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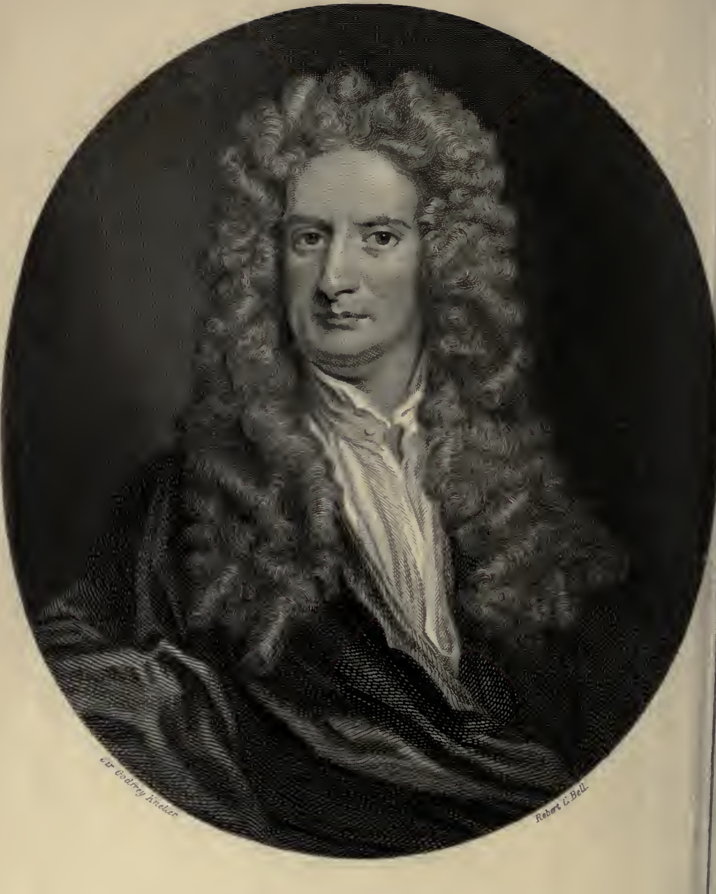

Isaac Newton.

A.R. McLeod
26 Oct 1921
Cambridge.

MEMOIRS

OF

THE LIFE, WRITINGS, AND DISCOVERIES

OF

SIR ISAAC NEWTON.

BY SIR DAVID BREWSTER, K.H.

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Second Edition.

VOL. I.

EDINBURGH:

EDMONSTON AND DOUGLAS.

MDCCCLX.

Ergo vivida vis animi pervicit, et extra
Processit longe flammantia mœnia mundi;
Atque omne immensum peragravit mente animoque.
LUCRETIUS, Lib. i. l. 73.

TO HIS ROYAL HIGHNESS

PRINCE ALBERT, K.G.

CHANCELLOR OF THE UNIVERSITY OF CAMBRIDGE.

Sir,

In dedicating this Work to your Royal Highness, I seek for it the protection of a name indissolubly associated with the Sciences and the Arts. An account of the Life, Writings, and Discoveries of Sir Isaac Newton might have been appropriately inscribed to the Chancellor of the University of Cambridge, the birthplace of Newton's genius, and the scene of his intellectual achievements; but that illustrious name is more honourably placed beside that of a Prince who has given such an impulse to the Arts and Sciences of England, and whose views, were they seconded by Statesmen willing to extend Education and advance Science, would raise our country to a higher rank than it now holds among the nations of Europe, in the arts of Peace and of War. It is from the trenches of Science alone that war can be successfully waged; and it is in its patronage and liberal endowment that nations will find their best and cheapest defence.

That your Royal Highness may be enabled to realize those noble and patriotic views respecting the national encouragement of Science, and the consolidation of our Scientific Institutions, which you have so much at heart, and that you may long live to enjoy the reputation which you have so justly earned, is the ardent wish of,

SIR,

Your Royal Highness's

Humble and obedient Servant,

DAVID BREWSTER.

ST. LEONARD'S COLLEGE,
ST. ANDREWS, *May* 12, 1855.

PREFACE.

In consequence of the wide circulation of the Life of Sir Isaac Newton, which I drew up for the "Family Library" in 1831, I was induced to undertake a larger work, in order to give a more detailed account of his Life, Writings, and Discoveries. For this purpose I applied in 1837 to the Honourable Newton Fellowes, one of the trustees of the Earl of Portsmouth, for permission to inspect the Manuscripts and Correspondence of Sir Isaac, which, through his grandniece, Miss Conduitt, afterwards Lady Lymington, had come into the possession of that noble family. Mr. Fellowes kindly granted my request, and his amiable and accomplished son, Mr. Henry Arthur Fellowes, who, had he lived, would now have been Earl of Portsmouth, met me in June 1837, at Hurtsbourne Park, to assist me in examining and making extracts from the large mass of papers which Sir Isaac had left behind him.

In this examination our attention was particularly directed to such letters and papers as were calculated to throw light upon his early and academical life, and, with the assistance of Mr. Fellowes, who copied for me several important documents, I was enabled to collect many valuable materials unknown to preceding biographers.

After the death of Sir Isaac, his nephew, Mr. Conduitt, drew up a memorial, containing a sketch of his life, for the use of Fontenelle, the Secretary to the Academy of Sciences in Paris, whose duty it was to write his Eloge, as one of the eight Associates of the Academy. This memorial was published by Edmond Turnor, Esq., in his "Collections for the History of the Town and Soke of Grantham," and was supposed to contain all the information that Mr. Conduitt could collect from persons then alive, and from other sources, respecting Sir Isaac's life. This, however, was a mistake. After the publication of Fontenelle's Eloge, Mr. Conduitt resolved to draw up a Life of his illustrious relative, and, with this view, he wrote the following letter, requesting the assistance of Sir Isaac's personal friends :[1]—

"*6th February* 172$\frac{7}{8}$.

"SIR,—I have taken the liberty to trouble you with some short hints of that part of our honoured friend, Sir I. Newton's life, which I must beg the favour of you to undertake, there being nobody, without dispute, so well qualified to do it as yourself. I send you, at the same time, Fontenelle's Eloge, wherein you will find a very imperfect attempt of the same kind ; but I fear he had neither abilities nor inclination to do justice to that great man, who had eclipsed the glory of their hero, Descartes. As Sir I. Newton was a national man, I think every one ought to contribute to a work intended to do him justice, particularly those who had so great a share in his esteem as you had ; and as I pretend to nothing more than to compile it, I shall acquaint the public in the Preface, to whom they are indebted for each particular part of it.

[1] This letter is docqueted by Conduitt, "Letter sent by me concerning Sir I. N.'s Inventions."

"I am persuaded that the hints I have sent you are very imperfect, and that your own genius will suggest to you many others much more proper and significant, and I beg of you to put down everything that occurs to your thoughts, and you think fit to be inserted in such a work.

"I conjure you not to put off what I take the liberty to recommend to you. As on one hand the complying with my request will be a mark of your gratitude to your old friend, and an eternal obligation on me, so your delaying it will be the most mortifying disappointment to,

"Sir,

"Your most humble Servant,

"JOHN CONDUITT."[1]

Although Mr. Conduitt had at this time resolved to compile a Life of Sir Isaac, and had obtained much information from Dr. Stukely, Mr. Wickins, and Dr. Humphrey Newton of Grantham, yet he seems to have so far relinquished his design, that in June 1729, nearly eighteen months after the date of his letter, he intimates to a friend[2] that "he has *some thoughts* of writing the Life of Sir Isaac Newton himself." That he made the attempt, appears from an indigested mass of manuscript which he has left behind him, and which does not lead

[1] I have not succeeded in ascertaining to whom this letter was addressed. It was probably a circular sent to more than one person. I have found a letter from John Craig, and a paper by De Moivre, which have the appearance of being answers to it, but the dates of both are earlier than that of Conduitt's letter. In a letter dated April 16, 1729, Conduitt made a similar application to Professor Machin.

[2] In a letter on the subject of a large "monumental picture to Newton's memory," for Conduitt himself. This letter is docqueted, "sent to Westgarth," who seems to have been then in Italy.

us to regret that he abandoned his design. The materials, however, which he obtained from Mrs. Conduitt, and from the friends of Newton then alive, are of great value ; and, in so far as Mr. H. A. Fellowes and I could make an abstract of these and other manuscripts during a week's visit at Hurtsbourne Park, I have availed myself of them in composing the first volume of this work, which was printed before the papers themselves came into my hands.

Before I began the second volume, which contains the history of the Fluxionary controversy, and the Life of Newton subsequent to the publication of the first edition of the *Principia*, I had the good fortune to obtain from the Earl of Portsmouth, through the kindness of Lord Brougham, the collection of manuscripts and correspondence which the late Mr. H. A. Fellowes had examined and arranged as peculiarly fitted to throw light on the Life and Discoveries of Sir Isaac. In these manuscripts I found much new information respecting the history of the *Principia*, which, though it might have been more appropriately placed in the first volume, I have introduced into those chapters of the second which relate to the period when the other editions of the *Principia* were published.

In the different controversies in which Newton's discoveries involved him, his moral character had never been the subject of suspicion. In Hooke, he found a jealous but an honest rival, who, though he claimed discoveries which substantially belonged to Newton, never cast a reproach upon his name ; and amid all the bitterness of the Fluxionary controversy, Leibnitz and Bernoulli, and their anonymous auxiliaries, never hesitated to acknowledge the purity of Newton's motives, and the scrupulous correctness of his conduct. It was reserved for two English astronomers, the one a contemporary and the other a disciple, to misrepresent and calumniate their illustrious countryman.

In 1835, the scientific world was startled by the publication of Baily's Life of Flamsteed, a huge volume, deeply affecting the character of Newton, and, strange to say, printed and circulated throughout the world, at the expense of the Board of Admiralty. The friends of the great philosopher were thus summoned to a painful controversy, which, had it been raised in his lifetime, would have been summarily extinguished ; but a century and a quarter had elapsed before the slumbering calumnies revived, and it was hardly to be expected that the means of defence would have enjoyed the same vitality. Under these circumstances Mr. Fellowes and I anxiously searched, but in vain, for the letters of Flamsteed to Newton, and other relative documents which were necessary for his defence. In this difficulty, some of the admirers of Newton, among whom I must mention my friend Mr. Robert Brown, the distinguished President of the Linnean Society, sent me some important facts ; but valuable as they were, they were not sufficient to refute the calumnies of the Astronomer-Royal. From this embarrassment, however, I have been relieved by the receipt of all Flamsteed's letters and other important papers which Newton had carefully preserved, and which Mr. Fellowes had discovered and set aside for my use. With these documents I trust I have been able, though at a greater length than I could have wished, to defend the illustrious subject of this work against a system of calumny and misrepresentation unexampled in the history of science.

When I published my *Life of Newton* in 1831, I had not seen his correspondence with Mr. Cotes and other mathematicians in the Library of Trinity College. Mr. Halliwell, however, who had made copious extracts from these manuscripts, kindly put them into my hands; but the subsequent publication of the correspondence by Mr. Edleston, has enabled me to make a more advantageous use of these valuable materials.

Dr. Monk, Bishop of Gloucester and Bristol, had "often expressed in private a wish and request that some one of the many accomplished Newtonians who are resident in that society would favour the world by publishing the whole collection,"[1] and I have no doubt that it was from this public expression of it, in his able and interesting *Life of Dr. Bentley*, that the Masters and Seniors of Trinity College resolved to publish the correspondence.

This valuable work, edited by Mr. Edleston, Fellow of Trinity, is a most important contribution to the History of Mathematical and Physical Science. The admirable synopsis which it contains of Newton's Life;—the learned and able annotations illustrative of his history; and the explanatory notes on the letters themselves, throw much light on the subjects to which they refer, and have been of essential service to me in the composition of this work. But in addition to the obligations which I owe to Mr. Edleston, in common with every friend of science, I have to acknowledge others of a more personal kind. During the printing of the second volume, which he has had the kindness to peruse, I have received from him much new and important information, and availed myself of his judicious criticisms and useful suggestions.

To Professor De Morgan, to whom the public owes a brief

[1] *Life of Bentley*, p. 180.

but interesting biographical sketch of Newton, and who has carefully investigated various points in the Fluxionary controversy, I have been indebted for much information, and for his kind revision of the sketch I had given of the early history of the Infinitesimal Calculus. On a few questions in the life of Newton, and the history of his discoveries, my opinion differs somewhat from his ; but I have been able to confirm, from the documents in my possession, many of his views on important points which he was the first to investigate and to publish.

From my late amiable and distinguished friend Professor Rigaud of Oxford, too early cut off in his scientific career, I obtained valuable aid whenever I encountered difficulties or required information. His "Historical Essay on the Principia," which he generously offered to withhold from the public, till I had finished the present work, is a most important contribution to the history of Newton's discoveries, and I am glad to be able to complete the correspondence between Newton and Halley, which Mr. Rigaud was the first to publish in its genuine state.

The Rev. Jeffrey Ekins, Rector of Sampford, whose family, from their connexion with Newton, have been long in possession of several of his theological manuscripts and letters, has obligingly sent me copies of many of them, and has otherwise favoured me with much useful information.

To Lord Brougham, Sir John Lubbock, Mr. Cutts Barton, and other friends, I have to return my best thanks for the assistance they have given me.

In concluding this Preface, I can hardly avoid referring to Sir Isaac Newton's religious opinions. In the chapter which relates to them I have touched lightly, and unwillingly, on a subject so tender; and in publishing the most interesting of the manuscripts in which these opinions are recorded, I have done little more than submit them to the judgment of the reader. Though adverse to my own, and I believe to the opinions of those to whom his memory is dearest, I did not feel myself justified, had I been so disposed, to conceal from the public that which they have long suspected, and must have sooner or later known. What the gifted mind of Newton believed to be truth, I dare not pronounce to be error. By the great Teacher alone can truth be taught, and it is only at His tribunal that a decision will be given on those questions, often of words, which have kept at variance the wisest and the best of men.

St. Leonard's College,
St. Andrews, *May* 12, 1855.

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VOL. I.

VOL. II.

MEMOIRS

OF THE

LIFE AND WRITINGS OF SIR ISAAC NEWTON.

CHAPTER I.

Great discoveries previous to the birth of Sir Isaac Newton—Pre-eminence of his reputation—The interest attached to the study of his life and writings—His birth and parentage—An only and posthumous child—Notice of his descent—Inherits the small property of Woolsthorpe—His mother marries again—Is sent to a day-school—His education at Grantham School—His idle habits there—His love of mechanical pursuits—His windmill, water-clock, self-moving carriage, and kites—His attachment to Miss Storey—His love of drawing and poetry—His unfitness to be a farmer—His dials, water-wheels, and anemometer—Leaves Grantham School—His commonplace book and college expenses.

The seventeenth century has always been regarded as the most interesting and eventful period in the history of positive knowledge. The discoveries and speculations of a preceding age had prepared the way for some grand generalization of the phenomena of the material world; and sages of lofty intellect heralded the advent of that Master-mind by which it was to be accomplished. The establishment by Copernicus of the true Solar System, and of its independence of the sidereal universe, led to the investigation of those general laws with which Kepler laid the foundations of Physical Astronomy; while, in combination with these, the observations of Tycho, the telescopic discoveries of Galileo, and the speculations of Hooke and Borelli, contributed in no slight degree to the establishment of the theory of universal gravitation, by which Sir Isaac Newton has immortalized his name, and perpetuated the intellectual glory of his country.

A generalization of such vast extent, enabling us to determine the position and aspects of the planets during thousands of years that are past, and for thousands of years to come, could not but be regarded as an achievement of the highest order : and the name of Newton, therefore, has, by universal consent, been placed at the head of those great men who have been the benefactors and ornaments of their species. Imposing as are the attributes with which Time has invested the sages of antiquity—its poets and its philosophers ; and dazzling as are the glories of its heroes and its lawgivers, their reputation pales in the presence of his ; and the vanity of no presumptuous school, and the partiality of no rival nation, has ventured to question the ascendency of his genius. The philosopher, indeed, to whom posterity will probably assign the place next to Newton, has characterized his great work,—*The Principles of Natural Philosophy*, as pre-eminent above every other production of human genius,[1] and has thus divested of extravagance the encomium of contemporary friendship.

> Nec fas est propius mortali attingere Divos.
>
> HALLEY.
>
> So near the gods—man cannot nearer go.

But while the history of such discoveries must, to the intellectual world, be a subject of exciting interest, the biography of him who made them,—the details of his life, his studies and his opinions, cannot fail to arrest the attention and influence the judgment of every cultivated mind. Though the path of such a man may have lain in the secluded vale of humble life, unmarked by those dramatic incidents which throw a lustre even round perishable names, yet the inquiring spirit will linger over the history of a mind so richly endowed, will study its intellectual and moral phases, and will seek the shelter of its

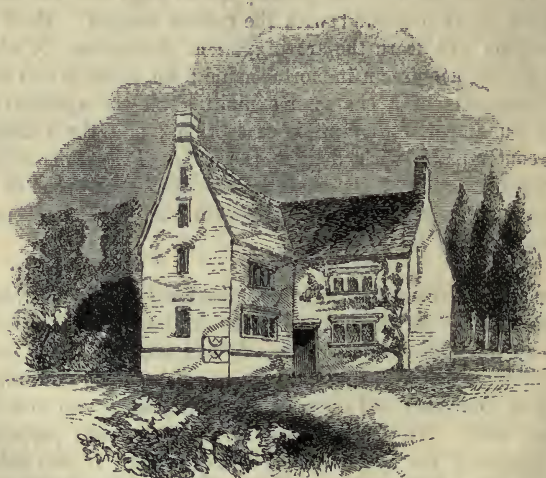
[1] The Marquis La Place.—See his *Exposition du Système du Monde*, Livre cinquième, chap. vi. p. 336.

authority on those solemn questions which Reason has abandoned to Faith and Hope.

If we look for instruction from the opinions of ordinary men, and watch their conduct as an exemplar for our own, how interesting must it be to follow the most exalted genius through the labyrinth of common life,—to mark the steps by which he attained his lofty pre-eminence,—to see how he performs the functions of the social and the domestic compact;—how he wields his powers of invention and discovery;—how he comports himself in the arena of intellectual strife; and in what sentiments, and with what aspirations, he leaves the world which he has adorned.

In each and all of these phases, the writings and the life of Sir Isaac Newton abound with the richest counsel. Here the philosopher will learn the art of patient observation by which alone he can acquire an immortal name; the moralist will trace the lineaments of a character exhibiting all the symmetry of which our imperfect nature is susceptible; and the Christian will contemplate with delight the High Priest of Science quitting the study of the material universe—the scene of his intellectual triumphs, to investigate with humility and reverence the mysteries of his faith.

Isaac Newton was born in the Manor-house of Woolsthorpe, a hamlet in the parish of Colsterworth, in the county of Lincoln, close to the village of Colsterworth, and about six miles south of Grantham, between one and two o'clock in the morning of the 25th December, old style, 1642, in the same year in which Galileo died. His father, Isaac Newton, who was proprietor and farmer of the manor of Woolsthorpe, died in the thirty-seventh year of his age, a little more than a year after the death of his father Robert Newton, and only a few months after his marriage to Hannah Ayscough, daughter of James Ayscough of Market Overton, in Rutlandshire. Mrs. Newton had thus been left in a state of pregnancy, and appears to have given a premature birth to her only and posthumous child. The infant thus ushered into the world was of such a diminutive size, that, as his mother afterwards expressed it to Newton himself, he might have been put into a quart-mug, and so feeble apparently was his constitution, that two women who were sent to Lady Pakenham's at North Witham, to obtain for him some tonic medicine, did not expect to find him alive on their return. Providence, however, disappointed their fears, and that frail tenement which seemed scarcely able to imprison its immortal mind, was destined to enjoy a vigorous maturity, and to survive even the average term of human existence.



Manor-house, Woolsthorpe; the Birthplace of Sir Isaac Newton, showing the Solar Dials which he made when a boy.

The small Manor of Woolsthorpe is said to have been more than a hundred years in the possession of the family, who, according to one account, were descended from Sir John Newton of Westby, in Lincolnshire, and, according to another, from a Scotch family in East Lothian. The Manor-house is situated in a pleasant little hollow on the west side of the valley of the river Witham, which rises near it, and one spring of which is in the Manor. From the house there is an agreeable prospect of the village of Colsterworth to the east, and, according to Dr. Stukely, the air is so good, combining the sharpness of the midland part of the kingdom with the more genial temperature of the low parts of Lincolnshire, that the country round Woolsthorpe was called the Montpellier of England. The Manor-house consists of two storeys, and is built of stone. Sir Isaac's study before he went to college, and when he visited his mother from the University, was in the upper flat. The book-shelves are described by Dr. Stukely as having been made by Sir Isaac himself with pieces of deal-boxes, and as having contained 200 or 300 books belonging to his father-in-law, Dr. Smith, which Sir Isaac presented to Dr. Newton of Grantham.

The Manor of Woolsthorpe, Sir Isaac's paternal estate, purchased by his grandfather in 1623, from Robert Underwood, was worth only £30 per annum, but his mother possessed a small estate at Sewstern, on the borders of Leicestershire, and about three miles south-east of Woolsthorpe, which was worth about £50 per annum; and it is probable that the cultivation of the little farm, on which she resided, added to the limited rental upon which she had to support herself and educate her son.

Under the guardianship of his uncle, James Ayscough, and the tender care of his mother, young Newton remained at Woolsthorpe acquiring gradually that strength of constitution which was essential to the development of his intellectual powers. Before, however, he had reached his fourth year, he was deprived of his mother's care, in consequence of her marriage, on the 27th January 1645, to the Rev. Barnabas Smith, rector of North Witham;[1] and her duties devolved upon her mother, the wife of James Ayscough, and a daughter of Mr. Blythe of Stroxton, who, for this purpose, took up her residence at Woolsthorpe. At the usual age Isaac was sent to two little day-schools at Skillington and Stoke, two hamlets about a mile to the north of Woolsthorpe, and about the same distance from each other, acquiring the education in reading, writing, and arithmetic, which such seminaries afforded.

When he reached the age of twelve he was sent to the public school at Grantham, then taught by Mr. Stokes, who had the character of being a good teacher, and was boarded at the house of Mr. Clark, an apothecary in the town, whose grandson, Mr. Clark, exercised the same profession there in 1727, the year of Newton's death. The house in which our young philosopher lodged, was next to the George Inn, "northward in the High Street, which was rebuilt about 1711." According to the confessions which Sir Isaac himself made to Mr. Conduit, he was extremely inattentive to his studies, and stood very low in the school. When he was the last in the lowermost form but one, the boy next above him, as they were going to school, gave him a kick on the stomach, which occasioned a great degree of pain. As soon as the scholars were dismissed, New-

[1] The issue of this marriage was a son and two daughters—Benjamin, Mary, and Hannah Smith, from whom were descended the four nephews and nieces who inherited Sir Isaac's personal estate.

The following account, from Conduit's MSS., of Mrs. Newton's marriage to Mr. Smith, was given to Mr. Conduit "by Mrs. Hutton, whose maiden name was Ayscough:—

"Mr. Smith, a neighbouring clergyman, who had a very good estate, had lived a bachelor till he was pretty old, and one of his parishioners advising him to marry, he said he did not know where to meet with a good wife. The man answered, The widow Newton is an extraordinary good woman. But, saith Mr. Smith, how do I know she will have me, and I don't care to ask and be denied; but if you will go and ask her, I will pay you for your day's work. He went accordingly. Her answer was, she would be advised by her brother Ayscough. Upon which Mr. Smith sent the same person to Mr. Ayscough on the same errand, who, upon consulting with his sister, treated with Mr. Smith, who gave her son Isaac a parcel of land, being one of the terms insisted upon by the widow if she married him." This parcel of land was given by Mrs. Smith, and was probably her property of Sewstern.—See the *Annual Register* 1776, Characters, p. 25.

ton challenged the boy to fight, and for this purpose they went into the churchyard. The schoolmaster's son came up to them during the fight, and, "clapping one on the back and winking to the other," encouraged them both to continue the encounter. Though Sir Isaac was not so robust as his antagonist, yet he had much more spirit and resolution, and therefore succeeded in the combat, beating his opponent till he declared he would fight no more. The schoolmaster's son, who seems to have been an amateur in the art, told Sir Isaac that he must treat the other as a coward by rubbing his nose against the wall. The victor accordingly took the advice, and dragging his victim by the ears, thrust his face against the wall of the church. The success which thus attended his first struggle for superiority induced him to repeat it in a better cause. Although vanquished in the churchyard, his antagonist still stood above him in the school, a victory more honourable than that which Newton had achieved; and though the schoolmaster and his son would have given a different decision on the relative merits of the youthful combatants, yet Newton took the right view of his own position, and resolved to possess the moral as well as the physical superiority. He accordingly exerted himself in the preparation of his lessons, and, after many a severe struggle in which he and his adversary were alternately successful, he not only gained the individual victory, but rose to the highest place in the school.

It is very probable that Newton's idleness arose from the occupation of his mind with subjects in which he felt a deeper interest. He had not been long at school before he exhibited a taste for mechanical inventions. With the aid of little saws, hammers, hatchets, and tools of all sorts, he was constantly occupied during his play-hours in the construction of models of known machines, and amusing contrivances. The most important pieces of mechanism which he thus constructed, were a wind-mill, a water-clock, and a carriage to be moved by the person who sat in it. When a wind-mill was in the course of being erected near Grantham, on the way to Gunnerby, Sir Isaac frequently watched the operations of the workmen, and acquired such a thorough knowledge of its mechanism, that he completed a working model of it, which Dr. Stukely says was "as clean and curious a piece of workmanship as the original." This model was frequently placed upon the top of the house in which he lived at Grantham, and was put in motion by the action of the wind upon its sails. In calm weather, however, another mechanical agent was required, and for this purpose a mouse was put in requisition, which went by the name of the miller. It does not distinctly appear how the mouse was compelled to perform a function so foreign to its ordinary habits, but it was supposed to act upon something like a tread-wheel when attempting to reach some corn placed above it; or, according to another supposition, it was placed within a wheel, and by pulling a string tied to its tail, it went forward "by way of resistance," as Dr. Stukely observes, and thus turned the mill.

The water-clock constructed by Sir Isaac was a more useful piece of mechanism than his wind-mill. It was made out of a box which he begged from Mrs. Clark's brother, and, according to Dr. Stukely, to whom it was described by those who had seen it, "it resembled pretty much our common clocks and clock-cases," but was less in size, being about four feet in height, and of a proportional breadth. There was a dial-plate at top with figures of the hours. The index was turned by a piece of wood, which "either fell or rose by water dropping." The clock stood in Sir Isaac's bedroom, and it was his daily practice to supply it every morning with the proper quantity of water. It was frequently resorted to by the inmates of Mr. Clark's house to ascertain the hour of the day, and it remained there long after Sir Isaac went to Cambridge. Dr. Stukely informs us, that having had occasion to talk of clepsydræ, or water-clocks, Newton remarked that their chief inconvenience arose from the furring up of the small hole through which the water passed, by the impurities which it contained,—a cause of inequality in its measure of time, the reverse of what takes place in clocks made with sand, which enlarges the hole through which it descends.

The mechanical carriage which Sir Isaac is said to have invented, was a four-wheeled vehicle, and was moved with a handle or winch wrought by the person who sat in it. We can find no distinct information respecting its construction or use, but it must have resembled a Merlin's chair, which is fitted only to move on the smooth surface of a floor, and not to overcome the inequalities of a common road.[1]

Although Sir Isaac was at this time a "sober, silent, and thinking lad," who never took part in the games and amusements of his school-fellows, but employed all his leisure hours in "knocking and hammering in his lodging-room," yet he was anxious to please them by "inventing diversions for them above the vulgar kind." In this way he often succeeded in alluring them from trifling amusements, and teaching them, as Dr. Stukely says, "to play philosophically;" or, as Dr. Paris has better expressed it, in the title of his charming little work, to make "philosophy in sport science in earnest." With this view he introduced the flying of paper kites, and he is said to have investigated their best forms and proportions, as well as the number and position of the points to which the string

[1] It is a curious fact that Leibnitz, the rival of Newton, had laboured at similar inventions. In a letter written to Sir Isaac from Hanover, about a month after Leibnitz's death, on the 14th November 1716, the Abbé Conti informs him that Leibnitz had laboured all his life to invent machines, which had never succeeded, and that he was particularly desirous of constructing a wind-mill for mines, and a carriage to be moved without horses. Fontenelle, in his Eloge on Leibnitz, mentions these two inventions in different terms. He had bestowed, says he, much time and labour upon his wind-mill for draining the water from the deepest mines, but was thwarted in its execution by certain workmen who had opposite interests. In the matter of carriages, his object was merely to render them lighter and more commodious; but a doctor, who believed that Leibnitz had prevented him from getting a pension from the King of Hanover, stated in some printed work, that he had contemplated the invention of a carriage which would perform the journey from Hanover to Amsterdam in twenty-four hours.—*Mém. Acad. Par.* 1718. Hist. p. 115.

should be attached. He constructed also lanterns of "crimpled paper," in which he placed a candle to light him to school in the dark winter mornings; and in dark nights he tied them to the tails of his kites, in order to terrify the country people, who took them for comets.

Hitherto the attention of Sir Isaac had not been directed to any of the celestial phenomena, and when he did study the apparent daily motion of the sun, he was probably led to it by the imperfect measure of time which he obtained from his water-clocks. In the yard of the house where he lived, he was frequently observed to watch the motion of the sun. He drove wooden pegs into the walls and roofs of the buildings, as gnomons to mark by their shadows the hours and half-hours of the day. It does not appear that he knew how to adjust these lines to the latitude of Grantham; but he is said to have succeeded, after some years' observation, in making them so exact, that anybody could tell what o'clock it was by *Isaac's Dial*, as it was called. It was probably at the same time that he carved two dials on the walls of his own house at Woolsthorpe; but, though we have seen them there, we were not able to determine whether they were executed by a tentative process like those in Mr. Clark's yard, or were more accurately projected, from a knowledge of the doctrine of the sphere.[1]

But saws and hammers were not the only tools which our young philosopher employed. He was expert also with his pencil and his pen, drawing with the one and inditing verses with the other. It is not improbable that he received some in-

[1] One of these dials was taken down in 1844, along with the stone on which it was cut, by Mr. Turnor of Stoke Rochford, and presented by his uncle, the Rev. Charles Turnor, to the Museum of the Royal Society. The dial was traced on a large stone in the south wall, at the angle of the building, and about six feet from the ground. The name NEWTON, with the exception of the first two letters, which have been obliterated, may be seen under the dial in rude and capital letters. The other dial is smaller than this, but not in good preservation. The gnomons of these dials have unfortunately disappeared. In the woodcut representing the manor-house of Woolsthorpe, the birthplace of Sir Isaac, are shown the places on the wall where the dials were traced.—See *Phil. Trans.* 1845, pp. 141, 142.

struction in drawing from his writing-master, called "Old Barley," who lived in the place occupied, in Dr. Stukely's time, by "the Millstone Alehouse in Castle Street." But whether he was instructed or self-taught, he seems to have made some progress in the art. His room was furnished with pictures drawn by himself, some of them being copied from prints, and some from life. The frames of these pictures were made by himself, and "coloured over in a workmanlike manner." Among these portraits Dr. Stukely enumerates "several of the King's heads, Dr. Donne, Mr. Stokes, his teacher at Grantham, and King Charles I." In addition to these portraits, there were well-designed drawings of "birds, beasts, men, ships, and mathematical diagrams, executed with charcoal on the wall, which remained till the house was pulled down in 1711."

Although Sir Isaac told Mr. Conduit that he "excelled particularly in making verses," yet it is strange that no authentic specimen of his poetry has been preserved. Beneath his portrait of Charles I. the follow verses were written :—

A secret art my soul requires to try,
If prayers can give me what the wars deny.
Three crowns distinguished here, in order do
Present their objects to my knowing view.
Earth's crown thus at my feet I can disdain,
Which heavy is, and at the best but vain.
But now a crown of thorns I gladly greet,—
Sharp is this crown, but not so sharp as sweet;
The crown of glory that I yonder see,
Is full of bliss and of eternity.

Mrs. Vincent, who repeated these lines to Dr. Stukely from memory, fancied that they were written by Sir Isaac; but even if he had thus early tasted of the Pierian spring, he must have lost his relish for its sparkling waters, as he often expressed in his later years a dislike for poetry ;—"not unlike Plato," as Conduit observes when mentioning this fact, "who, though he had addicted himself to poetry in his younger days, would not, in his serious years, allow even Homer a place in his commonwealth."

During the seven years which Sir Isaac spent at Grantham, there were some female inmates in Mr. Clark's house, in whose society he took much pleasure, and spent much of his leisure time. One of these, Miss Storey, sister to Dr. Storey, a physician at Buckminster, near Colsterworth, and the daughter of Mr. Clark's second wife, was two or three years younger than Newton, and seems to have added to great personal attractions more than the usual allotment of female talent. To the society of his school-fellows he preferred that of the young ladies at home, and he often made little tables, cupboards, and other utensils for Miss Storey and her playfellows, to set their dolls and their trinkets upon. Miss Storey, who after her second marriage bore the name of Mrs. Vincent, confessed to Dr. Stukely, when he visited her at Grantham in 1727, when she had reached the age of 82, that Newton had been in love with her, but that the smallness of her portion and the inadequacy of his own income, when the fellow of a college, prevented their marriage. Newton's esteem for her continued unabated during his life. He paid her a regular visit whenever he went to Woolsthorpe, and he liberally relieved her from little pecuniary embarrassments which seem to have occasionally beset her family.

At the death of the Rev. Mr. Smith in 1656, his widow, Sir Isaac's mother, left the rectory of North Witham, and, accompanied with her three children, Mary, Benjamin, and Hannah Smith, took up her residence at Woolsthorpe, which Mr. Smith had rebuilt. At this time Newton had reached his fifteenth year, and had acquired all the learning which a provincial school could supply. It does not appear that he had thought of following any particular profession, and it is probable that his mother intended to bring him up as a farmer and grazier,[1] and, like his ancestors, to take charge of her little property. He was therefore recalled from the school at Grantham, and

[1] Mrs. Hutton mentioned to Mr. Conduit that this was the profession to which Newton was to be brought up.

entered upon the new and not very welcome duties of tilling the ground and disposing of its produce. He was thus frequently sent to Grantham on Saturday, the market-day, in order to dispose of grain and other kinds of agricultural produce, and purchase articles of a domestic nature which the family required. On these occasions he was accompanied by an old and trustworthy man-servant, till he acquired sufficient experience to do business by himself. The inn which they frequented was the Saracen's Head in Westgate, but no sooner had they put up their horses than Isaac deserted his commercial duties, intrusted his marketings to the management of his rural Mentor, and went in search of knowledge to his former haunt in Mr. Clark's garret, where a parcel of old books afforded an interesting occupation of his time till the hour arrived when it was necessary to return. When the luxuries in the garret had lost their novelty, our young philosopher thought it a waste of time to go so far as Grantham and do nothing; he deserted his duties, therefore, at an earlier stage, and entrenched himself under a hedge on the wayside between Woolsthorpe and Grantham, devouring some favourite author till his companion roused him on his return. With such tastes and habits it was not to be expected that the more urgent affairs of the farm would prosper under his management. When his mother ordered him into the fields to look after the sheep, or to watch the cattle when they were treading down the crops, he was equally negligent of the obligations which were imposed upon him. The sheep went astray, and the cattle enjoyed themselves among the growing corn, while he was perched under a tree with a book in his hands, or shaping wooden models with his knife, or luxuriating over the movements of an undershot water-wheel whirling the glittering spray from its float-boards, or arresting the passing traveller by its aqueous pulsations.

It was about this time, also, that he seems to have paid some attention to the subject of the resistance of fluids, to which his experiments with water-wheels would naturally lead him. Mr. Conduit,[1] apparently on the authority of Mrs. Vincent, informs us that even when he was occupied with his paper kites, he was endeavouring to find out the proper form of a body which would experience the least resistance when moving in a fluid. Sir Isaac himself told Mr. Conduit that one of the earliest scientific experiments which he made was in 1658, on the day of the great storm when Cromwell died, and when he himself had just entered his sixteenth year. In order to determine the force of the gale, he jumped first in the direction in which the wind blew, and then in opposition to the wind; and after measuring the length of the leap in both directions, and comparing it with the length to which he could jump in a perfectly calm day, he was enabled to compute the force of the storm. Sir Isaac added, that when his companions seemed surprised at his saying that any particular wind was a foot stronger than any he had known before, he carried them to the place where he had made the experiment, and showed them the measure and marks of his several leaps. This mode of jumping to a conclusion, or reaching it *per saltum*, was not the one which our philosopher afterward used. Had he, like Coulomb, employed a shred of paper instead of his own person, and observed the time that it took to fly through a given distance, he would have obtained a better substitute for an anemometer.

Such were the occupations of Newton when his mother intrusted to him the management of her farm. Experience soon convinced her that he was not destined to be a cultivator of the soil; and as his love of study and dislike of every other occupation increased with his years, she resolved to give him all the advantages which education could bestow. He was accordingly sent back to the school at Grantham, where he remained for nine months in active preparation for his academical studies. His uncle, the Rev. W. Ayscough, who was rector of Burton Coggles, about three miles east of Woolsthorpe,

[1] MSS. of Conduit among the family papers.

having one day discovered Newton under a hedge, occupied in the solution of a mathematical problem, confirmed Mrs. Smith in the resolution which she had taken ; and as he had himself studied in Trinity College, it was arranged that Newton should follow his example, and proceed to Cambridge at the approaching term.

We have not been able to discover the exact year in which Newton was sent back to school, or the nature of the studies by which he was to be prepared for the University. It is stated by Conduit that he went to Cambridge in 1660 ; but the records of the University place it beyond a doubt that he was not admitted there till 1661, so that he had a year more than has been supposed to fit him for college. This period of preparation must have extended from 1658 to 1661, from the 16th to the 19th year of his age, and we accordingly find in one of his memorandum books, a small volume of about $3\frac{1}{2}$ inches square, and dated March 19, 1659,[1] that he was en-

[1] Mr. Conduit, in his MS. notes, mentions *two* of these memorandum books in the following manner :—" I find in a paper book of his to which he has put his name, and dated 1659,—Rules for drawing and making colours ;" and in another of the same year, "*Prosodia* written out." The *first* of these books I did not find among the family papers ; but the *second* is the one referred to in the text. The following is its title :—

Quisquis in hunc librum
Teneros conjecit ocellos,
Nomen subscriptum perle-
gat ipse nomen.
Isaac Newton,
Martii 19, 1659.

On the second page is the title *Utilissimum Prosodiæ Supplementum*, which terminates on the 33d page with the date March 26, and is followed by an Appendix of three pages.

At the end of the book there is a list of his expenses, entitled *Impensa propria*, occupying fourteen pages. On the 4th page the expenses are summed up thus:—

Totum, ...	£3	5	6
Habui, ...	4	0	0
Habeo, ...	0	14	6

On the 5th page there are fourteen loans of money extended thus:

Lent Agatha, . .	£0	11	1
Lent Gooch, . .	1	0	0

and he then adds at the bottom of the page, Lent out 13 shillings more than £4.

gaged in the study of prosody. This little volume contains various entries of his expenses during the first year that he was at college, but nothing, excepting the purchase of a dial, to indicate that he was engaged in physical or mathematical studies.

The day in which he quitted Grantham was one of much interest not only to himself but to his school-fellows and his venerable teacher. Mr. Conduit[1] has recorded it as a tradition in Grantham, that on that day the good old man, with the pride of a father, placed his favourite pupil in the most conspicuous part of the school, and having, with tears in his eyes, made a speech in praise of his character and talents, held him up to the scholars as a proper object of their love and imitation. We have not heard that the schoolmaster of Grantham lived long enough to feel a just pride in the transcendent reputation of his pupil; but many of the youth to whom his affectionate counsel was addressed, may have had frequent opportunities of glorying in having been the school-fellows of Sir Isaac Newton.

Among the entries are	Chessmen and dial, .	£0	1	4
	Effigies amoris, . .	0	1	0
	Do.	0	0	10

and on the last page are entered seven loans, amounting to £3, 2s. 6d. There is likewise an entry of "Income from a glasse and other things to my chamber-fellow, £0 0 9." Another page is entitled

OTIOSE ET FRUSTRA EXPENSA.

Supersedeas.	Sherbet and reaskes.
China ale.	Beere.
Cherries.	Cake.
Tart.	Bread.
Bottled beere.	Milk.
Marmelot.	Butter.
Custards.	Cheese.

[1] MSS. of Conduit among the family papers.

CHAPTER II.

Newton enters Trinity College, Cambridge—Origin of his Love of Mathematics—Studies Descartes' Geometry, and the Writings of Schooten and Wallis—Is driven from Cambridge by the Plague—Observes Lunar Halos in 1664—Takes his degree of B.A. in 1665—Discovers Fluxions in the same year—His First speculations on Gravity—Purchases a Prism to study Colours—Revises Barrow's Optical Lectures—But does not correct his erroneous Opinions about Colours—Is elected a Minor Fellow of Trinity in 1667—and a Major Fellow in 1668—Takes his degree of M.A.—His Note-book, with his expenses from 1666 to 1669—Makes a small Reflecting Telescope—His Letter of advice to Francis Aston, when going upon his Travels—His Chemical Studies—His Taste for Alchemy—His Paper on Fluxions sent to Barrow and Collins in 1669.

To a young mind thirsting for knowledge, and ambitious of the distinction which it brings, the transition from a provincial school to a university like that of Cambridge,—from intellectual solitude to the society of men imbued with all the literature and science of the age, must be an event of the deepest interest. To Newton it was a source of peculiar excitement. The history of science affords many examples where the young aspirant had been early initiated into her mysteries, and had even exercised his powers of invention and discovery before he was admitted within the walls of a college; but he who was to give Philosophy her laws did not exhibit such early talent. No friendly counsel regulated his youthful studies, and no work of a scientific character guided him in his course. In yielding to the impulse of his mechanical genius, his mind obeyed the laws of its own natural expansion, and following in the line of least resistance, it was thus drawn aside from the precipitous path which it was fitted to climb, and the unbarred strongholds which it was destined to explore.

When Newton, therefore, entered Trinity College, he brought with him a more slender portion of science than at his age falls to the lot of ordinary scholars ; but this state of his acquirements was perhaps not unfavourable to the development of his powers. Unexhausted by premature growth, and invigorated by healthful repose, his mind was the better fitted to make those vigorous and rapid shoots which soon covered with foliage and with fruit the genial soil to which it had been transferred. Cambridge was consequently the real birthplace of Newton's genius. Her teachers fostered his earliest studies,—her institutions sustained his mightiest efforts,—and within her precincts were all his discoveries made and perfected. When he was called to higher official functions, his disciples kept up the pre-eminence of their master's philosophy, and their successors have maintained this seat of learning in the fulness of its glory, and rendered it the most distinguished among the universities of Europe.

With letters of introduction from his uncle, the Rev. James Ayscough, to his friends in Cambridge, Sir Isaac left Woolsthorpe in June 1661, and was admitted Subsizar at Trinity College on the 5th of that month, and matriculated Sizar[1] on the 8th of July. Neither history nor tradition has handed down to us any distinct account of the studies which Newton pursued at Cambridge during the first three or four years of his residence in that University. In Conduit's Memoirs of Newton,

[1] "This class of students," says Mr. Edleston, "were required to perform various menial services, which now seem to be considered degrading to a young man who is endeavouring, by the force of his intellect, to raise himself to his proper position in society. The following extract from the *Conclusion Book* of Trinity College, while it affords an example of one of their duties, will also serve to illustrate the rampant buoyancy of the academic youth at the time of the Restoration."

"Jan. 1660-1. Ordered also that no Bachelor, of what condition soever, nor any Undergraduate, come into the upper butteries, save only a Sizar that is sent to see his tutor's quantum, and then to stay no longer than is requisite for that purpose, under penalty of 6d. for every time ; but if any shall leap over the hatch, or strike a butler or his servant upon this account of being hindered to come into the butteries, he shall undergo the censure of the Masters and Seniors."—Edleston's *Correspondence of Sir Isaac Newton and Professor Cotes*, Lond. 1850, p. xli.

transmitted to Fontenelle,[1] we find very little information on this point, and even that little is by no means correct. Before Newton left Woolsthorpe, his uncle had given him a copy of Sanderson's Logic, which he seems to have studied so thoroughly, that when he afterwards attended the lectures on that work, he found that he knew more of it than his tutor. Finding him so far advanced, his tutor intimated to him that he was about to read Kepler's Optics to some Gentlemen Commoners, and that he might attend the Readings if he pleased. Newton immediately studied the book at home, and when his tutor gave him notice that his Lectures upon it were to commence, he was was surprised to learn that it had been already mastered by his pupil.

About the same time probably he bought a book on Judicial Astrology at Stourbridge fair,[2] and in the course of perusing it he came to a figure of the Heavens, which he could not understand without a previous knowledge of trigonometry. He therefore purchased an English Euclid, with an index of all the problems at the end of it, and having turned to two or three which he thought likely to remove his difficulties, he found the truths which they enunciated so self-evident that he expressed his astonishment that any person should have taken the trouble of writing a demonstration of them. He therefore threw aside Euclid "as a trifling book," and set himself to the study of Descartes' Geometry,[3] where problems not so simple seem to have baffled his ingenuity. Even after reading a few pages, he got beyond his depth, and laid aside the work; and he is said to have resumed it again and again, alternately retreating and advancing till he was master of the whole,

[1] Collections for the History of the Town and Soke of Grantham, &c. By EDMUND TURNOR, F.R.S., F.S.A. Lond. 1806, pp. 159, 160. Conduit's MSS. were written subsequently to the Memoirs above referred to.

[2] Demoivre says that the book on Astrology was bought at Stourbridge, the seat of the Cambridge fair, close to the town.

[3] Newton's copy of Descartes' Geometry I have seen among the family papers. It is marked in many places with his own hand, *Error, Error, non est Geom.*

without having received any assistance.[1] The neglect which he had shown of the elementary truths of geometry he afterwards regarded as a mistake in his mathematical studies; and on a future occasion he expressed to Dr. Pemberton his regret that "he had applied himself to the works of Descartes, and other algebraic writers, before he had considered the Elements of Euclid with that attention which so excellent a writer deserved." [2]

The study of Descartes' Geometry seems to have inspired Newton with a love of the subject, and to have introduced him to the higher mathematics. In a small commonplace book, bearing on the 7th page the date of Jan. 1663-4, there are several articles on angular sections, and the squaring of curves and "crooked lines that may be squared," several calculations about musical notes;—geometrical propositions from Francis Vieta and Schooten;—annotations out of Wallis's Arithmetic of Infinites, together with observations on Refraction,—on the grinding of "spherical optic glasses,"—on the errors of lenses, and the method of rectifying them, and on the extraction of all kinds of roots, particularly those "in affected powers." [3]

This commonplace book is particularly interesting from its containing the following important entry by Newton himself, after the lapse of thirty-five years, and when he had completed all his discoveries.

"*July* 4, 1699.—By consulting an account of my expenses at Cambridge,[4] in the years 1663 and 1664, I find that in the year 1664, a little before Christmas, I, being then Senior

[1] This statement is different from that of Conduit in his *Memoirs*, but I give it on his own authority, as founded on later inquiries.

[2] Pemberton's *View of Sir Isaac Newton's Philosophy*. PREF.

[3] In this commonplace book we find the date November 1665, so that its contents were written in 1664 and 1665.

[4] In the commonplace book which contains the "annotations out of Schooten and Wallis," no expenses are entered, so that there must be another note-book which I have not found, in which the purchase of Schooten's Miscellanies and Descartes' Geometry is recorded. It is not likely that the *second* note-book of 1659, mentioned by Conduit, contained expenses incurred in 1663 and 1664.

Sophister, bought Schooten's Miscellanies and Cartes' Geometry (having read this Geometry and Oughtred's *Clavis*[1] clean over half a year before), and borrowed Wallis's works, and by consequence made these annotations out of Schooten and Wallis, in winter between the years 1664 and 1665. At such time I found the method of Infinite Series; and in summer 1665, being forced from Cambridge by the plague,[2] I computed the area of the Hyperbola at Boothby,[3] in Lincolnshire, to two and fifty figures by the same method.

Is. Newton."

In consequence of the devotion of his mind to these abstract studies, and his long-continued observations upon a comet in 1664,[4] which made him sit up late at night, Sir Isaac's health was impaired to such a degree, as Mr. Conduit informs us, that from this illness "he learnt to go to bed betimes." In the beginning of the same year, on the 19th February, Sir Isaac's attention was directed to the subject of circles round the moon, by two coronas of three and five-and-a-half degrees each, accompanied by the halo of 22° 35′, of which he subsequently gave the theory in his Treatise on Optics.[5] In this year there were forty-four vacancies in the scholarships of Trinity College, and Newton was elected to one of them on the 28th of April. On this occasion he was examined in Euclid by Dr. Barrow, who

[1] Conduit remarks that in reading this work he did not entirely understand it, especially what "relates to Quadratic and Cubic Equations."—MSS. A translation of the Clavis was published and recommended by Halley in 1694.

[2] The plague commenced in Westminster about the end of 1664. It raged during the hotter months of 1665, and had so far abated before the end of the year, that the inhabitants returned to their homes in December. The date of Newton's quitting Cambridge, viz., 1665, as written under his own hand in his commonplace book, coincides with these facts, and is on this account probably the correct one; but Pemberton makes the date 1666, which is adopted by Professor Rigaud, and seems to be given by Newton himself in the *Phil. Trans.* vol. vi. p. 3080. Rigaud's *Hist. Essay on the first publication of Sir Isaac Newton's Principia*, p. 1, note.

[3] A village in Lincolnshire, near Sleaford, where Newton was probably on a visit.

[4] This comet passed its perihelion on the 4th December at midnight.

[5] Book II. Part IV. Obs. 13.

formed an indifferent opinion of his knowledge, and hence he was led not only to read Euclid with care, but to form a more favourable estimate of the ancient geometer when he came to the interesting propositions on the equality of parallelograms on the same base and between the same parallels.[1] In the month of January 1665, Newton took the degree of Bachelor of Arts, along with twenty-five other members of Trinity College, but we are not able to ascertain the academical rank which he held among the graduates, as the grace for that year does not contain the order of seniority of the Bachelors of Arts. The Proctors at this time were John Slader of Trinity, and Benjamin Pulleyn of Trinity, Newton's tutor, and the persons appointed in conjunction with them to examine the Questionists, were John Eachard of Catherine Hall, the satirical author of the *Grounds, &c., of the Contempt of the Clergy*, and Thomas Gipps of Trinity.[2]

In the same year Newton committed to writing his first discovery of Fluxions. This paper, written by his own hand, and dated May 20, 1665, represents in pricked letters the fluxions applied to their fluents, and in another leaf of the same waste book the method of fluxions is described without pricked letters, and bears the date of May 16, 1666. In the same book, with the date of November 13, 1665, there is another paper on Fluxions, with their application to the drawing of tangents, and "the finding the radius of curvity of any curve."[3] In the month of October 1666, Newton drew up another small tract, in which the method of Fluxions is again put down without pricked letters, and applied to Equations involving roots or surds.[4]

[1] Conduit's MSS.

[2] Edleston's *Correspondence*, &c. &c , App. xxi. xlv.

[3] Rigaud's *Hist. Essay*, &c., App. No. II. p. 20. From the Macclesfield MSS. Raphson *Historia Fluxionum*, Cap. I. p. 1, Cap. xiii. p. 92, and English Edition, pp. 115, 116.

[4] These papers in the Macclesfield Collection are quoted by Newton himself in his Observations on Leibnitz's celebrated Letter to the Abbé Conti, dated 9th April 1716. See Raphson's *Hist. of Fluxions*, pp. 103 and 116.

It was doubtless in the same remarkable year 1666, or perhaps in the autumn of 1665, that Newton's mind was first directed to the subject of Gravity. He appears to have left Cambridge some time before the 8th of August 1665, when the College was "dismissed" on account of the Plague, and it was therefore in the autumn of that year, and not in that of 1666, that the apple is said to have fallen from the tree at Woolsthorpe, and suggested to Newton the idea of gravity. When sitting alone in the garden, and speculating on the power of gravity, it occurred to him that as the same power by which the apple fell to the ground, was not sensibly diminished at the greatest distance from the centre of the earth to which we can reach, neither at the summits of the loftiest spires, nor on the tops of the highest mountains, it might extend to the moon and retain her in her orbit, in the same manner as it bends into a curve a stone or a cannon ball, when projected in a straight line from the surface of the earth. If the moon was thus kept in her orbit by gravitation to the earth, or, in other words, its attraction, it was equally probable, he thought, that the planets were kept in their orbits by gravitating towards the sun. Kepler had discovered the great law of the planetary motions, that the squares of their periodic times were as the cubes of their distances from the sun, and hence Newton drew the important conclusion that the force of gravity or attraction, by which the planets were retained in their orbits, varied inversely as the square of their distances from the sun. Knowing the force of gravity at the earth's surface, he was, therefore, led to compare it with the force exhibited in the actual motion of the moon, in a circular orbit; but having assumed that the distance of the moon from the earth was equal to sixty of the earth's semidiameters, he found that the force by which the moon was drawn from its rectilineal path in a second of time was only 13·9 feet, whereas at the surface of the earth it was 16·1 in a second. This great discrepancy between his theory and what he then considered to be the fact, induced him to abandon the subject, and pursue other studies with which he had been previously occupied.[1]

It does not appear from any of the documents which I have seen, at what time Newton made his first optical discoveries. On the authority of one of his memorandum books, containing an account of his expenses, it is stated by Conduit that he purchased a prism, in order to make some experiments on Descartes' Theory of Colours, and that he not only detected the errors of the French philosopher, but established his own views of the subject; but this is contradicted by Newton himself, who distinctly informs us that it was in the beginning of the year 1666, that he procured a glass prism "to try therewith the phenomena of colours."[2] There is no evidence, however, that he used it for this purpose, and there is every reason to believe that he was not acquainted with the true composition of light when Dr. Barrow completed his Optical Lectures, published in 1669.[3] In the preface of this work, Dr. Barrow acknowledges his obligation to his colleague Mr. Isaac Newton, as a man of a fine disposition and great genius, for having revised the MSS., and corrected several oversights, and made some additions of his own.[4] Now, in the twelfth Lecture there

[1] Neither Pemberton nor Whiston, who received from Newton himself the history of his first ideas of Gravity, records the story of the falling apple. It was mentioned, however, to Voltaire by Catherine Barton, Newton's niece, and to Mr. Green by Martin Folkes, the President of the Royal Society. We saw the apple-tree in 1814, and brought away a portion of one of its roots. The tree was so much decayed that it was taken down in 1820, and the wood of it carefully preserved by Mr. Turnor of Stoke Rocheford. See Voltaire's *Philosophie de Newton*, 3me part. Chap. III. Green's *Philosophy of Expansive and Contractive Forces*, p. 972, and Rigaud's *Hist. Essay*, p. 2.

[2] *Phil. Trans.* vol. vi. p. 3075.

[3] "Verum quod tenellæ matres factitant, a me depulsum partum amicorum haud recusantium nutriciæ curæ commisi, prout ipsis visum esset, educandum aut exponendum, quorum unus (ipsos enim honestum duco nominatim agnoscere) *D. Isaacus Newtonus*, collega noster (peregreglæ vir indolis ac insignis peritiæ) exemplar revisit, aliqua corrigenda monens, sed et de suo nonulla penu suggerens quæ nostris alicubi cum laude inexa cernes." The other friend was John Collins, whom he calls the Mersennus of our nation. *Epist. ad Lectorem.* The imprimatur of this volume is dated March 1668-9.

[4] The addition by Newton is a singularly elegant and expeditious method at the end

are some observations on the nature and origin of colours, which are so erroneous and unphilosophical, that Newton could not have permitted his friend to publish them had he been then in the possession of their true theory. According to Barrow, who introduces the subject of colours as an unusual digression, *White* is that which discharges a copious light, scattered equally in every direction. *Black* is that which emits light not at all, or very sparingly. *Red* is that which emits light more condensed than usual, but interrupted by shady interstices. *Blue* is that which discharges a rarefied light, or one excited by a weaker force, as in bodies which consist of white and black particles arranged alternately, such, for example, as the *clear ether* in which there float fewer particles that reflect light, while the rest take away light, the *sea* in which the *white* salt is mixed with the *black* water, and the *blue shadows* seen at the same time by candle and day light, which are produced by the whiteness of the paper mixed with the faint light or blackness of the twilight. *Yellow* consists of much *white* and a little *red* interspersed, and *Purple* of much blue and some red. *Green* seems to have puzzled Dr. Barrow. He says that it is somehow allied to *Blue;* but he adds, let wiser men find out the *difference*, I dare not conjecture. These opinions are so unsound, that they could not fail to have attracted the attention of Newton, who had certainly begun to study the subject of colours; and if he had discovered at this time that *white* was a mixture of all the colours, and *black* a privation of them all, he could not have permitted the absurd speculations of his friend and master to pass uncorrected.[1]

While Newton was thus occupied with the subjects of Fluxions and Gravity, he "applied himself also to the grinding of optic glasses of other figures than spherical." Descartes, in

of Lect. xiv., of determining geometrically in every case the image formed by lenses, and describing the lens which projects the image on a given point.

[1] Barrow introduces the subject of colours by the following remarkable sentence: "*Quoniam colorum incidit mentio, quid si de illis (etsi præter morem et ordinem) paucula divinavero?*"—*Lect.* xii. *ad finem.*

his *Dioptrics*, published in 1629, and more recently James Gregory, in his *Optica Promota*, published in 1663, had shown that parallel and diverging rays could only be reflected or refracted to a point or focus by mirrors or lenses, whose surfaces were paraboloidal, ellipsoidal, or hyperboloidal, or of some other form not spherical. Descartes had even invented and described machines by which lenses of these shapes could be ground and polished, and it was the universal opinion that the perfection of refracting telescopes and microscopes depended on the degree of accuracy with which lenses of these forms could be executed.

While engaged in this work Newton made his first experiments with the prism, and he was soon induced to abandon what he calls his "glass-works," in consequence of having found "that the perfection of telescopes was limited not so much for want of glasses truly figured according to the prescriptions of optick authors (which all men have hitherto imagined), as because *light* itself is a heterogeneous mixture of differently refrangible rays, so that were a glass so exactly figured as to collect any one sort of rays into one point, it could not collect those also into the same point, which having the same incidence upon the same medium, are apt to suffer a different refraction." He was therefore led to "take reflections into consideration," but in consequence of the interruption produced by the Plague, "it was more than two years before he proceeded."

After his return to Cambridge,[1] on the disappearance of the

[1] The only information which we have relative to the times of Newton's leaving and returning to Cambridge, in consequence of the Plague, is contained in the following note by Mr. Edleston:—

"The College was 'dismissed' June 22d, on the reappearance of the Plague. The Fellows and Scholars were allowed their commons during their absence. Newton received on this account 3s. 4d. weekly, for 13 weeks, ending Michaelmas 1666.

,,	,,	,,	12 ,,	Dec. 21.
,,	,,	,,	5 ,,	Ladyday 1667."

The College had been also dismissed the previous year, August 8th, on the breaking out of the plague, but Newton must have left Cambridge before that, as his name does

Plague, he was, on the 1st of October 1667, elected Minor Fellow, and an apartment called "The Spiritual Chamber," assigned to him by the Master,—a locality which Mr. Edleston conjectures to be the ground room next the chapel in the north-east corner of the great court. A few weeks after this he went to Lincolnshire, and returned on the 12th February 1667-8. On the 16th March 1668, he took his degree of M.A., and was the twenty-third on the list of 148 signed by the Senior Proctor.[1]

About this time, and during the period extending from 1666 to 1669, when he succeeded to the Lucasian chair, his studies were of a very miscellaneous kind, and were doubtless interrupted not only by the appearance and reappearance of the plague, but by the preparations necessary for taking his degree. In his common note-book,[2] which I found among the family papers, and which, along with a number of problems in geometry and the conic sections, contains an account of his expenses from 1665 to 1669, there are many entries which throw some light upon his social character as well as upon his studies. During his absence from College in 1665 and 1666, we find him purchasing Philosophical Intelligences, the History of the Royal Society, Gunter's Book and Sector from Dr. Fox, together with magnets, compasses, glass-bubbles, drills, mandrels, gravers, hones, and hammers. In 1667, he purchased Bacon's

not appear in the list of those who received *extra commons* for 6½ weeks on the occasion. "Aug 7, 1665.—A month's commons (beginning Aug. 8th) allowed to all Fellows and Scholars which now go into the country upon occasion of the pestilence."—(*Conclusion Book.*)

"On the continuance of the scourge, we find him with others receiving the allowance for commons for 12 weeks, in the quarter ending Dec. 21, 1665, and for 13 weeks ending Ladyday 1666."—Edleston's *Correspondence, &c.* p. xlii. note 8.

[1] Thomas Burnet, author of the *Theoria Telluris Sacra*, and a future friend and correspondent of Sir Isaac.

[2] This note-book, of which three-fourths is white paper, begins at one end with three pages of short-hand, which is followed by his expenses. At the other end of the book there is a *Novi Cubi . . . . Tabella*, and a number of problems in geometry and the conic sections.

Miscellany, three prisms, and four ounces of putty.[1] He records his jovial expenses, not only on the occasion of his taking his two degrees, but "at the tavern several other times." He acknowledges his having "lost at cards twice;" but this is compensated by his liberality to his "cousin Ayscough," on whom, and "on other acquaintance," he "spends" considerable sums,—by his generosity to his sister, for whom he buys oranges,—and his kindness to D. Wickins, to whom he lends considerable sums of money. It appears, too, from this note-book, that Newton went to London on Wednesday the 5th August 1668, and returned to Cambridge on Monday the 28th September, after an absence of nearly two months; but the object of his journey is nowhere mentioned. It is not improbable that he went there to purchase lenses, and apparatus and materials for chemical experiments,—a new branch of science which seems at this time to have occupied his attention, and which he continued to prosecute with much zeal during the most active period of his life. In April 1669, he records the purchase of lenses in London, and there follows a long list of chemical substances, headed by mercury, together with a furnace, and an air-furnace.[2]

[1] Flowers of Putty, an oxide of zinc used in polishing lenses and metallic specula.

[2] As this list of expenses is very interesting, and as the book which contains them has obviously been preserved by Newton himself as evidence of the priority of some of his researches, the following abstract of it is presented to the reader:—

1665.

Received, May 23d, whereof I gave my tutor 5s., ...	. £5 0 0
Remaining in my hands since last quarter, ...	. 3 8 4
In all, ...	. £8 8 4

This account of expenses extends only to six and a half pages, and records many loans. The following are among the entries:—

Drills, gravers, a hone, a hammer, and a mandrel, . . .	. £0 5 0
A magnet, ...	. 0 16 0
Compasses, ...	. 0 3 6
Glass bubbles, ...	. 0 4 0
My Bachelor's account, ...	. 0 17 6

Towards the end of 1668, Newton carried into effect, on a small scale, his resolution to "take reflections into consideration." Thinking it "best to proceed by degrees," he first "made a small perspective to try whether his conjecture would hold good or not."[1] The telescope was six inches long. The

At the tavern several other times,	£1	0	0
Spent on my cousin Ayscough,	0	12	6
On other acquaintance,	0	10	0
Cloth, 2 yards, and buckles for a vest,	2	0	0
Philosophical Intelligences,	0	9	6
The Hist. of the Royal Society,	0	7	0
Gunter's Book and Sector to Dr. Fox,	0	5	0
Lost at cards twice,	0	15	0
At the tavern twice,	0	3	6
I went into the country, Dec. 4, 1667.			
I returned to Cambridge, Feb. 12, 1667.			
Received of my mother,	30	0	0
My journey,	0	7	6
For my degree to the College,	5	10	0
To the proctor,	2	0	0
To three prisms,	3	0	0
Four ounces of putty,	0	1	4
Lent to D. Wickins,	1	7	6
Bacon's Miscellanies,	0	1	6
Expenses caused by my degree,	0	15	0
A Bible binding,	0	3	0
For oranges for my sister,	0	4	2
Spent on my journey to London, and 4s. or 5s. more which my mother gave me in the country,	5	10	0
I went to London, Wednesday, August 5th, and returned to Cambridge on Monday, September 28, 1668.			
Lent D. Wickins,	0	11	0
April 1669.			
For glasses in Cambridge.			
For glasses in London.			
For aquafortis, sublimate, oyle pink, fine silver, antimony, vinegar, spirit of wine, white lead, salt of tartar, ☿	2	0	0
A furnace,	0	8	0
Air furnace,	0	7	0
Theatrum chemicum,	1	8	0
Lent Wardwell 3s., and his wife 2s.,	0	5	0

[1] See Letter to Oldenburgh, Feb. 1671-2, in Newtoni *Opera*, by Horsley, tom. iv. p. 295; and Letter to a Friend, Feb, 23, 1668-9, in Gregory's *Catoptrics*, edit. 3d, p. 259; or in the *Macclesfield Collections*, vol. ii. p. 289.

aperture of the large speculum was something more than an inch, and, as the eye-glass was a plano-convex lens, with a focal length of one-sixth or one-seventh of an inch, "it magnified about forty times in diameter," which he believed was more than any six-feet refracting telescope could do with distinctness. Owing to the badness of the materials which he used, and the want of a good polish, it did not represent objects so distinctly as a six-feet refractor, yet Sir Isaac was of opinion that it would discover as much as any three or four feet refractor, especially if the objects are luminous. He saw with it Jupiter distinctly round, with his four satellites, and also the horns or "moonlike phase of Venus," though this last phenomenon required a nice adjustment of the instrument. He therefore considered this small telescope as "an epitome" of what may be done by reflections; and he did not doubt that, in time, a six-feet reflector might be made which would perform as much as any sixty or hundred feet refractor. In consequence of interruptions, Sir Isaac did not proceed any farther in the construction of reflectors till the autumn of 1671.

It was during this period of his history, on the 18th of May 1669, that Sir Isaac wrote the celebrated letter of advice to his young friend, Mr. Aston, who, at the age of twenty-seven, was about to make a tour on the Continent. This "letter" is a very interesting production.[1] It does not evince much acquaintance with the ways of the world, but it shows some knowledge of the human heart, and throws a strong light on the character and opinions of its author. In his chemical studies, which, as we have just seen, he had recently commenced, his mind was impressed with some belief in the doctrines of alchemy, and he certainly pursued his experiments to a late period of his life, with the hope of effecting some valuable transmutations. Among the subjects, therefore, to which he requests Mr. Aston to pay attention, there are several which indicate this tendency of his mind. He desires him to

[1] See APPENDIX, No. I.

observe the products of nature, especially in mines, with the circumstances of mining, and of extracting metals or minerals out of their ores, and refining them ; and, what he considered as far more important than this, he wishes him to observe if there were any transmutations out of one species into another, as, for example, out of iron into copper, out of one salt into another, or into an insipid body, &c. Such transmutations, he adds, are above all others worth his noting, being *the most luciferous, and many times lucriferous experiments, too, in philosophy!* Among the particular observations to which he calls the attention of his friend, is that of a certain vitriol, which changes iron into copper, and which is said to be kept a secret for the lucrative purpose of effecting that transmutation. He is to inquire also whether in Hungary, or in the mountains of Bohemia, there are rivers whose waters are impregnated with gold, dissolved by some corrosive fluids like aqua regis ; and whether the practice of laying mercury in the rivers till it be tinged with gold, and then separating the gold by straining the mercury through leather, be still a secret or openly practised. There was at this time in Holland a notorious alchemist of the name of Bory, who, as Sir Isaac says, was some years since imprisoned by the Pope, in order to extort from him secrets of great worth, both " as to medicine and profit," and who made his escape into Holland, where they granted him a guard. " I think," adds Sir Isaac, " he usually goes clothed in green : pray inquire what you can of him, and whether his ingenuity be any profit to the Dutch !" We have not been able to discover the results of Mr. Aston's inquiries, but whatever they were they did not damp the ardour of Newton in his chemical researches, nor extinguish the hope which he seems to have cherished, of making " philosophy lucriferous," by transmuting the baser metals into gold.

But however fascinating these studies were to our young philosopher, he did not permit them to interfere with his nobler pursuits. At the very time when writing to Mr. Aston, we find him occupied with his fluxionary calculus, and transmitting to Dr. Barrow his celebrated paper *On Analysis by Equations with an infinite number of terms*, with permission to communicate it to their mutual friend, Mr. Collins. In announcing this communication on the 20th June 1669, and promising to send it by the next opportunity, Dr. Barrow keeps the name of its author a secret, and merely tells Mr. Collins that he is a friend staying at Cambridge, who has a powerful genius for such matters. In his next letter of the 31st July, accompanying the paper, he expresses the hope that it will not a little delight him: and, in a third letter to Collins of the 20th August, he mentions how much he is pleased with the favourable opinion which his correspondent has of it, and adds, that "the name of the author is Newton, a Fellow of our College, and a young man, who is only in his second year since he took the degree of Master of Arts, and who, with an unparalleled genius, has made very great progress in this branch of mathematics."

CHAPTER III.

Newton succeeds Barrow in the Lucasian Chair—Hyperbolic Lenses proposed by Descartes and Others—Opinions of Descartes and Isaac Vossius on Colours—Newton discovers the Composition of White Light, and the different Refrangibility of the Rays that compose it—Having discovered the cause of the imperfection of Refracting Telescopes, he attempts the construction of Reflecting ones—Constructs a second Reflecting Telescope in 1668, which is examined by the Royal Society, and shown to the King—Discussions respecting the Gregorian, Newtonian, and Cassegrainian Telescope—James Gregory the Inventor of the Reflecting Telescope—Attempts to construct one—Newton makes a Speculum of silvered glass—Glass Specula by Short in 1730, and Airy in 1822—Hadley constructs two fine Reflecting Telescopes—Telescopes by Bradley, Molyneux, and Hawksbee—Short's Reflecting Telescopes with Metallic Specula—Magnificent Telescope of Sir William Herschel with a four-feet Speculum—Munificence of George III.—Astronomical Discoveries of Sir Wm. Herschel—Telescopes of Sir J. Herschel and Mr. Ramage—Gigantic Telescope of the Earl of Rosse with a six-feet Speculum—Progress of Telescopic Discovery—Proposal to send a fine Telescope to a Southern Climate.

In 1669, when Dr. Barrow had resolved to devote himself to the studies and duties of his profession, he resigned the Lucasian Professorship of Mathematics in favour of Newton. His appointment took place on the 29th October, and we may now consider him as having entered on that brilliant career of discovery, the history of which will form the subject of some of the following chapters. It had been long known to every writer on optics, and to every practical optician, that lenses with spherical surfaces, such as those now in common use, did not give distinct images of objects. This indistinctness was believed to arise solely from their spherical figure, in consequence of which the rays which passed through the marginal or outer parts of the lens were refracted to a focus nearer the lens than those which passed through its central parts. The distance between these foci was called the *spherical aberration* of the lens, and various methods were suggested for diminishing or removing this source of imperfection. Descartes[1] had shown that hyperbolic lenses refracted the rays of light to a single focus, and we accordingly find the early volumes of the Philosophical Transactions filled with schemes for grinding and polishing lenses of this form. Newton had made the same attempt, but finding that a change of form produced a very little change in the indistinctness of the image, he thought that the defect of lenses, and the consequent imperfection of telescopes, might arise from some other cause than the imperfect convergency of the incident rays to a single point. This happy conjecture was speedily confirmed by the brilliant discovery of the different refrangibility of the rays of light,—a discovery which has had the most extensive applications to every branch of science, and (what is very rare in the history of inventions) one to which no other person has made the slightest claim.

No plausible conjecture, even, had been formed by the predecessors of Newton respecting the nature and origin of colours. Descartes believed them to be a modification of light depending on the direct or rotatory motion of its particles. Grimaldi, Dechales, and others, regarded them as arising from different degrees of rarefaction and condensation of light. Gregory defines colour to be the hue (*tinctura*) of igneous corpuscles emerging from radiant matter,[2] and we have already seen that the views of Barrow on this subject were equally absurd. In recounting the opinions of preceding writers, Newton alleges that in all of them the colour is supposed not to be innate in light, but produced by the action of the bodies which reflect or refract it. This, however, is not strictly true, as Isaac Vossius, in a dissertation which Newton probably never saw, distinctly maintains that all the colours exist in light itself, or, to use another of his expressions, that all light carries its colours along

[1] Dioptrice, cap. viii. ix. 1629.

[2] Optica Promota: *Definitiones*, 3. Lond. 1663.

with it.[1] This, however, was a mere conjecture, which cannot be regarded as in any way anticipating the great discovery of Newton, "that the modification of light from which colours take their origin, is innate in light itself, and arises neither from reflection, nor refraction, nor from the qualities or any other conditions of bodies whatever, and that it cannot be destroyed or in any way changed by them."

After our author had purchased his glass prism at Stourbridge Fair, he made use of it in the following manner. Having made a hole H in his window-shutter SHT, and darkened the room, he admitted a ray of the sun's light RR, which after

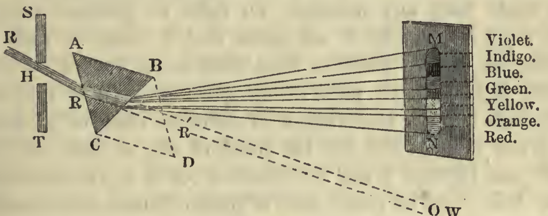


Fig. 2.

refraction at the two surfaces AC, BC of the prism ABC, exhibited on the opposite wall MN what is called the *Solar* or *Prismatic Spectrum*. This spectrum was an elongated image of the sun

[1] Isaaci Vossii *De Lucis Natura et Proprietate*, Amstel. 1662. As the opinions of Vossius have not been referred to by any of our historians of science, the following passages may be interesting.

"Primus itaque color, si tamen color dicendus sit, is est albus, pelluciditatem proxime hic accedit. Insunt itaque et lumini omnes colores, licet non semper visibiliter; nempe ut flamma intensa alba et unicolor apparet, eadem si per nebula aut aliud densius corpus spectetur, varios induit colores. Pari quoque ratione, Lux, licet invisibilis aut alba ut sic dicam, si per prisma vitreum, aut aerem roridum transeat, similiter varios colores induit."—P. 6.

"Omnem tamen lucem secum colores deferre et eo colligi potest quod si per lentem vitream, aut etiam per foramen, lumen in obscurum admittatur cubiculum in muro aut linteo remotiore manifeste omnes videantur colores, cum tamen in punctis decussationis radiorum et locis minimum lenti vicinis, nullus color sed purum tantum compareat lumen."—P. 64.

"Quapropter non recte ii sentiunt qui colorem vocant Lumen modificatum."—P. 59.

about *five* times as long as it was broad, and consisted of *seven* different colours, *Red*, *Orange*, *Yellow*, *Green*, *Blue*, *Indigo*, and *Violet*. "It was at first," says Newton, "a very pleasing divertisement to view the vivid and intense colours produced thereby;" but this pleasure was immediately succeeded by surprise at various phenomena which were inconsistent with the received laws of refraction. The "extravagant disproportion between the length of the spectrum and its breadth," excited him to a more than ordinary curiosity of examining from whence it might proceed. He could scarcely think that the various thickness of the glass, or the termination with shadow or darkness could have any influence on light to produce such an effect; yet he thought it not amiss first to examine these circumstances, and he therefore tried what would happen by transmitting light through parts of the glass of different thickness, or through holes in the window of different sizes, or by setting the prism without (on the left hand of ST), so that the light might pass through it and be refracted before it was terminated by the hole; but he found none of these circumstances material. The fashion of the colours was in all these cases the same.

Newton then suspected that by some unevenness of the glass, or other accidental irregularity, the colours might be thus dilated. In order to try this he took another prism BCD, and placed it in such a manner that the light passing through them both might be refracted contrariwise, and thus returned by BCD into the path RRW, from which the first prism ABC had diverted it, for by this means he thought that the regular effects of the prism ABC would be destroyed by the second prism BCD, and the irregular ones more augmented by the multiplicity of refractions. The result was, that the light which by the first prism was diffused into an oblong form MN, was reduced by the second prism into a circular one W with as much regularity as when it did not pass through them, so that whatever was the cause of the length of the image MN, it did not arise from any irregularity in the prism.

Sir Isaac next proceeded to examine more critically the effect that might be produced by the difference in the angles of incidence, at which rays from different parts of the sun's disc fell upon the face AC of his prism, and for this purpose he measured the lines and angles belonging to the spectrum MN, and obtained the following results :

Distance of MN from the hole H, . . .	22 feet.
Length of MN, ...	13¼ inches.
Breadth of MN, ...	2⅝ ,,
Diameter of the hole H, ...	0¼ ,,
Angle of WR with the middle of MN, . . .	44° 56′.
Angle ABC of the prism, ...	63° 12′.
Refractions at R and R′, ...	54° 4′.

"Now, subducting the diameter of the hole from the length and breadth of the image, there remains 13 inches in the length and 2⅜ inches in the breadth comprehended by those rays which passed through the centre of the hole, and consequently the angle of the hole which that breadth subtended was about 31′, answerable to the sun's diameter ; but the angle which its length subtended was more than five such diameters, namely, 2° 49′."

With the refractive power of the prism, which he found to be 1·55, he found the refractions of two rays proceeding from opposite parts of the sun's disc, so as to differ 31 minutes in their obliquity, to be such as to comprehend an angle of 31 or 32 minutes.

Although Newton could not doubt the correctness of the law of the Sines on which these calculations were founded, yet "his curiosity caused him again to take his prism, and satisfy himself by direct experiment that even a motion of the prism about its axis of four or five degrees, did not sensibly change the position of the spectrum MN on the wall," so that "there still remained some other cause to be found out," from which the spectrum could subtend an angle of 2° 49′.

Having set aside all these explanations of the length of his spectrum, Newton hazarded the strange suspicion that the rays after passing through the prism "might move in curve lines, and according to their more or less curvity lead to different parts of the wall," and "it increased his suspicion," he adds, "when he remembered that he had often seen a tennis-ball struck with an oblique racket describe such a curve line. In this case a circular and a progressive motion being communicated to it by that stroke, its parts on that side where the motions conspire, must press and beat the contiguous air more violently than on the other, and there excite a reluctancy and reaction of the air proportionally greater. And for the same reason, if the rays of light should possibly be (composed of) globular bodies, and by their oblique passage out of one medium into another acquire a circulating motion, they ought to feel the greater resistance from the ambient ether on that side where the motions conspire, and thence be continually bowed to the other. But notwithstanding this plausible ground of suspicion, when I came to examine it, I could observe no such curvity in them. And besides (which was enough for my purpose) I observed that the difference betwixt the length of the image, and the diameter of the hole through which the light was transmitted, was proportional to their distance."

Having thus gradually removed these different hypotheses, or suspicions, as Newton calls them, he was led to the *experimentum crucis* for determining the true cause of the elongation of the spectrum MN. He placed a board with a hole in it behind the face BC of the prism, and close to it, so that he could transmit through the hole any one of the colours in MN, and keep back the rest. When the hole was near C, for example, no other rays but the *red* fell on the wall at N. He then placed behind the *red* space at N another board with a hole in it, and behind this board he placed another prism, so as to receive the red light at N, which passed through the hole in the board. He then turned round the first prism ABC, so as to make all the colours pass successively through the two holes, and he marked their places on the wall. From the variation of these places he saw that the *red* rays at N were less refracted by the second prism than the *orange* rays, the *orange* less than the *yellow*, and so on, the *violet* being more refracted than all the rest. Hence he arrived at the grand conclusion, *that light was not homogeneous, but consisted of rays of different refrangibility.*

We have given this full account of Newton's mode of investigation, in order to show the cautious manner in which he proceeded ; and were it not for the inconceivable stupidity of the men who called in question his results, we should have considered all his suspicions and precautions unnecessary, and adopted the opinion of Arago, that the *compound nature of white light* was clearly involved in the very phenomenon of the prismatic spectrum, and that the words in which Newton stated it as a conclusion, were "nothing else than a literal description or translation of that familiar experiment."[1]

Having established this important truth, Newton immediately perceived that the different refrangibility of the rays of

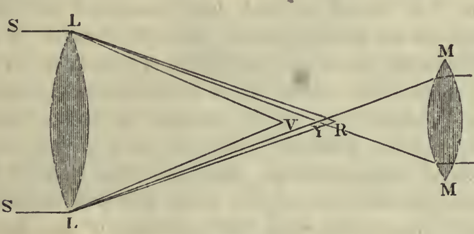


FIG. 3.

light was the real cause of the imperfection of refracting telescopes. If LL is a convex lens receiving rays SL, SL, the *violet* rays in the *white* ray SL will be refracted in the line LV to V, the *yellow* rays to Y, and the *red* rays to R, forming a

[1] *Phil. Trans.* vol. vii. No. 80. Feb. 19, 1672.

violet image of the *sun*, or any other object from which the white light proceeds at the point V, a *yellow* image at Y, and a *red* one at R, images of intermediate colours being formed at intermediate points between V and R. If this image is received on a sheet of white paper at V, or Y, or R, it will be exceedingly indistinct, and tinged with these different colours. Newton found that the space VR, which is called the *chromatic aberration*, or the *aberration of colour*, was in glass the fiftieth part of the diameter LL of the lens, so that in lenses about six inches in diameter, such as those used in the telescopes about 150 feet long, of Campani, Divini, and Huygens, the space VR would be about ·17 of an inch. Hence if LL be the object-glass of a telescope directed to any luminous body, and MM an eye-glass through which the eye sees magnified the image or picture of the body between V and R, it cannot see distinctly all the different images of the body formed there. If it is adjusted to see distinctly the *yellow* image at Y as it is in the figure, it will not see distinctly either the *red* or the *violet* images, nor indeed any but the *yellow*, and that very imperfectly, as it is mixed up with hazy images of all the other colours, producing great confusion and indistinctness of vision.

As soon as Sir Isaac saw this result of his discovery, he left off his "glass-works," as he called his attempts to improve the refracting telescope, and, in the autumn of 1668, constructed the little reflecting telescope which we have already described. The success of this experiment, small as it was, inspired Newton with fresh zeal, and, though his mind was now occupied with his optical discoveries, with the elements of his method of fluxions, and with his speculations on gravity, yet, with all the ardour of youth, he set himself to the task of executing another reflecting telescope with his own hands. This telescope, of which we have given a drawing in the annexed figure, was a better one than the first; and, we presume from its not being much superior either to the first, or to the one executed by his colleague, he allowed it to lie by him for several years. The existence of these telescopes having become known to some of the members of the Royal Society, Newton was requested to

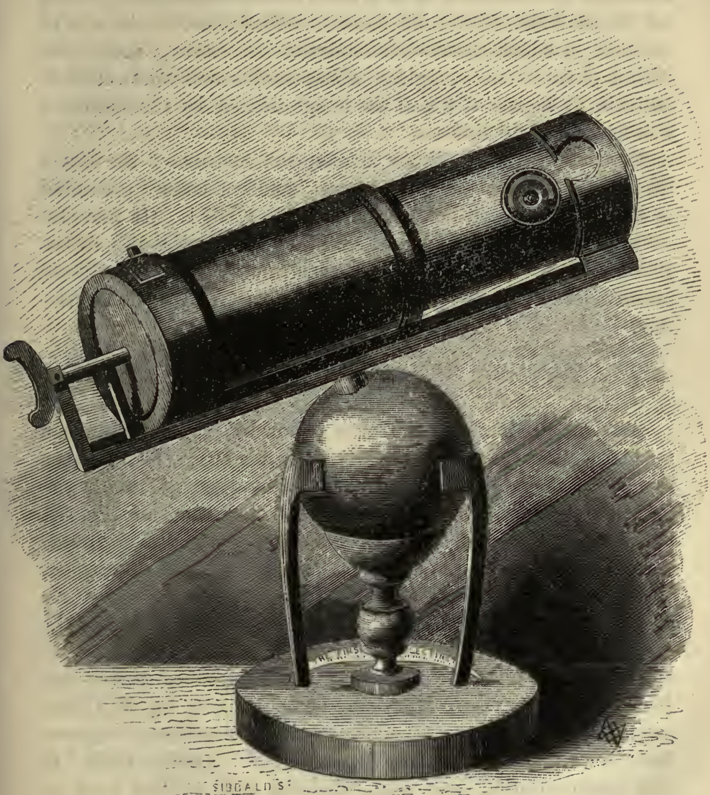


FIG. 4.

send his instrument to that learned body. This telescope consisted of a concave metallic speculum, the radius of curvature of which was $12\frac{2}{3}$ or 13 inches, so that "it collected the sun's rays at the distance of $6\frac{1}{3}$ inches." The rays reflected by the speculum were received upon a plain metallic speculum inclined 45° to the axis of the tube, so as to reflect them to the side of the tube in which there was an aperture to receive a small tube with a plano-convex eye-glass, whose radius was one-twelfth of an inch, by means of which the image formed by the speculum was magnified 38 times.

Newton did not hesitate to obey the request of the Royal Society, and it was accordingly sent, and we believe presented to that distinguished body near the end of 1671. It was also shown to the King, and a description of it published in the Philosophical Transactions.[1] The instrument itself is carefully preserved in the Library of the Royal Society, with the inscription,—

"THE FIRST REFLECTING TELESCOPE INVENTED BY SIR ISAAC NEWTON, AND MADE WITH HIS OWN HANDS."

Previous to March 16, 1672, a Fellow of Trinity College had made a similar telescope of nearly the same size, which Newton found to "magnify more, and also more distinctly," than a six-feet refractor, and which he considered better than his own. A description of Newton's instrument in Latin was drawn up and corrected by Newton, and when signed by Lord Brouncker, Wren, and Hooke, was sent to Huygens, who expressed his approbation of it, and suggested the propriety of giving the concave speculum a parabolic form. Various observations were made upon the instrument, particularly by Monsieur Auzout and Monsieur Denys; and Monsieur Bercé claimed for M. Cassegrain the invention of a telescope which he considered "almost like Newton's," and "more ingenious."[2] Newton replied to this communication, and acknowledging that he had been ac-

[1] *Phil. Trans.* vol. vii. No. 81, p. 4004 March 25, 1672.

[2] See *Journal des Savans*, 1672, pp. 80 and 121; and *Phil. Trans.* No. 83, p 4056, May 20, 1672.

quainted with the telescope proposed by Gregory before he had contrived his own, he points out the superiority of the Gregorian to the Cassegrainian form, and of his own to both. This letter led to a little amiable controversy between Gregory and Newton on the merits of their reflecting telescopes, in which neither of them gained the victory.[1]

Newton's occupations were at this period too numerous, and his time too valuable to be spent in mechanical labour ; and he therefore never resumed the construction of reflecting telescopes. The Royal Society, however, employed a London optician of the name of Cox to execute a Newtonian reflector, with a speculum whose focal length was no less than *four feet*, but he failed in polishing the speculum ; and though Sir Isaac himself contemplated the construction of another instrument, he seems to have wholly abandoned the attempt, and to have bequeathed to another age the honour of making his telescope an instrument of discovery. The want of a good material for the specula seems to have been the difficulty which perplexed the optician ; and it would appear from the following observations of Newton himself, that the specula for the instrument, ordered by the Royal Society, were to be made of a new material. "You will gratify me much," says he in a letter to Oldenburg, "by acquainting me with the particular dimensions, fashion, and success

[1] Gregory's *Catoptrics*, App. 261. In this controversy, Newton never claimed any credit for the invention of a new form of the reflecting telescope, and was certainly surprised at the notice it excited among persons that either were, or ought to have been, acquainted with the previous invention of Gregory. In his letter to Mr. Collins, he speaks in the kindest manner of Gregory. "I doubt not that when Mr. Gregory wrote his *Optica Promota*, he could have described more fashions than one of these telescopes, and perhaps have run through all the possible cases of them, if he had thought it worth his pains. Because Mr. Cassegrain propounded his supposed invention pompously, as if the main business was the contrivance of these instruments, I thought fit to signify that that was none of his contrivance, nor so advantageous as he imagined. And I have now sent you these farther considerations on Mr. Gregory's answer, only to let you see that I chose the most easy and practicable way to make the first trials. Others may try other ways, nor do I think it material which way these instruments are perfected, so they be perfected.—Dec. 10, 1672." See the *Macclesfield Collections*, vol. ii. pp. 346, 347, or Newtoni *Opera* by Horsley, vol. iv. p. 288.

of the four-feet tube, which, I presume, Mr. Cox by this time hath finished. And to inform myself of the advantages of the steely matter which is made use of, you will much oblige me if you can procure me a fragment of it. I suppose it is made by melting steel with a little antimony, perhaps without separating the sulphureous from the metalline part of that mixture. And so though it may be very hard, and capable of a good polish, yet I suspect whether it be so strongly reflective as a mixture of other metals. I make this inquiry, because if I should attempt anything farther in the fabric of the telescope, I would first inform myself of the most advantageous materials. On which account, also, you would farther oblige me if you can inquire whether Mr. Cox, or any other artificer, will undertake to prepare the metals, glass, tube, and frame of a four-feet telescope, and at what rates he will do it, so that there may remain nothing for me to do but to polish the metals. A gross account of this will at present suffice, until I send you a particular design of the fabric of the instrument, if I resolve upon it."[1]

Such is a brief account of the first reflecting telescope that was successfully constructed and applied to the heavens; but though we make this admission in its favour, we must also acknowledge that it was a small and ill-made instrument, incapable of showing the beautiful celestial phenomena which had been long seen by the refracting telescopes of Hevelius and Huygens. No discovery was made by any of the three instruments to which we have referred, and more than fifty years elapsed before telescopes of the Newtonian form became useful in astronomy. A similar fate befell the reflecting telescope of James Gregory, who was the undoubted inventor of that noble instrument, and whose merits were thrown into the shade by the display which accompanied the invention of his friend. In his *Optica Promota*, published in 1663, Gregory describes a reflecting telescope, with the view of making telescopes shorter and more manageable. When compared with other telescopes,

[1] July 13, 1672, in the *Macclesfield Collections*, vol. ii. p. 333.

he gives it the character of a *golden one*, as "it has no inconveniences, and may have all the properties of the other telescopes, whether dioptric or catoptric." He then goes on to describe "a telescope of this most perfect kind." It consists of a parabolic concave speculum, with a hole in its centre, having near its focus a small elliptic concave speculum. The image formed by the large parabolic speculum is received by the small elliptical one, and reflected through the aperture in the former upon a lens which magnifies it. In the reflecting telescope proposed by Cassegrain, the image formed by the larger speculum is received by a small *convex* speculum, the effect of which is to shorten the telescope, and prevent the crossing, or "decussation of the rays," as Newton calls it, in the focus of the larger speculum.[1] Gregory never attempted to construct this instrument with his own hands, but he employed Messrs. Reeves and Cox, celebrated glass-grinders, to execute a concave speculum three feet in focal length, together with a little concave and a little convex speculum; but as Mr. Reeves "could not polish the large concave on the tool, but merely with cloth and putty,"[2] and as Gregory was on the eve

[1] Sir Isaac seems to have been the first person who suggested the idea that vision might be rendered indistinct by the collision of the rays when they cross one another at the focus of mirrors or lenses. In speaking of the use of more than one eye-glass in the Gregorian telescope, he states, that "*by the iterated decussations of the rays, objects will be rendered less distinct*, as is manifest in dioptric telescopes, where two or three eye-glasses are applied to erect the object."—Letter to Collins, Dec. 10, 1672; *Macclesfield Collections*, vol. ii. p. 344. In the course of some experiments on this subject, I found that the sections of the cone of rays are never so distinct and well-defined *after the rays have crossed as before*.—(*Treatise on New Phil. Inst.* pp. 44 and 193.) And Captain Kater, in comparing two equal telescopes, the one Gregorian and the other Cassegrainian, found that the intensity of the light within the focus was nearly double of what it was without the focus. In other experiments, he found the ratio as 1000 to 788.—*Phil. Trans.* pp. 13, 14. Mr. Tulley, however, in making similar experiments, did not confirm the results obtained by Captain Kater. I have found, in confirmation of these facts, that the negative diffractive fringes produced by rays which do not cross one another before they enter the eye, are more distinct than the positive ones which do cross.—*Treatise on Optics*, Edit. of 1853, p. 117.

[2] Dr. Hooke made several experiments with the speculum executed by Mr. Reeves, and did not find it so bad as Gregory thought. See Newton's Letter to Collins above referred to.

"of going abroad, he thought it not worth the pains to trouble himself any farther with it, so that the tube was never made. Yet," he adds, "I made some trials with a little concave and *convex* speculum, which were but rude, seeing I had but transient views of the object."[1]

Although Newton did receive through Oldenburg the information he requested from Mr. Cox,[2] yet he never availed himself of it in proceeding any farther with metallic reflectors. In consequence, however, of Gregory having suggested to him the use of glass specula silvered on the back for burning glasses, and shown how to make the foci of each surface coincident, Newton proposed, we believe in 1678, to substitute these specula instead of metallic ones in the reflecting telescope. In this manner he attempted to make a telescope four feet long, and with a magnifying power of 150; but though the glass was wrought by a London artist, and seemed well finished, yet, when it was quicksilvered on its convex side, it exhibited all over the glass innumerable inequalities, which rendered every object indistinct. He expresses, however, his conviction, that nothing but good workmanship is wanting to perfect such telescopes, and he recommends their consideration "to the curious in figuring glasses." This recommendation remained unnoticed for upwards of fifty years. At last Mr. James Short, a Scotch artist of consummate skill, executed, about the year 1730, no fewer than six reflecting telescopes, with glass specula, three of which were fifteen inches, and three nine inches in focal length; but some of them turned out useless from the veins in the glass. Maclaurin,[3] who, with one of nine inches, could read the Philosophical Transactions very easily at the distance of 130 feet, informs us that they were excellent instruments. Short, however, found that their light was fainter than he expected, and from this cause, combined with the difficulty of

[1] Letter from Gregory to Collins and Newton, Sept. 26, 1672.
[2] *Biog. Brit.*, Art. Newton, p. 3217.
[3] Smith's *Optics*, vol. ii. Remarks, p. 80.

finishing them, he afterwards limited himself to the use of metallic specula.[1]

The subject of glass specula was resumed in 1822 by Mr. Airy, one of the distinguished successors of Newton in the Lucasian chair. Having demonstrated that the aberration both in figure and colour might be corrected in these instruments, he executed more than one; but though the result of the experiment was such as to excite hopes of ultimate success, the construction of such an instrument is still a desideratum in practical science.

Notwithstanding these failures, we would not discourage the young artists of the present day from endeavouring to surmount the difficulties experienced by their predecessors. Discs of glass can now be obtained entirely free of veins, and what is of great importance, instead of coating the convex surface with a plate of mercury and tin, which reflects even less light than speculum metal, we can now, by the electrotype, deposit pure silver on the glass, and give it a reflective power far surpassing that of any other metal.

Such is a brief history of the attempts which were made by Newton and Gregory to construct reflecting telescopes. They were certainly far from being successful; nor were their contemporaries more fortunate, though guided by the light of their experience.

After the lapse of fifty years, however, and several years before his death, Sir Isaac had the satisfaction of seeing a Newtonian telescope, six feet long, mounted upon a commodious stand, and capable of exhibiting some of the most interesting phenomena in the heavens. A Gregorian telescope, of an inferior size, was executed with similar success, and from that time the art of making telescopes with metallic reflectors was gradually brought to perfection. The history of these improve-

[1] Caleb Smith proposed to correct the colour produced by the two refractions, by a concave lens placed between the speculum and the small receiver, or by making the surface of a rectangular glass prism concave.—*Phil. Trans.* 1739, p. 326.

ments, and of the grand discoveries in astronomy to which they led, would of itself form an interesting volume. We shall endeavour, in a few pages, to present it to our readers.

The person to whom we owe the first step in the improvement of the reflecting telescope, was John Hadley, one of the inventors of the Reflecting Quadrant, which bears his name.[1] This gentleman, who was a Fellow of the Royal Society, and possessed of considerable scientific attainments, began his experiments in 1719, and, probably after many failures, completed a telescope toward the end of 1720. It was shown and presented to the Royal Society, in whose Journals for January 12, 1721, the following notice of it occurs:—"Mr. Hadley was pleased to show the Royal Society his reflecting telescope, made according to our President's (Sir Isaac Newton) directions in his Optics, but curiously executed by his own hand, the force of which was such as to enlarge an object near *two hundred times*, though the length thereof scarce exceeds *six* feet; and having shown it he made a present thereof to the Society, who ordered their hearty thanks to be recorded for so valuable a gift." The instrument consisted of a metallic speculum, about six inches in diameter, and its focal length was five feet two inches and a half. Its plane speculum was made of the same metal, about the 15th of an inch thick, and it had six eye-pieces, three convex lenses 1-3d, 3-10ths, and 11-40ths of an inch, magnifying 190, 208, and 230 times, two concave lenses magnifying 200 and 220 times, and an erecting eye-piece of three convex lenses, magnifying about 125 times. It had also a small refracting telescope as a finder, which, we believe, was first suggested by Descartes, and the whole was mounted upon a stand, ingeniously and elegantly constructed.[2] The celebrated Dr. Bradley, and the Rev. Mr. Pound of Wanstead, compared it with the great Huygenian refractor 123 feet long, and they saw with the reflector, though less brightly, "whatever they had hitherto disco-

[1] See Prof. Rigaud's *Biographical Account of John Hadley, Esq.*, pp. 7-11.

[2] *Phil Trans.* vol. xxxii. No. 376, March and April, 1723, p. 303.

vered with the Huygenian, particularly the transits of Jupiter's satellites, and their shadows over the disc of Jupiter, the black list in Saturn's ring, and the edge of the shadow of Saturn cast on his ring. They also saw with it several times the five satellites of Saturn."[1] Mr. Hadley himself and others likewise saw the preceding phenomena together with the belts of Saturn, and the first and second satellites of Jupiter, as bright spots on the body of the planet.[2]

After executing another Newtonian telescope of the same size, Mr. Hadley directed his attention to those of the Gregorian form, upon which he made great improvements. In 1726 he communicated to Dr. Desaguliers an account of the instrument as perfected by himself, with tables showing the relative proportions of its different parts; and in 1734 he made an additional communication to the same writer, in reference to the use of a double eye-glass, for "preventing the objects being coloured near the edges of the field."[3] Not content with the labours of his own hands, Mr. Hadley, who was now Vice-President of the Royal Society, was desirous of enabling astronomers and opticians to manufacture these valuable instruments, the former for use in their observatories, and the latter for public sale. He accordingly inspired Dr. Bradley with the desire of constructing these instruments, and with his directions "he succeeded pretty well, and would probably have perfected one of them, had he not been obliged suddenly to remove from the place where he then dwelt, and been since diverted from it by other avocations." Soon afterwards, however, Dr. Bradley with Mr. Samuel Molyneux, renewed the attempt at Kew, by making an instrument about 26 inches long; but notwithstanding Dr. Bradley's experience and Mr. Hadley's frequent instructions, a long time elapsed before they could "tolerably succeed." At last, however, they completed to their satisfaction a telescope of the Newtonian form of the above focal length. They afterwards

[1] *Phil. Trans.* July and August 1723, p. 382.

[2] Gregory's *Catoptrics*, pp. 250, 285.

[3] *Ibid.*, p. 385.

made a pretty good one of seven inches, and one of eight feet, the largest that had yet been made.[1] The first of these instruments was elegantly fitted up on a highly ornamented stand, and presented by Mr. Molyneux to his Majesty John v. of Portugal.[2]

Hitherto no optician but Mr. Hawksbée had ventured to construct these instruments for sale. He executed a good one of about 3½ feet in focal length,[3] and other two of six and twelve feet, and he was the first person, as Molyneux informs us, "who had attempted it without the assistance of a fortune, which could well bear the disappointment."

Having acquired, by his own experience and Mr. Hadley's instructions, a sufficient knowledge of the art, Mr. Molyneux communicated the whole process[4] to Mr. Edward Scarlet, his Majesty's optician, and to Mr. Hearne, a mathematical instrument maker, and both these artists attained to such perfection in constructing them, that they manufactured them for public sale. In this way the Reflecting Telescope came into general use, and, principally in the Gregorian form, it has been an article of trade with every regular optician.

While the English opticians, with the aid of Molyneux and Hadley, were thus practising the new art of grinding and polishing specula, Mr. James Short of Edinburgh, without any such aid, was devoting to the subject all the energies of his youthful mind. In the year 1732, and in the 22d year of his age, he began his labours; and to such perfection did he carry the art of grinding and polishing metallic specula, and of giving them the true parabolic figure, that with a telescope of 15 inches in focal length, he and Mr. Bayne, Professor of Law

[1] The Hon. Samuel Molyneux and Hadley in Smith's *Optics*, vol. ii. p. 302, § 782.

[2] *Ibid.*, p. 363, § 913.

[3] This telescope, according to Dr. Smith, was so excellent that it was scarcely inferior to Hadley's of 5 feet 2½ inches in length. It bore a power of 226, as determined by Mr. Hawksbee, Mr. Folkes, and Dr. Jurin. See Smith's *Optics*; Remarks, p. 79.

[4] This process, drawn up partly by Molyneux and partly by Hadley, is printed in Dr. Smith's *Optics*, vol. ii. p. 301.

in the University of Edinburgh, read the Philosophical Transactions at the distance of 500 feet, and several times, particularly on the 24th of November and the 7th of December 1734, they saw the five satellites of Saturn together, an achievement beyond the reach of Hadley's six-feet telescope. Mr. Short had constructed several instruments, 9, 6, 4, and $2\frac{6}{10}$ inches in focal length. With those four inches long he saw the satellites of Jupiter very well, and read in the Philosophical Transactions at the distance of 125 feet. With the six-inch ones he read at the distance of 160 feet, and with the nine-inch ones at the distance of 160 feet. The celebrated Colin Maclaurin compared one of the six-inch ones with one of the best London ones of $9\frac{3}{10}$ inches, and found that it exceeded it in brightness, distinctness, and magnifying power. It surpassed also another London one, $11\frac{1}{2}$ inches in focal length.[1] After Short had established himself in London in 1742, he received £630 for a twelve-feet reflector from Lord Thomas Spencer. In 1752 he executed one for the King of Spain for £1200; and a short time before his death, which took place in 1768, he finished the specula of the magnificent telescope which was mounted equatorially for the Observatory of Edinburgh, by his brother Thomas Short. The King of Denmark offered twelve hundred guineas for this instrument, through which we have often seen the leading celestial phenomena, but not till the large speculum had been greatly injured in consequence of having been repolished by an inferior artist.[2]

Notwithstanding these great improvements on the Reflecting Telescope, no discovery of importance had yet been achieved by them. The ordinary refractors of Huygens, and those of Campani in the hands of Cassini, though they laboured under all the imperfections of coloured light, had made the latest dis-

[1] Maclaurin in Smith's *Optics*, vol. ii., Remarks, p. 81.

[2] This telescope was removed from the Observatory upon the establishment of the Astronomical Institution, and is, we believe, now lying dismantled in some garret of the city.

coveries in the heavens ; and nearly three quarters of a century had elapsed without any extension of our knowledge of the Solar and Sidereal Systems. This, however, was only one of those stationary intervals during which human genius holds its breath, in order to take a new and a loftier flight. The power of the Refracting Telescope, extended to the unmanageable length of above *two hundred* feet, had been strained to the very utmost, and the Reflectors, vigorous and promising in their infancy, were about to attain an efficiency and magnitude which the most sanguine astronomer had never ventured to anticipate. It was reserved for Sir William Herschel and the Earl of Rosse to accomplish this great work, and by the construction of telescopes of gigantic size to extend the boundaries of the Solar System—to lay open the hitherto unexplored recesses of the sidereal world, and to bring within the grasp of reason those nebular regions to which imagination had not ventured to soar.

Anxious to observe with his own eyes the wonders of the planetary system, and, fortunately for science, unable to purchase a telescope for himself, Sir William Herschel resolved, in 1774, to construct one with his own hands. With this instrument, which was a Newtonian reflector of five feet, he saw distinctly the ring of Saturn and the satellites of Jupiter. Dissatisfied with its performance, he afterwards executed *two hundred* specula of *seven* feet focal length, *one hundred and fifty* of *ten* feet, and above *eighty* of *twenty* feet ! In 1781 he began a thirty-feet aerial reflector, with a speculum three feet in diameter, but as it was cracked in the operation of annealing, and as another of the same size was lost in the fire from a failure in the furnace, his hopes were disappointed. In minds like his, however, disappointment is often a stimulus to higher achievements, and the double accident which befell his specula suggested, no doubt, the idea of making a still larger instrument, and of obtaining pecuniary aid for its accomplishment. He accordingly conveyed, through Sir Joseph Banks, to the King his intention to execute such a telescope, and his Majesty, with the munificent spirit of a great sovereign, instantly offered to defray the whole expense of its construction. Encouraged by this noble act of liberality, Sir William Herschel began in 1785, and completed in 1789, his gigantic telescope, *forty feet* in focal length, with a speculum *forty-seven and a half inches* in diameter ! Its tube, about *forty feet* long and *five* wide, was made of iron, and the observer, suspended in a moveable seat at the mouth of it, examined, with what is called the *front view*, the celestial objects to which it was directed. This noble instrument, now dismantled, stood in the lawn of Sir William Herschel's house, and some of our readers may remember, like ourselves, its extraordinary aspect when visiting the great astronomer himself, or resting in the Crown Hotel at Slough, or journeying on their way to Windsor.

It is due to the memory of George III., that the friends of science should cherish it with respect and gratitude. By enabling Sir William Herschel to construct his colossal tube, and to spend the whole of his time in applying it to the heavens, he was entitled to share in the glory of his discoveries ; and we owe it to historical truth to say, that none of the sovereigns who either preceded or followed him have an equal claim on the homage of astronomers. If, in his imperial rule, he sometimes transcended the limits of constitutional government, let us remember that he left the throne more secure and glorious than he found it. If he ventured, on some occasions, to thwart the counsellors of his choice, we may find some apology for the exercise of a high prerogative in the factious character of the age, and in the acknowledged incapacity of his advisers ;—and if he lost a transatlantic empire by persisting to levy tribute from its people, he followed the advice of distinguished counsellors, and was but the instrument of a higher power in establishing a mighty nation veined with Saxon blood, and nerved with British spirit,—destined to give lessons of civilisation to the Eastern World—to afford a home to science unpatronized—to religion in persecution, and to patriotism in exile.

Stimulated by such patronage, the genius and perseverance which created instruments so transcendent in magnitude, were not likely to be baffled in their practical application. In the examination of the starry heavens, the ultimate object of his labours, Sir William Herschel exhibited the same exalted qualifications ; and in a few years he rose from the level of humble life to the enjoyment of a name more glorious than that of the sages and warriors of antiquity, and as enduring as the objects with which it will be for ever associated. Nor was it in the ardour of the spring of life that these triumphs were achieved. He had reached the middle of his appointed course before his career of discovery began, and it was in the autumn and winter of his days that he reaped the full harvest of his glory. The discovery of a new planet at the verge of the Solar System, was the first trophy of his skill, and new double and multiple stars, and new nebulæ and groups of celestial bodies, were added in hundreds to the system of the universe. The spring tide of knowledge, which was thus let in upon the human mind, continued for a while to spread its waves over Europe, but when it sank to its ebb in England, there was no other bark left upon the strand but that of the Deucalion of science, whose home had been so long upon its waters.[1]

When Sir William Herschel's great telescope was taken down in 1822, a telescope of 20 feet in focal length, and with an aperture of $18\frac{1}{4}$ inches, was erected in its place by his son, Sir John Herschel. This instrument, with three mirrors of the same size, was carried to the Cape of Good Hope, and it was with it that Sir John made those valuable observations which have added so greatly to our knowledge of Sidereal Astronomy.

About the same time, the late Mr. John Ramage, a merchant

[1] For an account of the Decline of Science in England, here alluded to, we refer the reader to Sir John Herschel's *Treatise on Sound*, to Mr. Airy's *Report on Astronomy*, in the Report of the British Association for 1833, and to Mr. Babbage's interesting volume, *On the Decline of Science*. See also *Quarterly Review*, October 1830, and *North British Review*, vol. xiv. p. 235.

in Aberdeen, devoted much of his attention to the construction of large Newtonian reflectors. He ground and polished specula of 13½, 15, and 21 inches in diameter. One of these was erected at the Royal Observatory of Greenwich, in 1820,[1] with a focal length of 25 feet, and a speculum 15 inches in diameter; another of the same size at Sir John Ross's Observatory, near Stranraer;—and the large speculum of 21 inches is, we believe, in the Observatory of Glasgow.[2]

The long interval of half a century seems to be the period of hybernation during which the telescopic mind rests from its labours, in order to acquire strength for some great achievement: Fifty years elapsed between the dwarf telescope of Newton and the large instruments of Hadley: Other fifty years rolled on before Sir William Herschel constructed his magnificent telescope; and fifty years more passed away before the Earl of Rosse produced that colossal instrument which has already achieved such brilliant discoveries.

This distinguished nobleman began his experiments so early as 1828, and he ground and polished specula fifteen inches, two feet, and three feet in diameter, before he commenced the Herculean attempt of executing a speculum *six feet* in diameter, and with a focal length of *fifty* feet. The speculum was cast on the 13th April 1842, ground in 1843, polished in 1844, and, in February 1845, the telescope was ready to be tried. The focal length of the speculum is fifty-four feet. It weighs four tons, and, with its supports, it is seven times as heavy as the four-feet speculum of Sir William Herschel. The speculum is placed in one of the sides of a cubical wooden box s, *Fig.* 6, about eight feet wide; and to the opposite end of this box is fastened the tube, which is about fifty feet long, eight feet in diameter in the middle, but tapering to seven at the extremities, and

[1] See *Transactions of the Astronomical Society*, vol. ii. p. 413.

[2] A fine reflecting telescope, with a speculum two feet in diameter, and a focal length of twenty feet, has been recently constructed by Mr. Lassels, who has made with it several important discoveries within the limits of our own system.

furnished with diaphragms $6\frac{1}{2}$ feet in aperture. The tube is made of deal staves an inch thick, hooped with strong iron clamp rings, and it carries at its upper end, and in the axis of the tube, the small oval speculum A, six inches in its lesser diameter.

The telescope, as shown in the annexed figure, is established between two lofty castellated piers sixty feet high, and is raised to different altitudes by a strong chain cable B attached to the top of the tube. This cable passes over a pulley T on the frame

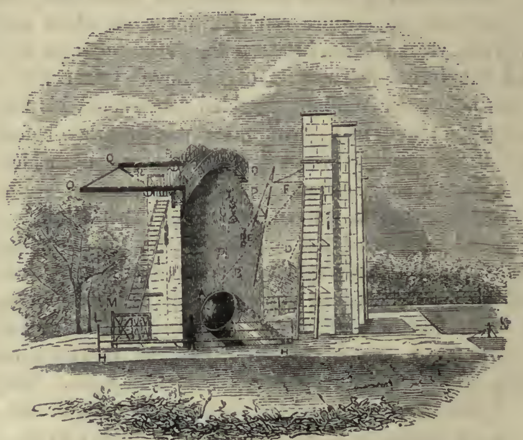


FIG. 5.—Lord Rosse's Telescope from the South-East.

F down to a windlass shown at U in *Fig.* 6, on the ground, which is wrought by two assistants. To the frame F are attached, at X, X, chain guys fastened to the counterweights E, E. The telescope is balanced by these counterweights suspended by chains D, D, which are fixed to the sides of the tube, and pass over large iron pulleys C, C.

To the *eastern* pier is fixed a strong semicircle of cast-iron

VV, about eighty-five feet in diameter. The telescope is connected with this circle by a strong racked bar W, with friction-rollers attached to the tube by wheel-work, so that by means of a handle near the eye-piece, the observer can move the telescope along the bar on either side of the meridian to the distance of an hour for an equatorial star.

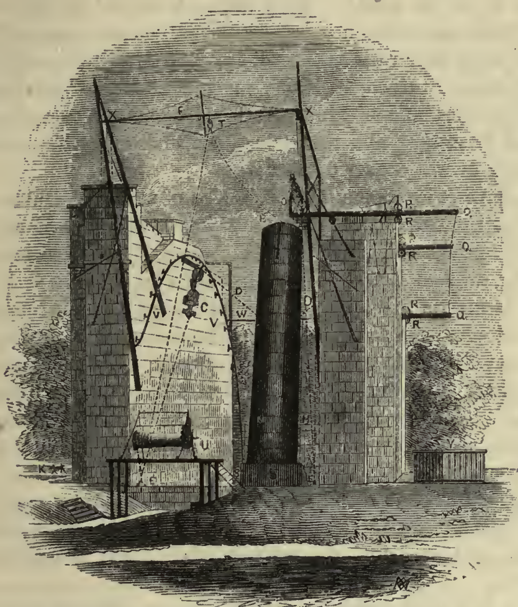


FIG. 6.—Lord Rosse's Telescope from the North-West.

On the *western* pier are erected the stairs and galleries for the observers. The *first* gallery, shown at H, H below the tube, *Fig.* 5, commands an altitude of 42°. It is a light but strong framing of wood, which slides between two ladders I, I, fixed to the southern face of the piers. It is counterpoised by a weight, and raised to the height required by a windlass K. Upon its upper plane is a railway upon which the observing gallery L can be moved about 24 feet east and west by means of two wheels turned by a winch M near the observer. Other three galleries, N, O, P, command all altitudes above 42°, and within 5° of the zenith. They are each carried by two beams Q, Q, which run between pairs of grooved wheels R, R, and these beams, with their respective galleries, are drawn forward when the wheels are turned by a very ingenious piece of mechanism. These galleries hold twelve persons, and strangers are not a little startled when they find themselves suspended, midway between the piers, over a chasm 60 feet deep.[1]

We have enjoyed the great privilege of seeing and using this noble instrument, one of the most wonderful combinations of art and science which the world has yet seen. We have, in the morning, walked again and again, and ever with new delight, along its mystic tube, and, at midnight, with its distinguished architect, pondered over the marvellous sights which it discloses,—the satellites, and belts and rings of Saturn,—the old and new ring, which is advancing with its crest of waters to the body of the planet,—the rocks, and mountains, and valleys, and extinct volcanoes of the moon,—the crescent of Venus, with its mountainous outline,—the systems of double and triple stars,—the nebulæ and starry clusters of every variety of shape,—and those spiral nebular formations which baffle human comprehension, and constitute the greatest achievement in modern discovery.

Such is a brief description of the gigantic telescope completed by the Earl of Rosse. In order to form a correct idea of its effective magnitude, we must compare it with other instruments, as in the following table, in which the specula are supposed to be square in place of round :—

[1] A box containing a second speculum is shown at Y.

Names of Makers.	Diameter of Speculum.	Area of Surface.
Newton,	1 inch,	1 square inch.
——	2·37 ,,	5·6 ,,
Hadley.	4·5 ,,	20 ,,
——	5 ,,	25 ,,
Hawksbee,	9 ,,	81 ,,
Ramage,	21 ,,	441 ,,
Lassels,	2 feet,	576 ,,
Lord Rosse,	2 ,,	576 ,,
——	3 ,,	1296 ,,
Herschel,	4 ,,	2304 ,,
Lord Rosse,	6 ,,	5184 ,,

Next in interest to the telescopes of Lord Rosse are those of M. Foucault, who deposits a film of pure silver upon the spherical surface of a disc of glass. After the silver surface has been polished by the hand, he modifies the figure by local retouches, and converts the sphere into an ellipsoid, and then into a paraboloid, so as to remove the spherical aberration. By this method he has produced a telescope with a speculum *thirteen* inches in diameter, and *eight* feet in focal length, which separates the two small stars which compose the *blue* star of γ Andromedæ, a result obtained by M. W. Struve with the great achromatic of Palkowa.

In looking back on what the telescope has accomplished since the time of Newton, and in reflecting on the vast depths of ether which have been sounded ;—on the number of planetary bodies which have been added to our system, and on the extensive fields of sidereal space which have been explored, can we hesitate to believe it to be the Divine plan that man shall yet discover the whole scheme of the visible universe, and that it is his individual duty, as well as his high prerogative, to expound its mysteries and to develop its laws ? Over the invisible world he has received no commission to reign, and into its secrets he has no authority to pry. It is over the material and the visible that he has to sway the intellectual sceptre,—it is among the structures of organic and inorganic being that his functions of combination and analysis are to be chiefly exercised. However great have been the achievements of the past, and however magnificent the instruments to which we owe them, the limits of telescopic vision have not been reached, and space has yet marvellous secrets to surrender. A reflector *ten feet* in diameter will be due to science before the close of the century, and a disc of flint-glass,[1] 29 *inches* in diameter, awaits the command of some liberal government, or some munificent individual, to be converted into an achromatic telescope of extraordinary power.

In cherishing these sanguine expectations, we have not forgotten that the state of our northern atmosphere must set some limit to the magnifying power of our telescopes. In a variable climate, indeed, the vapours and local changes of temperature, and consequent inequalities of refraction, offer various obstructions to astronomical research. But we must meet the difficulty in the only way in which it can be met. The astronomer cannot summon the zephyrs to give him a cloudless sky, nor command a thunderstorm to clear it. He must transport his telescope to the purer air of Egypt or India, or climb the flanks of the Himalaya or the Andes, to erect his watch-tower above the grosser regions of the atmosphere. In some of those brief yet lucid intervals, when distant objects present themselves in sharp outline and minute detail, discoveries of the highest value might be grasped by the lynx-eyed astronomer. The resolution of a nebula,—the bisection of a double star,—the detection of new asteroids ;—the details of a planet's ring,—the evanescent markings on its disc,—the physical changes on its surface, and perchance the display of some of the dark worlds of Bessel, might be the revelations of a moment, and would amply repay in national glory the transportation of a huge telescope to the shoulder or to the summit of a lofty mountain.[2]

[1] This disc of flint-glass was executed by Messrs. Chance Brothers and Company, of the Smethwick Glass-works, and was rewarded with a council medal of the Great Exhibition.—See *Reports of the Juries*, p. 529.

[2] This proposal, which was first made by the author in September 1844, is likely to be now carried into effect. A committee of the British Association, and of the Royal Society, have applied to Government for the necessary funds.

CHAPTER IV.

Newton writes Notes on Kinkhuysen's Algebra—and on Harmonic and Infinite Series—Delivers Optical Lectures at Cambridge—Is elected a Fellow of the Royal Society—Communicates to them his Discoveries on the different Refrangibility and Nature of Light—Popular account of them—They involve him in various Controversies—His Dispute with Pardies—with Linus—with Gascoigne and Lucas—The Influence of these Disputes on his Mind—His Controversy with Dr. Hooke and Monsieur Huygens, arising from their Attachment to the Undulatory Theory of Light—Harassed with these Discussions he resolves to publish nothing more on Optics—Intimates to Oldenburg his Resolution to withdraw from the Royal Society from his inability to make the Weekly Payments—The Council agree to dispense with these Payments—He is allowed by a Royal Grant to hold his Fellowship along with the Lucasian Chair without taking Orders—Hardship of his situation in being obliged to plead Poverty to the Royal Society—Draws up a Scheme for extending the Royal Society, by paying certain of its Members—The Scheme was found among his Papers—Soundness of his Views relative to the Endowment of Science by the Nation—Arguments in support of them.

WHILE Newton was constructing his Reflecting Telescope, and discussing with Gregory and others the question of its superiority to instruments of the Gregorian and Cassegrainian form, his mind was directed to a variety of other subjects. Dr. Barrow had requested him, through Collins, to write some notes to be appended to a Latin translation from the Dutch, of Kinkhuysen's Algebra, a task which he readily undertook, and which occupied some considerable portion of his time during the years 1669 and 1670. He at first did not think the work "worth the pains of a formal comment," and returned the book with his notes, "intermixed with the author's discourse," requesting Collins not to mention his name, but merely to say that "it was enriched by another author." In thanking him for his valuable additions, Collins intimated that the part on surd numbers had been "too lightly handled," and requested Newton to point out in several books on surds which he sent to him, such passages as might be added to Kinkhuysen, to supply the defect. Newton kindly offered to make the necessary additions, and having learned from his correspondent that his "pains" in this matter "would be acceptable to some very eminent grandees of the Royal Society, who must be made acquainted therewith," he got back his MSS., and added only two or three examples more, as upon revising the papers he "judged it (the part on surds) not so imperfect as he thought it had been."[1]

His attention had also been directed by Collins to problems on the summation of harmonic series, and in the determination of the rate per cent. in annuity problems, when all the other quantities were given. In sending the solution of the problems, he gives Collins permission "to insert it in the Philosophical Transactions, so it be without his name to it." "For I see not," he adds, "what there is desirable in public esteem were I able to acquire and maintain it. It would perhaps increase my acquaintance, the thing which I chiefly study to decline."

In the month of July 1670, he had intended, during the Duke of Buckingham's installation as Chancellor of the University of Cambridge, to pay a visit to his friends in London, and to give Mr. Collins "a verbal acknowledgment of his undeserved favours;" but he was prevented "by the sudden surprisal of a fit of sickness, which not long after (God be thanked) I again recovered of."

During the winter of this year, Newton had begun to "methodize his Discourse of Infinite Series,[2] designing to illustrate it with problems," but he was "suddenly diverted from it by some business in the country," and was not able to resume the

[1] Letters to Collins from 1669 to September 27, 1670.—*Macclesfield Correspondence*, vol. ii.

[2] This work was never finished. It was published by Horsley, under the title of *Geometria Analytica*, from three different MSS.—See Newtoni *Opera*, tom. i. pp. 391-518., A translation of it had been published by Colson in 1736.

subject till towards the end of the year, when he was prevented by other avocations from preparing it for the press.

Although our author had read a course of lectures on Optics, in the University of Cambridge, in the years 1669, 1670, and 1671, containing his principal discoveries regarding the different refrangibility of light, and towards the end of 1671 was preparing a series of twenty of them for the press, yet it is a singular fact that these discoveries should not have become public through the conversation or correspondence of his pupils. The members of the Royal Society even had acquired no knowledge of them till the beginning of February 1672, and it was chiefly on his Reflecting Telescope that his reputation in that body was founded. So great indeed was the interest which it excited, that Dr. Seth Ward, Bishop of Salisbury, who had written some able works on Astronomy, and filled the Savilian Chair of Astronomy at Oxford, proposed Mr. Newton as a Fellow of the Royal Society on the 23d December 1671. In a letter to its secretary, Mr. Oldenburg, of the 6th January, he expressed his satisfaction with this event in the following words :—" I am very sensible of the honour done me by the Bishop of Sarum in proposing me a candidate, and which I hope will be further conferred upon me by my election into the Society ; and if so, I shall endeavour to testify my gratitude by communicating what my poor and solitary endeavours can effect towards the promoting your philosophical designs." He was accordingly elected on the 11th January, on which day the Society, with the view of securing his invention of the telescope from foreign piracy, agreed to transmit a drawing and account of it to Huygens at Paris. The notice of his election, and the thanks of the Society for the communication of his telescope, were contained in the same letter, with an assurance that the Society " would take care that all right should be done him in the matter of this invention." In replying to this letter, Newton very justly expressed his surprise to see " so much care taken about securing an invention of which I have hitherto had so little value, and therefore since the Royal Society is pleased to think it worth the patronizing, I must acknowledge it deserves much more of them for that, than of me, who, had not the communication of it been desired, might have let it still remain in private, as it hath already done some years."

Thus encouraged by the Royal Society, Newton lost no time in making other communications to them. In his very next letter to their secretary, dated 18th January 1672, he announces his optical discoveries in the following manner:—" I desire that in your next letter you would inform me for what time the Society continue their weekly meetings; because, if they continue them for any time, I am purposing them to be considered of and examined on account of a philosophical discovery, which induced me to the making of the said telescope, and which I doubt not but will prove much more grateful than the communication of that instrument, being in my judgment the oddest if not the most considerable detection which hath hitherto been made in the operations of nature."

This " oddest and most considerable detection" was the discovery of the different refrangibility of the rays of light, which it was necessary to explain in a previous chapter, as having been made before the construction of his telescope. It was communicated in a letter to Oldenburg on the 6th of February 1672, and excited great interest when read on the 8th February to " that illustrious company." The " solemn thanks of the meeting were voted to its author for his very ingenious discourse;" and it was immediately printed in the 80th Number of their Transactions, namely, on the 19th February, both for the purpose of having it well considered by philosophers, and for " securing the considerable notices thereof to the author against the arrogations of others." At the same time a committee, consisting of Dr. Seth Ward, Bishop of Salisbury, Mr. Boyle, and Dr. Hooke, was appointed to peruse and consider it, and to give in a report upon it to the Society.

The kindness of this distinguished body, and the anxiety which they had already shown for Newton's reputation in the affair of his telescope, excited on his part a reciprocal feeling, and he accepted of their proposal to print his discourse in the following humble terms :—" 'Twas an esteem," he says, " of the Royal Society, for candid and able judges in philosophical matters, which encouraged me to present them with that discourse of light and colours, which since it has been so favourably accepted of, I do earnestly desire you to return them my cordial thanks. I before thought it a great favour to have been made a member of that honourable body ; but I am now more sensible of the advantage. For believe me, Sir, I do not only esteem it a duty to concur with them in the promotion of real knowledge, but a great privilege, that instead of exposing discourses to a prejudiced and censorious multitude (by which means many truths have been baffled and lost), I may with freedom apply myself to so judicious and impartial an assembly. As to the printing of that letter, I am satisfied in their judgment, or else I should have thought it too strait and narrow for public view. I designed it only to those that know how to improve upon hints of things, and therefore, to shun tediousness, omitted many such remarks and experiments as might be collected by considering the assigned laws of refraction, some of which I believe, with the generality of men, would yet be almost as taking as any of those I described. But yet since the Royal Society have thought it fit to appear publicly, I leave it to their pleasure ; and, perhaps, to supply the aforesaid defects, I may send you some more of the experiments, to record it (if it be so thought fit) in the ensuing Transactions."

Having in the preceding chapter given an account of the leading doctrine of the different refrangibility of the rays of light, and of the attempts to improve the reflecting telescope which that discovery suggested, we shall now endeavour to make the reader acquainted with the other discoveries respecting colours, which he at this time communicated to the Royal Society.

We have already seen that a beam of white light emitted from the sun, and refracted by a prism, is decomposed by its action into seven different colours, which compose what is called the *Prismatic Spectrum*, and which is nothing more than an elongated image of the sun, its length being *five* times its breadth, and the coloured spaces having the proportions shown in the annexed figure.

When this spectrum is distinctly formed by a good prism, so so that the different colours are clearly separated, Newton found that any particular colour, such as *red*, was not susceptible of any change either by refraction through prisms, or reflection from mirrors, or from natural bodies, nor by any other cause that he could observe, notwithstanding his utmost endeavours to change it. It might become fainter or brighter, but its colour never changed. Its refrangibility, too, was equally unchangeable, and hence he drew the conclusion that the same degree of refrangibility always belonged to the same colour, and the same colour to the same degree of refrangibility.

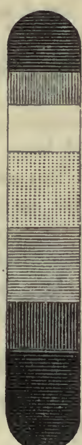


Fig. 7.

But while the colours in the spectrum are original and simple, such as *red*, *orange*, *yellow*, *green*, *blue*, *indigo*, and *violet*, other colours may be compounded of these, "for a mixture of *yellow* and *blue* makes *green*, and *red* and *yellow* makes *orange*, and *orange* and *yellowish green* makes *yellow*." These compound colours, however, may be separated by the prism into their simple colours, and hence we are enabled by the prism to decompose all such colours, and however similar they may be to the primitive ones, their difference may always be discovered by the different refrangibility of their elements.

But, as Newton remarks, "the most surprising and wonderful composition is that of *whiteness*. No one sort of rays is alone capable of exhibiting it. It is ever compounded, and for its composition all the primary colours in their due proportion are required." In order to prove this doctrine, which is called the *Recomposition* of white light, he employed three different methods. When the beam of white light RR, *Fig.* 8, was separated into its component colours, as in the spectrum MN, he received the refracted pencil R′ on a second prism BCD, held

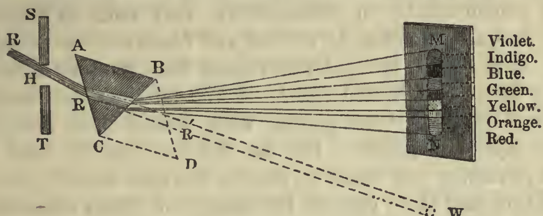


FIG. 8.

either close to the first, or a little behind it, and by the opposite refraction of this prism they were all refracted back into a beam of perfectly white light BW, which projected a white circular spot on the wall at W, exactly similar in form and in colour to the spot formed there by the beam RR, before the prism intercepted it.

Another mode of recomposing white light, which Newton tells us he "often beheld with admiration," is to cause the spectrum to fall upon a large lens at some distance from the prism, and then to converge all the colours into a spot, and mix them again as they were in the light before its incidence on the prism. The light thus reproduced is "entirely and perfectly white," and does not "at all sensibly differ from the direct light of the sun, unless when the glasses used were not sufficiently clear." Hence our author concludes, "that whiteness is the usual colour of light; for light is a confused aggregate of rays endued with all sorts of colours, as they are promiscuously darted from the various parts of luminous bodies." When there is a due proportion of the ingredients, that is, of all the simple colours, *whiteness* is generated, but if any one colour predominate, the light will incline to that colour, as in the yellow flame of a candle, the blue flame of brimstone, and the various colours of the fixed stars.

From a consideration of these facts, our author regards it as very evident how colours are produced by the prism. Since such of the rays constituting white light as differ in colour, differ proportionally in refrangibility, they must in virtue of their unequal refraction be severed and dispersed into an oblong form, as in *Fig.* 7, in regular succession from the least refracted *red* to the most refracted *violet*. "And for the same reason it is," says Newton, "that objects, when looked upon through a prism, appear coloured. For the difform rays, by their unequal refraction, are made to diverge towards several parts of the retina, and there express the images of things coloured, as in the former case they did the sun's image upon a wall."

Having established these principles, Newton applies them to the explanation of several interesting phenomena. He shows that the colours of the primary and secondary rainbow are prismatic spectra, produced by the refraction of the drops of water. He explains the odd phenomena of *lignum nephriticum*, *leaf gold*, *fragments of coloured glass*, and other bodies, which appear in one position of one colour, and of another in another, in consequence of their disposition to reflect one sort of light and transmit another. He assigns the reason of Hooke's beautiful experiment, with two wedge-like transparent vessels, the one filled with *red*, the other with a *blue* liquor. Although they are each very transparent, yet when the two are put together they are opaque; for since the one transmits only *red*, and the other only *blue*, no rays whatever can pass through both of them. And without giving more instances, he concludes with this general one, "That the colours of all natural bodies have no other origin than this; that they are variously qualified to reflect one sort of light in greater plenty than another. For if we illuminate these bodies with uncompounded light of different colours, they always appear of the colour of the light cast upon them, the colour being most vivid in the light of their own daylight colour. *Minium*, for example, though *red*, appears indifferently of any colour, though most luminous in *red;* and *bise*, though blue, appears indifferently of any colour cast upon it, though most luminous in *blue.* Hence, since *minium* reflects most copiously the red rays, it must appear *red* when illuminated with daylight, that is, with all sorts of rays promiscuously blended, and for the same reason *bise* appears *blue.*

No sooner were these important discoveries given to the world, than they were criticised and assailed with a degree of virulence and ignorance which have not often been combined in scientific controversy. The Royal Society unfortunately contained few individuals of pre-eminent talent capable of appreciating the value of his discoveries, and of entering the lists against his envious and ignorant assailants. While they held his labours in the highest esteem, they regarded his discoveries as fair subjects of discussion, and their secretary regularly communicated to him, and even published in their Transactions, almost all the papers which were written in opposition to his views.

The first communication on this subject was the suggestion of four experiments with the prism, which seems to have been made by some friend at Cambridge, as Newton communicated them to the editor of the Philosophical Transactions, with his own observations.[1] This letter was followed by a communication from a Jesuit, Ignatius Pardies, Professor of Mathematics in the Parisian College of Clermont, containing animadversions upon the new Theory of Colours. Although Newton in his original discourse had demonstrated the reverse, yet the French professor pretended that the elongation of the sun's image arose from the unequal incidence of the different rays on the first face of the prism ; that the mixture of differently coloured

[1] The communication is dated 13th April 1672, and is published in the *Transactions*, No. 82, p. 4059, April 22, 1672.

powders was not white, but dun and grey; and that opacity was not produced when the two coloured liquors were mixed in the same vessel. Newton answered these shallow objections in the most satisfactory manner;[1] but this disciple of Descartes, unwilling to be vanquished, took up a new position, and maintained that the elongation of the sun's image by the prism, might be explained by the diffraction of light on the hypothesis of Grimaldi, or by the diffusion of undulations on the hypothesis of Hooke.[2] Newton replied to these silly speculations on the 11th of June; but he contented himself with reiterating his original experiments, and confirming them by more popular arguments.[3] Pardies replied on the 9th July, in terms highly complimentary to Newton.[4] He expressed himself satisfied with Newton's explanations, acknowledged that his only difficulty had been wholly removed, and that he cherished the warmest gratitude for the kindness with which his annotations had been examined and answered.

About this time Newton seems to have been peculiarly sensitive about the reception of his Doctrine of Colours. On the 8th of July, a month after he had written his second Reply to Pardies, he published in the Philosophical Transactions, with his name, "A Series of Queries, propounded by Mr. Isaac Newton, to be determined by experiment, positively and directly concluding his new Theory of Light and Colours, and here recommended to the industry of the lovers of Experimental Philosophy, as they were generously imparted to the Publisher in a letter of the said Mr. Newton."[5] This paper consists of eight queries, which are merely the different propositions he had established put into that form as if they were still matters of doubt, and it concludes with expressing the wish of the author, "that all objections be suspended taken from *Hypotheses*, or any other heads than these two; of showing the insufficiency

[1] *Phil. Trans.* No. 84, p. 4091, June 17, 1672.
[2] *Phil. Trans.* No. 85, p. 5012, July 15, 1672. [3] *Ibid.* p. 5014. [4] *Ibid.* p. 5018.
[5] *Phil. Trans.* No 84, p. 4080, June 17, 1672. This paper is part of a letter to Oldenburg, dated July 6, 1672, from Stoake Park, Northamptonshire.

of experiments to determine these *Queries*, or prove any other parts of my theory by assigning the flaws and defects in my conclusions drawn from them ; or of producing other experiments which directly contradict me, if any such may seem to occur." In order to "invite and gratify foreigners" to consider and put to trial these Queries, the publisher "delivers Mr. Newton's letter in the language also of the learned."

This challenge to foreigners on the part of Oldenburg, summoned into the field a new combatant, in the person of Francis Linus, a physician in Liege, who, on the 6th October 1674, addressed a letter to a friend in London, entitled, "Animadversions on Newton's Theory of Light and Colours."[1] He asserts that he has often observed the same difference between the length and breadth of the spectrum ; but that he never found it so when the sky was clear and free of clouds near the sun. The difference only appeared when the sun either shone through a white cloud, or enlightened some such clouds near him. The elongation of the spectrum was therefore not affected by the true sunbeams, "and consequently the theory of light grounded on the experiment cannot subsist." In support of these gratuitous assertions, Linus appeals to frequently repeated experiments on the refractions and reflections of light which he had exhibited nearly thirty years ago, "together with divers other experiments on light, to that worthy promoter of experimental philosophy, Sir Kenelm Digby, who coming into these parts to take the Spa waters, resorted oftentimes to my darkened chamber, and took notes upon them ;" and he adds, that if Newton had used the same industry as he did, "he would never have taken so impossible a task in hand," as "to endeavour to explicate the aforesaid difference between the length and breadth of this coloured *spectrum*, by the received laws of refraction." When this letter was shown to Newton he refused to answer it ; but Linus was referred to his answer to Pardies, and assured that the experiments on which he animad-

[1] *Phil. Trans.* No. 110, p. 217.

verted were made in clear days when there was no bright cloud in the heavens. The Dutch philosopher, however, was not satisfied with this reply. He confesses in a second letter to his friend, that "*if the assertions be admitted*, they do indeed directly cut off what he had said of Mr. Newton's being deceived by a bright cloud;" but he endeavours to prove that Newton did not make the experiment in a clear day, because Newton describes the ends of the spectrum as semicircular, "and these semicircular ends are never seen in a clear day!" The rest of the letter abounds with the most erroneous statements, indicating the grossest ignorance, and calculated to irritate even the patient mind of Newton.

Oldenburg again attempted to prevail upon Newton to answer these observations, but he once more declined, on the ground that the dispute referred merely to simple matters of fact, which could only be decided before competent witnesses. The entreaties of Oldenburg, however, prevailed, and "lest Mr. Linus should make the more stir," this great man condescended to write a grave reply to reasonings utterly contemptible, and to assertions wholly unfounded. In this answer, dated November 13, 1675, and which Linus, who died on the 15th November, probably never saw, Newton gives the most minute and simple instructions for producing the prismatic spectrum. He mentions the size of the hole, "about the bigness of a pease," the position of the prism close to the hole, the mode of turning the prism round till the spectrum is placed in its stationary position, when the rays are equally refracted on both sides of the prism, and the nature and order of the colours. He tells him also that the experiment will not succeed well if the day is not clear, and he begs that "when Mr. Line has tried this he will proceed to try the *experimentum crucis*, which," he adds, "may be done, though not so perfectly, even without darkening a room, or the expense of any more time than half a quarter of an hour."[1]

[1] *Phil. Trans.* No. 121, p. 503.

After the death of Linus, his pupil, Mr. Gascoigne, entered the field, and declared that Linus had shown to various persons in Liege his experiment, proving the spectrum to be circular, and that Mr. Newton could not be more confident on his side than they were on the other, being fully "persuaded, that unless the diversity of placing the prism, or the bigness of the hole, or some other such circumstance, be the cause of the difference between them, Mr. Newton's experiment will hardly stand."[1] Pleased with "the handsome genius" of Mr. Gascoigne's letter, Newton replied again,[2] exerting himself to discover the reason why the elongated spectrum was not as visible to others as it was to himself. With this view, he describes the three different kinds of images that may be seen upon the wall when a prism refracting the sun's rays is turned round its axis. The first of these is the regular elongated coloured spectrum; the second a round white image formed by reflection from one of the faces of the prism; and the third, an image formed by two refractions and one reflection, which, with a good equi-angular prism, would be a round white image of the aperture, but more or less elongated and coloured, if the two refracting angles were more or less inequal. From this it becomes very probable that Linus never saw the real prismatic spectrum.[3]

As Mr. Gascoigne had not the means of making the experiment thus pointed out, he requested Mr. Anthony Lucas of Liege to make it for him. This ingenious individual, who succeeded Linus in the mathematical chair at Liege, confirmed the leading results of Newton, in so far as the prismatic spec-

[1] *Phil. Trans.* No. 121, p. 503.

[2] *Ibid.* No. 123, p. 556.

[3] A short time before the commencement of this controversy, Linus communicated to the Royal Society a paper entitled *Optical Assertions concerning the Rainbow,* which appeared in their *Transactions,* No. 117, p. 386. How such a paper could have been published by so learned a body seems very incomprehensible. Linus was celebrated as a dial-maker. Mr. Charles Ellis mentions one of his dials at Liege, in which the hours were distinguished by touch, and says that they were "the originals of those formerly in our Privy Gardens."—*Phil. Trans.* No. 283, 1703, vol. xv. p. 1418.

trum was concerned; but he refused to acknowledge the truth of his theory, and made a number of experiments with coloured silks and coloured fluids, which he considered to be subversive of it. His experiments on the length of the spectrum, however, possess a peculiar interest. With a prism having an angle of 60°, and a refractive power of 1·500, he formed the spectrum at the distance of eighteen feet from the window. The hole in the shutter was sometimes one-fifth and sometimes one-tenth of an inch, the distance of the prism from the hole about two inches, and the darkness of the room equal to that of the darkest night when the hole was shut. Under these circumstances, he never could find the spectrum longer than *thrice the diameter* of its breadth, or, at most, *three and a half times* that diameter when the refractions on both sides of the prism were equal; whereas Newton found it to be *five times* that diameter, with a prism whose refracting angle was 63° 12′.

In taking into consideration this new difficulty, Newton acknowledges that a difference of 3° 12′ in the refracting angle of the prism is too little to reconcile the two results, and he conjectures that Mr. Lucas may have set down the round number of 60° as the angle of his prism, in the same manner as he set down its refractive power, or the ratio of the sines, as two to three, or 1·500. "Then," he adds, "if it be two or three degrees less than 60°, if not still less, all this would take away the greatest part of the difference between us." In order, however, to determine the point experimentally, he measured the length of the spectrum with prisms of different angles, and obtained the following results:—In the first column of the following table, he gives the six angles of two prisms which he used, and "which were measured as exactly as he could *by applying them to the angle of a sector*." In the second he gives in inches the length of the image made by each of these angles, its breadth being *two* inches, its distance from the prism *eighteen feet* and *four* inches, and the breadth of the hole in the window-shutter one-fourth of an inch. We have added a third column, showing the ratio of the length to the breadth of the spectrum.

	Angles of the Prism.	Length of Image.	Ratio of its Length to its Breadth.
The first Prism,	56° 10′	$7\frac{3}{4}$ inches.	$3\frac{7}{8}$ to 1.
	60 24	$9\frac{1}{2}$,,	$4\frac{3}{4}$ to 1.
	63 26	$10\frac{1}{3}$,,	$5\frac{1}{6}$ to 1.
The second Prism,	54° 0′	$7\frac{1}{3}$,,	$3\frac{2}{3}$ to 1.
	62 12	$10\frac{1}{3}$,,	$5\frac{1}{16}$ to 1.
	63 48	$10\frac{3}{4}$,,	$5\frac{3}{8}$ to 1.

On a clearer day, with the second prism, he found the lengths of the spectrum to be as follows, about one-fourth of an inch greater than before :—

The second Prism,	54° 0′	$7\frac{2}{3}$ inches.	$3\frac{5}{6}$ to 1.
	62 12	$10\frac{1}{2}$,,	$5\frac{1}{4}$ to 1.
	63 48	11 ,,	$5\frac{1}{2}$ to 1.

In noticing the other experiments of Lucas with differently coloured silks, which he placed in a line, and viewing both through a prism and when placed at the bottom of a square vessel of water, Newton found that "unconcerned persons" always saw them in a line as if they had all suffered the same refraction. He does not, however, point out their insufficiency to prove an equality of refraction, but thanks Mr. Lucas for taking so much pains in examining them, "and so much the more, as he was the first that had sent him an experimental examination of them." He even goes so far as to say that, in a little Treatise on the subject, written before his first communication to the Royal Society, he had actually written down the principal of the experiments which Mr. Lucas had now sent him.

We have been thus minute in describing the experiments of Lucas and Newton on the length of the spectrum, because they have a close connexion with the determination of the different dispersive powers of bodies, which was one of the greatest discoveries of the following century, and led to the invention of the achromatic telescope. There are only two ways in which we can account for the shortness of the spectrum observed by Lucas. His eyes may have been to some extent insensible to violet and blue light, and therefore the spectrum would appear to him much shorter than it really was. If we cut off from Newton's spectrum one and a half inches, to reduce it to Lucas's, we cut off the whole of the indigo and violet spaces; and, unless from an imperfection of vision, Lucas could not have failed to see these colours in an apartment so very dark as his. If he had no such imperfection, it becomes highly probable that his prism was made of glass of a low dispersive power. Newton's prisms may have been of flint-glass, and Lucas's of crown-glass; and it is a remarkable circumstance, that in all these controversies the nature of the glass is never once mentioned. Had Newton been less confident than he was, that all other prisms must give a spectrum of the same length as his, in relation to its refracting angle and index of refraction, the invention of the achromatic telescope would have been the necessary result. The objections of Lucas drove Newton to make experiments which he never contemplated, namely, to measure accurately the lengths of spectra formed with prisms of different angles and different refractive powers; and had the Dutch Professor maintained his opinions with more obstinacy and perseverance, he would have conferred a distinguished favour upon science, and rewarded Newton for all the vexation which had arisen from the minute discussion of his optical discoveries.

Thus terminated the disputes with Pardies, Linus, Gascoigne, and Lucas, and we think it can scarcely be doubted that Newton found it a more difficult task to detect the origin of his adversaries' blunders, and to expose their fallacy, than to establish the great truths which they had attempted to overturn.

Harassing as such a controversy was to a philosopher like Newton, yet it did not touch those deep-seated feelings which characterize the noble and generous mind. It was with ignorance and incapacity only that he had to strive. No personal invective ruffled his equanimity;—no vulgar jealousy roused his indignation ;—no charge of plagiarism called in question his veracity or his honour. These aggravations of scientific controversy, however, he was destined to endure, and in the disputes which he was called to maintain against Hooke, Huygens, and Leibnitz, the agreeable consciousness of grappling with minds of kindred power was painfully embittered by the personal feelings which were thrown into the contest.

Dr. Robert Hooke, born in 1635, was about seven years older than Newton, and was one of the ninety-eight original or unelected Fellows of the Royal Society. He possessed great versatility of talent; yet though his genius was of the most original cast, and his acquirements extensive, he had not devoted himself with fixed purpose to any particular branch of knowledge. His numerous and ingenious inventions, of which we cannot speak too highly, gave to his studies a practical character, unfitting him for that continuous labour which physical researches so imperiously demand. The subjects of light and colours, however, seem to have deeply occupied his thoughts before Newton descended into the same arena, and there can be no doubt that he had made considerable progress in their study. With a mind less divergent in its pursuits, and more fixed in its purpose, he might have unveiled the mystery in which both these subjects were enveloped, and preoccupied the intellectual throne which was destined for his rival; but the infirm state of his health, the peevishness of temper to which it gave rise, the number of unfinished inventions from which he looked both for fortune and fame, and above all, his inordinate love of reputation, distracted and broke down the energies of his powerful intellect. In the more matured inquiries of his rivals he recognised, and often truly, his own incompleted speculations; and when he saw others reaping the harvest for which he had prepared the ground, and of which he had sown the seed, it was not easy to conceal the mortification which their success inspired. In the arbitraments of science, it has always been a difficult task to adjust the rival claims of competitors, when the one was allowed to have completed what the other was acknowledged to have begun. He who commences an inquiry, and publishes its results, often goes much farther than he has announced to the world, and pushing his speculations into the very heart of the subject, frequently submits them to the ear of friendship. From the pedestal of his published labours his rival begins his researches, and brings them to a successful issue, while he has in reality done nothing more than complete the unfinished labours, and demonstrate the imperfect speculations of his rival or his predecessor. To the world and to himself he is no doubt in the position of the principal discoverer, but there is still some apology for his rival, when he brings forward his unpublished labours, and some excuse for the exercise of personal feeling, when he measures the speed of his rival by his own proximity to the goal.

The conduct of Dr. Hooke would have been viewed with some such feeling, had not his arrogance on other occasions checked the natural current of our sympathy. When Newton presented his Reflecting Telescope to the Royal Society, Dr. Hooke not only criticised the instrument with undue severity, but announced, what was never realized, that he possessed an infallible method of perfecting all kinds of optical instruments, so that "whatever almost hath been in notion and imagination, or desired in optics, may be performed with great facility and truth."

Descartes had long ago maintained that an ethereal medium pervaded all transparent bodies;—that light consists in the action of this medium;—that the ether is less implicated in the parts of solid bodies;—that it moves more freely in them, and transmits light more readily through them, so as to accelerate the rays in a certain proportion;—that refraction arises from this acceleration, and has the sines of incidence and refraction proportional;—that light is at first uniform;—that its colours are some disturbance or new modification of its rays by refraction or reflexion ;—that the colours of a prism are made by means of the quiescent medium accelerating some motion of the rays on one side where *red* appears, and retarding it on the other side where *blue* appears, and that there are but these two original colours, or colour-making modifications of light, which, by their various degrees or dilutings, as Hooke calls them, produce all intermediate ones.

These views were adopted by Dr. Hooke, who "changed Descartes' pressing or progressive motion of the medium to a vibrating one ;—the rotation of the globuli to the obliquation of pulses, and the accelerating their rotation on the one hand, and retarding it on the other, by the quiescent medium to produce colours, to the like action of the medium on the two ends of his pulses for the same end."[1]

Such were Hooke's opinions of the nature of light when Newton published his Theory of Colours, and it was through this theoretical medium that he viewed Newton's discoveries, when he sent his observations upon them to the Royal Society, on the 15th February 1672. Dr. Hooke was thanked "for the pains he had taken in bringing in such ingenious reflections ;" but it was not "thought fit to print the two papers together, lest Mr. Newton should look upon it as a disrespect in printing so sudden a refutation of a discourse of his which had met with so much applause at the Society but a few days before."

It is not easy to follow the train of thought which runs through the observations of Dr. Hooke. While he praises "the niceness and curiosity" of Newton's experiments, and expresses an entire agreement with him as to the truth of those which he brought forward, founded on hundreds of trials made by himself, yet he "cannot see in his hypothesis of solving the phenomena of colours thereby, any undeniable argument to convince

[1] This view of Descartes' theory and of Hooke's opinions, is given by Newton in his letter to Oldenburg, dated 21st December 1675. General Dict. vol. vii. p. 783, or *Macclesfield Correspondence*, vol. ii. p. 378.

him of its certainty." He considers them as proving his own hypothesis, which he endeavours, without much success, to explain and establish. This, indeed, seems to be the principal object of his paper, but even if he had succeeded, the truth of his theory would not have invalidated in the slightest degree the doctrines of Newton. "I most readily agree," says he, "with them (Newton's experiments) in every part thereof, and esteem it (his hypothesis) very subtle and ingenious, but I cannot think it to be the only hypothesis, nor so certain as mathematical demonstration." In remonstrating with Newton "on his wholly laying aside the thought of improving telescopes and microscopes by refractions," he is more successful; but though this assertion, that the difficulties of removing the effects of colour are not insuperable, has received ample confirmation, yet the result was not obtained by any of the contrivances which he pretended to possess.

Newton lost no time in replying to Hooke's communication, and he expressed to Oldenburg the gratification which he felt, "that so acute an objector as Hooke had said nothing that could enervate any part of his theory." On the 11th July 1672, he transmitted to Oldenburg an elaborate answer to Hooke,[1] expressing his conviction that both of them "had a sincere endeavour after knowledge, without valuing uncertain speculations for their subtleties, or despising certainties for their plainness." After admitting that he had deduced the "corporeity of light" from his theory of colours, he asserts that the properties of light were in some measure capable of being explained, not only by that theory, but by many other mechanical hypotheses, and that "he had therefore declined them all, and spoken of light in general terms, considering it abstractedly as *something or other* propagated every way in straight lines from luminous bodies, without determining what that thing is." Conscious of the ingenuity and mental power of his opponent, Newton left him no loop-hole for escape, but replied to every

[1] Newtoni *Opera*, tom. iv. pp. 322-342.

objection with a precision and force of argument which Hooke found to be unanswerable. In this remarkable discussion Newton pointed out the true character of experimental philosophy, and the duties of those who cultivate it when rival theories demand their attention. He has shown that the properties of light may be investigated, and its physical laws determined without any other principle than that it is "something propagated every way in straight lines," and that discoveries are not to be valued from their coincidence with the theoretical views of him who made them, or their repugnance to those of his opponents. The discovery of an important fact, or a new law, may confirm one theory and shake another, but he is not a friend to truth who would over-estimate it in the one case, or depreciate it in the other. The true philosopher who forgets his own reputation amid the triumphs of advancing science, and who confides in a theory as a branch of eternal truth, will be the last to spurn from him even experimental results, that may put his own views to the torture. It is the self-seeking sciolist alone who pilfers a laurel at the expense of truth, or the intellectual coward who dreads the ordeal, and questions the decision of experiment and observation. Should the eye of youthful genius rest upon these pages, we would counsel him to ponder over the reply to Hooke, and to remember, in the ardour of his pursuit, that Science has a court of appeal in which posterity is the arbiter.

It would have been well for the progress of science and the tranquillity of its friends, if experiment and observation had been, more than they have, our guides in philosophical inquiry. Even in the present day the disciples of Hooke, who "split pulses" with more success than he did, and whose theory of light has attained a lofty pre-eminence, have not scrupled to imitate their master in measuring optical truths by the undulatory standard, and in questioning and depreciating labours, that it cannot explain, or that run counter to its deductions. There is fortunately, however, a small remnant in the Temple of

Science, who, while they give to theory its due honours and its proper place, are desirous, as experimental philosophers, to follow in the steps of their great Master.

After silencing the most powerful of his adversaries, Newton was unexpectedly summoned to defend himself against a new enemy. The celebrated Christian Huygens, an eminent mathematician and natural philosopher, who, like Hooke, had maintained the undulatory theory of light, transmitted to Oldenburg on the 14th January 1673, a letter from Paris, containing some considerations on Newton's Theory of Light ; but though his knowledge of optics was of the most extensive kind, his objections were as groundless, and his speculations as erroneous as those of his less enlightened countrymen. Attached to the undulatory hypothesis, he seems, like Dr. Hooke, to have viewed the theory of Newton as calculated to overturn it, and he therefore objects to its two leading doctrines, namely, the composition of white light by the union of all the colours, and the generality of the doctrine of their different refrangibilities. The objection which he urges against the theory of *whiteness* is, that it may be produced equally well by *yellow* and *blue*, and "he does not see why Mr. Newton doth not content himself with these two colours, as it will be much more easy to find a *hypothesis* by motion that will explicate these two differences, than for so many diversities as there are of other colours ; and till he hath found this hypothesis, he has not taught us what it is wherein consists the nature and difference of colours, but only this accident (which certainly is very considerable) of their different refrangibility." He then proposes that the experiment should be tried of stopping all the colours but *yellow* and *blue* and *green*, and then mixing them on paper to see if they make the paper white, "as well as when they all give light." Nay, he adds the following extraordinary opinion, as if it were a new and happy thought. "I even doubt," says he, "whether the *lightest place of the yellow colour may not all alone produce that effect*, and I mean to try it at the first conveniency ; for this thought never came into my mind but just now. Meantime you may see that if these experiments do succeed it can no more be said that all the colours are necessary to compound white; and that 'tis very probable that all the rest are nothing but degrees of *yellow* and *blue* more or less changed."

On the subject of the difference of refrangibility, he is equally wrong, though with more reason for his error. He remarks, that the picture formed in a dark room by an object-glass of twelve feet, is too distinct and too well defined to be "produced by rays that would stray the fiftieth part of the aperture; so that (as I believe I have told you heretofore) the difference of the refrangibility doth not, it may be, always follow the same proportion in the great and small inclinations of the rays upon the surface of the glass."[1]

To these extraordinary objections, Newton replied on the 3d April 1673,[2] and also in another paper which immediately follows the observations of Huygens, the first of these answers having been, as we are informed by the editor, mislaid, otherwise it should have also immediately followed the letter of Huygens. In these answers, Newton shows that the *yellows* and *blues* which could produce *white*, are not simple but compound; and he explains more minutely how the existence of an aberration equal to the fiftieth of the aperture, is compatible with the distinctness of a picture formed by a twelve-feet object-glass. Huygens, still dissatisfied with the explanations so patiently given to him, informs Oldenburg that he has still "matter to answer them, but seeing that Newton maintains his opinion with so much concern, he list not to dispute." Newton was not pleased with this criticism upon his explanations, and says in his letter to Oldenburg,—"As for Mr. Huygens' expression, I confess it was a little ungrateful to me to meet with objections which had been answered before, without

[1] *Phil. Trans.* vol. viii. No. 96, p. 6086, July 1693.
[2] *Phil. Trans.* No. 97, p. 6108.

having the least reason given me why those answers were insufficient."[1]

But though Huygens appears in this controversy as a rash and unreasonable objector to the Newtonian doctrine of colours, it was afterwards the destiny of Newton to play a similar part against the Dutch philosopher. When Huygens published his beautiful law of double refraction, founded on the finest experimental analysis of the phenomena, though presented as a result of the undulatory theory, Newton not only rejected it, but substituted for it another law entirely incompatible with the experiments of Huygens, which Newton himself had praised, but with those of all succeeding philosophers.[2]

Although Hooke and Huygens were now driven from the field, and the views of Newton established upon an impregnable

[1] Letter to Oldenburg, without a date, but probably in April 1673.

[2] It is curious to observe how little accurate knowledge of the great optical discoveries of the age was possessed by Leibnitz. In a letter addressed to Huygens, dated 8th September 1679, he says,—"I hear from Mr. de Mariotte that you are about to give us your Dioptrics, so long wished for. I have a great desire to know beforehand if you are satisfied with the ratio of refraction proposed by Descartes. I confess that I am neither wholly satisfied with it, nor with the explanation of Mr. Fermat, given in the third volume (Lett. 51) of Descartes' Letters."—Ch. Hugenii *Exercit. Math.*, tom. i. pp. 7, 8: lett. iv. Hag. Com. 1833. Huygens made no reply to this question, though he answered Leibnitz's letter on the 22d November. In reply to this letter, Leibnitz repeats the same question, confessing that he was neither satisfied with the ratio of Descartes, nor that of Fermat deduced from an opposite supposition. To this question he adds,—"I wish to know also if you believe that the irregularity of refraction,—for example, that which Mr. Newton has remarked,—ought to hurt telescopes considerably?"—*Ibid.* lett. vi. p. 17. An answer to this question was given by Huygens in a subsequent letter, for we find Leibnitz, in a letter dated 26th June 1680, expressing his satisfaction that Huygens had formed the same opinion of the "pretended demonstration of the laws of refraction given by Descartes."—*Ibid.* lett viii. p. 20. No reply is made to the question about Newton's doctrine of the cause of the imperfection of refracting telescopes; but *ten* years afterwards, when Leibnitz had received from Huygens a copy of his *Traité de la Lumière*, we find the following curious passage in his letter to Leibnitz, dated 24th August 1690:—"I have said nothing respecting colours in my *Traité de la Lumière*, finding this subject very difficult, and particularly from the great number of different ways in which colours are produced. Mr. Newton promised something on the subject, and communicated to me some very fine experiments which he had collected. It seems that you have also thought on the subject, and apparently to some purpose."—*Ibid.* lett. xi. pp. 27, 28.

basis, yet these prolonged and exciting controversies ruffled his temper, and disturbed his tranquillity. Even the satisfaction of humbling all his antagonists he did not regard as a compensation for the time he had wasted, and the intellectual labour which he had thrown away. "I intend," says he to Oldenburg, "to be no farther solicitous about matters of philosophy; and therefore I hope you will not take it ill if you never find me doing anything more in that kind; or rather that you will favour me in my determination, by preventing, so far as you can conveniently, any objections or other philosophical letters that may concern me." In a subsequent letter in 1675, he says,—"I had some thoughts of writing a farther discourse about colours, to be read at one of your assemblies, but find it yet against the grain to put pen to paper any more on that subject;" and in a letter to Leibnitz, of the 9th December 1675, he observes,—"I was so persecuted with discussions arising out of my theory of light, that I blamed my own imprudence for parting with so substantial a blessing as my quiet to run after a shadow." Nor was this a temporary resolution arising from some disagreeable expressions of a personal nature, which often embitter controversy even in its most temperate form. Nearly a year after his complaint to Leibnitz, he uses the following remarkable expressions in a communication to Oldenburg:—"I see I have made myself a slave to philosophy; but if I get free of Mr. Linus's business, I will resolutely bid adieu to it eternally, excepting what I do for my private satisfaction, or leave to come out after me; for I see a man must either resolve to put out nothing new, or to become a slave to defend it."[1]

In this state of mind, perplexed, as we shall presently see, with some pecuniary difficulties, and feeling, as he expressed it to Collins in 1674, "that mathematical speculations were at least dry, if not somewhat barren," there is reason to believe

[1] This letter is dated November 18, 1676, and was written after receiving an account of the experiments of Lucas.—*Macclesfield Correspondence*, vol. ii. p. 405.

that Newton, "who, in the usual course of things, would vacate his Fellowship in a few months, had seriously thought of directing his mind to the study of law." In an obituary notice of the Rev. Robert Uvedale, Rector of Langton, in Lincolnshire,[1] it is stated that his grandfather, Mr. Uvedale, when one of the Divinity Fellows of Trinity College, Cambridge, had become candidate for the Law Fellowship in that College when made vacant, on the 14th February 1673, by the death of Dr. Crane ;—that Mr. Newton was his competitor ;—that Dr. Barrow, as Master of Trinity, decided it in favour of Mr. Uvedale ; and that the ground of his decision was, that though Mr. Uvedale and Mr. Newton were at that time equal in literary attainments, yet he must give the Fellowship to Mr. Uvedale as the senior. Mr. Edleston[2] is disposed to consider this story as mythical, and he thinks that the real facts of the case were, that Uvedale was appointed to a Law Fellowship, and that Newton would have been glad to have had one. This opinion he rests on the ground that the tenure of the Law Fellowship could scarcely be considered compatible with the duties of the Lucasian chair, and "he believes that it would argue much misconception of the characters of the two great men concerned, to suppose them capable of being parties to a lax interpretation of the statute which they had sworn to obey." We can hardly admit the force of this argument in opposition to the precise statements, even if traditionary, of the Uvedale family. The necessities of Newton, and the ardent friendship of Barrow, might have induced the one to adopt a lax interpretation of the Lucasian statutes, and the other to accept the Fellowship, had it been in his power, without any great loss of character ; and we are the more inclined to adopt this opinion, when we know that in modern times the same statutes have been imperfectly observed.

While Newton was harassed with these discussions, and

[1] *Gentleman's Magazine*, 1799, Supplement, pp. 1186 and 999.

[2] *Correspondence, &c.*, pp. xlviii. xlix. *note*, 38.

chagrined, it may be, with the loss of the Law Fellowship, he came to the resolution of resigning his place in the Royal Society. On the 8th of March 1673, he writes in the following terms to Oldenburg :—"SIR,—I desire that you will procure that I may be put out from being any longer a member of the Royal Society ; for though I honour that body, yet, since I see I shall neither profit them, nor (by reason of this distance) can partake of the advantage of their assemblies, I desire to withdraw." Oldenburg expressed his surprise[1] "at his resigning for no other cause than his distance, which he knew as well at the time of his election ;" and he probably then intimated to him, that he would apply to the Society to excuse him his weekly payments. That such an intimation was made, appears from Newton's letter to Oldenburg, dated June 23, 1673, in which he says,—"For your proffer about my quarterly payments, I thank you, but I would not have you trouble yourself to get them excused, if you have not done it already." Nothing farther seems to have been said on the subject till the 28th January 1675, when Mr. Oldenburg mentioned "to the Society, that Mr. Newton was now in such circumstances that he desired to be excused from the weekly payments."[2] Upon which "it was agreed to by the council that he should be dispensed with, as several others were." It does not appear, from any documents we have seen, what the change of circumstances was to which Oldenburg alludes, but Mr. Edleston thinks it probable that it refers to the expected vacating of his Fellowship, from his being appointed to the Lucasian chair, which, in the usual course of things, would expire in the following autumn. This anticipated event, however, did not take place, for, on the 27th April 1675, he obtained a patent from the Crown, permitting the Lucasian Professor to hold a Fellowship, without being obliged to go into orders.

[1] This appears from a memorandum on the back of Newton's letter to him.

[2] The admission-money to the Royal Society was £2, and the payments one shilling a week.

This permission seems to have been obtained on the application of Newton; and Mr. Edleston is of opinion, that the draught of it in Newton's own hand, among the Lucasian papers, was composed by himself, and that his visit to London in February may have been connected with this application to the Crown. When the grant was submitted to the King, the following memorandum, found also in Newton's handwriting, was recorded at Whitehall on the 2d March 1674:—"His Majesty, being willing to give all just encouragement to learned men who are and shall be elected into the said Professorship, is graciously pleased to refer this draught of a patent unto Mr. Atturney-Generall to consider the same, and to report his opinion what his Majesty may lawfully do in favour of the said Professors, as to the indulgence and dispensation proposed and desired." The original draught, which has been published by Mr. Edleston, was adopted, excepting in two unimportant particulars, and there is a copy of it in the archives of Trinity College, with the heading,—*Indulgentia Regia Professori Mathematico concessa, dignissimo viro Magistro Isaaco Newtono, hujus Collegii Socio, istud munus tunc temporis obeunte.*

It is obvious, we think, from these proceedings, that the change in Newton's circumstances must have been of a distressing nature, otherwise he would hardly have permitted Oldenburg to apply to the Royal Society for a remission of his weekly payments. At no period of his life had he any regard for money, and, as he was always punctual and accurate in his pecuniary concerns, it is very probable, that when the income of his Fellowship[1] and the Lucasian chair were united, he may have resumed his payments to the Royal Society.[2] If he did

[1] In reference to an application from Francis Aston for a dispensation similar to that received by Newton, Dr. Barrow, then Master of Trinity, in declining to grant it, says, —"Indeed a Fellowship with us is now so poor, that I cannot think it worth holding by an ingenuous person upon terms liable to so much scruple."—Edleston's *Correspondence*, p 1.

[2] In a volume of MSS. in the British Museum relating to the Royal Society, there is, as Mr. Weld informs us, a sheet containing the names of Fellows who will *probably pay*.

not do this, it could not have been from poverty, as we find him in 1676 subscribing forty pounds to the new Library of Trinity College.

But however this may be, it cannot fail to be remarked, especially by foreigners, as a singular example of the illiberality of England to her scientific institutions, that a Society, founded by the sovereign, and bearing the name of Royal, should have been established without any provision for the support of its members, for carrying on scientific inquiries, or for the publication of its Transactions. Nor is it less remarkable, that an Institution so useful to the country, so bright with immortal names, and so fitted to promote the intellectual glory of the nation, should have been continued under royal patronage for nearly two hundred years without any attempt being made to extend its usefulness, by placing it in the same advantageous position as the Academy of Sciences in Paris, and other similar institutions in the metropolitan cities of Europe.

If Newton did not feel it a hardship to pay a weekly pittance into the treasury of the Royal Society, he must have felt it a degradation to plead poverty for its remission. His colleagues in the Society, and men of science in a succeeding age, on whom the wealth of this world is never abundantly bestowed, must have often smarted under the injustice of paying for the publication of discoveries which it cost them much time, and frequently much money, to complete. Of all the taxes upon knowledge this is the most oppressive, and not the less oppressive that it is exacted from the feelings and patriotism of its victims.

There is reason to believe that Newton took this view of his own position, and of the inefficiency of any scientific body constituted upon the voluntary principle; and it is not improbable,

and give yearly one entertainment to the Society. Opposite the names of Dr. Grew, Hooke, and Newton, are the words, "No pay, but will contribute experiments." The date of this list, if it has any, is not mentioned. See Baily's *Life of Flamsteed*, p. 90, *note*, and Weld's *Hist. of the Royal Society*, vol. i. p. 250, *note*.

that he committed to writing his opinions on this subject at the time when he had resolved to withdraw from the Society. In support of this opinion, we have great pleasure in submitting to the reader a very remarkable document in Newton's handwriting, which we found among the family papers at Hurtsbourne Park, entitled "A Scheme for Establishing the Royal Society." We give it without abridgment or change, as the opinions of so competent a judge on the subjects which ought to occupy the attention of a national institute, and on the best method of making it efficient in promoting the advancement of profound science and of useful knowledge, cannot fail to be appreciated by every class of readers.[1]

"SCHEME FOR ESTABLISHING THE ROYAL SOCIETY.

"Natural Philosophy consists in discovering the frame and operations of Nature, and reducing them, as far as may be, to general Rules or Laws,—establishing these rules by observations and experiments, and thence deducing the causes and effects of things; and for this end it may be convenient, that *one* or *two* (and at length perhaps *three* or *four*) Fellows of the Royal Society, well skilled in any one of the following branches of Philosophy, and as many in each of the rest, be obliged by pensions and forfeitures (as soon as it can be compassed), to attend the meetings of the Royal Society.—The Branches are—

"1. Arithmetic, Algebra, Geometry, and Mechanics, with relation to the figures, surfaces, magnitudes, forces, motions, resistances, weights, densities, centres of gravity, and other mathematical affections of solids and fluids;—the composition of forces and motions;—the shocks and reflexions of solids;—the centrifugal forces of revolving bodies;—the motion of pen-

[1] We found six copies of this scheme, one of which is more complete than the others. The first paragraph of the copy given in the text is wanting in the less perfect copies, but in other respects they are nearly the same. There is no date upon any of the copies.

dulums, projected and falling bodies ;—the mensuration of time and distance ;—the efficacy of the five powers, the running of rivers ;—the propagation of light and sound, and the harmony and discord of tunes and colours.

"2. Philosophy relating to the Heavens, the Atmosphere, and the surface of the Earth, viz., Optics,—Astronomy,—Geography,—Navigation, and Meteorology ; and what relates to the magnitudes, distances, motions, and centrifugal forces of the heavenly bodies ; and to the weight, height, form, and motions of the Atmosphere, and of the things therein, and to instruments for observing the same ; and to the figure and motions of the Earth and Sea.

"3. Philosophy relating to animals, viz., their species,—qualities,—passions,—anatomy, diseases, &c., and the knowledge of the frame and use of their Stomachs,—entrails, blood-vessels, heart, lungs, liver, spleen, glands, juices, and organs of sensation, motion, and generation.

"4. Philosophy relating to vegetables, and particularly the knowledge of their species, parts, leaves, flowers, seeds, fruits, juices, virtues, and properties, and the manner of their generation, nutrition, and vegetation.

"5. Mineralogy[1] and Chemistry, and the knowledge of the nature of Earths, Stones, Corals, Spars, Metals, semi-metals, Marchasites, Arseniates, Bitumens, Sulphurs, Salts, Vitriols, Rain-Water, Springs, Oils, Tinctures, Spirits, Vapours, Fumes, Air, Fire, Flames and their parts, Tastes, Smells, Colours, Gravity, Density, Fixity, Dissolutions, Fermentations, Coalitions, Separations, Congelations, Liquefactions, Volatility, Distillation, Sublimation, Precipitation, Corrosiveness, Electricity, Magnetism, and other qualities ;—and the causes of subterraneous Caves, Rocks, Shells, Waters, Petrifactions, Exhalations, Damps, Heats, Fires, and Earthquakes, and the rising or falling of Mountains and Islands.

"To any one or more of these Fellows, such Books, Letters,

[1] Written by mistake *Meteorology ;* but in one of the other copies it is *Mineralogy.*

and things as deserve it, may be referred by the Royal Society at their meetings from time to time; and as often as any such Fellowship becomes void, it may be filled up by the Royal Society with a person who hath already invented something new, or made some considerable improvement in that branch of philosophy, or is eminent for skill therein, if such a person can be found. For the reward will be an encouragement to Inventors; and it will be an advantage to the Royal Society to have such men at their meetings, and tend to make their meetings numerous and useful, and their body famous and lasting."

It is very evident, from this interesting document, that Newton was desirous of converting the Royal Society into an institution like that of the Academy of Sciences in Paris; but we have not been able to learn that he ever communicated this plan either to the Society itself, or to any of its members. During the last twenty years, and long before we could have known the views of so competent a judge, we have cherished the same desire, and embraced every opportunity of pressing it upon the notice of the public.[1] Several years ago we communicated Sir Isaac Newton's scheme to Sir Robert Peel, and it was so far carried into effect by the establishment of the *Museum of Practical Geology*, which is neither more nor less than an enlargement of the *Mineralogical*, *Geological*, *and Chemical* sections of an Academy of Sciences, or a National Institute. The services of all the members of this important body are of course at the entire disposal of the State, though its members are frequently employed in other duties than those which strictly belong to their office. If mineralogy, geology, and chemistry, therefore, have obtained a national establishment for their improvement and extension,—astronomy, me-

[1] See especially the *Quarterly Review*, October 1830, vol. xliii. pp. 305-342; *Edinburgh Review*, January 1835, vol. lx. p. 363; *Edinburgh Journal of Science, passim; North British Review*, vol. iv. pp. 410-412; vol. vi. p. 506; vol. xiv. pp. 281-288; from the last of which articles some of the paragraphs in the text are transferred.

chanics, natural history, medicine, and literature, and the arts, are entitled to the same protection. If any real objections exist to such an establishment, they can be founded only upon two causes ;—on the unwillingness of existing voluntary societies to be merged in a general institution, and on the apprehension that the expense would be a burden to the state. Men will always be found who oppose every change, however salutary, and who regard the reform of existing institutions as dangerous innovations. In political and educational questions, the rights and interests of individuals often obstruct the march of civilisation, but in matters of science and literature, such rights have neither been conferred nor claimed. Were the Royal, the Astronomical, the Geological, the Linnæan, the Zoological, and the Geographical Societies, together with the Society of Civil Engineers, and the Museum of Practical Geology, all united into an Academy of Sciences, and divided into distinct sections as in France, the really working members would occupy a more distinguished position, while the nobility and gentry would preserve all their rights and privileges as honorary members.[1] The Royal Society of Literature, and the Antiquarian Society, would readily coalesce into the Academy of Belles Lettres, and the existing Royal Academy would form the Academy of the Fine Arts, divided, as in France, into the three sections of Painting, Sculpture, and Engraving. In the magnificent grove acquired by Prince Albert and the Royal Commissioners at Kensington Gore, a Palace of Arts would be reared for the Institute, and there would be one library, one museum, and one record of their weekly proceedings. Each member of the now insulated societies would listen to the memoirs and discussions of the assembled Academy, and science and literature would thus receive a new impulse from the number and variety of their worshippers.

The second difficulty to which we have referred, namely, the expense of endowment, scarcely merits our consideration. A

[1] Corresponding to the *Académiciens Libres* of the Academy of Sciences in Paris.

very large sum is annually expended by the State in support of the existing societies, and a considerable number of those who would be members of the General Institute, already enjoy the liberality of Government. But, independently of these considerations, the organization of a National Institute would be a measure of real and direct economy. The inquiries connected with the arts, whether useful or ornamental, which are required by the Government, have hitherto been carried on by Committees of Parliament; and had we a return of all the sums annually spent in scientific inquiries, and for scientific purposes, the amount would be found to exceed greatly that of the annual expense, however liberal, of a National Institution. Every question connected with ship-building, with our steam navy, our light-houses, our harbours, our railways, our mines, our fisheries, our sanitary establishments, our agriculture, our statistics, our fine and useful arts, would be investigated and reported upon by a Committee of Academicians; and while the money of the State would thus be saved, the national resources would be augmented, and all the material interests of the country, under the combined energies of her Art and her Science, would advance with a firm and accelerated step.

But there are grounds higher than utilitarian, on which we would plead the national endowment of science and literature. In ancient times, when knowledge had a limited range, and was but slightly connected with the wants of life, the sage stood even on a higher level than the hero and the lawgiver, and History has preserved his name in her imperishable record, when theirs have disappeared from its page. Archimedes lives in the memory of thousands who have forgotten the tyrants of Syracuse, and the Roman consul who subdued it. The halo which encircled Galileo under the tortures of the Inquisition, extinguishes in its blaze even the names of his tormentors; and Newton's glory will throw a lustre over the name of England, when time has paled the light reflected from her warriors. The renown of military achievements appeals but to the country which they benefit and adorn : It lives but in the obelisk of granite : It illuminates but the vernacular page. Subjugated nations turn from the proud monument that degrades them, and the vanquished warrior spurns the record of his humiliation or his shame. Even the patriot traveller makes a deduction from military glory, when he surveys the red track of desolation and of war, and the tears which the widow and the orphan shed corrode the inscription that is written in blood. How different are our associations with the tablet of marble, or the monument of bronze, which emblazon the deeds of the sage and the philanthropist ! Their paler lustre irradiates a wider sphere, and excites a warmer sympathy. No trophies of war are hung in the temple which they adorn, and no assailing foe desecrates its shrine. In the anthem from its choir the cry of human suffering never mingles, and in the procession of the intellectual victor, ignorance and crime are alone bound to his car. The achievements of genius, on the contrary, could the wings of light convey them, would be prized in the other worlds of our system,—in the other systems of the universe. They are the bequests which man offers to his race,—a gift to universal humanity—at first to civilisation—at last to barbarism.

Views like these must have influenced the mind of Newton, when, in an elaborate document which he left in duplicate behind him, he recommended the systematic endowment of Science. Were the British Parliament to try this question at its bar, and summon as witnesses the wisest of their race, what name, or what constellation of names, could countervail against the High Priest of Science, when he proposes to rebuild its Temple upon a broader basis, and give its arches a wider span, and its domes a loftier elevation !

CHAPTER V.

Mistake of Newton in supposing the Length of the Spectra to be the same in all Bodies—And in despairing of the Improvement of Refracting Telescopes—In his Controversy with Lucas he was on the eve of discovering the different Dispersive Powers of Bodies—Mr. Chester More Hall makes this Discovery, and constructs Achromatic Telescopes, but does not publish his Discovery — Mr. Dollond re-discovers the Principle of the Achromatic Telescope, and takes out a Patent—Principle of the Achromatic Telescope explained—Dr. Blair's Aplanatic Telescope—Great Improvement on the Achromatic Telescope by the Flint-Glass of Guinant, Fraunhofer, and Bontemps—Mistake of Newton in forming his Spectrum from the Sun's Disc—Dark Lines in the Spectrum—Newton's Analysis of the Spectrum incorrect—New Analysis of the Spectrum by Absorption, &c., defended against the Objections of Helmholtz, Bernard, and others—Change in the Refrangibility of Light maintained by Professor Stokes—Objections to his Theory.

The two great doctrines of the different refrangibility of the rays of light, and of the composition of white light, by mixing all the rays of the spectrum, having been established by Newton on an impregnable basis, we come now to describe some of the other results which he obtained regarding the prismatic spectrum and its colours, to point out the errors which he committed, to show the influence which they had on the progress of optics, and to give an account of the remarkable discoveries which have been made in this branch of science during the last and the present century.

There are few facts in the history of optics more singular than that Newton should have believed that all bodies when shaped into prisms produced prismatic spectra of equal length, or separated, or dispersed the red and violet rays to equal distances, when the mean refraction, or the refraction of the middle ray of the spectrum, was the same. This opinion, which he deduced from no direct experiments, and into which no theoretical views could have led him, seems to have been impressed on his mind with all the force of an axiom. In one of his experiments he had occasion to counteract the refraction of a prism of glass by a prism of water; and had he completed the experiment, and studied the result of it when the mean refraction of the two prisms was the same, he could not have failed to observe that the prism of water did not correct the colour of the prism of glass, and would have thus been led to one of the most important truths in optics,—that different bodies have different dispersive powers, or produce prismatic spectra of different lengths, when their mean refraction is the same. It is curious to observe, as happened in this experiment, what trifling circumstances often arrest the philosopher when on the very verge of a discovery. Newton had mixed with the water which he used in his prism a little *sugar of lead*, in order to increase the refractive power of the water; but the sugar of lead having a higher dispersive power than water, made the dispersive power of the water prism equal to that of the prism of glass; so that if Newton had completed the experiment, the use of the sugar of lead would have prevented him from making an important discovery, which was almost in his possession. Had he, on the contrary, increased the angle of his water prism till it produced the same deviation of the mean ray of the spectrum, he would have found that the one prism did not correct the colour of the other, and that the glass had a greater dispersive power than the water, and gave a longer spectrum.

Nor is it less extraordinary that the same discovery escaped from his grasp during his controversy with Lucas. When the Dutch philosopher and his numerous friends who saw his experiments, pronounced his spectrum to be only 3½ times its breadth, Newton found it to be at least five times its breadth; and it is strange that neither party ever thought that this might arise from using different kinds of glass, and never made the least inquiry regarding the material of which their prism was made. It is highly probable that Lucas's prism had a very low dispersive power, which would account for the great difference between his spectra and those of Newton, but whether this was the case or not, Newton, under the blind conviction that all spectra must, *cæteris paribus*, be of equal length, pronounced "the improvement of telescopes by refractions to be desperate,"[1] and thus checked for a long time the progress of this branch of science.

About two years after the death of Sir Isaac, an individual unknown to fame, broke the spell in which the subject of the spectrum had so long been bound. In the year 1729, Mr. Chester More Hall, of More Hall in Essex, while studying the mechanism of the human eye, was led to suppose that telescopes might be improved by forming their object-glass with two lenses of different refractive powers. He published no memoir on the subject, and has not even left behind him any record of the steps by which he arrived at such a conclusion. It is probable that he may have adopted David Gregory's idea of combining lenses of different density, and as crown and flint-glass differed most in this respect, that in combining them he discovered the great difference in their dispersive powers, and was thus led to the invention of the achromatic telescope. Mr. Hall employed working opticians to grind his lenses, and furnished them with the proper radii of their surfaces for correcting the colour arising from the difference of refrangibility in the rays, and the aberration occasioned by the spherical

[1] Optics, Prop. vii. Book ii. p. 91. In his reply to Hooke, who justly "reprehended him for laying aside the thoughts of improving optics by refractions," he seems to modify his opinion by saying that he tried what might be done "by two or more glasses or crystals, with water or some other fluid between them." "But what the results by theory or by trials have been, he might possibly find a more proper occasion to declare." This was written in 1672, and we can therefore say with certainty that he failed in this attempt, as it was in 1684 that he pronounced the case to be desperate. It is a curious circumstance that David Gregory, in his Lectures delivered in Edinburgh in 1684, suggests that, in imitation of the human eye, the object-glasses of telescopes might be composed of media of different density. In Brown's translation of Gregory, the sense of the passage is not brought out. See Gregory's *Catoptrics*, Prop. xxiv. Schol. pp. 110, 111.

figure of the lenses. Mr. Bass, a well-known working optician, was one of his assistants, and it was probably through him that the knowledge of Mr. Hall's invention has been preserved. About the year 1733 he had completed several achromatic object-glasses, which bore an aperture of more than $2\frac{1}{2}$ inches, though their focal length did not exceed twenty inches. One of these telescopes, which in 1798 was in the possession of the Rev. Mr. Smith of Charlotte Street, Rathbone Place, was examined by several gentlemen of scientific eminence, and found to be a genuine achromatic telescope.

Many years after the death of Mr. More Hall, Mr. John Dollond and others had turned their attention to the improvement of telescopes. Euler, believing the eye to be achromatic, had attempted, but in vain, to discover a combination of media, by which the object-glasses of telescopes could give colourless images. Klingenstierna had endeavoured to show that refraction without colour might be produced according to the laws of refraction laid down by Newton himself; but none of these philosophers made a single step towards the great discovery which was made by Mr. Dollond, when the previous labours of Hall were unpublished. In 1758, he communicated to the Royal Society an account of his experiments on the different refrangibility of light. In this valuable paper, he proved that glass had a greater dispersive power than water, and attempted to make achromatic object-glasses by enclosing water between two lenses of glass. In this attempt he found the spherical aberration difficult to correct, and he was therefore led to try crown and flint glass, which he found to have such different dispersive powers, that he was at once able to make achromatic object-glasses. In order to secure his right to this invention, Dollond took out a patent; but in consequence of its having been discovered that the same invention had been made before, some of the London opticians tried the question at law, and produced in court the telescope of Mr. Hall. It was in vain to deny the prior claims of Mr. Hall;

but as it was certain that Dollond was unacquainted with his labours, and as no achromatic telescope had ever been exposed to sale, Lord Mansfield justly decided the case in favour of Dollond.[1]

It is not easy to explain to the general reader the principle of the Achromatic Telescope; but we think it may be apprehended from an inspection of the annexed diagram. In crown glass the index of refraction is 1·526 for red rays, and 1·547 for violet rays. If L L then be a convex lens of crown glass, it will refract the violet rays more than the red, the former in the direction L R, and the latter in the direction L V, so that R will be the focus of red, and V that of the violet rays. If we now place behind it a concave lens C C of the same kind of glass and the same curvature, it will by its opposite and equal refractions unite again the rays L R, L V, in the direction L *l*, so as to form a white ray; but in this case the compound lens acts like a piece of plane glass, or rather like a watch glass which

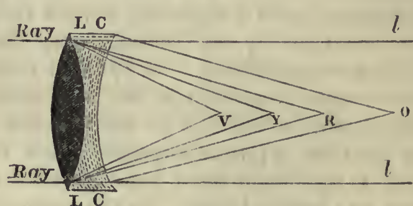


FIG. 9.

has no focus. But if we make the concave lens C C of flint glass with less curvature than L L, then since it has a greater refractive and dispersive power than the lens L L of crown glass, it will, notwithstanding its inferior curvature, unite the rays L R, L V, and leave such a balance of refraction in favour of the lens L L, that the rays will be united, and a colourless image

[1] See Tilloch's *Philosophical Magazine*, Nov. 1789, vol. ii. p. 177.

formed at O, so that the double object-glass L L C C will be an achromatic one.

If the prismatic spectrum formed by crown and flint glass had been exactly the same, that is, if the coloured spaces in each were of the same length, telescopes constructed upon the preceding principle would have been perfect, in so far as colour is concerned; but this is not the case, and consequently in the very best achromatic telescopes, there is left what has been called a *secondary spectrum*, consisting of *green* and *purple* colours, which appear on the border of the images of all luminous objects.

This *secondary* or *residual* spectrum, arising from what has been called the *irrationality* of the coloured spaces in the two equal spectra of crown and flint glass, may be corrected by an ingenious contrivance discovered by Dr. Blair. He found that muriatic acid produced a prismatic spectrum, in which the coloured spaces were nearly the same as in crown glass, and that he could increase its low refractive and dispersive power, by mixing it with metallic solutions, so as to fit it for being used like flint glass for correcting the colour of the crown glass without balancing its refraction. This increase in its refractive and dispersive powers, did not alter the proportion of the coloured spaces in its spectrum, so that it was capable of giving a perfectly colourless image, when placed as a concave lens between two convex ones of crown glass. The metallic solution used by Dr. Blair was *muriate of antimony*, and in the lens which he constructed, the rays of different colours were bent from their rectilineal course with the same equality and regularity as in reflexion. To this telescope he gave the name of *Aplanatic*. According to the testimony of Professor Robison, those he examined surpassed greatly the best ordinary achromatic telescopes; but they have been found difficult to construct, and in so far as we know, there is not in existence a single aplanatic telescope.

The Achromatic Telescope, on the contrary, even with the imperfection of its secondary spectrum, has undergone great improvements, and promises to rival Reflectors in excellence and power. By the labours of Guinand, Fraunhofer, and M. Bontemps, discs of flint glass of 12, 15, 24, and even 29 inches in diameter, have been made, and we hope soon to see the largest of them converted into a magnificent telescope. The disc of 24 inches has been converted into a telescope by the Rev. Mr. Craig of Leamington.[1]

But while Newton overlooked the remarkable property of the prismatic spectrum, on which the improvement of Refracting Telescopes depends, he committed other considerable mistakes in his examination of the spectrum. It does not seem to have occurred to him that the *Solar Spectrum* was not the spectrum from which the properties of the sun's rays ought to be deduced, and that the relations of the coloured spaces must depend on the angular magnitude of the luminous body, or of the aperture from which the spectrum is obtained. Misled by an apparent analogy between the length of the coloured spaces and the divisions of a musical chord,[2] which he ascertained " by an assistant whose eyes were more critical than his own," he adopted that division as representing the proportion of the coloured spaces in every dispersed beam of light. Had he studied the prismatic spectrum in Mercury and Jupiter by the same instruments, he would have obtained quite different results. In Mercury, where the sun's apparent magnitude is very large, he would have seen a spectrum without any green, and having *red*, *orange*, and *yellow* at one end, *white* in the middle, and *blue* and *violet* at the other end. In Jupiter, on the contrary, he would have obtained a spectrum in which the coloured spaces were much more condensed, and the pure colours more separated. The Solar spectrum described by Newton, has an intermediate character between these two extremes, and had he examined it under the same circumstances

[1] See my *Treatise on Optics*, new edit. p. 506.
[2] *Optics*, Part ii. Prop. iii. p. 110.

in winter and in summer, he would have found the analysis of the beams more perfect in summer, on account of the sun's diameter being less. We are entitled, therefore, to assert, that neither the number nor the extent, nor the limits of the coloured spaces, as given by Newton, are those which belong to the true prismatic spectrum.

Had Newton received upon his prism a beam of light transmitted through a very narrow aperture, he would have anticipated Wollaston and Fraunhofer in their fine discovery of the lines in the prismatic spectrum. In 1802, Dr. Wollaston, by transmitting the light of the sky through an aperture the twentieth of an inch wide, discovered *six* fixed dark lines in the spectrum, one in the red, one in the orange, one in the blue, and one in the violet spaces. Without knowing of Wollaston's observations, the late celebrated M. Fraunhofer of Munich, discovered in sun light, nearly 600 lines, the largest of which subtended an angle of from 5″ to 10″. We have found this angle to increase enormously by atmospherical absorption, as the sun passes from the meridian to the horizon, and in a long series of observations we have observed upwards of *two thousand* lines in the prismatic spectrum formed from the sun's rays.

From his analysis of the Solar spectrum, by examining with the prism its separate colours, Newton concluded, *that to the same degree of refrangibility ever belonged the same colour, and to the same colour ever belonged the same refrangibility*, and hence he inferred that *red*, *orange*, *yellow*, *green*, *blue*, *indigo*, and *violet*, were primary and simple colours. This proposition is true in so far as the analysis of the spectrum by the prism is concerned ; but we have found another species of analysis, by which the colours of the spectrum may be decomposed. Though we cannot separate the *green* rays in the spectrum into *yellow* and *blue* by the refraction of prisms, yet if we possessed any solid or fluid which had a specific attraction for *blue* rays, that is, which absorbed them during the passage of the *green* light through the medium, and allowed the *yellow* rays to pass, we should then analyse the *green* into its component elements as effectually as if we separated them by the prism. We have in this way subjected the colours in the spectrum to the analysis of a great variety of solid and fluid bodies of different colours, and we have found that in every part of the spectrum, the colours are more or less changed or decomposed by absorption.

The simplest way of observing these changes is to receive the spectrum in the eye by looking through the prism at a narrow line of light from the sky. If we now interpose between the eye and the prism a plate of purplish blue glass, about the twentieth of an inch thick, we shall see the prismatic spectrum with its bright colours completely metamorphosed. The *red* part of the spectrum is divided into *two red* spaces, separated by a dark interval. Next to the inner red space comes a space of bright yellow, separated from the red by a visible interval. After the yellow comes the *green*, with an obscure space between them, then follow the *blue* and the *violet*, the last of which has suffered little or no diminution. Now, in this experiment, the *blue* glass has absorbed the *red* rays which, when mixed with the *yellow*, on one side constituted *orange*, and the *blue* rays which, when mixed with the *yellow* on the other side, constituted *green*, so that the insulation of the yellow rays thus effected, and the disappearance of the *orange* and of the greater part of the *green* light, places it beyond a doubt that the *orange* and *green* colours in this spectrum are component colours, the former consisting of *red* and *yellow*, and the latter of *yellow* and *blue* rays *of the very same refrangibility.* If we compare the *two red* spaces seen through the *blue* glass, with the red spaces seen without the *blue* glass, it will appear that the *red* has experienced such an alteration in its tint by the action of the blue glass, as would be effected by the absorption of a small portion of yellow light; and hence we conclude that the red of this spectrum contains a slight tinge of yellow, and that the yellow space extends over more than one half of the spectrum, including the *red*, *orange*, *yellow*, *green*, and *blue* spaces.

By varying the absorptive media, I have found that *red* light exists in the *yellow* space, and we have ocular evidence, that in the *violet* space *red* light is combined with the *blue* rays. From these and other facts, which it would be out of place here to enumerate, I have been led to the conclusion that *the prismatic spectrum consists of three different spectra*, viz., *red*, *yellow*, and *blue*, *all having the same length*, *all superposed*, *and each having its maximum intensity at the point where it predominates in the combined spectrum.* Hence it follows :—

1. That *red*, *yellow*, and *blue*, rays of the same refrangibility exist at every point of the spectrum of intensities, represented by the ordinates of the curve of intensity in each separate spectrum.

2. That the colour of the spectrum at any one point will be that of the predominant ray modified by the smaller quantities of the other two rays ; and,

3. That if we could absorb the two predominant rays at any one point of the spectrum, in such quantities as when mixed with the remaining or unabsorbed ray, would make white light, we should be able to insulate *white light indecomposable by the prism.*

This view of the structure of the spectrum will be understood from the annexed diagrams, where *Figs.* 10, 11, and 12, represent the three separate spectra, which are shown in their combined state in *Fig.* 13. In all these figures, the point M is the *red* or *least* refrangible extremity of the spectrum, and N the *violet* or *most* refrangible extremity. The maximum intensity of each spectrum is opposite R, Y, and B, the intensity diminishing to nothing at the extremities M and N. When these three spectra are superposed, they will exhibit the colours shown in *Fig.* 13, in which we have inserted the three curves which represent the intensities in each spectrum.

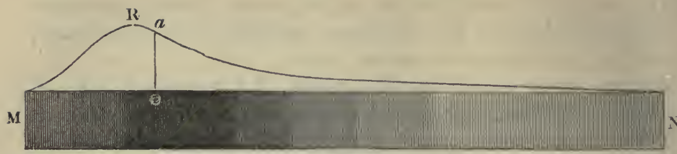


FIG 10.

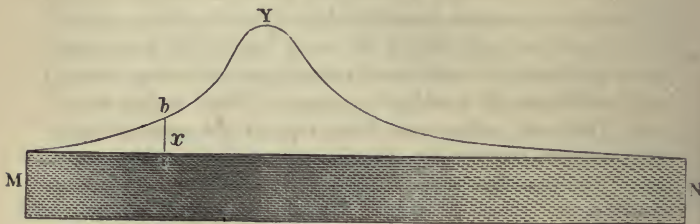


FIG. 11.

FIG. 12.

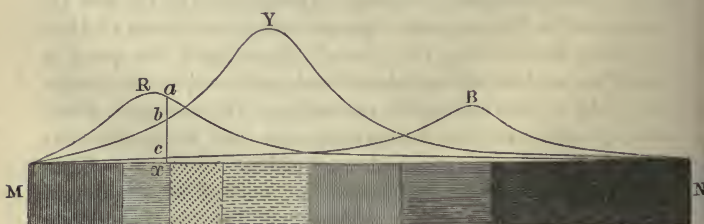


FIG. 13.

In order to explain how the *seven* colours, observed by Newton, are produced by the three primitive colours, we shall take the case of the *orange*, as shown in *Fig.* 13, where the *three* ordinates ax, bx, cx, will indicate the relative intensities of the *three* colours, combined at any point x of the spectrum. Thus let

The ordinate for red light be		$ax =$	30
,,	yellow	$bx =$	16
,,	blue	$cx =$	2
Then $ax + bx + cx$		$=$	48 rays.

Hence the point x will be illuminated with forty-eight rays, namely, thirty of red, sixteen of yellow, and two of blue light. Now, as there must be certain quantities of red and yellow light, which, when combined with two blue rays, will form *white*, let us suppose that white light, whose intensity is ten, will be formed by three red, five yellow, and two blue rays, then it follows that the point x will be illuminated with

Red rays, ...	30 — 3	or 27 rays.
Yellow rays, ...	16 — 5	or 11 ,,
White light + 3 red + 5 yellow + 2 blue,		or 10 ,,
Orange = red + yellow + white, .		= 48 rays.

That is, the point x will have the colour of *orange* rendered brighter by a mixture of *white* light. The *blue* rays consequently which exist at x will not communicate any *blue* tinge to the prevailing *orange*.

In submitting to the scientific world this new analysis of light, by absorption, we were fully aware of the difficulties which we had to encounter, and we anticipated the opposition which would be made to it. "Even in physical science," we said,[1] "it is an arduous task to unsettle long-established and deeply-rooted opinions; and the task becomes Herculean when these opinions are intrenched in national feeling, and associated with immortal names. There are cases, indeed, where the

[1] *Edinburgh Transactions*, 1831, vol. xii. p. 124.

simple exhibition of new truths is sufficient to dispel errors the most deeply cherished, and the most venerable from their antiquity ; but it is otherwise with doctrines which depend on a chain of reasoning where every step in the inductive process is not rigorously demonstrative ; and of this we require no other proof than is to be found in the history of Newton's optical discoveries, and particularly in the opposition they experienced from such distinguished men as Dr. Hooke and Mr. Huygens."

The preceding analysis of the spectrum embraces three propositions, which, to a certain extent, are independent of each other.

1. That the colours of the coloured spaces may be changed by absorbing media, acting by reflexions and transmissions.

2. That in pure spectra, *white light* can be insulated.

3. That the Newtonian spectrum of *seven* colours consists of *three* equal primary spectra, *red*, *yellow*, and *blue* superposed, having their maximum intensity of illumination at different points, and shading to nothing at their extremities.

The *first* of these propositions may be true, even though we could not insulate white light at any point of the spectrum ; and both the first and second may be true, without our being able to demonstrate that the three spectra have the same length, and diminish in intensity from their maxima to their extremities.

The general proposition that the colours of the spectrum are changed by absorption, has been questioned by three classes of critics,—by Mr. Airy,[1] M. Melloni,[2] and Mr. Draper,[3] who have never repeated our experiments, but made some very imperfect ones of their own ;—by Dr. Whewell,[4] and the Abbé Moigno,[5] who have made no experiments at all ;—and by M.

[1] *Phil. Mag.* vol. xxx. p. 73.

[2] *Bibl. Univers.* Août 1847.

[3] Silliman's *Journal*, vol. iv. p. 388. 1847.

[4] *Hist. of Inductive Sciences*, vol. ii. p. 361 ; and *Edinburgh Review*, vol. lxvi. p. 136 ; and vol. lxxiv. p. 288.

[5] *Répertoire d'Optique*, tom. ii. p. 459.

Helmholtz[1] in Prussia, and M. Bernard[2] in France. We have replied to the three first of these writers, and shall now make a few observations on the results obtained by MM. Helmholtz and Bernard.

M. Helmholtz has candidly stated, in contradiction of Mr. Airy, that "the changes of colour" which we have described, as produced by absorption, "are for the most part sufficiently striking to be observed without difficulty;" and he adds, that "a careful repetition of at least the most important of my experiments, carried out in exact accordance with my method, and with every precaution hitherto deemed necessary, has indeed taught me that the facts which he affirms to have observed, are described with perfect accuracy."

The change of colour, thus admitted as a physical fact, M. Helmholtz ascribes to two causes :—

1. To the possible admixture of rays scattered from the prism, and the other transparent bodies used in the experiment; and

2. To the mixture of complementary colours produced by the action of the other colours of the spectrum on the retina.

The first of these, as M. Helmholtz almost admits, is wholly uninfluential, and the second, if it does disturb the colorific impressions on retinæ tender and sensitive, had no such effect on ours.

If the subjective perception of colour, when we view the spectrum, or make experiments in which more than one colour reaches the eye, is capable of masking the colours under examination, then all that has been written on colours, thus seen, must be erroneous, and all the gay tints of art or of nature are but false hues under the metamorphosis of a subjective perception. We must not now pronounce a *rose* to be *red*, and its leaves *green*, till we have stared at them through a chink, or torn them from their foot-stalk! The phenomena of ab-

[1] Poggendorff's *Annalen* 1852, No. 8.

[2] *Ann. de Chim. et de Phys.* tom. xxxv. p. 385, &c.

sorption which we have described *we have seen*, just as Newton saw his seven colours in the spectrum, and Hooke his composite tints in the soap-bubble; and now that our eyes have nearly finished their work, we are not disposed to mistrust, without reason, such good and faithful servants.[1]

The observations of M. Bernard, who has repeated only a few of our experiments, differ very little from those of M. Helmholtz. He maintains that the conversion of the *blue* space into *violet* arises from the light being diminished. If the colours of the spectrum thus change, as he maintains, by their becoming fainter, we would desire to ask at what degree of illumination are we to see the spectrum in its *true colours?* *Colour* cannot depend upon refrangibility, if the *blue space* is converted into *violet* either by diminution of light or absorption; and therefore the doctrine of M. Bernard is as fatal to Newton's as to ours. If M. Bernard's experiment be correct, it only proves that the *blue rays, when enfeebled, lose their power over the retina sooner than the red.*

The Newtonian doctrine, "that the degree of refrangibility proper to any particular sort of rays is not mutable by refraction, nor reflection, nor by any other cause,"[2] has been recently questioned by Professor Stokes, one of the distinguished successors of Newton in the Lucasian chair. Mr. Stokes[3] found that the chemical rays in the violet space, between the lines G and H of the spectrum, produce, in a solution of sulphate of quinine, *light of a sky-blue* colour, which he assumes to have the refrangibility of that portion of the spectrum. By refracting

[1] The changes of colour in the spectrum at different seasons of the year, and the different hours of the day, and when formed from different portions of the illuminated sky, as well as from the direct light of the sun, are very remarkable. We have mentioned one or two of them in the *Edinburgh Review*, vol. lxxiv. p. 284. Jan. 1842. One of these observations is as follows:—"October 23, 1832. 11th, The *yellow* comes distinctly up to F, and a little beyond it; *i.e.*, the *blue* has been all absorbed in the *green* space of Fraunhofer's spectrum from E to F." In another observation on the 5th February 1833, the *green* space was wholly *yellow*.

[2] Letter to Oldenburg, Feb. 6, 1672, in *Phil. Trans.* No. 80, p. 3081, § 3.

[3] *Phil. Trans.* 1852.

this light through a prism, he converts the *sky-blue rays* into a spectrum of all colours, and all refrangibilities. Hence he concludes, that the *sky-blue light* having the fixed refrangibility due to its locality between G and H, is changed by refraction into all the other colours, with their respective refrangibilities. If this conclusion be admitted, our doctrine of the severance of colour and refrangibility is placed beyond a doubt. We have in the first experiment *sky-blue* light with the refrangibility of *violet* light between G and H; and, in the second experiment, we have the same *blue* light changed by refraction into *all the colours* of the spectrum.

We cannot, however, avail ourselves of this last fact, for, after a careful consideration of Mr. Stoke's important results,[1] we cannot but regard the *sky-blue* light as a *phosphorescence*, produced in the quinine solution by the chemical rays, which, like all other phosphorescences, is decomposable by the prism.

[1] See my *Treatise on Optics*, new edition, pp. 182, 183.

CHAPTER VI.

Newton on the cause of the Moon's Libration—Is occupied with the subject of planting Cider Trees—Sends to Oldenburg his Discourse on Light and Colours, containing his Hypothesis concerning Light—Views of Descartes and Hooke, who adopt the Hypothesis of an Ether, the Vibrations of which produce Light—Rejected by Newton, who proposes a Modification of it, but solely as an Illustration of his Views, and not as a Truth—Light is neither Ether, nor its Vibrating Motion—Corpuscles from the Sun act upon the Ether—Hooke claims Newton's Hypothesis as contained in his Micrographia—Discussions on the Subject—Hooke's Letter to Newton proposing a Private Discussion as more suitable—Newton's Reply to this Letter, acknowledging the Value of Hooke's Discoveries—Oldenburg the Cause of the Differences between Hooke and Newton—Newton's Letter to Boyle on the Subject of Ether—His Conjecture on the Cause of Gravity—Newton supposed to have abandoned the Emission Theory—Dr. Young's Supposition incorrect—Newton's mature Judgment in favour of the Emission Theory.

In the years 1675 and 1676, when Newton was engaged in his fruitless controversy with the Dutch professors, his mind was directed to a great variety of subjects. Collins[1] informs his correspondent, James Gregory, that he had not written to Newton or even seen him for these *eleven* or *twelve* months; that he did not wish to trouble him, as he was "intent upon chemical studies and practices," and that Newton and Barrow had "begun to think mathematical speculations at least dry, if not somewhat barren." His attention was at this time occupied with the subject of the moon's libration. In a letter to Oldenburg in 1673, in reference to Huygen's work on Central Forces, he mentions that "he had sometimes thought that the moon's libration might depend upon her *conatus* from the sun and earth compared together, till he apprehended a better cause." This better cause he communicated in 1675 to Nicholas

[1] October 19, 1675. *Macclesfield Correspondence,* vol. ii. p. 280.

Mercator, who published it in the following year in his Astronomical Institutions.[1] Galileo had discovered and explained the diurnal libration, arising from the spectator not viewing the moon from the centre of the earth, but it was reserved for Newton to explain the libration in longitude, which Hevelius, its discoverer, had ascribed to the displacement of the centre of the moon's orbit from the centre of motion. He showed that it was occasioned by the inequalities of the moon's motion in an elliptic orbit round the earth, combined with the uniformity of her motion round her axis. In the same letter to Mercator he showed that the libration in latitude arose from her axis of rotation being inclined 88° 17′ to the ecliptic.

About this time we find Newton occupied with a subject very different from his usual pursuits—taking an interest, like a country gentleman, in the planting of fruit trees for the manufacture of cider. It does not appear how his attention was directed to this subject. A reference is made to it in a letter to Oldenburg, in November 1676; but we have been fortunate enough to find among his papers a previous letter to the same gentleman, in September, which we need make no apology for inserting here.

"*September* 2, 1676.

"SIR,—I have now made what inquiry I can into the state we are in for planting, and find there are some gentlemen that of late have begun to plant, and seem to incline more and more to it, but I cannot hear of any professed nurseryman we have. Our gardeners find more profit in cherry trees, and so stock their ground almost wholly with them. The chief of them plant some fruit trees, but it is to find the gentry with plants: to whom I am apt to think your proposition will prove a very reasonable one, considering the new humour of planting that begins to grow among them. But in order to

[1] "Harum . . . librationum causas Hypothesi elegantissima explicavit nobis vir cl. Isaac Newton, cujus humanitati hoc et aliis nominibus plurimum debere me lubens profiteor."—Mercator's *Institutiones Astronomicæ*, p. 286.

promote the design, I am desired to inquire what sort of trees your friend can furnish us with, at what rates, which way they can most conveniently be conveyed to so great a distance, and what may be the charges of carriage. Also, whether they are to be sent in cions or grafts; the first being more convenient for carriage, and so rather to be wished, unless those trees be found best which are grafted on their native soil. I perceive the gardener I mentioned (Mr. Blackley by name) would gladly embrace the proposal, and provide himself with more ground than he has, for a nursery, to stock his neighbours, if he found he can have good sorts of trees, and the carriage make them not too dear.

"But, upon discoursing with people, I find we lye under one great difficulty; which is an opinion generally taken up here, that Red Streaks (the famous fruit for cyder in other parts) will not succeed in this country. The tree thrives well here, and bears as much fruit, and as good to look as in other countries; but the cyder made of it they find harsh and churlish, and so this fruit begins here to be generally neglected, and other fruit, and which they find does pretty well, but the cyder will not keep above a year, whereas that made of Red Streaks in other parts will keep three years or more. The ill success of Red Streaks here, I perceive, is generally imputed to the soil; but since the tree thrives, and bears as well here as in other parts, I am apt to think it is in the manner of making the cyder. For upon inquiry of the gardiners, I cannot find that they mixed any other fruit with the Red Streaks, which I have been told they do in the cyder countries, and am apt to believe it necessary; the juice of the finer fruit, on the one hand, sweetening and ripening the harsh juice of the Red Streaks, as that juice, on the other hand, by its slow ripening, makes the cyder keep long. Sir, if this prejudice we have against Red Streaks could be removed, it would much promote the design of planting, and double the benefit of it to us by bettering the cyder; and therefore I make bold to desire you to inform me, if you know of any practical description of making cyder, printed in any author ; and if not, to desire you, if it lye in your way at any time, to inquire, about the manner of making and ordering of it. For which end give me leave to make these queries :—What sort of fruit are best to be used, and in what proportion they are to be mixed, and what degree of ripeness they ought to have ? Whether it be material to press them as soon as gathered, or to pare them ? Whether there be any circumstances to be observed in pressing them ? or what is the best way to do it ? If you can direct us to, or procure for us a short narrative of the way of making and ordering cyder in the cyder countries, which takes in a resolution of these, or the most material of these queries, you will oblige your humble servant,

"Is. Newton."

"Sr. If my last letter be not yet sent to Mr. Lucas, I desire you would, for preventing any suspicion of insincerity, insert this parenthesis (as is well known here) between the words [and written a tractate on that subject], and [wherein I had set down] in the latter part of my letter."[1]

In November 1676, Newton addresses another letter to Oldenburg in the following terms :—

"I am desired to write to you about procuring a recommendation of us to Mr. Austin, the Oxonian planter. We hope your correspondent will be pleased to do us that favour as to recommend us to him, that we may be furnished with the best sort of cider fruit-trees. We desire only about 30 or 40 graffs for the first essay, and if these prove for our purposes, they will be desired in greater numbers. We desire graffs rather than sprags, that we may the sooner see what they will prove. They

[1] Newton's letter had been forwarded to Mr. Lucas, and therefore the sentence does not appear in it.—See *Phil. Trans.* No. 128, p. 703.

are not for Mr. Blackley, but some other persons about Cambridge."[1]

The friend mentioned in one of these letters, and the correspondent in the other, was the Rev. Dr. John Beal, Rector of Yeovil, in Somersetshire, who, in imitation of his father and great-grandfather, had distinguished himself by his zeal in the plantation of orchards for the making of cider.[2]

But though thus occasionally occupied with other subjects, he was at this time diligent in the prosecution of his optical researches. On the 13th November 1675, he intimated to Oldenburg, "that he had some thoughts of writing a further discourse about colours, to be read at one of your assemblies, but find it yet against the grain to put pen to paper any more on that subject. But, however, I have one discourse by me on that subject, written when I sent my first letter to you about colours, and of which I then gave you notice. This you may command when you think it may be convenient, if the custom of reading weekly discourses still continues." Mr. Oldenburg having been desired by the Society to thank him for this offer, and to desire him to send this discourse as soon as he pleased, Newton again writes to him on the 30th November, "that he intended to have sent the papers this week, but that upon reviewing them it came into his mind to write another little scribble to accompany them." This little scribble was his "Hypothesis," to which we shall presently refer.

The discourse above referred to was produced in manuscript on the 9th December 1675, with the title of—"A Theory of Light and Colours, containing partly an Hypothesis to explain the properties of light discoursed of by him in his former papers, partly the principal phenomena of the various colours exhibited by thin plates or bubbles, esteemed to be of a more difficult consideration, yet to depend also on the said properties of

[1] Edleston's *Correspondence*, App. No. xvi. p. 260.

[2] He wrote a work entitled, *Herefordshire Orchards a Pattern for England*, 1656. See Birch's *Hist. of the Royal Society*, vol. iv. p. 235.

light." This paper was introduced by the following letter to Oldenburg, which possesses considerable interest.

"SIR,—I have sent you the papers I mentioned, by John Stiles. Upon reviewing them I find some things so obscure as might have deserved a further explication by schemes; and some other things I guess will not be new to you, though almost all was new to me when I wrote them. But as they are, I hope you will accept of them, though not worth the ample thanks you sent. I remember in some discourse with Mr. Hooke, I happened to say that I thought light was reflected, not by the parts of glass, water, air, or other sensible bodies, but by the same confine or superficies of the ethereal medium which refracts it, the rays finding some difficulty to get through it in passing out of the denser into the rarer medium, and a greater difficulty in passing out of the rarer into the denser; and so being either refracted or reflected by that superficies, as the circumstances they happened to be in at their incidence make them able or unable to get through it. And for confirmation of this, I said further, that I thought the reflexion of light, at its tending out of glass into air, would not be diminished or weakened by drawing away the air in an air-pump, as it ought to be if they were the parts of air that reflected; and added, that I had not tried this experiment, but thought he was not unacquainted with notions of this kind. To which he replied, that the notion was new, and he would the first opportunity try the experiment I propounded. But upon reviewing the papers I sent you, I found it there set down for trial; which makes me recollect that about the time I was writing these papers, I had occasionally observed in an air-pump here at Christ's College, that I could not perceive the reflexion of the inside of the glass diminished in drawing out the air. This I thought fit to mention, lest my former forgetfulness, through my having long laid aside my thoughts on these things, should make me seem to have set down for certain what I never tried.

"Sir,—I had formerly purposed never to write any hypothesis of light and colours, fearing it might be a means to engage me in vain disputes; but I hope a declared resolution to answer nothing that looks like a controversy, unless possibly at my own time upon some by-occasion, may defend me from that fear. And therefore, considering that such an hypothesis would much illustrate the papers I promised to send you, and having a little time this last week to spare, I have not scrupled to describe one, so far as I could on a sudden recollect my thoughts about it; not concerning myself, whether it should be thought probable or improbable, so it do but render the paper I send you, and others sent formerly, more intelligible. You may see by the scratching and interlining it was done in haste; and I have not had time to get it transcribed, which makes me say I reserve a liberty of adding to it, and desire that you would return these and the other papers when you have done with them. I doubt there is too much to be read at one time, but you will soon see how to order that. At the end of the hypothesis you will see a paragraph, to be inserted as is there directed. I should have added another or two, but I had not time, and such as it is I hope you will accept it.—SIR, I am your obedient servant, "IS. NEWTON."

The Hypothesis,[1] to which this letter is introductory, possesses many points of historical interest. Descartes was the first philosopher who maintained the existence of an ether, a medium more subtle than air, filling the interstices of air, and occupying the pores of glass and all transparent bodies. He considered the ether to be composed of a continued series of molecular globules, along which a motion was propagated constituting light and colour.[2] Dr. Hooke, who adopted the

[1] See APPENDIX, No. II.

[2] Dr. Whewell states that Descartes regarded light as "consisting of small particles *emitted* by the luminous body," but Mr. Vernon Harcourt (Letter to Lord Brougham, p. 32) has shown the incorrectness of this opinion. See *Œuvres* de Descartes, tom. vii. pp. 193, 240.

general view of Descartes, maintained that "the parts of bodies when briskly agitated excite vibrations in the ether which are propagated every way from these bodies in straight lines, and cause a sensation of light by beating and dashing against the bottom of the eye; something after the manner that vibrations in the air cause a sensation of sound by beating against the organs of hearing."[1] In his reply to Hooke, on the 11th of July 1673, Newton distinctly states that this, which he calls the fundamental supposition in Hooke's hypothesis, "*seems itself impossible;* namely, that the waves or vibrations of any fluid can, like the rays of light, be propagated in straight lines, without a continual and very extravagant spreading and bending every way into the quiescent medium where they are terminated by it. *I am mistaken if there be not both experiment and demonstration to the contrary.*"

In thus summarily rejecting Hooke's hypothesis, Newton suggests a modification of it, or a form in which it will be better fitted to account for the phenomena, or to use his own expression,—"The most free and natural application of this hypothesis I take to be this—that the agitated parts of bodies, according to their several figures, sizes, and motions, do excite vibrations in the ether of various depths or sizes, which being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour; but if by any means those of unequal sizes be separated from one another, the largest beget a sensation of a red colour, the least or shortest of a deep violet, and the intermediate ones of intermediate colours."[2] Now this modification of Hooke's hypothesis has been very erroneously regarded as an expression of Sir Isaac's own views, whereas he merely gives it as a better form of a hypothesis, the fundamental position of which he pronounces impossible, and contrary both to experiment and demonstration. In judging of Sir Isaac's Hypothesis of 1675,

[1] Newtoni *Opera*, tom. iv. pp. 325, 326.

[2] *Phil. Trans.* 1672, No. 88, p 5088.

it is necessary to keep this in view, as it appears to be quite clear that this hypothesis is not what he believes, but what he found it necessary to draw up for the information of many of his friends. "Having observed," he says, "the heads of some great virtuosos to run much upon hypotheses, as if my discourses wanted a hypothesis to explain them by, and found that some, when I could not make them take my meaning, when I spoke of the nature of light and colours abstractedly, have readily apprehended it when I *illustrated* my discourse with an hypothesis; *for this reason* I have here thought fit to send you a description of the circumstances of this hypothesis, as much tending to the *illustration* of the papers I herewith send you."

In order to prevent any misapprehension of his meaning, he goes on to say, "that he shall not assume either this or any other hypothesis;" yet while he is describing this hypothesis "he shall *sometimes, to avoid circumlocution, and to represent it more conveniently, speak of it as if he assumed it, and propounded it to be believed.*"

With this caution, he supposes an ethereal medium rarer than air, subtler, and more elastic, not one uniform matter, but "compounded of various ethereal spirits or vapours, with the phlegmatic body of ether. The whole frame of nature may be nothing but various contextures condensed by precipitation, and after condensation, wrought into various forms, at first by the immediate hand of the Creator, and ever since by the power of nature; which, by virtue of the command, increase and multiply, became a complete imitator of the copies set her by the protoplast." "Thus," he adds, "perhaps may all things be originated from ether." Newton then proceeds to describe an electrical experiment, which afterwards excited much interest in the Society. He laid upon a table a round piece of glass about two inches broad, set in a brass ring, so as to keep the glass about the sixth of an inch from the table, the air being enclosed on all sides by the ring. Having placed some small pieces of paper within the ring, and rubbed the glass briskly with some rough substance, the pieces of thin paper began to be attracted and fly about even after the friction had ceased. From this result he conceived that some subtle matter lying condensed in the glass was rarefied by friction as water is rarefied into vapour by heat, and by "moving and circulating variously, actuates the pieces of paper till it returns into the glass and be re-condensed there." He next supposes that this ether may be imbibed by the earth, and also copiously by the sun, in order to preserve his shining, and keep the planets from receding farther from him; that is, to increase his "gravitating attraction, which may be caused by the continual condensation of some very subtle gummy or unctuous substance diffused through the ether." And as if he were amusing himself with the extravagance of his speculations, he adds, "And *they that will may also* suppose that this spirit affords, or carries with it thither, the solary fuel, and material principle of light, and that the vast ethereal spaces between us and the stars are for a sufficient repository for this food of the sun and planets!" If we laugh at Kepler's firm belief that the earth and other planets are enormous living animals taking their daily and nightly alternations of sleeping and waking, we may be allowed to smile when Newton condescends to feed them with the nectar and ambrosia of the ethereal domains. In the same extravagance of speculation he supposes that the soul may have an immediate power over the whole ether in any part of the body, producing, by processes which he invents, the swelling and shrinking of the muscles, and the animal motions which result from it.

In passing from "the effects and uses of ether" to the "consideration of light," he supposes that light "is neither ether, nor its vibrating motion, but something of a different kind propagated from lucid bodies," such as "multitudes of small and swift corpuscles of various sizes springing from shining bodies, at great distances, one after another, but yet without any sensible interval of time." That it is different from the vibrations of the ether, he infers from the existence of shadows, and the colours of thin plates. His next supposition is, "that light and ether mutually act upon one another, ether in refracting light, and light in warming ether;" and, after some farther observations on this mutual action, he goes on to explain the manner in which refraction and reflexion are produced upon this hypothesis, and the cause of transparency, opacity, and colour. His discourse concludes with an application of the hypothesis to the colours of thin plates, to the inflexion of light, and to the colours of natural bodies,—subjects to which we shall presently direct the reader's attention.

After the reading of the first part of this discourse on the 9th December, Mr. Hooke said, "that the main of it was contained in his *Micrographia*, which Mr. Newton had only carried farther in some particulars." When this remark was communicated to Newton, he seems to have been greatly offended, and, on the 21st December, he wrote a letter to Oldenburg, pointing out the difference between his hypothesis and that of Dr. Hooke. Although "he is not much concerned at the liberty of Mr. Hooke's insinuation," yet he wishes to "avoid the savour of having done anything unjustifiable or unhandsome" to him. He therefore separates the part of the hypothesis that belongs to Descartes and others, and leaves to Hooke the merit of having changed Descartes' progressive motion of the ether into a vibrating one,—"the rotation of the globuli to the obliquation of pulses, and the accelerating their rotation on the one hand, and retarding it on the other, by the quiescent medium to produce colours, to the like action of the medium on the two ends of his pulse for the same end." He gives Hooke the credit also of explaining the phenomena of thin plates, and also the colours of natural bodies, fluid and solid.[1] In the other two paragraphs of the letter, he details more

[1] Newtoni *Opera*, tom. iv. pp. 378-381; or Birch, vol. iii. p. 278.

specifically the difference between his explanations and those of his rival.[1]

These controversial discussions seem to have annoyed Hooke as much as they did Newton, and, instead of publicly replying to the two last communications of Newton, he addressed a letter to him, which, with Newton's answer, we had the good fortune to discover among the family papers. These letters are highly interesting; and we are persuaded that those who have had occasion to animadvert on the conduct of Hooke, will peruse this letter with much satisfaction.

Robert Hooke—"These to my much esteemed friend, Mr. Isaack Newton, at his chambers in Trinity College in Cambridge.

"Sʀ,—The hearing a letter of yours read last week in the meeting of the Royal Society, made me suspect that you might have been some way or other misinformed concerning me; and this suspicion was the more prevalent with me, when I called to mind the experience I have formerly had of the like sinister practices. I have therefore taken the freedom, which I hope I may be allowed in philosophical matters to acquaint you of myself. First, that I doe noe ways approve of contention, or feuding or proving in print, and shall be very unwillingly drawn to such kind of warre. Next, that I have a mind very desirous of, and very ready to embrace any truth that shall be discovered, though it may much thwart or contradict any opinions or notions I have formerly embraced as such. Thirdly, that I do justly value your excellent disquisitions, and am extremely well pleased to see those notions promoted and improved which I long since began, but had not time to compleat.

[1] In a paper entitled "Observations," which accompanied this letter, but which was not printed, Newton says that Hooke, in his *Micrographia*, had "delivered many very excellent things concerning the colours of thin plates, and other natural bodies, which he had not scrupled to make use of as far as they were for his purpose."

That I judge you have gone farther in that affair much than I did, and that as I judge you cannot meet with any subject more worthy your contemplation, so I believe the subject cannot meet with a fitter and more able person to inquire into it than yourself, who are every way accomplished to compleat, rectify, and reform what were the sentiments of my younger studies, which I designed to have done somewhat at myself, if my other more troublesome employments would have permitted, though I am sufficiently sensible it would have been with abilities much inferior to yours. Your design and mine are, I suppose, both at the same thing, which is the discovery of truth, and I suppose we can both endure to hear objections, so as they come not in a manner of open hostility, and have minds equally inclined to yield to the plainest deductions of reason from experiment. If, therefore, you will please to correspond about such matters by private letters, I shall very gladly embrace it; and when I shall have the happiness to peruse your excellent discourse (which I can as yet understand nothing more of by hearing it cursorily read), I shall, if it be not ungrateful to you, send you freely my objections, if I have any, or my concurrences, if I am convinced, which is the more likely. This way of contending, I believe, to be the more philosophical of the two, for though I confess the collision of two hard-to-yield contenders may produce light, [yet] if they be put together by the ears by other's hands and incentives, it will [produce rath]er ill concomitant heat, which serves for no other use but . . . . kindle—cole. S^r^, I hope you will pardon this plainness of, your very affectionate humble serv^t^,

"1675-6. ROBERT HOOKE."

To this letter Newton sent the following reply :—

"CAMBRIDGE, *February* 5, 1675-6.

"DR. SIR,—At the reading of your letter I was exceedingly pleased and satisfied with your generous freedom, and think you have done what becomes a true philosophical spirit. There is nothing which I desire to avoyde in matters of philosophy more than contention, nor any kind of contention more than one in print; and, therefore, I most gladly embrace your proposal of a private correspondence. What's done before many witnesses is seldom without some further concerns than that for truth; but what passes between friends in private, usually deserves the name of consultation rather than contention; and so I hope it will prove between you and me. Your animadversions will therefore be welcome to me; for though I was formerly tyred of this subject by the frequent interruptions it caused to me, and have not yet, nor I believe ever shall recover so much love for it as to delight in spending time about it; yet to have at once in short the strongest objections that may be made, I would really desire, and know no man better able to furnish me with them than yourself. In this you will oblige me, and if there be any thing else in my papers in which you apprehend I have assumed too . . . . . . If you please to reserve your sentiments of it for a private letter, I hope you [will find that I] am not so much in love with philosophical productions but that I can make them yield. . . . . . . But, in the mean time, you defer too much to my ability in searching into this subject. What Descartes did was a good step. You have added much several ways, and especially in considering the colours of thin plates. *If I have seen farther, it is by standing on the shoulders of giants.* But I make no question you have divers very considerable experiments beside those you have published, and some, it's very probable, the same with some of those in my late papers. Two at least there are, which I know you have often observed,—the dilatation of the coloured rings by the obliquation of the eye, and the apparition of a black spot at the contact of two convex glasses, and at the top of a water-bubble; and it's probable there may be more, besides others which I have not made, so that I have reason to defer as much or more in this respect to you, as you would to me.[1] But not to insist on this, your letter gives me occasion to inquire regarding an observation you was propounding to me to make here of the transit of a star near the zenith. I came out of London some days sooner than I told you of, it falling out so that I was to meet a friend then at Newmarket, and so missed of your intended directions; yet I called at your lodgings a day [or] two before I came away, but missed of you. If, therefore, you continue . . . . . . to have it observed, you may, by sending your directions, command . . . . . . your humble servant, "Is. Newton."

These beautiful letters, emulous of good feeling and lofty principle, throw some light on the character and position of two of the greatest of our English philosophers, and we cannot read their mutual confessions and desires without an anxious hope that two such men may never again be placed in a state of intellectual collision. In alluding to the sinister practices of some intermeddling friend, and to the evil consequences of two hard-to-yield contenders being put together by the ears by other's hands and incentives, Hooke evidently refers to his colleague, Mr. Oldenburg. It was not unlikely that the secretary to the Royal Society, and its Curator and Professor of Mechanics, might have occasional grounds of difference without any imputation upon their social or moral character; but this official jealousy, whatever was its amount, was increased in a high degree during the disputes between Hooke and Hevelius on the subject of plain and telescopic sights, and between Hooke and Huygens respecting the invention of pendulum clocks. These disputes were running high about the time when Newton's discourse on colours was before the Royal Society, and in both of them Oldenburg took a keen and active part against Hooke. It was, therefore, no improbable supposition, that in communicating to Newton what Hooke had said at the Society,

[1] In his *Optics*, published many years after this, in 1704, Newton does not give Hooke the credit of having made these observations.

Oldenburg had given it too high a colouring, or even artfully misrepresented it. In a subsequent dispute, in 1686, about the law of gravity, when Newton made some severe animadversions on Hooke's claim, Dr. Halley informs him in reply, that "he feared Mr. Hooke's *manner* of claiming the discovery had been *represented in worse colours than it ought.*" With his usual good feeling, Newton thus expressed his regret: "Now that I understand he was *in some respects misrepresented to me, I wish I had spared the postscript in my last.*"

When Hooke, in the case more immediately before us, stated "that the main of Newton's discourse was contained in his *Micrographia*, which he had only carried further in some particulars," he did not do justice to the valuable communication of his rival; but, on the other hand, we have it on the evidence of Newton himself, that he did not, in his discourse, give Hooke the same credit for his discoveries which he afterwards did in the letter that he addressed to him. It has been too much the practice of the admirers of Newton to assail the memory of Hooke with ungenerous animadversions, and unmanly abuse. M. Biot has even ventured to describe him as "a bad man," as if he added to the intellectual fame of Newton by the moral depreciation of his rival. We cannot give our sanction to so harsh a judgment. Under a due sense of the imperfections of our common nature, and influenced by the charity which thinketh no evil, we may find in the physical constitution and social position of Hooke, and to a certain extent in the injustice of his enemies, some apology for that jealousy and quickness of temper which may have been more deeply regretted by himself than it was felt by others.

After the publication of his "Hypothesis, explaining the Properties of Light," Newton seems to have been conversing with Robert Boyle on its application to chemistry, and on the 28th February 1679, he addressed a letter to him on the subject, in fulfilment of a long deferred promise. The views which he here presents to his friend, he characterizes as in digested and unsatisfactory to himself, and he adds, that "as it is only an explication of qualities that is desired," he "sets down his apprehensions in the form of suppositions." He supposes a subtle and elastic ether to pervade all gross bodies, and to stand rarer in their pores than in free space, being so much the rarer as their pores are less. The ether within solid and fluid bodies diminishes in density towards their surface, while the ether without all such bodies diminishes in density towards their surface. According to this theory there is a certain space within solid and fluid bodies, and a certain space without them, which Newton calls "the space of the ether's graduated rarity." On these suppositions he tries to explain the inflexion of light in passing through this space, the colours of minute particles, and of natural bodies, the repulsion and attraction of bodies coming into contact, the action of menstrums upon bodies, the phenomena of effervescence and ebullition, and the transmutation of gross substances into aërial ones. He conceives the confused mass of vapours, air, and exhalations, which we call the atmosphere, to be nothing else but the particles of all sorts of bodies of which the earth consists, separated from one another, and kept at a distance by the said principle, and he concludes this remarkable speculation with a conjecture about the cause of gravity.

"I shall set down," he says, "one conjecture more, which came into my mind even as I was writing this letter ; it is about the cause of gravity. For this end I will suppose ether to consist of parts differing from one another in *subtlety* by indefinite degrees ; that in the pores of bodies there is less of the grosser ether in proportion to the finer, than in open spaces ; and consequently, that in the great body of the earth there is much less of the grosser ether in proportion to the purer, than in the regions of the air ; and that yet the grosser either in the air affects the upper regions of the earth, and the finer ether in the earth the lower regions of the air, in such a manner, that from the top of the air to the surface of the earth, and again from the surface of the earth to the centre thereof, the ether is insensibly finer and finer. Imagine now any body suspended in the air or lying on the earth; and the ether being by the hypothesis grosser in the pores which are in the upper parts of the body, than in those which are in its lowest parts, and that grosser ether being less apt to be lodged in these pores than the finer ether below, it will endeavour to get out and give way to the purer ether below, which cannot be without the bodies descending to make room above for it to go out into."[1]

The Hypothesis of Newton, and his other speculations regarding ether, have led some writers to suppose that he had abandoned the corpuscular or emission theory, in which light is supposed to be produced by material particles projected from luminous bodies, and that he had adopted views not very different from those of the supporters of the undulatory theory. This opinion has been entertained chiefly on the authority of Dr. Thomas Young, in his theory of light and colours.[2] In introducing this theory, he remarks, that "a more extensive examination of Newton's writings has shown me, that he was in reality the first that suggested such a theory as I shall endeavour to maintain; and that his own opinion varies less from this theory than is now almost universally supposed."[3] "I shall collect," he adds, "from Newton's various writings, such passages as seem to be most favourable to its admission (Dr. Young's theory), and although I shall quote some papers which may be thought to have been *partly retracted* at the publication of the 'Optics,' yet I shall borrow nothing from them that can be supposed to militate against his *maturer judgment*." In another place he states in language still more explicit, "that *Newton considered the operation of an ethereal medium as absolutely necessary to the production of the most remarkable effects of light.*"

[1] Letter to Boyle, Newtoni *Opera*, tom. iv. pp. 385-395.
[2] *Phil. Trans.* 1801; or, Lectures on Natural Philosophy, vol. ii. p. 614.
[3] *Ibid.* vol. i. p. 477.

In direct contradiction to these statements, we have already found Newton distinctly maintaining "that light is neither ether nor its vibrating motion, but something of a different kind propagated from lucid bodies," such as "multitudes of small and swift corpuscles of various sizes springing from shining bodies;" and when in order to please his friends and illustrate his views, he invents a speculation "not propounded to be believed," he cannot be regarded as maintaining views at all approximating to the undulatory theory. We cannot understand how Dr. Young could overlook the language of caution in which he everywhere guards himself against its being supposed that he believes even in the existence of an ether,—language, too, so precise, that the honest meaning of its author cannot be misinterpreted.

The matured judgment of Newton, of which Dr. Young speaks, and against which his quotations directly militate, is given in the following explicit passage, published in 1717, in the second edition of his *Optics*, revised by himself.[1]

"Are not all hypotheses erroneous in which light is supposed to consist in pression or motion propagated through a fluid medium? For in all these hypotheses the phenomena of light have been hitherto explained by supposing that they arise from new modifications of the rays, which is *an erroneous supposition*.

"If light consisted only in pression propagated without actual motion, it would not be able to agitate and heat the bodies which refract and reflect it. If it consisted in motion propagated to all distances in an instant, it would require an infinite force every moment in every shining particle to generate that motion. And if it consisted in pression or motion propagated either in an instant or in time, it would bend into the shadow. For pression or motion cannot be propagated in a fluid in right lines, beyond an obstacle which stops part of the

[1] *Optics*, edit. 3d, 1720, pp. 336, 339.

motion, but will bend and spread every way into the quiescent medium which lies beyond the obstacle. . . .

"And it is as difficult to explain by such hypotheses how rays can be alternately in fits of easy reflexion and easy transmission ; unless perhaps one might suppose that there are in all space two ethereal vibrating mediums, and that the vibrations of one of them constitute light, and the vibrations of the other are swifter, and as often as they overtake the vibrations of the first, put them into those fits. But how two *ethers* can be different through all space, one of which acts upon the other, and by consequence is reacted upon, without retarding, shattering, dispersing, and compounding one another's motions, is inconceivable. And against filling the heavens with fluid mediums, unless they be exceeding rare, a great objection arises from the regular and very lasting motions of the planets and comets in all manner of courses through the heavens. For thence it is manifest that the heavens are void of all sensible resistance, and by consequence of all sensible matter."

That this passage contains the mature and the latest judgment of Newton on the subject of light cannot be doubted. All the quotations from Newton referred to by Dr. Young bear the date of 1672 and 1675, and the letter to Boyle the date of 1679 ; but the preceding passage was published in 1704, 1717, and 1721, in the lifetime of Newton, when it was in his power to alter or retract it. But in addition to this argument, we have the evidence of Leibnitz in a letter to Huygens, dated 26th April 1694, that Newton at that time was more convinced than ever of the truth of the emission theory. "I have learned," says Leibnitz, "from Mr. Fatio,[1] by one of his friends, that Mr. Newton and he have been more than ever led to believe that light consists of bodies which come actually to us from the sun, and that it is in this way that they explain the different refrangibility of light and colours, as if there were primitive bodies

[1] Fatio D'huillier, the particular friend of Newton.

which always kept their colours, and which come materially from the sun to us. The thing is not impossible, but it appears to me difficult to understand how by means of these little arrows which, according to them, the sun darts, we can explain the laws of refraction."[1]

[1] Huygenii *Exercitationes Mathematicæ, &c.*, Fascic. i. p. 173.

CHAPTER VII.

Newton's Hypothesis of Refraction and Reflexion—Of Transparency and Opacity—Hypothesis of Colours—The Spectrum supposed to be divided like a Musical String—Incorrectness of this Speculation—Hooke's Observations on the Colours of thin Plates explained by the Vibrations produced in the Ether by the luminous Corpuscles—Hooke claims this Theory as contained in his Micrographia—Newton's Researches on the Colours of Thin Plates—Previous Observations of Boyle—Hooke's elaborate Experiments on these Colours—His Explanation of them—Dr. Young's Observations upon them—Newton acknowledges his Obligations to Hooke—Newton's Analysis of the Colours seen between two Object-Glasses—Corrections of it by MM. Provostayes and Desains—Newton's Theory of Fits of easy Reflexion and Transmission—Singular Phenomenon in the Fracture of a Quartz Crystal—Newton's Observations on the Colours of Thick Plates—Recent Experiments on the same Subject.

In the preceding chapter we have given an account of the first part of Newton's discourse on light and colours, read on the 9th December 1675, and explaining his hypothesis concerning "ether and ethereal substances, and their effects and uses." In the second part of the portion read at the same meeting he proceeds to "the consideration of light" as connected with the supposed ether, that is to the cause of refraction, reflexion, transparency, and opacity.

Regarding the ether as more dense in free space than in solid bodies, and as diminishing in density towards their surface both from without and from within, Newton supposes the incurvation or bending of a ray of light, incident on such a surface, in one direction to produce refraction, and in another to produce reflexion, to be effected within "the space of ether's graduated rarity," or "physical superficies." In the case of refraction, from air to glass, the ray passes from denser into rarer ether, and is incurvated from the perpendicular in its passage through the physical superficies ; whereas in reflexion from a dense medium, such as glass into air, it is incurvated upwards or towards the glass, and the incurvation may be such that the ray does not emerge but suffer total reflexion.

In order to account by the agency of ether for the simultaneous refraction and reflexion of light incident upon the same surface of glass or water, Newton supposes "that ether in the confine of two mediums is less pliant and yielding than in other places, and so much the less pliant (or, 'more rigidly tenacious') by how much the mediums differ in density." When light therefore, that is small corpuscles, falls upon "this rigid resisting ethereal superficies, it puts it into a vibrating motion, so that *the ether therein is continually expanded and compressed by turns.*" When a ray of light is incident upon it "while it is much compressed, it is too dense and stiff to let the ray pass through, and so *reflects* it ; but the rays that are incident upon it at other times, when it is either expanded by the interval of two vibrations, or not too much compressed or condensed, go through and are *refracted.*"

When the ether is of the same rarity in every pore, or when the ether is evenly spread by its continual vibrations into all the pores when they do not exceed a certain size, the light will pass freely through the body, or the body will be *transparent.* But when the pores exceed a certain size, the density of the ether will be greater than that which surrounds it, and the light being refracted or reflected at its superficies, the body will be *opaque.*

On the 16th December the second portion of Newton's discourse was read, in which he applies his hypothesis to the explanation of *colours.* For this purpose he supposes the particles of light to have different degrees of "bigness, strength, or power," *red* having the *largest*, and *violet* the *least* degree of any of these qualities. When light, therefore, is incident on the "refracting superficies," the smallest particles, namely, the violet, will be most incurvated or refracted, and the red the least; and when these fall upon the refracting superficies of the retina, they will there excite "the sensation of various colours according to their bigness and mixture, the *biggest* with the *strongest* colours *reds* and *yellows*, the *least* with the *weakest blues* and *violets*, the *middle* with *green*, and a *confusion of all* with *white;* much after the manner that in the sense of hearing, nature makes use of aërial vibrations of several bignesses to generate sounds of divers tones." Pursuing this idea, "the analogy of nature," he conjectures, "that colour may possibly be distinguished into its principal degrees, *red*, *orange*, *yellow*, *green*, *blue*, *indigo*, and *deep violet*, on the same ground that sound within an eighth is graduated into tones." In order to test this speculation by experiment, he forms a distinct spectrum, and, "because his own eyes are not very critical in distinguishing colour," he employs a friend to whom he has not communicated his thoughts, to measure the lengths of the different coloured spaces. The differences between the measures thus obtained, he says, "were but little, especially towards the red end, and taking means between these differences, the length of the image (reckoned not by the distance of the verges of the semicircular ends, but by the distance of the centres of those semicircles, or length of the strait sides as it ought to be) was divided *in about* the same proportion *that a string is between the end and the middle to sound the tones in the eighth.*"

Ingenious as this speculation is, it is contradicted by all the recent discoveries respecting the prismatic spectrum, of which we have given an account in a preceding chapter. It is not even true in the spectrum which Newton himself observed. There are not *seven* colours in any spectrum, and even if we divide it into such a number of parts, the divisions have no resemblance to those of a musical string.

From the explanation of colours produced by refraction, Newton proceeds to explain those produced by reflexion, namely, the colours of thin plates described by Hooke in his Micrographia.

In order to do this, he supposes that the ethereal vibrations excited by a ray move faster than the ray itself, and so "overtake and outrun it, one after another." When light, therefore, is incident upon a thin transparent plate, the waves, excited by its passage through the first surface, overtaking it one after another, till it arrive at the second surface, will cause it to be there reflected or refracted according as the condensed or the expanded part of the wave overtakes it there. If the plate be so thin that the condensed part of the first wave overtakes the ray at the second surface, it must be reflected there; if *double* that thickness, so that the following rarified part of the wave, that is, the space between that and the next wave, overtake it, there it must be transmitted; if *triple* the thickness, so that the condensed part of the *second* wave overtake it, there it must be reflected, and so where the plate is *five*, *seven*, or *nine* times that thickness, it must be *reflected* by reason of the *third*, *fourth*, or *fifth* wave overtaking it at the second surface; but when it is *four*, *six*, or *eight* times that thickness, so that the ray may be overtaken there, by the dilated interval of those waves, it shall be *transmitted*, and so on; the second surface being made able or unable to reflect according as it is condensed or expanded by the waves.

In this way he explains the coloured rings produced by pressing a convex lens against a plain glass; and he concludes this portion of his discourse, namely, his "Hypothesis," by applying it to certain phenomena of Inflexion or Diffraction, as observed by Grimaldi.

It was after the reading of this portion of his discourse that Hooke said, "that the main of it was contained in his Micrographia, which Mr. Newton had only carried farther in some particulars,"—a remark which led to the correspondence with Oldenburg and Hooke, which we have given in the preceding chapter.

In the remainder of his discourse, Newton gives an account of his beautiful experiments on the colours of thin plates; but before we enter upon their consideration, we must notice the previous observations of Boyle and Hooke, in order that we may apportion to Hooke and to Newton the discoveries which they actually made. In the details into which this will lead us, we shall see two great minds striving for victory,—calling forth all their powers to surmount the difficulties which beset them in their path,—deviating from the rigorous process of research which both of them recognised, and perhaps forgetting, in the ardour of their pursuit, some of those courtesies which are now deemed essential in intellectual warfare.

In his book on Colours,[1] Mr. Boyle informs us, that divers, if not all essential oils, as also spirit of wine, when shaken, "have a good store of bubbles, which appear adorned with various and lively colours." He mentions also, that bubbles of soap and turpentine exhibit the same colours, which "vary according to the incidence of the sight and the position of the eye;" and he had seen a glass-blower blow bubbles of glass, which burst, and displayed "the varying colours of the rainbow, which were exceedingly vivid."

In the year 1664, Hooke published, in his *Micrographia*,[2] a very interesting *chapter of the colours observable in Muscovy glass* (mica), *and other thin bodies*, in which he has described many new phenomena.

1. In several parts of plates of mica, he found white specks or flaws diversely coloured with all the colours of the rainbow, the colours being ranged in rings, encompassing, and having the same form as the speck. The colours from the middle of the spot were *blue*, *purple*, *scarlet*, *yellow*, and *green*, the same series of colours recurring *nine* or *ten* times.

[1] Experiments and Observations touching Colours. Exp xix. p. 243. London, 1664.

[2] "*Micrographia*, or some Physiological Descriptions of Minute Bodies made by magnifying-glasses, with Observations and Inquiries thereupon." In many of the copies the date is 1667, but the title-page which bears this date was a trick of the printer, to indicate a second edition, which was never printed. The imprimatur of the President of the Royal Society is Nov. 23, 1664. See Ward's *Life of Hooke*, in the Lives of the Gresham Professors, p. 190.

2. By pressing together two pieces of plate-glass with his forefingers and thumbs, he produced the same series of colours as in mica, the colours changing with the thin plate of air between the glasses. The same phenomena were produced by placing different fluids between the plates, the colours being more strong and vivid in proportion as the refractive power of the fluids differed from that of the glass-plates.

3. If the plate of air or fluid is *thickest* in the middle like a *convex* lens, or *thinnest* as in a *concave* lens, the colours will also be produced, the order of colours in the *first* case being *red*, *yellow*, *green*, *blue*, &c. ; and, in the *second*, quite *contrary*.

4. As the colours cease when the plates have a certain thickness, so they cease also when the plate has a certain thinness, the colours ending in a white and colourless ring.

5. When we cleave a plate of mica with a needle, we shall come to one of such a thickness as to exhibit a uniform colour, every different degree of thinness below this giving a different colour.

6. When *two* or *three* or more of these coloured plates are laid one upon another, they exhibit such compound colours "as one would scarce imagine would be the result of such ingredients." A faint *yellow*, for example, and a *blue*, may produce a very deep *purple*.

7. The same coloured laminæ may be obtained by blowing glass very thin; and also from bubbles of pitch, rosin, colophony, turpentine, solutions of gums, or any glutinous liquor, such as wort, wine, spirit of wine, oil of turpentine, glare of snails, soap-water, &c.

8. The same colours are produced upon polished steel by gradually tempering or softening it with a sufficient degree of heat. They are also produced on brass, copper, silver, gold, tin, but most conspicuously upon lead ; and the colours that cover the surface of the metal are nothing else than a very thin vitrified part of the heated metal.

9. The same colours are exhibited in animal bodies, as in

pearls, mother-of-pearl shells, oyster shells, and almost all other kinds of stony shells. They are seen also in muscles and tendons.

10. If we take any glutinous substance, and run it exceedingly thin upon the surface of a smooth glass, or a polished metalline body, the same colours are produced; "and in general wheresoever you meet with a transparent body thin enough, that is terminated by reflecting bodies of differing refractions from it, there will be a production of these pleasing and lovely colours."

Such is a brief account of Hooke's elaborate inquiry into the colours of thin plates. We shall now consider the theory which he invented to explain them. He considers light as produced by "a very short vibrating motion propagated every way through a homogeneous medium by direct or straight lines extended every way like rays from the centre of a sphere, and with equal velocity, so that the pulse or vibration of the luminous body will generate a sphere which will continually increase, and grow bigger, just after the same manner (though indefinitely swifter) as the waves on the surface of the water do swell into bigger circles about a point of it where, by the sinking of a stone, the motion was begun ;—whence it necessarily follows, that all the parts of these spheres, undulated through a homogeneous medium, cut the rays at right angles." Our author then proceeds to explain how refraction and reflexion take place at the confines of media, in which the "fluid undulating substance" (or ether) has different densities.

In applying this theory to the explanation of the colours of thin plates, he considers it "most evident that the reflexion from the under or farther side of the body, is the principal cause of the production of these colours." Supposing a ray "to fall obliquely on the thin plate, part thereof is reflected back by the first superfices," but, as the body is transparent, another part of the ray is refracted by the first surface, reflected by the second, and refracted again by the first surface, so that after two refractions and one reflexion, there is propagated a kind of fainter ray, whose pulse, by reason of the time spent in passing and repassing between the two surfaces, comes behind the former reflected pulse, so that hereby (the surfaces being so near together that the eye cannot discriminate them from one) this confused or duplicated pulse, whose strongest part precedes, and whose weakest follows, does produce on the retina the sensation of a yellow. If the two reflecting surfaces be yet farther removed asunder, then will the weaker pulse be so far behind, that it may be coincident with the second, third, fourth, fifth, &c., as the plate grows thicker ; "so that if there be a thin transparent body that, from the greatest thinness requisite to produce colours, does, in the manner of a wedge, by degrees grow to the greatest thickness that a plate can be of to exhibit a colour by the reflexion of light from such a body, there shall be generated such a consecution of colours, whose order, from the thin end towards the thick, shall be *yellow*, *red*, *purple*, *blue*, *green*, and these so often repeated, as the weaker pulse does lose pace with its primary or first pulse, and is coincident with a second, third, fourth, &c., pulse behind the first. And this, as it is coincident, or follows from the first hypothesis I took of colours, so upon experiment have I found it in multitudes of instances that seem to prove it."

Dr. Thomas Young has quoted nearly the whole of these passages as such an approximation to the true explanation of the colours of thin plates, that if he had not satisfied himself respecting the phenomena of this class of colours, these passages would have led him earlier to a similar opinion. The doctrine of interference is distinctly stated in them, and had Hooke adopted Newton's views of the different refrangibility of light, and applied them to his own theory of the coincidence of pulses, he would have left his rival behind in this branch of discovery.

Relying on the correctness of his views respecting the colours produced by *reflexion*, Hooke very ingeniously applied the same principle to the colours produced by *refraction;* and his objection to Newton's doctrine always was, that it was contrary to his theory. It is very obvious that hypotheses, however much they were abjured by the experimental philosophers of that day, were not only invented but admired; and Newton was thus driven to propose a hypothesis to satisfy his friends, he himself declaring that he neither believed it, nor wished them to believe it.

When this hypothesis was read, Hooke, as we have already seen, stated "that the main of it was contained in his *Micrographia*, which Mr. Newton had only carried farther in some particulars." The reader will, we think, be able to judge, from our abstract of Hooke's theory and observations, of the truth of this remark. We think it substantially true, and do not hesitate to say, that Newton has not done justice to Hooke. Excepting once, in reference to the inflexion of light, Hooke's name is never mentioned. The results of his experiments are made use of, and his theory partly adopted and altered, without any acknowledgment of the one, or notice of the other. In his vindication, read on the 21st December 1675, Newton admits that he made use of some of Hooke's observations; that he adopted the idea of a vibrating ether; and he thanks him for his explanation of opacity, and for his notice of the colours of plated bodies. In his interesting letter to Hooke, which we have given in the preceding chapter, he goes much farther, acknowledging that Hooke had added much several ways to Descartes' theory, especially in considering the colours of thin plates, and giving him the credit of two important discoveries (which we do not find in the *Micrographia*), namely, the dilatation of the coloured rings by the obliquation of the eye, and the apparition of a black spot at the contact of two convex glasses, and at the top of a water bubble. In thus justifying the criticism of Hooke, and throwing some blame on Newton, we revert with pleasure to the noble amends which he made in his private letter, when there was no "intermeddling friend" to pervert the native generosity of his character.

We have hitherto considered only that part of Newton's discourse which contained his hypothesis, and its application to refraction, reflexion, transparency, and opacity. The remaining portions of it were read at the Royal Society on the 20th January, the 3d and the 10th February 1675-6, and contain all the optical discoveries of Newton.

The portion which was read on the 20th January, contains fifteen observations. In the first three of these he describes the arcs and circles of colours, which are exhibited by pressing together the imperfectly flat surfaces of two prisms. The place where they touched was absolutely transparent, appearing like a black spot "when looked upon," and "when looked through" it seemed like a hole in the thin plate of air between the prisms. The arcs and rings were generally of many colours, and about eight or nine in number. By turning the prisms about their common axis, the rings became black and white, and were sometimes about *thirty* in number. In order to see them distinctly, and without any other colour, it was necessary to hold the eye at a considerable distance from them, and also to view them through a slit or oblong hole narrower than the pupil of the eye.

In order to observe the order of the colours more correctly, and obtain measures of the rings at different thicknesses of the plate of air between the glasses, Newton took two object-glasses, the one a plano-convex for a *fourteen* feet telescope, and the other a large double convex for one of *fifty* feet, and having laid upon this the other with its plane side downwards, he pressed them slowly together, and observed the following orders of colours, next to the pellucid or dark central spot.

Order 1st,—Dark spot, violet, blue, white, yellow, and red.
Order 2d,—Violet, blue, green, yellow, and red.
Order 3d,—Purple, blue, green, yellow, and red.
Order 4th,—Green and red.

The succeeding orders became more and more imperfect, "till after three or four more revolutions they ended in perfect whiteness."[1]

When his eye was placed perpendicularly over the glasses, he found the diameter of the first six rings, at the most luminous "part of their orbits," to be, when squared in arithmetical progression of the odd numbers, 1, 3, 5, 7, 9, 11, and the diameter of the dark rings between the more luminous ones, when squared, to be in arithmetical progression of the even numbers, 2, 4, 6, 8, 10, 12. When the rings were viewed obliquely, they became bigger, as Hooke had observed, continually swelling as the eye was removed farther from their axis.

"By measuring the diameter of the same ring at several obliquities of the eye, partly by other means, as also by making use of the two prisms for very great obliquities," Newton found its diameter, and consequently the thickness of the air at its perimeter, to be "proportional to the secant of an angle whose sine is a certain mean proportional between the sines of incidence and refraction. And that mean proportional is the first of 106 arithmetical mean proportionals between the sines of incidence and refraction counted from the lesser sine, that is, from the sine of refraction when the refraction is made out of air into water, otherwise from the sine of incidence."[2] That is, the angle to whose secant the thickness of the air is proportional, is one whose sine is to the sine of the real angle of incidence in the constant ratio of

$$\frac{106 + \frac{1}{m}}{107}$$

m being the index of refraction of the glass.

In repeating the experiment with the light of a monochromatic lamp, and measuring the angles with great care, and at

[1] The reader will observe that the orders here given, and their colours, differ somewhat from those published nearly thirty years afterwards in his "Optics."

[2] *Optics*, Book ii. Part i. Obs. 7, 18.

incidences so great as 85° 21′, MM. Provostayes and Desains obtained the following results. At an incidence of 85° 21′ the diameter of the *seventh* black ring in millionths of a millimetre, was

By observation, . . .	47·53
By Newton's Formula, . .	40·11

According to the doctrine of interference, the thickness of the plate of air should be proportional to the secant of the angle of incidence, which, in the present case, would give 47·55 for the diameter of the seventh ring, a coincidence with the experiment so remarkable, as to leave no doubt of the truth of the theory.[1]

The difference between Newton's experiment and the result of theory, is so great as to call forth the remark from Sir John Herschel,[2] that "it might be drawn into an argument against the theory, were we sure that the law of refraction at extreme incidences, and with very thin laminæ, does not vary sensibly from that of the proportional sines." The important results obtained by MM. Provostayes and Desains will teach us rather to doubt the accuracy of an unconfirmed experiment, and carefully to repeat it, than to explain it by calling in question a well established law.

By various modes of observation, Newton found the following relations between the diameter of the rings and the thickness of the plate of air :—

Diameter of the ring,	10,	$10\frac{1}{13}$,	$10\frac{1}{3}$,	$10\frac{3}{4}$,	$11\frac{2}{5}$,	$12\frac{1}{2}$,	14,	$15\frac{1}{4}$,	$16\frac{4}{5}$,	$19\frac{1}{4}$.	$22\frac{5}{7}$,	29,	35.
Thickness of the plate of air,	10,	$10\frac{2}{13}$,	$10\frac{2}{3}$,	$11\frac{1}{2}$,	13,	$15\frac{1}{2}$,	20,	$23\frac{1}{3}$,	$28\frac{1}{2}$,	37,	$52\frac{1}{4}$,	84,	$122\frac{1}{2}$.

Our author next proceeded to examine the effects of homogeneous coloured light, and was thus led to more important results. In place of *eight* or *nine* rings which he saw in the open air, he now saw more than *twenty*. In *red* light the rings

[1] *Comptes Rendus*, &c. &c., tom. xxv. p. 498. 1850

[2] *Treatise on Light*, Art. 670.

were much *larger* than in *blue* and *violet*. The thickness of the plate of air at which any red ring was produced, was to that at which the same *violet* ring was produced, as *nine* to *fourteen*. The rings were not of various colours, as before, when white light was used, but of the prismatic colour which was employed, and each ring was separated from the other by a dark ring or space. Upon placing a white paper behind the rings, Newton observed rings painted upon it of the same colour with those which were reflected, and of the same size as their intermediate dark space. Hence he concluded that the light which fell on the dark spaces was transmitted through the glasses without any change of colour, and that *the aërial interval of the glasses according to its various thickness is disposed in some places to reflect, and in others to transmit, the light of any colour, and in the same place to reflect one colour where it transmits another.*

From the examination of the colours of thin plates of air, Newton proceeded to that of the colours of thin plates of water, as exhibited in the soap-bubble. Having covered the soap-bubble with a glass shade, he saw its colours emerge in a regular order, like so many concentric rings encompassing the top of it. As the bubble grew thinner by the continual subsidence of the water, the rings dilated slowly and overspread the whole of it, descending to the bottom, where they vanished successively. When the colours had all emerged from the top, there arose in the centre of the rings a small round black spot, like that in the centre of the rings formerly described, dilating it to more than half an inch in breadth till the bubble burst.

Upon examining the rings between the object-glasses, Newton found that when they were only *eight* or *nine* in number, more than *forty* could be seen by viewing them through a prism; and even when the plate of air seemed all over uniformly white, multitudes of rings were disclosed by the prism. The same result was obtained with thin plates of water, mica, and glass.

By means of these interesting observations, Newton proceeds to show how the system of coloured rings exhibited by white light, are produced by the superposition of the rings belonging to each separate colour in the spectrum, and he constructs a diagram, explaining a method of finding the colours of which the rings are composed at any distance from their centre. He then concludes this part of his discourse with a table showing, in millionths of an inch, the different thicknesses of plates of air, water, and glass, when they exhibit the different colours in the seven rings or orders of colours. The thicknesses, for example, of *air*, *water*, and *glass*, at which no light is reflected, or at which the black of the first ring is produced, are 2, $1\frac{1}{2}$, $1\frac{1}{4}$ millionths of an inch respectively, and the thicknesses at the margin of the seventh ring are 84, 63, and $54\frac{1}{2}$ millionths of an inch. This Table, which is known by the name of *Newton's Scale of Colours*, is of great value in all optical researches, and is constantly referred to by modern writers on Optics.

This celebrated discourse is concluded by nine propositions, showing how the phenomena of thin transparent plates stand related to the colours of all natural bodies, and how the size of the component parts of such bodies may be conjectured by their colours,—a subject which will be discussed in another chapter.

Such is a brief account of Newton's discoveries respecting the colours of thin plates, and of the hypothesis of ethereal vibrations, by which he proposed to explain them. The experiments from which they were deduced were all made previous to 1675; and it does not appear that, during the remaining fifty-two years of his life, he made any other communications on optical subjects to the Royal Society. In the preface to his Treatise on Optics, dated 1704, he tells us that "*part* of the ensuing discourse about light was written at the desire of some gentlemen of the Royal Society in the year 1675, and then sent to their secretary and read at their meetings; and *the rest* was added about *twelve years after*, to complete the Theory, except the third book and the last proposition of the second, which were since put together out of scattered papers." These additions to the discourse, which were made in 1687, are no doubt his ampler discussion of the theory of the colours of natural bodies, and his theory of *fits of easy reflexion and easy transmission*, by which he explains the colours of thin plates; and what was since put together out of scattered papers, was the first part of the third book on the *inflexion of light*, and the fourth part of the second book on the *colours of thick plates*. An explanation, therefore, of the theory of fits, will form an appropriate conclusion of our account of Newton's discoveries respecting the colours of thin plates.

In the propositions of his Optics, where he explains this theory, Newton does not attempt to assign any cause by which these fits are produced. He does not inquire whether the kind of action or disposition in which they originate "consist in a circulating or vibrating motion of the ray or of the medium, or something else;" but he says, that those who require a hypothesis, "which, whether it be true or false, he does not consider, may for the present adopt the one previously explained, in which the rays of light, by impinging on any refracting or reflecting surface, excite vibrations in the refracting or reflecting medium or substance," and that the ray is refracted or reflected according as it is in that part of the vibration which conspires with or impedes its motion.[1] A popular idea may be formed of these fits of reflexion and transmission, by supposing that each particle of light, after its emission from a luminous body, revolves round an axis perpendicular to the direction of its motion, and presenting alternately to a refracting surface, which it approaches, an attractive and a repulsive pole, in virtue of which it will be refracted if the attractive pole is

[1] It is curious that Newton here makes no mention of an ethereal medium as that in which the vibrations are executed, as he does in his Hypothesis, formerly described. See p. 118.

nearest the refracting surface, and reflected if the repulsive pole is nearest that surface.

In order to explain this more clearly, let S be a ray of light which falls upon a transparent surface MN, and is *transmitted* by that surface. It is obvious that it must have been nearer its fit of transmission than its fit of reflexion when it met the surface MN at T; but whether it was exactly in its fit of transmission, or a little way from it, the theory supposes that it is put by the action of the surface into the same state as if it had begun its fit of transmission at T. Let us now suppose that its fit of reflexion takes place at R, and that these fits recur at T′, R′, T″, R″, &c., so that if there was a second transparent surface at T′ or T″, the ray would be transmitted; and if there was a second transparent surface at R′, R″, it would be reflected. The spaces TT′, T′T″, are called the intervals of the fits of transmission, and the spaces RR′, R′R″, the intervals of the fits of reflexion. Now, as the spaces TT′ RR′ are equal for light of the same colour, it is obvious that the ray R will be transmitted, if the thickness of the body is TT′, TT″, &c., that is TT′, 2 TT′, 3 TT′, or any multiple whatever of TT′, the interval of a fit of easy transmission; and as TT′ is equal to RR′, the ray R will be reflected when the thickness of the body

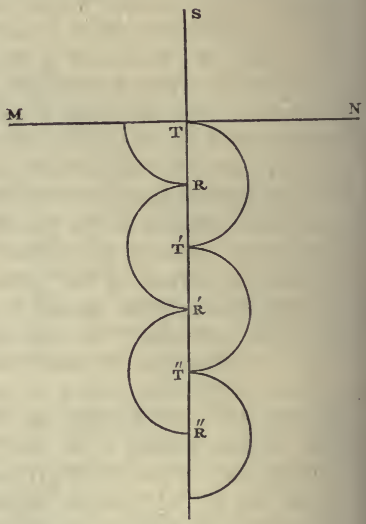


FIG. 14.

is $\frac{1}{2}$ TT′, $1\frac{1}{2}$ TT′, $2\frac{1}{2}$ TT′, $3\frac{1}{2}$ TT′, &c. If the body MN, therefore, were a plate with parallel surfaces, and if the eye were placed above it so as to receive the rays reflected perpendicularly, it would in every case see the first surface MN by the portion of light uniformly reflected from that surface ; but if the thickness of the body were TT′, 2 TT′, 3 TT′, 4 TT′, or 1000 TT′, the eye would receive no rays from the second surface, because they would be all transmitted ; and, in like manner, if the thickness were $\frac{1}{2}$ TT′, $1\frac{1}{2}$ TT′, $2\frac{1}{2}$ TT′, or $1000\frac{1}{2}$ TT′, the eye would receive all the light reflected from the second surface, because it would be all reflected. When this reflected light meets the first surface MN, on its way back from the second surface, it will be all transmitted, because it is then in its fit of transmission. At intermediate thicknesses, such as $\frac{3}{4}$ TT′, a portion only of the light will be reflected from the second surface, increasing as the thickness increased from TT′ to $1\frac{1}{2}$ TT′, and diminishing again as the thickness increased from $1\frac{1}{2}$ TT′ to 2 TT′.

Let us now suppose that the plate whose surface is MN has

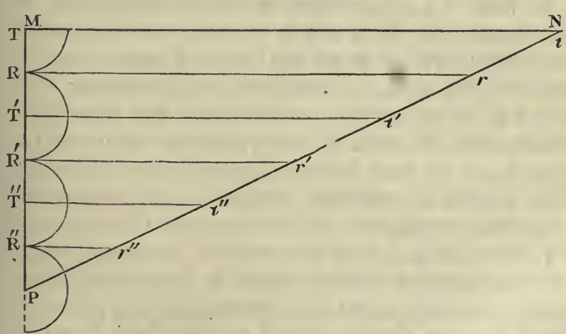


FIG. 15.

its thickness varying like a wedge MNP, *Fig.* 15, and that the eye is placed above it to receive the light which it reflects.

The interval of the fits being TT′, RR′, as before, it is evident that, near the point N, the light that falls upon the second surface tP, will be transmitted, because it is in a fit of transmission, but at the thickness TR the light will be reflected from the second surface at r, because it is then in a fit of reflexion, and again transmitted in returning through the first surface MN. In like manner, the light will be transmitted at t' and t'', and reflected at r' and r'', so that the observer will see a series of dark and luminous bands, the middle of the dark ones being at t, t', t'', and of the luminous ones at r, r', r''.

Let us now suppose that the figure is adapted to *red* light; then since the length of a fit is *greatest* in *red*, *least* in *violet*, and of an intermediate size in *yellow* light, it is obvious that in *yellow* light there will be a set of dark and luminous bands less than those in the figure, and in violet light another set less than in the yellow. When, therefore, the light incident on the plate is white, all these bands will be superimposed, and form the coloured bands already described. When the thin plate is wedge-shaped, the bands will be parallel. When it has the form of a concave lens, like air or vacuity between two object-glasses, the bands will be circular, with the lowest tints in the centre. When it has the form of a convex lens, like the plates of air in mica, the bands will also be circular, with the lowest tints at the margin; and when the thin plate has different thicknesses like a film of blown glass, the bands will have no regular shape, the same thickness giving always the same colour.

The preceding doctrine of fits has always been regarded as an ingenious explanation of the colours of thin plates. It is not given by its author as a theory or a hypothesis, but simply as an expression of the facts which he has observed; and yet it has to a certain extent the character of a hypothesis, in so far as it assumes that the second surface of the plate does not in every part of it reflect light like the first, whereas in the theory of interference, certain portions thus reflected are destroyed before they reach the eye of the observer.

With the exception of the interesting observations of MM. Provostayes and Desains already referred to, no discovery of any great importance has been made on the subject of thin plates since the time of Newton. We have had occasion to observe a number of curious phenomena in the thin plates of decomposed glass when acting upon light in a state of combination. The colours which they reflect and transmit are not deducible from any theory of light, and have an intimate connexion with the absorption of light by coloured media.[1]

Among natural phenomena illustrative of the colours of thin plates, we have found none more remarkable than one exhibited by the fracture of a large crystal of quartz of a smoky colour, and about two and a quarter inches in diameter. The surface of fracture, in place of being a face of cleavage, or irregularly conchoidal, as we have sometimes seen it, was filamentous like a surface of velvet, and consisted of short fibres so small as to be incapable of reflecting light. Their size could not have been greater than the third of the millionth part of an inch, or one-fourth of the thinnest part of the soap-bubble when it exhibits the black spot where it bursts.[2]

Although Newton did not communicate his observations on the colours of *thick plates* to the Royal Society in his discourse on light and colours, but "put them together out of scattered papers" some time before the publication of his Optics,[3] yet this is the proper place for bringing them under the notice of the reader.

The colours of thick plates arise from a quantity of light scattered in all directions from the little inequalities or imperfections which exist in the surface of a glass mirror either silvered or unsilvered. In order to observe them, a sunbeam is

[1] "On the Connexion between the Phenomena of the Absorption of Light and the Colours of Thin Plates."—*Phil. Trans.* 1837, p. 245.

[2] *Edinburgh Journal of Science*, vol. i. p. 108. June 1824.

[3] These observations, thirteen in number, entitled, "*Observations concerning the Reflections and Colours of thick transparent polished Plates*, form the fourth part of the Second Book of Optics.

admitted through a small hole about a third of an inch in diameter into a dark room. This beam is received perpendicularly on a concavo-convex glass mirror, a quarter of an inch thick, and having each surface ground to a sphere six feet in radius. When the sunbeam passes through a small hole in the middle of a sheet of white paper placed in the centre of the mirror's concavity, the whole is surrounded with four or five coloured rings. The rings resembled those seen by transmission through two object-glasses, but were larger and fainter in their colours. When mirrors of different thicknesses were used, the diameters of the rings were reciprocally as the square roots of the thicknesses; and in homogeneous light they were largest in the red, and smallest in the violet rays, like those formed by thin plates.

These and other phenomena described by Newton, he explains by taking into consideration the fits of easy reflexion and transmission of the faint scattered light already mentioned. On the undulatory theory they are explained by the interference of the portions of light scattered at the first surface by the rays in passing and repassing through it.

The Duke de Chaulnes[1] observed similar rings when the surface of the mirror was covered with fine gauze, or with a thin film of milk dried upon it, and Sir William Herschel[2] noticed analogous colours when hair-powder was scattered in the air before a metallic mirror, on which a beam of light was incident.

When we look through two plates of parallel glass of exactly the same thickness, at a circular disc of light 1° or 2° in diameter, no coloured bands will be seen when the light is incident perpendicularly, and when the plates are parallel. But if we incline them slightly to one another, we shall see, beside the direct image of the luminous body which is crossed with no fringes, a series of lateral images formed by successive reflexions between the surfaces of the plates, which are crossed with

[1] *Mém. Acad. Par.* 1705. [2] *Phil. Trans.* 1807.

fifteen or sixteen highly coloured bands parallel to the common section of the surfaces of the plates. The breadth of these bands is inversely as the inclination of the plates, and at a given inclination their magnitudes are inversely as the thickness of the plates employed.

These brilliant bands, which we have described minutely in a separate memoir,[1] are explicable by the doctrine of fits of easy reflexion and transmission. They have been explained also on the undulatory hypothesis by Dr. Thomas Young,[2] and in greater detail by Sir John Herschel.[3]

Another species of coloured fringes, produced by the reflexion of a pencil of light between the lenses of a double or a triple achromatic object-glass, is equally explicable by Newton's theory of fits, and by the doctrine of interference. Owing to the curvature of the surfaces which produce them, the forms of the isochromatic lines, or the lines of equal tint, are various and beautiful.[4]

[1] *Edinburgh Transactions*, 1815, vol. vii. p. 435.
[2] Art. CHROMATICS in *Encyclopædia Britannica*.
[3] *Treatise on Light*, § 688-695.
[4] *Edinburgh Transactions*, 1832, vol. xii.

CHAPTER VIII.

Influence of Colour in the Material World—Newton's Theory of the Colours of Natural Bodies—Coloured Bodies reflect only Light of their own Colour, absorbing all the other parts of White Light—The Colours of Natural Bodies are those of Thin Plates—The Transparent Parts reflecting one Colour and transmitting another—Arrangement of the Colours exhibited in Natural Bodies into Seven Classes—Coloured Juices and Solutions, Oxidated Films, Metals, &c. &c.—Newton's Theory applicable only to one Class of Colours—Objections to it stated—M. Jamin's Researches on the Colours of Metals—The Cause of Colours must be in the Constitution of Bodies—Examples of the effect of Heat upon Rubies and Nitrous Gas—Effect of Sudden Cooling—On Phosphorus—Effect of Mechanical Action on Iodide of Mercury—Indication of a New Theory—And of the Cause of the Absorption of Definite Rays—Illustration of these views in a remarkable Tourmaline.

HAD the objects of nature been rendered visible only by white light, and exercised upon it the same action in refracting and reflecting it to the human eye, all the combinations in the material world, and all the various forms of life, would have displayed no other tint than that which they exhibit in a pencil sketch, a China-ink drawing, or a photographic picture. The magnificent foliage of the vegetable world might have filled the eye with its picturesque and lovely forms, and given protection to its fruit and its flowers, but we should not have rejoiced in the verdure of its youth, nor mourned over the yellow of its age. The sober mantle of twilight would have replaced the golden vesture of the rising and the setting sun. The stars would have twinkled colourless in a grey sky, and the rainbow would have dwindled into a narrow arch of dusky light. The diamond, the ruby, and the sapphire might have displayed to science the nice geometry of their forms, and yielded to the arts their adamantine virtues; but they would have ceased to sparkle in the chaplet of beauty, or adorn the diadem of princes. The human face divine might have expressed all the qualities of the mind, and beamed with all the affections of the heart; but the purple light of love would not have risen on the cheek of beauty, nor the hectic flush have heralded its decay. Life would have breathed and perished in its pale marble; and nature would have sprung and decayed in its russet brown. The material world, however, has been otherwise framed, and those exquisite models of organic and inorganic life, into which the great sculptor has chiselled the furniture of his terrestrial temple, have been enhanced by that ethereal beauty which the play of light and colour can alone impart.

Many attempts were made previous to the time of Newton, to explain the colours of natural bodies; but they all necessarily failed, while philosophers were ignorant of the true nature of colours themselves. In his earliest communications to the Royal Society, Newton had clearly indicated his views respecting the colours of natural bodies; and after showing "that they appear of divers colours, according as they are disposed to reflect most copiously the rays endued by these colours," he proceeds, in the last part of his "Discourse," read to the Royal Society on the 10th February 1675-6, to consider "the constitution of bodies on which their colours depend." This curious subject continued to occupy the attention of Newton, and he enters upon it more fully in two different parts of his *Optics*, where "by the discovered properties of light he explains the permanent colours of natural bodies,"[1] and points out the "analogy between such colours, and the colours of thin transparent plates."[2]

After showing that all bodies, whatever were their colours, exhibited these colours best in white light, or in light which contained their peculiar colour, he proves by experiment, that when coloured bodies are illuminated with homogeneous *red*

[1] *Optics*, Book i. Part ii. Prop. 10.

[2] *Optics*, Book ii. Part iii.

light, they appear *red*, with homogeneous *blue* light, *blue*, and so on, " their colours being most brisk and vivid under the influence of their own daylight colours." The leaf of a plant, for example, appeared *green* in the white light of day, because it had the property of reflecting *green* light in greater abundance than any other. When the leaf was placed in homogeneous *red* light, it no longer appeared *green*, because there were no green rays in the red; but it reflected red light in a small degree, because there were some red rays in the compound green, which it had the property of reflecting. If the leaf had originally reflected a pure homogeneous green, unmixed with red, and reflected no white light (as all leaves do) from its outer surface, it would have appeared quite black, in pure homogeneous red light, as this light does not contain a single ray which the leaf is capable of reflecting. Hence it follows that the colours of natural bodies are owing to the property which they possess of stopping or absorbing certain rays of white light, while they reflect or transmit to the eye the rest of the rays of which white light is composed. The *green* leaf, for example, stops or absorbs the red, blue, and violet rays of the white light which falls upon it, and reflects and transmits only those which compose its peculiar green.

To this extent the views of Newton are demonstrable, and have been universally adopted; but when he attempts to determine the manner in which the colour of any body is insulated from the other colours which fall upon it, and in which these other colours are stopped or lost, or, in other words, the physical constitution of natural bodies by which these processes are effected, he enters the region of hypothesis and fails in bringing conviction to the mind. His theory, however, is grand and imposing, but standing as it does, and as we shall presently show, on a perishable basis, it must soon be swept away in the progress of optical discovery.

The following are the principles on which this theory is founded.

1. Bodies that have the highest refractive powers, reflect the greatest quantity of light from their surfaces, and at the confines of equally refracting media there is no reflexion.

2. The least parts of almost all natural bodies are in some measure transparent.

3. Between the parts of opaque and coloured bodies, are many spaces or pores, either empty or filled with media of other densities.

4. The parts of bodies and their interstices or pores must not be less than of some definite bigness, to render them coloured.

5. The transparent parts of bodies, according to their several sizes, reflect rays of one colour, and transmit those of another, on the same grounds that thin plates do reflect or transmit these rays.

6. The parts of bodies on which their colours depend, are denser than the medium which pervades their interstices.

7. The bigness of the component parts of natural bodies may be conjectured by their colours.

In illustration of the *fifth*, or leading proposition of the theory, Newton remarks, "that if a thinned or plated body, which being of an even thickness, appears all over of one uniform colour, should be slit into threads or broken into fragments of the same thickness with the plate, he sees no reason why every thread or fragment should not keep its colour, and by consequence why a heap of those threads or fragments should not constitute a mass or powder of the same colour which the plate exhibited before it was broken. And the parts of all natural bodies being like so many fragments of a plate, must on the same grounds exhibit the same colours."

In order to prove this, Newton proceeds to describe various kinds of colours, to which he considers the theory specially applicable; but before we follow him in this investigation, we must endeavour to classify all the varieties of colours which are exhibited in the natural world.

Colours may be arranged into *seven* classes, in each of which the colour has a different origin.

1. Transparent coloured fluids, such as the juices obtained from the coloured parts of plants, and coloured solutions, whether natural or artificial. Transparent coloured solids, such as coloured minerals, glasses, powders, and vegetable tissues.

2. Oxidated films on metals—colours of precious and hydrophanous opal—of Labrador felspar—of the feathers of birds—of the wings, &c., of insects—of the scales of fishes—of the tapetum of animals—of the *internal* films of mother-of-pearl and various shells—and of decomposed glass.

3. Superficial colours of mother-of-pearl, striated and grooved surfaces, which can be communicated by pressure to other surfaces.

4. Opalescences, or colours dispersed from the particles of different solid and fluid and gaseous bodies, some of which are coloured, and others colourless. These colours appear in ice, in water, in the atmosphere—in fluorspar and several glasses—in solutions of sulphate of quinine, &c., and in the juices of plants and several oils.

5. At the surfaces of media of different dispersive powers, and in which the index of refraction is the same in each medium for certain rays, but different for all the rest.

6. The colours produced by heat, and during combustion.

7. The colours of metals.

The colours referred to in the *first* of these classes are represented by red and yellow wines—by the coloured fluids shown in the windows of the apothecary—by the green leaves of plants—by the ruby, the cairngorm, the topaz, and the sapphire—and by the powders of cinnabar, red lead, ultramarine, sulphur, &c.

In all these bodies Newton supposes that the colour peculiar to each, namely, that which passes through its substance, is the *tint* reflected from the minute particles of which it is composed, the opposite or complementary tint, which is transmitted by the particles being lost within the body by a multitude of internal reflexions. In the *ruby*, for example, the particles are supposed to have such a size as to appear *red* by reflexion, and the *green* light which would be seen by transmission through a single particle, is supposed to be lost by repeated reflexions within the body composed of such particles. If we now examine the *ruby*, or any other coloured solid or fluid, we shall find that neither *red* nor *green* light is reflected from any of its external surfaces, or any of its internal parts. The *red* light which characterizes the body is seen *only* by transmission through its substance. If we now analyze the *red* light by the prism, we shall find that it has not the composition of any of the red rings in Newton's scale of colours.

In the case of the *ruby*, which we have purposely selected, we are able to apply another test, and one which Newton himself authorizes, when he remarks that changes of colour may be produced by the swelling or shrinking of the tinging corpuscles. In subjecting the *balas ruby* to a high degree of heat, which must have had the effect of swelling the tinging corpuscles, I found that it became *green*, which, as the cooling advanced, gradually faded into *brown*, the ruby resuming its original brilliant *red* when it had returned to its former temperature. Berzelius observed an analogous fact in the *spinelle* of Ceylon and Aker, which became *brown* by heat, then *black*, and *opaque* as the heat increased. Upon returning to its former temperature, it passed through a fine *chrome green* before it recovered its *red* colour. Hence it is obvious that these colours and changes of transparency, which have no relation to those of thin plates, could not have arisen from the gradual swelling and subsequent shrinking of tinging corpuscles.

A still more striking proof of the want of analogy between the colours of natural bodies and those of thin plates, may be obtained from the prismatic analysis of certain colours in which Newton himself believed that analogy to exist. A *green* of the *third* order of colours is, as he observes, "constituted principally of original *green*, but not without a mixture of some *blue* and *yellow*," and contains not a single ray of *orange*, *red*, *indigo*, or *violet*. He considers the *green* of all vegetables, to be a green of the *third* order, not only because this green is the purest and most intense in colour, but because when vegetables wither, some of them turn to a *greenish yellow*, and others to a more perfect *yellow* or *orange*, or perhaps to *red*, passing first through all these intermediate colours. "Now," he adds, "the *green* is *without doubt* one of the same orders with those colours into which it changeth, because the changes are gradual, and those colours, though usually not very full, yet are often too full and lively to be of the *fourth* order." These changes from *green* to *red*, he considers as "effected by the exhaling of the moisture which may leave the tinging corpuscles more dense, and something augmented by the accretion of the oily and earthy part of that moisture."

In order to put these opinions to the test of direct experiment, we examined the brilliant *green juice* extracted by alcohol from the leaves of twenty different plants, and also the same juice when taken from the leaves in their yellow, orange, and red state, and found that their composition had not the least resemblance to that of the colours of any order whatever, and least of all to those of the *greens* of the third order. The spectrum obtained from a sunbeam passing through these juices is one of singular beauty, divided by dark spaces into several coloured bands of unequal breadths, and possessing all the colours which ought not to exist in the green of the third order. When the green fluid thus analyzed has stood for three or four days it loses its bright green colour, and becomes of an *olive green*, which grows more and more of a *brownish yellow*, till it becomes almost *colourless*, a series of changes which have no relation whatever to the effects that might be expected to arise from an increase or decrease in the density or size of the tinging corpuscles.

In some plants the green leaf decays in a different manner from that described by Newton. In place of becoming *yellow*, the green leaves of the privet become of a deep *black violet*, when they wither ; a colour which has no resemblance whatever to any of the colours of thin plates. The fluid obtained from these violet leaves was of a *deep red colour*,—much deeper than that of the *darkest port wine*. It divided the red space of the spectrum into two red bands, absorbed the violet and blue spaces generally, and obliterated the middle of the green space. Its action was so different from that of the green juice, that the two tints had no resemblance to those of adjacent colours of the same order.[1]

The pale *blue* of the sky is regarded by Newton as a blue of the first order, produced by the minute particles of "vapours which have not arrived at that grossness which is requisite to reflect other colours ;" and while he considers the *whiteness* of froth, paper, linen, &c., as that which arises "from a mixture of the colours of several orders," that is, from the action of particles of a much greater size than those of vapours which produce the blue of the first order. Now, it is obvious that *froth*, when seen under a clear blue sky, must have the colour of the sky itself, as it is nothing more than an accumulation of images of the sky reflected from the innumerable aqueous vesicles which compose it. The colour of froth, wherever it is placed, must be the average tint of all the differently coloured rays which fall upon it and are reflected to the eye.

The colours referred to in the *second* class are undoubtedly analogous to those of thin plates. Newton has himself mentioned the colours of the feathers of some birds as those of thin plates, and the fine colours of the diamond and other beetles obviously have the same origin. The splendid colours of the tapetum, or membrane behind the retina of animals, afford an interesting example of this class of colours. Even when the membrane has been taken out, it exhibits the most beautiful colours by reflexion, but it becomes absolutely black

[1] A full account of these experiments, with coloured drawings of the spectra, will be found in the *Edinburgh Transactions*, 1833, vol. xii. pp. 538-545.

when dry. The colours, however, may be revived by moisture, and, after remaining in the dry state for upwards of twenty years, we have succeeded in restoring the colours by steeping the membrane in warm water. The black passes into a bright *blue*, the *blue* into *green*, and the *green* into *greenish yellow.*

In placing the internal colours of mother-of-pearl under this class, we must carefully distinguish them from the external colours communicable to wax. By reducing the mother-of-pearl to exceedingly thin plates, we are able to exhibit the action of the colorific films which they enclose, and which, like those of thin plates, give one colour by reflexion, and its complementary colour by transmission.[1]

The splendid colours exhibited by decomposed glass, both in the light which it reflects and transmits, belong also to colours of the second class ; and though they are clearly those of thin plates, yet they exhibit peculiarities when produced by a great number of films, which place them in a certain interesting relation with the colours of the first class.[2]

The colours of Labrador felspar, and of precious and hydrophanous opal, which we have shown to be produced by thin plates and minute pores and tubes, belong also to the second class of colours.[3]

The superficial colours which we have placed in the *third* class, have obviously no relation whatever to the colours of thin plates. They are spectra produced by interference, and, had he been acquainted with them, they would have been regarded by Newton himself as inexplicable by his theory.[4]

The very remarkable colours produced by internal dispersion, and which have recently excited so much interest from the discoveries of Professor Stokes, form a *fourth* class, which has

[1] See *Phil. Trans.* 1814, p. 397 ; and 1836, pp. 55, 56.

[2] See Layard's *Discoveries in the Ruins of Nineveh*, 1853, pp. 674-676 ; and *Phil. Trans.* 1837, p. 249.

[3] See *Edinburgh Transactions*, 1829, vol. xi. p. 322 ; and *Reports of the British Association*, 1844, p. 9.

[4] See *Phil. Trans.* 1814, p. 397 ; and 1829, p. 301.

not been identified with those of thin plates. The light thus dispersed must be reflected, in cases of ordinary opalescence, from the faces of minute pores in solids, or from particles of different densities disseminated through solids or suspended in fluids. The beautiful colours exhibited by fluor spar, by solutions of the sulphate of quinine, and various other solids and fluids, are emanations of a phosphoric nature, generated by certain rays in the solar spectrum, and have therefore no analogy with the colours of thin plates. These emanations have all colours,—*red* in the alcoholic juices of leaves, *violet*, *blue*, *pink*, and *whitish* in fluor spar, *sky-blue* in sulphate of quinine, bright *green* in alcoholic solutions of the *colchicum autumnale*, and in various glasses and oils, and *violet* in an alcoholic solution of guiacum.[1]

In the *fifth* class we have placed a new species of colours which we discovered many years ago, and which we believe have never been studied, or even alluded to by any other person. In the year 1814, when investigating the law of polarization for light reflected at the separating surface of different media, we had occasion to enclose oil of cassia between two flint-glass prisms, and were surprised to observe that the colour of the reflected light was *blue*. The cause of this we had some difficulty in discovering. The refractive power of oil of cassia exceeds greatly that of flint-glass for the mean rays of the spectrum, while the action of the two bodies on the less refrangible rays is nearly the same. Hence the *red* rays must be in a great measure *transmitted*, while there will be reflected a small portion of the *orange*, a greater portion of the *yellow*, and a much greater proportion of the *blue* and *violet*, so that the colour of the pencil, formed by reflexion, must necessarily be *blue*, mixed with some of the less refrangible rays.

By employing different kinds of glass, and different oils, we

[1] See *Edinburgh Transactions*, 1833, vol. xii. p. 542; and 1846, vol. xvi. p. 111; *Reports of the British Association*, 1838, pp. 10-12; *Phil. Trans.* 1845, p. 143; and 1852, p. 463.

obtained various analogous results, in which rays of different colours were extinguished from the reflected pencil according to the part of the spectrum where an equilibrium had been established between the refractive powers of the media in contact. When the refractive indices were equal in the *blue* rays, the colour of the reflected pencil was *yellow*. As the indices of refraction are the same for all obliquities of incidence,[1] the tint of the reflected pencil, though it must vary in intensity, can never vary in colour; and as that colour is abstracted from the white incident light, its complementary tint must appear, however faintly, in the transmitted pencil. Hence it follows as a general result, that as all reflecting surfaces are the separating surfaces of two media, the pencils which they reflect and transmit must necessarily have a different tint from the incident pencil, excepting in the extreme case, and one not known to exist, where the two bodies in contact have the same refractive power, or the same differences of refractive power for every ray of the spectrum.

Hitherto we have supposed the *irrationality* of the coloured spaces to be *simple*, but it may be *compound*, and there may be *two*, *three*, or more points in the spectrum of two adjacent media where the indices of refraction are the same, or have equal differences, while in other points they are not the same, or have their differences unequal. In these cases the reflected and transmitted tints will be compound; but as such colours have not been observed, it would be out of place to make any farther reference to such a supposition. It may be sufficient to remark, that even if we never discover spectra of such a character, they may exist in the refractions at the separating surfaces of the tinging corpuscles of Newton, and the media which fill their interstices.

In the *sixth* class of colours may be ranked those produced

[1] In his Memoir on Diffraction, Fresnel has thrown out the idea that, at great incidences, and with very thin laminæ, the law of refraction may not follow the proportionality of the sines.

by heat in metals and other substances, the colours of different bodies in combustion, and those exhibited in the deflagration of metals. There is no reason to believe that any of these colours have the slightest analogy with those of thin plates, and their nature and origin remain to be investigated.

The colours of metals, which form the *seventh* class in our enumeration, have been referred by Newton to those of thin plates, but without any plausible reason. The polarization of light by metallic bodies required to be investigated before the problem of their colour could be solved, and we owe its solution to the recent and beautiful researches of M. Jamin. As it would be foreign to the character of the present work to give an account of the process by which M. Jamin obtained his results, we must content ourselves with presenting them in the following Table :—

		D.		C.
COLOURS AFTER ONE REFLEXION.				
Copper, ...	Orange, very red, . .	69°	56′	0·113
Brass, ...	Yellow, ...	103	13	0·112
Bell metal, . . .	Orange, yellow, . . .	83	10	0·065
Speculum metal, . .	Orange, very red, . .	67	25	0·027
Zinc, ...	Blue, ...	180	57	0 021
Silver, ...	Orange, yellow, . . .	89	00	0·013
Steel, ...	White, ...	—	—	——
COLOURS AFTER TEN REFLEXIONS.				
Copper, ...	Red, middle, ...	42°	29′	0·812
Brass, ...	Orange, very red, . .	62	50	0·349
Bell metal, . . .	Red, ...	40	40	0·767
Speculum metal, . .	Red, orange, ...	53	59	0·292
Zinc, ...	Blue, indigo, ...	267	58	0.188
Silver, ...	Orange, yellow, . . .	84	32	0·124
Steel, ...	White, ...	—	—	——

The numbers in the column marked D are the distances of the tint of the metal from the red end of the spectrum, whose whole length is 360° ; and those under the letter C are the intensities of the tints, that of the incident *white* light being 1·000.

After a careful study of these different classes of colours, philosophers will have no hesitation in concluding that Newton's theory of the colours of natural bodies has only a limited application, and that instead of any general theory such as he contemplated, we must look for a separate explanation of the different classes of phenomena. The *first* class of our enumeration, which comprehends the largest number of coloured bodies, is the one which presents the greatest difficulty to philosophers; and to it the Newtonian hypothesis is certainly inapplicable. Within the solids, fluids, and gases of this class, certain rays of the intromitted pencil are absorbed or lost, while others are transmitted, or, what is the same thing, the coloured body has different degrees of transparency for different rays, being opaque for different portions of the spectrum at different thicknesses; whereas, in colourless bodies, the rays are absorbed in equal proportions, so that the transmitted beam emerges colourless. The colour of a body, therefore, is not produced by particles having the same colour as itself, but it is the colour which arises from the mixture of all the transmitted rays, and these rays proceed from every part of the spectrum, though in different proportions. Hence we must look for the cause of the colour in the constitution of the body itself, that is, in the manner in which its atoms are combined, and not in the size or nature of the atoms themselves.

In support and in illustration of this opinion, we may mention a few remarkable examples, in which the colour is changed by a change in the condition of its particles. The most remarkable of these is *nitrous gas.* This body is almost transparent in small thicknesses, and at low temperatures. By heating it, its colour becomes in succession *straw yellow*, *orange*, *red*, and even absolutely *black.*[1] When *phosphorus*, which in its ordinary state is of a *pale yellow* colour, is melted and thrown into cold water, it becomes *black*, and recovers its original colour when again melted. It is therefore obvious, that in both these cases the blackness could not be produced by any diminution in the size

[1] See *Edinburgh Transactions*, vol. xii. p. 523.

of the particles. A similar change of colour is produced by simple mechanical pressure on the crystals of *iodide of mercury*, which change their colour by simply pricking them with a sharp point.

The various phenomena of colour in crystallized bodies, and the influence of the continued action of light upon coloured substances, indicate the existence of different causes of colour; and the influence of structure, as one of these causes, is finely shown in the relation of the colours of dichroitic crystals to their axes of double refraction or crystallization.

The great diversity in the constitution of coloured bodies is peculiarly shown in the diversity of their action on the different rays of the spectrum; and it is therefore probable that the cause of their difference of colour may be found in the diversity of action exercised upon light by their particles or elementary atoms. In describing the colours of the *fifth* class, we have already mentioned an experiment with flint glass and oil of cassia, and its indication of a new theory of the colours of natural bodies of the first class. In Fraunhofer's spectrum, the principal black lines which it contains are represented by the letters A, B, C, D, E, F, G, H, I;—AI being nearly the whole of its length. If *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, *i*, represent the same lines in a spectrum of equal length formed by any fluid or solid different from that which produces the spectrum AI, then though *ai* be equal to AI, it frequently happens, and we venture to say, always happens, that *ab* is not equal to AB, nor *cf* to CF, while *ac* may be equal to AC, and *dh* to DH. The equal spectra may coincide in particular points, that is, individual lines in the one, indicating particular colours, may coincide with individual lines marking the same colours in the other spectrum, and yet other lines may not coincide, indicating different colours. When a ray of white light, therefore, is incident on the separating surface of the two media which give these two spectra, a very large portion, or rather the whole of the colours, indicated by the coincident lines, will be transmitted, while a very small portion of the colours indicated by the non-coincident lines will be reflected, the greatest quantity of the colours being reflected where the non-coincidence is greatest, and the greatest quantity being transmitted at the points of coincidence. Where there are many separating surfaces, and many elements in the body, the spectrum obtained by the prismatic analysis of the transmitted light will be cut up by obscure portions exactly as it is found to be in all coloured media.

When the constitution of any coloured body is altered by heat or pressure, the refractive and dispersive powers of its elements are changed, and the resulting colour altered, according to the ratio in which the refracting forces are changed in the elementary molecules. Changes of this kind are finely exhibited in the growth of certain coloured crystals. In the *tourmaline*, for example, we have sometimes a *red* nucleus which absorbs one of the doubly refracted pencils, namely, the *green* one, and transmits only the *red*. When this nucleus was completed, some change had taken place in the circumstances under which the crystallization was proceeding, and the molecules, though still combining as tourmaline, combine in such a manner as to produce no colour—no difference in the tint of the pencils—and no absorption of one of them. At a subsequent stage, the structure which produces the red colour again appears and disappears, forming in succession coloured and colourless laminæ round the original nucleus!

Another example of great interest is afforded by certain specimens of *fluor spar*, in which the colours of the *fourth* class are produced.[1] The structure which produces a white phosphorescence, is succeeded by one which produces a coloured phosphorescence, and this again by a structure which produces no phosphorescence at all. The changes of structure to which these different effects are owing, arise, in all probability, from a change in the arrangement of the atoms in the molecular groups of which the body is composed.

[1] See *Edinburgh Transactions*, vol. xvi. p. 112.

CHAPTER IX.

Newton's Discoveries on the Inflexion of Light—Previous Researches of Hooke—Newton's Animadversions on them offensive to Hooke—Newton's Theory of Inflexion as described by Grimaldi, having made no experiments of his own—Discoveries of Grimaldi, which anticipate those of Hooke—Hooke suggests the Doctrine of Interference—Newton's Experiments on Inflexion—His Views upon the Subject unsettled—Modern Researches—Dr. Young discovers the Law of Interference—Discoveries of Fresnel and Arago—Fraunhofer's Experiments—Diffraction by Grooved Surfaces—Diffraction by Transparent Lines—Phenomena of Negative Diffraction—Experiments and Discoveries of Lord Brougham—Explanation of Diffraction by the Undulatory Theory.

Among the optical discoveries of Newton, those which he made on the *inflexion* of light hold a high place. They were first published in his Treatise on Optics in 1704, but we have not been able to ascertain at what period they were made. In the preface to this work, Sir Isaac informs us, that the third book, which contains his experiments on inflexion, "was put together out of scattered papers;" and he adds, at the end of his observations, that "he designed to repeat most of them with more care and exactness, and to make some new ones for determining the manner how the rays of light are bent in their passage by bodies for making the fringes of colours with the dark lines between them. But we were then interrupted, and cannot now think of taking these things into consideration."

The earliest notice of the inflexion of light by English philosophers was taken by Dr. Hooke in a discourse read to the Royal Society on the 27th November 1672, "containing diverse optical trials made by himself, which seemed to discover some new properties of light, and to exhibit several phenomena in his opinion not ascribable to reflection or refraction, or any other till then known properties of light." The Society desired him to pursue these experiments, and to register some account of them, in order "to preserve his discoveries from being usurped."

After an interval of more than two years, he communicated to the Society a second discourse "on the nature and properties of light, in which were contained several new properties of light, not observed that he knew of by optical writers." These properties were,—

"1. That there is an *inflexion of light* differing both from refraction and reflexion, and seeming to depend upon the unequal density of the constituent parts of the ray, whereby the light is dispersed from the place of condensation, and rarefied, or gradually diverged into a quadrant.

"2. That this *deflexion* is made towards the superficies of the opaque body perpendicularly.

"3. That in this deflexion of the rays, those parts of diverged radiation that are deflected by the greatest angle from the strait or direct radiations are faintest; those that are deflected by the least are the strongest.

"4. That rays cutting each other in one common foramen, do not make the angles *ad verticem* equal.

"5. That colours may be made without refraction.

"6. That the true bigness of the sun's diameter cannot be taken with common sights.

"7. That the same rays of light falling upon the same point of the object will turn into all sorts of colours, only by the various inclination of the object.

"8. That colours begin to appear when two pulses of light are blended so very well and near together, that the sense takes them for one."[1]

These observations of Hooke on the Inflexion of Light, were

[1] See Birch's *Hist. Royal Society*, vol. iii. pp. 63, 194, and Hooke's *Posthumous Works*, pp. 186-190.

referred to, not very courteously, by Sir Isaac Newton, at the close of the celebrated Discourse on Colours, of which we have already given an account. After treating of the colours of natural bodies, he says, "that there is another strange phenomenon of colours which may deserve to be taken notice of. Mr. Hooke," he adds, "you may remember, was speaking of an odd straying of light, caused in its passage near the edge of a razor, knife, or other opaque body, in a dark room; the rays which pass very near the edge being thereby made to stray at all angles into the shadow of the knife. To this Sir William Petty, then President, returned a very pertinent query,—Whether that straying was in curve lines? and that made me (having heard Mr. Hooke, some days before, compare it to the straying of sound into the quiescent medium) say, that I took it to be only a new kind of refraction, caused perhaps by the external ether's beginning to grow rarer a little before it came at the opaque body, than it was in free spaces, the denser ether without the body, and the rarer within it, being terminated not in a mathematical superficies, but passing into one another through all intermediate degrees of density; whence the rays that pass so near the body, as to come within that compass where the outward ether begins to grow rarer, must be refracted by the uneven denseness thereof, and bended inwards towards the rarer medium of the body. To this Mr. Hooke was then pleased to answer, that though it should be but a new kind of refraction, yet it was a new one. What to make of this unexpected reply I knew not,—having no other thoughts but that a new kind of refraction might be as noble an invention as any thing else about light; but it made me afterwards, I know not upon what occasion, happen to say, among some who were present to what passed before, that I thought I had seen the experiment before in some Italian author. And the author is Honoratus Faber, in his dialogue De Lumine, who had it from Grimaldi, whom I mention because I am to describe something further out of him."

This passage, which must have been very offensive to Hooke, may be fairly adduced as affording an additional apology for his statement that Newton had, in his Discourse, only carried farther, in some particulars, what was contained in his *Micrographia.* We have no doubt that Hooke discovered the *inflexion of light*, without knowing anything of the previous experiments of Grimaldi. Hooke was right in calling his discovery a new property of light, and Newton was wrong in calling it "*only* a new kind of refraction,"—thus stripping it of much of its value, and placing it in the same category with his own discoveries. Hooke felt the bitterness of the remark, and with more temper than might have been expected, replied, "that though it should be *but a new kind of refraction*, yet it *was a new one;*" thus taking to himself the credit of making a new discovery even when reduced in importance by another designation. Newton confesses that he knew not what to make of this unexpected reply. The reply was a proper one, and might have been expected; but though Newton felt its full significance, he had not the readiness to make the explanation which it required, and which he subsequently gave. He had no thoughts, he afterwards said, of undervaluing the discovery of a property of light by calling it "a new kind of refraction;" yet he did not give this explanation till he had ascertained that the new property had been previously discovered by Grimaldi; and though he now gave its true value to the new discovery, he but embittered the admission when he announced to the Society, that the discovery belonged to an Italian philosopher. On a former occasion, Newton had unnecessarily claimed for Descartes some of Hooke's theoretical opinions; and when a similar claim was made for Grimaldi, Hooke could not but feel the unkindness of his rival. Nearly two centuries have elapsed since these controversies raged; and it is not without its moral in intellectual strife, nor yet without its consolation to the humbler cultivators of science, that while Newton's Theory of the Inflexion of Light is maintained by nobody, the Theory of Hooke, imperfect as it is, is adopted by the greater number of modern philosophers.

It is obvious from these details, that Newton had at this time made no important experiments on the Inflexion of Light. "He propounded his theory with diffidence," as he had "not made sufficient observation about it." It is equally obvious that he had not seen the work of Grimaldi,[1] which he quoted from Honoratus Faber, although a copy of the work had been three years in the possession of the Royal Society, or at least of their secretary, Mr. Oldenburg, who published an analysis of it in the Philosophical Transactions for January 22, 1671-72. The analysis, indeed, is a very imperfect one, in so far as it refers to the *diffraction* of light, and could scarcely have led Hooke to his discovery, even if he had perused it with attention. "The author," says the reviewer, "explains how many ways light is propagated or diffused, viz., not only *directly*, and by *refraction* and *reflexion*, but also by *diffraction;* which last, according to him, is done when the parts of light, separated by a manifold dissection, do in the same medium proceed in different ways,"—a definition of diffraction which Newton could scarcely have comprehended, and which, if he had, he would not have accepted.

That Newton had not seen Grimaldi's work in 1675, is avowed by himself; and there is every reason to believe that he had not even seen it in 1704, when he published his Optics. If he had seen it, and was aware of the discoveries which it contains, he has not only done great injustice to the Italian philosopher, but neglected the opportunity which it afforded him of anticipating the discoveries of his successors. In the third book of his Optics, he gives to Grimaldi the credit merely of having observed that the shadows of all bodies, placed in light let into a dark room through a small hole, were larger than they ought to be, and that these shadows had three

[1] *Physico-Mathesis de Lumine, Coloribus, et Iride, aliisque annexis.* Bononiæ, 1665. 4to.

parallel fringes of coloured light adjacent to them, whereas the Italian philosopher had penetrated more deeply into the subject, and obtained, as we shall now see, very important results.

Having admitted a ray of light through a small hole into a dark room, Grimaldi observed that it was diffused in the form of a cone, and that all bodies placed in this light had their shadows larger than they should have been had the light passed by their edges in straight lines. Upon a closer examination of these shadows, he discovered that they were surrounded by *three* coloured fringes, growing narrower as they receded from the shadow. When the sun's light was strong, he perceived similar coloured fringes *within* the shadow of narrow bodies like a needle. These fringes were sometimes only *two* in number, and sometimes *four*, their number increasing and their size diminishing with the thickness of the body, and also, in the same shadow, when it was received on a sheet of paper at a greater distance from the body. From these new and valuable facts Grimaldi concluded that light is bent from its rectilineal direction in passing by the edges of bodies. By admitting the sun's rays through two small holes, so near each other that the two cones of light which they produced did not penetrate each other till they had reached to a considerable distance from the two holes, Grimaldi discovered the remarkable fact, that in consequence of the mutual interference of the two cones of light, the spot which was illuminated by both the pencils of light was more obscure than when it was illuminated by either of them singly, or, what is the same thing, "that a body actually illuminated may become more obscure by *adding* a light to that which it already receives." Grimaldi discovered also the beautiful phenomenon of the crested, or curved fringes exhibited within the shadow of the rectangular termination of bodies.

Although Hooke was anticipated by Grimaldi in the greater number of his observations, yet he is clearly entitled to share with the Italian philosopher in the discovery of the doctrine of the interference of light, though it was left to Dr. Thomas Young to complete the discovery.

Such was the state of the subject of Inflexion when Newton directed to it his powers of acute and accurate observation. His attention, however, was turned only to the enlargement of the shadow of inflecting bodies, and to the three fringes adjacent to it. He was therefore led to take exact measures of the shadow of a human hair, and of the breadth of the fringes at different distances behind it, and to repeat these observations with light of different colours. In this way he was led to two new and remarkable results.

1. That these breadths were not proportional to the distances at which they were measured; and,

2. That the fringes made in homogeneous *red* light were *red*, and the largest; that those made in *violet* light were *violet*, and the smallest; and that those made in *green* light were *green*, and of an intermediate size, the rays which formed the red fringe passing by the hair at a greater distance than those which formed the violet.

When Newton made the preceding observations, he intended to repeat most of them with more care, and to make "some new ones, to determine the manner how the rays of light are bent in their passage by bodies;" but having been then interrupted, he could not think of resuming the inquiry.

It is very difficult to ascertain his real views on the subject of *inflexion*. In his Discourse, read in 1675, he ascribes it to the variable density of the ether within and without the inflecting body, thus regarding it as a new species of refraction; and in his letter to Robert Boyle in 1679, he takes the same view of the subject, and considers the several colours of the fringes as produced "by that refraction." Pursuing the same idea, he asserts in the Scholium to the 96th Prop. of the first book of the Principia, that the rays of light, in passing near bodies, are bent round them as if by attraction; that the rays which pass nearest them are most bent, as if they were most attracted;

that those which pass at a greater distance are less bent ; and that those which pass at still greater distances, are bent in an opposite direction.

In this remarkable passage, Newton introduces, for the first time, the idea of a force bending the rays ; or of an *inflecting* force bending the rays inwards, accompanied with a *deflecting* force bending them outwards. This opinion, however, he subsequently abandoned ; for in the third book of his *Optics*, he refers all the phenomena to a force which "bends the rays *not towards*, but *from* the shadow ;" and he distinctly asserts, "that light *is never known to follow crooked passages, nor to bend into the shadow*."

These erroneous opinions, now wholly exploded, arose from Newton's having never observed the *internal fringes*, or those seen within the shadow. Grimaldi had described them minutely in his work, and, as they have been seen by every philosopher, it is not easy to explain how they should have escaped the notice of two such careful observers as Hooke and Newton. Without this cardinal fact our author stumbled in his path, and was misled into the erroneous propositions that bodies act upon light at a distance ;—that this action bends in rays with a force diminishing with the distance ;—and that rays which differ in refrangibility differ also in flexibility. Nor was he nearer the truth, when he conjectured in his third query that the rays of light, in passing by the edges of bodies, may be bent several times backwards and forwards with a motion like that of an eel, and that the three fringes of coloured light may arise from three such bendings.

A subject which had thus baffled the sagacity of Newton, was not likely to unfold its mysteries to ordinary observers. The experiments of Grimaldi and Newton were repeated by various philosophers in various lands. Observations better made, and measures more accurately taken, were continually accumulating. A Pelion of inferences was heaped upon an Ossa of facts, but no Baconian conjurer could elicit from them the vital spark. The cardinal facts were still wanting, and a century passed away before a single experiment dissipated the inflexion theories of a graduated ether, of refracting atmospheres, and molecular actions. This humble experiment, which neither merits nor claims any particular notice, was, we believe, first made by ourselves in 1798, and afterwards, extended in 1812 and 1813. We found that ice, cork, metals, and diamond, the lightest and the heaviest bodies, the least refractive and the most refractive substances, produced exactly the same fringes ; and that no change in the phenomena of inflexion was produced when a fibre of an opaque body was placed in fluids of precisely the same or of greater refractive power. Hence it followed that the light which passed by the edges of bodies was not inflected by any refracting agent, or by any action whatever of the bodies themselves.

It is to Dr. Thomas Young, however, that we owe the master fact which enabled philosophers to unveil the mysteries of diffraction, and to account for a great variety of hitherto unexplained phenomena. In studying the internal fringes, and the crested ones discovered by Grimaldi, he found that, by intercepting the rays which passed by one side of the diffracting body, the internal and the crested fringes completely disappeared ; and hence he concluded that the fringes were produced by the joint action, or by the interference, of the two portions of light which passed on each side of the diffracting body.

Having thus discovered the cause of the internal fringes, Dr. Young directed his attention to the external ones. He considered them as produced by the interference of the direct rays, or "those which have pursued their course without interruption," with those which are *reflected from* the margin of the diffracting body ; and as the fringes are on this supposition formed by light "*turned away* from the substance near which it passes," he has characterized the phenomenon as one of *deflected light.*

M. Fresnel, to whose fine researches we owe the best experiments on diffraction, and the most perfect theory of it, followed Dr. Young in ascribing the external fringes to the influence of light reflected from the edge of the diffracting body,—an opinion which we never could reconcile with the palpable fact, that the fringes had always the same character, whatever was the reflecting power, or the shape of the edge of the body. Fresnel, influenced no doubt by the same consideration, suggested a different origin for the rays which interfere with the direct ones, namely, that the rays which pass at a sensible distance from the diffracting body deviate from their primitive direction towards the shadow, and thus interfere with the direct rays that pass near the body. In comparing these two hypotheses, and assuming with Dr. Young that half an undulation was lost by the reflected rays, he found that the real place of the fringe, on the hypothesis of a reflection, would be $\frac{17}{100}$ths of a millimètre different from what it really was.

In conducting his experiments on diffraction, Fresnel adopted a new and accurate method of observing and measuring the fringes. In place of using a small hole, he employed a convex lens of short focal length, which collected the solar rays into a focus, from which they again diverged, as if they had proceeded from a small aperture.[1] When bodies were placed in this divergent light, he examined the fringes adjacent to their shadows by means of an eye-glass furnished with a micrometer, instead of receiving them upon a white surface ; and he was thus able to measure their breadths even to the one hundred or two hundredth part of a millimètre. In this way he traced the external fringes to their origin, and with a lens of short focus he perceived the *third* fringe at a distance of less than the one-hundredth part of a millimètre from the edge of the inflecting body.

By measuring the angular inflection of homogeneous *red* light, when the radiant point was placed at different distances

[1] A concave lens is preferable to a convex one, for reasons which will presently be seen ; and we recommend that it should be achromatic.

in front of the diffracting body, and also when the radiant point remained fixed at different distances of the fringes behind the inflecting body, he was led to two important discoveries—

1. That the angular inflexion diminishes with the distance of the inflecting body from the radiant point; and,

2. That when the radiant point remains fixed, the successive positions of the same fringe are not in a straight line, but form a curve whose concavity is turned towards the diffracting body,[1] the curves being hyperbolas, having for their common foci the radiant point and the edge of the diffracting body.[2]

The discovery of Dr. Young, that an opaque screen, on one side of the inflecting body, extinguished the interior fringes, was extended by M. Arago, who found that the same effect is produced by a transparent screen of sufficient thickness, and that thin screens merely displace the fringes, and transfer them from the side where they were formed. When such a screen is placed on each side of the diffracting body, the effect is equal to the difference of the transferences which each screen would have produced separately. As the amount of this transference may be computed theoretically from the thickness and refractive power of the screen, MM. Arago and Fresnel employed this method for measuring, with great exactness, the refractive power of gases.

The late M. Fraunhofer of Munich made a series of experiments on the diffraction of light on a large scale, and obtained many interesting results. The experiments were made with a telescope, which enabled him to obtain accurate measures of the fringes or rings produced by apertures of various forms; and he has published beautiful drawings of the spectra, and groups of spectra produced by a great number of diffracted rays,—by small apertures variously arranged, and by wire-gratings either acting singly, or crossed at right angles.

[1] This result had been previously obtained by Sir Isaac Newton.

[2] The hyperbolic form of the fringes had been previously discovered by Dr. Young.—*Lect.*, vol. i. p. 287.

We have had occasion to study some of the same phenomena, when produced by lines cut upon polished steel with a diamond. The grooved surfaces which we employed were executed for us by the late Sir John Barton, and contained groups of lines varying from 500 to 10,000 in an inch. When divergent light was reflected from these surfaces, the central image formed by ordinary reflexion from the original surface of the steel-plate was, in general, *white*, as observed in every case by Fraunhofer and others, and the other spectra had their usual character. But when the bright spaces in the plate, or those between the grooves had a certain relation to the width of the groove, or the part of the steel that was excavated by the diamond point, a series of new and remarkable phenomena were produced. The light reflected from the original surface of the steel forming the central image was no longer *white*, but coloured, the colour varying with the angle of incidence at which the steel-plate received the divergent beam. In some of the groups of lines, the colour varied slightly from 0° to 90° of incidence. In others, it passed through the first order of colours ; and in others, where the original steel surface was nearly removed, it passed through three or more orders of tints. The light which is obliterated from the central image, at any angle of incidence, or the complementary colour of the tint at that angle, is obliterated also from all the coloured spectra at less angles of incidence, the angle diminishing with the distance of the spectrum from the central one, and being less in each spectrum for the less refrangible rays.

If we cover the surface of the grooved steel with a fluid so as to reduce the refractive power of its surface, we develop more orders of colours on the *white* or central image, and consequently on all the spectra, higher tints being produced at a given incidence. But what is very remarkable, when the central image is perfectly white, and when the spectra are complete without any obliteration of their tints, the application of fluids to the grooved surface develops colours on the central or white image, and a corresponding obliteration of tints in the coloured spectra.[1]

In the experiments hitherto made on diffraction, the lines employed have been opaque, such as wires, hairs, or fibres of glass, which act upon light as if they were opaque. A series of beautiful phenomena are produced when we employ transparent lines drawn upon glass with solutions of gums of different kinds, and different degrees of strength. A section of these transparent lines varies with the nature and density of the solution, though it is generally thicker at its edges. The consequence of this is, that the light which passes through the transparent line not only interferes with that which passes on each side of it, but also with part of the light which has its direction changed by the refraction of its curvilineal edges. Hence it follows, that a series of new interferences takes place, and we accordingly have a splendid display of coloured fringes infinitely surpassing in variety and brilliancy of colour the ordinary phenomena of diffraction.[2]

In all the experiments on inflexion and diffraction made by Newton and Fresnel, the fringes were viewed either on paper, or in the focus of a lens when the rays had actually interfered and produced the coloured fringes. The fringes thus seen may be called *positive*, because they are formed in space and out of the eye, on the retina of which they are afterwards delineated; but there is another form of these fringes, which I have examined, and which may be called *negative*, because they are not brought to a positive focus in space, or do not interfere till they reach the retina. In order to see these fringes, place the lens behind the diffracting body, so as to see the *positive* fringes, and then move it forward till these fringes disappear. The diffracting edge will now be in the anterior focus of the lens.

[1] See the *Phil. Trans.* 1829, pp. 301-317.

[2] These effects are so beautiful, that we have recommended the use of a diffracting apparatus for suggesting patterns for ribands.—See *Reports of British Association*, 1838, vol. vii. p. 12; *Treatise on Optics*, Edit. 1853, p. 117.

If we advance the lens towards the diffracting body, the *negative* fringes will appear, and will increase in size till the lens touches the body, when they will have the same magnitude as the *positive* fringes have when the lens is placed behind the body, at the distance of twice its focal length.

If we wish to see the fringes larger, we must use a lens with a longer focus; and when it is placed in contact with the diffracting body, the fringes will in every case be the same as the positive ones seen by the same lens placed behind the body twice its focal length. If the diffracting body is included in a fluid lens, or even placed in *front of the lens*, the negative fringes will be seen. In producing the negative fringes, the interfering rays are those which virtually radiate from the anterior focus of the lens, and which being refracted into parallel directions, enter the eye, and interfere on the retina; and in consequence of their not interfering till they enter the eye, they are much more distinct than the positive fringes.[1]

The most recent experiments on the inflexion of light have been made by Lord Brougham, who had investigated the subject so early as 1796, and given an account of his experiments in two interesting papers printed in the Philosophical Transactions.[2] These investigations were published before Dr. Young discovered the key to this class of phenomena, and before Fresnel had explained them on the principles of the undulatory theory. In his early papers, Lord Brougham considered the phenomena as produced by inflecting and deflecting forces emanating from the diffracting body, and acting, as Newton supposed, upon the passing rays; but in his recent researches he has used these terms merely for the purpose of making the narrative shorter and more distinct, and has avoided all arguments and suggestions relating to the two rival theories.

The recent investigations of Lord Brougham were carried on under the clear sky of Provence, and with an excellent set of

[1] See *Reports of British Association*, vol. vii. p. 12. 1838.

[2] *Phil. Trans.* 1796, p. 227; and 1797, p. 352.

instruments constructed by M. Soleil of Paris. It would be impossible, without diagrams, to make them intelligible to the general reader, but some idea may be formed of the originality and importance of his discoveries from the two following propositions, which relate to a new property of the inflected and deflected rays :—

1. "The rays of light, when inflected by bodies near which they pass, are thrown into a condition or state which disposes them to be on one of their sides more easily deflected than before their first flexion, and disposes them on the other side to be less easily deflected; and when deflected by bodies, they are thrown into a condition or state which disposes them on one side to be more easily inflected, and on the other side to be less easily inflected than they were before the first flexion.

2. "The rays disposed on one side by the first flexion are polarized[1] on that side by the second flexion; and the rays polarized on the other side by the first flexion, are depolarized and disposed on that side by the second flexion."[2] In continuing his researches, Lord Brougham was led to conclude that the rays of light differ in deflexibility and inflexibility, the least refrangible being the most flexible; the law of different flexibility having this peculiarity, that the fringes or images by flexion are not rectilineal but curvilineal from the extreme violet to the extreme red.

Whatever opinion we may form of the undulatory theory in its physical aspect, the explanation which it affords of a vast variety of optical phenomena, entitles it to the highest consideration. With the exception of Lord Brougham's discoveries, and the peculiar colours on the central image formed by grooved surfaces, to which we have already referred, the undulatory theory gives a satisfactory explanation of the leading phenomena of diffraction, while the Newtonian or atomical hypothesis has not even ventured to suggest a probable explanation.

[1] Lord Brougham uses the term polarization "merely because the effect of the first edge resembles polarization, and without giving any opinion as to its identity."

[2] *Phil. Trans.* 1850, pp. 235-260.

CHAPTER X.

Miscellaneous Optical Researches of Newton—His Experiments on the Absolute Refractive Powers of Bodies—More recent Experiments—His Conjecture respecting the Inflammability of the Diamond confirmed by more direct Experiments—His Erroneous Law of Double Refraction—His Observations on the Polarity of Double Refracted Pencils—Discoveries on Double Refraction in the present Century—His Experiments on the eye of a Sheep—Results of them—His three Letters on Briggs's New Theory of Vision—His Theory of the Semi-Decussation of the Optic Nerves—Partly anticipated by Rohault—Opinions of later writers on Vision, of Reid, Brown, Wollaston, Twining, and Alison, discussed—The true laws of Sensation and Vision—Newton's Observations on the Impression of Strong Light upon the Retina—More recent Observations—His Reflecting Sextant—His Reflecting Microscope—His Reflecting Prism for Reflecting Telescopes—His Method of Varying the Magnifying Power of Newtonian Telescopes—Newton's Treatise on Optics—His Lectiones Opticæ.

Although the discoveries described in the preceding chapters are those on which Newton's reputation in optics chiefly rests, yet it is necessary to notice some of his less elaborate researches, which, though of inferior importance in the science of light, have either exercised an influence over the progress of discovery, or have been associated with the history of other branches of knowledge.

In the second book of his Optics,[1] Newton proves, with much fulness of detail, that "the cause of reflexion is not the impinging of light on the solid or impervious parts of bodies, as is commonly believed;" and that "bodies reflect and refract light by one and the same power variously exercised in various circumstances." He then proceeds to show, that "if light be swifter in bodies than in vacuo, in the proportion of the sines which measure the refraction of the bodies, the forces of the bodies to reflect and refract light are very nearly proportional to the

[1] Part iii. Prop. viii. ix. &c.

densities of the same bodies, *excepting that unctuous and sulphureous bodies refract more than others of the same density.*" This remarkable exception led our author to point out the connexion between the refractive powers and the chemical composition of bodies. Having obtained measures of the refractive powers and densities, or specific gravities of twenty-two substances varying in density between *air* and *diamond*, and having computed their refracting forces, and compared them with their densities, he calculated their *refractive powers in respect of their density.* From this comparison he found that *topaz*, *selenite*, *rock-crystal*, *Iceland spar*, *common glass*, *glass of antimony*, and *air*, have their refractive powers almost in the same proportion as their densities, "excepting that the refraction of that strange substance, *Iceland spar*, is a little bigger than the rest."—"Again," he adds, "the refraction of *camphor*, *olive oil*, *lintseed oil*, *spirit of turpentine*, and *amber*, which are fat sulphureous unctuous bodies, and *diamond*, which probably is an unctuous substance coagulated, have their refractive powers in proportion to one another as their densities, without any considerable variation. But the refractive powers of these unctuous substances are two or three times greater in respect of their densities than the refractive powers of the former substances are in respect of theirs. *Water* has a refractive power in a middle degree between these two sorts of substances . . . . *salts of vitriol* between those of earthy substances and water, and spirit of wine between *water* and *oily* substances." The following are a few of the numbers in Newton's Table :—

	Refractive Power.		Refractive Power.
Pseudo topaz,[1] . .	3,979	Rain water, . .	7,845
Air, ...	5,208	Spirit of wine, . .	10,121
Rock crystal, . .	5,450	Oil of olives, . .	12,607
Iceland crystal, .	6,536	Amber, . . .	13,654
Rock salt, . .	6,477	Diamond, . .	14,556

To the results in this table we have added the following, computed chiefly from observations of our own, and interesting

[1] Probably *Sulphate of Barytes.*

as being, with the exception of three in *italics*, below the lowest and above the highest in Newton's Table :—

	Refractive Power.		Refractive Power.
Tabasheer, . .	976	Realgar artificial, .	16,666
Cryolite, . . .	2,742	Ambergris, . .	17,000
Fluor spar, . .	3,426	Sulphur, . . .	22,000
Sulphate of Barytes,	3,829	Phosphorus, . .	28,857
Greenockite, . .	12,861	Hydrogen, . .	29,964
Octohedrite, . .	13,816	Hydrogen, . .	31,862
Diamond, . .	13,964		

The enormous refractive powers possessed by the last six bodies in the preceding table, when taken in connexion with those given by Newton, exhibit in a striking degree the connexion between a high degree of inflammability and a great refracting force. The conjecture of Newton that the diamond "is an unctuous substance coagulated," has been generally regarded as a proof of singular sagacity, and as an anticipation of the results of chemical analysis; but it is certainly not entitled to such praise. Its *solitary* position among the oils and inflammable bodies led to the conjecture; but had he known the refractive index and specific gravities of *greenockite* and *octohedrite*, he would have drawn the same conclusion respecting them, and been mistaken. The real inference respecting the composition of the diamond, which Newton's Table authorizes, is not that it should consist of carbon, but of sulphur. "So then," says he, "by the foregoing table, all bodies seem to have their refractive powers proportional to their densities (or very nearly), excepting so far as they partake more or less of sulphureous oily particles, and thereby have their refractive power made greater or less. Whence it seems rational to attribute the refractive power of all bodies chiefly, if not wholly, to the sulphureous particles with which they abound. For it is probable that all bodies abound more or less with sulphurs. And as light congregated by a burning glass acts most upon sulphureous bodies, to turn them into fire and flame ; so since all action is mutual, *sulphurs ought to act most upon light*."[1]

That diamond is *a soft substance coagulated*, has been rendered probable by experiments of a more direct nature. We have shown by the examination of a great number of diamonds in polarized light, that the little cavities which many of them contain, have been pressed outward by an elastic force emanating from some gas or fluid with which they had been filled. Several such cavities we found in the Koh-i-noor diamond, and in the two smaller ones which accompanied it ; and in a specimen in the British Museum, we found a yellow crystal of diamond that had crystallized upon the cleavage surface of another which was colourless, having been expelled from an adjacent cavity, in which it had existed in a fluid state.[2]

Among the more interesting optical researches of Newton, we rank his observations on the double refraction and polarization of light. On the 12th of June 1689, when Huygens was in England, during the presidency of Sir Robert Southwell, he attended a meeting of the Royal Society, at which Newton was present. Huygens informed the Society that he was about to publish a treatise concerning the cause of gravity, and another about refraction, giving, among other things, the reasons of the doubly refracting Iceland crystal. "Mr. Newton, considering a piece of the Iceland crystal, did observe that of the two species wherewith things do appear through that body, the one suffered no refraction when the visual ray came parallel to the oblique sides of the parallelopiped ; the other, as is usual in all other transparent bodies, suffered more when the beam came perpendicular to the planes through which the object appeared."[3] It is remarkable that this observation of Newton, which had been made long before by Bartholinus, as

[1] *Optics*, Book ii. Part iii. Prop. x.

[2] See *Transactions of the Geological Society*, 2d Series, vol. iii. p. 455 ; and *North British Review*, vol. xviii. p. 227.

[3] *Journal Book* of the Royal Society.

Huygens knew at the time, and as the Royal Society ought to have known,[1] should not have been claimed for that author.

In the admirable Treatise on Light, to which Huygens referred at the Royal Society, and which was published in 1690, he has shown that the observation of Bartholinus, adopted by Newton, is erroneous,[2] and has explained the law of *unusual refraction*, as exhibited in one of the two pencils formed by the double refraction of *Iceland* or *calcareous spar*. This law he deduced from the principles of the undulatory theory, and he confirmed it by direct experiment. Viewing it probably as a theoretical result, Newton seems to have regarded it as incorrect, and though he has given Huygens the credit of describing the phenomena more exactly than Bartholinus, who first discovered and described the remarkable property of this spar, yet without assigning any reason, or even referring to the law of Huygens, he substitutes another in its place. The observations of Newton were first published in his *Optics* in 1704,[3] fourteen years after the appearance of Huygens's work. The law of unusual refraction, adopted by Newton, is not given as the result of theory. It is stated as an undoubted truth, and no experiments whatever are referred to as having been made either by himself or others. "One of these refractions," he says, "is performed by the usual rule of optics, the sine of incidence out of air into this crystal being to the sine of refraction as five to three. The other refraction, which may be called the *unusual*[4] refraction, *is performed by the following rule*." This rule was first shown to be erroneous by the Abbé Hauy,[5] and it has been rejected by all succeeding philosophers.[6]

In his observations on the successive disappearance and re-

[1] It was published in the *Phil. Trans.* 1671, p. 2039.

[2] *Traité de la Lumière*, chap. v. p. 57; and Maseres' *Scriptores Opticæ*, p. 234.

[3] Query 25th and 26th at the end of the work.

[4] The term *unusual*, and the ratio of the sines, viz., 5 to 3, were given by Bartholinus in the abstract of his Paper in the *Phil. Trans.* No. 67, Jan. 1670-1, pp. 20, 39.

[5] *Traité de Mineralogie*, tom. i. p. 159, Note.

[6] Hauy's *Elements of Nat. Phil.* by Gregory, vol. ii. p. 337.

appearance of two of the four images which are formed when a luminous object is viewed through two rhombs of Iceland spar, one of which is made to revolve upon the other, Newton has been more successful, though he has omitted to give to Huygens the credit of having discovered these curious phenomena. He considers "every ray of light as having four sides or quarters, two of which are originally endued with the property on which the unusual refraction depends, and the other two opposite sides not endued with that property;" and he adds, that "it remains to be inquired whether there are not more properties of light by which the sides of the rays differ, and are distinguished from one another."

In animadverting on Huygens's theory of two vibrating media within the Iceland crystal, he asserts that the unusual refraction depends "not on new modifications, but on the original and unchangeable dispositions of the rays," which, he says, "had Huygens known, he would have found it difficult to explain how these dispositions, which he supposed to be impressed on the rays by the first crystal, could be in them before their incidence on that crystal; and in general, how all rays emitted by shining bodies can have these dispositions in them from the beginning. To me, at least," he adds, "this seems inexplicable, if light be nothing else than pression or motion propagated through ether."

After Newton wrote these imperfect observations, more than a century elapsed before the double refraction and polarization of light in Iceland spar and other bodies were reduced to regular laws. In 1810, Malus announced to the Academy of Sciences, the remarkable discovery that a ray of light reflected at a particular angle was polarized like one of the pencils formed by Iceland spar, that is, exhibited the same properties in its four sides or quarters which are exhibited in one of the pencils of Iceland spar; and the result of this fine discovery has been the establishment of a new branch of Physical Optics, which possesses the highest interest, not only from the beauty of its laws and the splendour of its phenomena, but from the new power with which it arms the philosopher in detecting organic or inorganic structures, which defy the scrutiny of the eye and the microscope.

Although Sir Isaac Newton has not published any of his opinions or experiments on Vision, or on the structure and functions of the eye, yet we fortunately possess some fragments of his researches, which are both valuable and interesting. Among these is a manuscript in his own handwriting, which we found among the family papers, containing some accurate observations and experiments on the form and dimensions of the eye of a sheep, and accompanied with an outline drawing, on a large scale, of a section of the eye.[1] The following are the most interesting results contained in this manuscript.

In the first part of it, which is written in Latin, he makes the outer surface of the cornea part of a prolate spheroid, the major axis coinciding with the optical axis, or that of the eye, and having to the transverse axis the ratio of 1350 to 972.

He places the focus for parallel rays of the first surface of the cornea at a point behind the eye, and as far beyond the sclerotic coat as one-seventh of the diameter of the eye-ball, which he makes an oblate spheroid, having its vertical axis 1025, and its horizontal one 975, the anterior portion of the spheroid coinciding nearly with the front of the iris.

He represents the crystalline lens as having a great degree of convexity, differing not much from a sphere, and he remarks that the anterior superficies of the crystalline is more full than the posterior surface, which is certainly not the case, and is not so represented in the diagram.

The second part of the manuscript, which contains minute measurements of every part of the eye, is written in English, and concludes with an expression of regret, that "he was prevented by an accident from taking the distance of the crystalline humour from the horny tunic (the sclerotic coat), which I

[1] See APPENDIX, No. III.

would gladly have done to have had the conformity of all the parts one to another, in one and the same eye." The elliptical form of the cornea was detected not many years ago by M. Chossat of Geneva, who, of course, could not know that he had been anticipated by Newton.

We have not been able to ascertain at what time these observations were made, but it appears from the correspondence of Newton with Dr. W. Briggs, published by Mr. Edleston,[1] that in 1682 his attention was called to the subject of binocular vision, in consequence of Dr. Briggs having communicated to the Royal Society on the 15th March, a paper entitled, "A New Theory of Vision."[2] Briggs, who was a contemporary of Newton's at Cambridge, and a Fellow of Corpus Christi College, seems to have sent him a copy of his paper, and to have solicited his opinion of it. The theory which he proposes evinces neither sagacity nor genius. Setting out on the erroneous principle which has so long disfigured the physiology of the senses, and which has not yet been exploded, that sensation is performed only in the brain, he seeks for an explanation of single vision with two eyes, and of other visual phenomena in "the rise of the optic nerve, the position of its fibres, and the manner of their insertion into the eye." He describes the optic nerves as arising "from two gibbous protuberances,"[3] in such a manner that those fibres that are in the *zenith* or apex of the thalami have the greatest tension, while those in the *nadir*, or opposite part, have the least tension by reason of a less flexure. Every fibre that passes into the upper part of the right eye from the upper part of one thalamus, has a corresponding one passing from the upper part of the other thalamus into the upper part of the left eye, and the same thing takes place with the lower fibres. The fibres which thus correspond in site

[1] *Correspondence*, &c. pp. 264-273. From the Originals in the British Museum, Add. MSS. 4237, fol. 32 and 34.

[2] Hooke's *Collections*, March 1682, No. 6, p. 167.

[3] The *Thalami Nervorum Opticorum*.

correspond also in tension, "so that when any impression from an object without moves *both fibres*, it causes not a *double sensation* any more than *unisons* in two *viols* struck together cause a *double sound*." This theory may be called the *theory of corresponding fibres*, and is doubtless the parent of one more modern though equally inadmissible—the *theory of corresponding points.*

In his first letter to Briggs, Newton tells him that he has "perused his very ingenious theory of vision, in which (to be free with you as a friend should be) there seems to be some things more solid and satisfactory, others more disputable, but yet plausibly suggested, and well deserving the consideration of the ingenious. The more satisfactory I take to be your asserting that we see with both eyes at once,—your speculation about the *musculas obliquus inferior*,[1]—your assigning every fibre in the optic nerve of one eye to have its correspondent in that of the other, both which make all things appear to both eyes, in one and the same place, and your solving hereby the duplicity of the object in distorted eyes, and confuting the childish opinion about the splitting the optic cone. The more disputable seems your notion about every pair of fellow fibres being unisons to one another, discords to the rest, and this consonance making the object seen with two eyes appear but one, for the same reason that unison sounds seem but one sound." Newton here terminates his letter to "his honoured friend, Dr. Briggs," with the observation that he had intended to state his objections "against this notion," but that he thought it better "to reserve it for discourse at their next meeting."

Briggs, probably anxious for an earlier discussion than one living at Cambridge could concede, seems to have requested him to make his objections in writing. Newton accordingly addressed to his honoured friend a long letter of nearly seven printed pages,[2] a letter of very great interest, and utterly sub-

[1] Briggs considers this muscle necessary to prevent squinting, by "keeping the eye *even and in sight*."—Hooke's *Coll.*, March 1682, p. 170.

[2] Dated Trin. Coll. Cambridge, September 12, 1682. APPENDIX, No. IV.

versive of the theory of his correspondent. In the commencement and conclusion of this letter, which is of a slightly personal nature, we see finely displayed the modesty and peculiar character of its author. "Though I am of all men," he begins, "grown the most shy of setting pen to paper about anything that may lead into disputes, yet your friendship overcomes me so far, that I shall set down my suspicions about your theory, yet on this condition, that if I can write but plain enough to make you understand me, I may leave all to your use without pressing it further on. For I design not to confute or convince you, but only to present and submit my thoughts to your consideration and judgment."

After showing that the *bending* of the nerves in the thalami is no proof of a difference of tension, he states, that when the ear hears two sounds in unison, it does not hear them as one sound, unless they come from nearly the same spot; and for the same reason a similar tension of the optic fibres will not make the object appear one to two eyes.

He then proceeds to show that the singleness of the picture arises from the coincidence of the two pictures, and therefore that the cause of single vision must be sought for in the *cause that produces the coincidence.* "But you will say," he adds, "how is this coincidence made? I answer, what if I know not? Perhaps in the sensorium after some such way as the Cartesians would have believed,[1] or by some other way. Perhaps by the mixing of the marrow of the nerves in their juncture before they enter the brain, the fibres on the right side of each eye going to the right side of the head, those on the left side to the left."[2]

In support of his theory, Briggs maintained that "it was

[1] Descartes himself distinctly states that we see objects single with two eyes in exactly the same way as we feel objects single with two hands, forgetting that we see them double by the displacement of the coincident images, and *never* feel them double by the two hands. See Descartes' *Dioptrice*, cap. 6, *De Visione*, Art. X. The experiment of feeling a pea double between two fingers, is not hostile to this observation.

[2] This is precisely the theory of Rohault, see p. 200.

not to be imagined that the nerves decussate one another, or are blended together," at the place where they approach each other before they set off to the right and left eye; and he adduces the case of many fishes, where the nerves are joined only by simple contact, "and in the *chameleon* not at all (as is said)," admitting, at the same time, that in *whitings*, and perhaps some other fishes, they do decussate.

To this Sir Isaac replies: "If you say that in the chameleon and fishes the nerves only touch one another without mixture, and sometimes do not so much as touch; 'tis true, but makes altogether against you. Fishes look one way with one eye, the other way with the other; to the right hand with this, to the left hand with that, twisting their eyes severally this way or that as they please. And in those animals which do not look the same way with both eyes, what wonder if the nerves do not join? To make them join would have been to no purpose; and nature does nothing in vain. But then, whilst in these animals, where 'tis not necessary, they are not joined, in all others which look the same way with both eyes, so far as I can yet learn, they are joined. Consider, therefore, for what reason they are joined in the one and not in the other. For God, in the frame of animals, hath done nothing without reason."

The last objection of Sir Isaac to the new theory is unanswerable. Admitting that consonance unites objects seen with the fibres of *two* eyes, "much more," says he, "will it unite those seen with those (consonant fibres) of the *same* eye, and yet we find it much otherwise."

"You have now seen," he says in conclusion, "the sum of what I think of worth objecting, set down in a tumultuary way, as I could get time from my Stourbridge Fair friends. If I have anywhere expressed myself in a more peremptory way than becomes the weakness of the argument,—pray, look on that as done not in earnestness, but for the mode of discoursing. Whether anything be so material as that it may prove any way useful to you, I cannot tell; but pray, accept of it as written for that end. For having *laid philosophical speculations aside*, nothing but the gratification of a friend would easily invite me to so large a scribble about things of this nature."[1]

Notwithstanding the force of these objections, Dr. Briggs continued to press his theory on public notice, and in May 1683, he published in Hooke's Philosophical Collections additional explanations of it, and a reply to *seven* different objections that had been sent him "by Mr. Newton, our worthy Professor of Mathematics at Cambridge, and other friends." It would be out of place to make any observations on this defence of his theory. We hear no more of it for two years; but it appears that Newton had requested Briggs to print a Latin version of it, and we accordingly find that it was published in London in 1685, with a curious letter of Newton's prefixed. This letter[2] must have been solicited by Briggs, in order to call the attention of philosophers to his book; and we confess that we feel great difficulty in appreciating the motives that could have induced its author to express the opinions which it contains.

In this letter,[3] written in Latin, Sir Isaac speaks of Briggs's two treatises[4] as advancing at once two sciences of great name, Anatomy and Optics. He compliments him on having diligently inquired into the mysteries of an organ so skilfully constructed, and he expresses the great delight which he had formerly received from the skill and dexterity with which he had dissected it. He tells him that he had so elegantly developed the muscles of the eye-ball, and expounded the other parts, that we could not only understand, but see the uses and functions of each,

[1] This letter contains, as will be seen in the Appendix, No. IV., a paragraph respecting the opinions of a Mr. Sheldrake, who, as Mr. Edleston informs us, was a Fellow of Corpus Christi College, and seven years senior to Newton. Mr. Sheldrake states that vision is more distinct when the eye is directed to the object, than when the object is *above* or *below* the optic axes. I do not recollect that this curious fact has been stated by any previous writer on vision.

[2] See APPENDIX, No. V.

[3] Dated Cambridge, May 1685.

[4] The one the Theory of Vision, and the other his *Ophthalmographia*. Cantab. 1676, and Lond. 1687.

and that this showed that nothing inaccurate could be expected from his scalpel. He then speaks of his excellent anatomical tract, in which he shows the value of accurate observation by "a most ingenious theory." After describing Briggs's theory in a few lines, and mentioning the analogy between unisons in music and in optics, he says that nature is simple—that a great variety of effects may be produced by the same mode of operation, and that this was probable in the causes of the cognate senses. But notwithstanding all this general praise, which is certainly not merited, Newton does not adopt the theory. For though he *may suspect that there is another analogy* between these senses, than that contained in the theory, he must willingly confess that that of Briggs is very ingeniously excogitated. He then remarks that he does not think the second dissertation useless in which he *dilutes* the objections made against the theory. "Go on, then," he adds, "illustrious sir, as you are doing, and advance these sciences by your very great inventions, and teach the world that those difficulties in investigating physical causes which usually yield with difficulty to vulgar attempts, may be so easily overcome by talent."

While Newton was writing this letter, there is reason to believe that he had himself conceived another theory of single vision with two eyes, proceeding on the supposition that Briggs was wrong in his Anatomy as well as in his Optics. This, we think, is indicated by the "other analogy" of the senses of sight and hearing which he then suspected, and to which he was no doubt led by his correspondence with Briggs. It is evident, that in September 1682, the date of his second letter, he had laid aside philosophical speculations, and that he unwillingly wrote his opinion "about things of that nature;" and it is equally obvious, from his supposition about the mixing of the marrow of the nerves in their juncture before they enter the brain, that if the idea of the semi-decussation of the fibres had been then in his view, he had not at that time given it any serious consideration.

That he had studied this subject with peculiar care, is manifest from the 15th Query of his Optics,[1] where he has given a brief abstract of his theory of corresponding points, or of the semi-decussation of the optic nerves, but particularly from an elaborate paper on the subject which was never published in his lifetime, but was found in MS. among the papers of William Jones, Esq., known as the celebrated Macclesfield Collection of scientific correspondence. A copy of this paper was given to Joseph Harris, who inserted it in his *Treatise of Optics*,[2] but from the manner in which he has garbled it, we cannot discover whether or not he has published the whole of the manuscript.[3]

The theory of Newton, as published in his Optics, and as more fully developed in the MS. in question, will be understood from the annexed diagram given by himself. Let P, Q represent the two eyes, TVEG, YXEH the optic nerves, crossing at what has been called the *sella turcica*, GH, and passing between IL or MK towards the brain. Newton observes, that if the nerve be cut crosswise anywhere between TG or YH, the section will

[1] See APPENDIX, No. VI. [2] See APPENDIX, No. VII.

[3] Although it is evident, from a careful perusal of the 15th Query, that it contains the same doctrine of the *semi-decussation of the optic nerves* which is given in the MS., yet it has been misunderstood by Dr. Reid, who obviously had not seen the copy of it in Harris's *Optics*. "Sir Isaac Newton," says Dr. Reid (*Inquiry*, cap. vi. sect 13), "who was too judicious a philosopher and too accurate an observer to have offered even a conjecture which did not tally with the facts which had fallen under his observation, proposes a query with respect to the cause of it (namely, the relation and sympathy between corresponding points of the two retinæ)."—*Optics*, Query 15. Dr. Reid seems not to have detected the doctrine of semi-decussation in the Query, and to have believed that individual nerves, not half-nerves, from the two sides of both eyes, united before they reached the brain, and there produced a joint and single impression; and Dr. Alison has either taken up Dr. Reid's opinion, or misunderstood the Query, and also the theory of semi-decussation. "It is well-known," he says, "that an explanation (of single vision by means of double images) was proposed by Newton, fully considered by Reid, and since supported by Wollaston (often called the theory of Wollaston, but quite incorrectly), proceeding on the supposition of *a semi-decussation of the human optic nerves* at their commissure, *whereby the fibres from the right half of the retina go to the right optic lobe in the brain*, and *vice versa*." This is the theory of Rohault, and not of Newton and Wollaston, in which the *half-fibres*, from the right half of the retina of each eye, unite into one fibre at their commissure GH in *Fig.* 12, and then go to the right optic lobe.

"appear full of spots or pimples, which are a little prominent, especially if the nerve be pressed or warmed at a candle; that these shoot into the very eye, and may be seen withinside where the retina grows to the nerve; and that they continue to the very juncture EFGH. But at the juncture they end on a sudden into a more tender white pap, like the anterior part

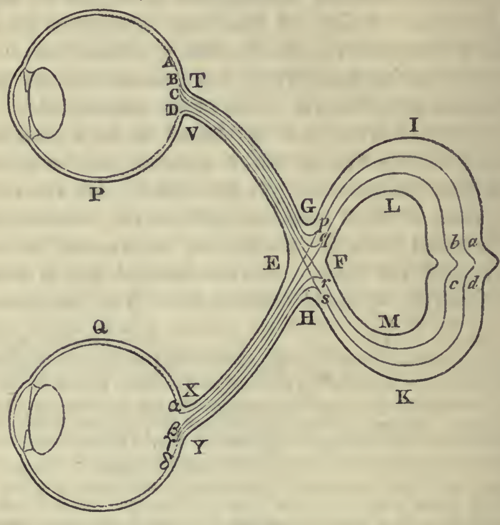


FIG. 16.

of the brain, and so the nerve continues after the juncture into the brain, filled with a white tender pap, in which can be seen no distinction of parts as betwixt the said juncture and the eye."

"Now I conceive," says he, "that every point in the retina of one eye hath its correspondent point in the other, from which two very slender pipes, filled with a most limpid liquor, do without any interruption, or any other unevenness or irregularity in their process, go along the optic nerves to the juncture EFGH, where they meet either betwixt GF or FH, and there unite into one pipe as big as both of them; and so continue in one passing either betwixt IL or MK into the brain, where they are terminated perhaps at the next meeting of the nerves between the *cerebrum* and *cerebellum*, in the same order that their extremities were situated in the retinas. And so there are a vast multitude of these slender pipes which flow from the brain, the one-half through the right-side nerve IL, till they come at the juncture GF, where they are each divided into two branches, the one passing by G and T to the right side of the right eye AB, the other half shooting through the space EF, and so passing by X to the right side of the left eye $\alpha\beta$. And, in like manner, the other half shooting through the left-side nerve MK, divide themselves at FH, and their branches passing by EV to the right eye, and by HY to the left, compose that half of the retina in both eyes which is towards the left side CD and $\gamma\delta$."

From this theory of the *semi-decussation* of the *optic nerves*, Newton draws the following conclusions :—

" Hence it appears," says he,—

" 1. Why the two images of both eyes make but one image, *abcd*, in the brain.

" 2. Why, when one eye is distorted, objects appear double, for if the image of any object be made upon A in the one eye, and β in the other, that object shall have two images in the brain at *a* and *b*. Therefore, the pictures of any objects ought to be made upon the corresponding points of the two retinas; if upon A in the right eye, then upon α in the left; if upon B, then also upon β. And so shall the motions concur after they have passed the juncture GH, and make one image at *a* or *b* more vivid than one eye alone could do.

" 3. Why, though one thing may appear in two places by distorting the eyes, yet two things cannot appear in one place. If the picture of one thing fall upon A, and of another upon α, they may both proceed to *p*, but no farther. They cannot both be carried on the same pipe *p a* into the brain; that which is strongest, or most helped by phantasy, will there prevail, and blot out the other.

"4. Why, if one of the branches of the nerve beyond the juncture, as at G F or F H should be cut, that half of both eyes towards the wounded nerve would be blind, the other half remaining."[1]

This ingenious theory, decidedly superior to that of Briggs, was to a considerable extent anticipated by M. Rohault, in his *Traité de Physique*, published in 1671, more than ten years before Newton's attention was called to the subject. Rohault gives the very same figure as the preceding one, with this difference, that the nerves neither cross nor split into two at G H. He supposes that the two optic nerves have their corresponding or sympathetic fibres, which unite in one point in the brain; and he thus explains single vision with two eyes, their duplicity by distortion, and the impossibility of two things appearing in one place.[2]

During the 120 years that have elapsed since the publication of Newton's Optics, we hear nothing more of the Theory of Vision in the 15th Query, and in the manuscript above referred to, till the year 1824, when Dr. Wollaston published in the *Philosophical Transactions* of that year, a paper *On Semi-decussation of the Optic Nerves*, in which he reproduces the very theory of Newton, in order to account for the curious disease of *hemiopsy*, or *amaurosis dimidiata*,[3] in which the patient sees with each eye only half of an object, being blind to the other

[1] Sir Isaac draws other four conclusions from his theory, but they will find a fitter place in the APPENDIX, No. VII.

[2] A Latin translation of Rohault's work was published in 1708, by Dr. Clarke, "with annotations chiefly from the philosophy of Newton, and yet no notice is taken of Newton's Theory, as contained in his 15th Query, although Dr. Clarke had translated the *Optics* into Latin. He adds a note stating that the conjecture respecting the fibres of the optic nerve had not yet been confirmed by dissection. Part I. cap. 31, p. 225, *note*.

[3] The *suffusio dimidians* of other authors.

half. This sympathy between the two eyes may certainly arise from structure, and depend upon "connexion of nervous fibres," and if it does, is very well explained either by the hypothesis of Rohault or of Newton; but we cannot attach any value to the invention of structural hypotheses when the phenomena may be explained by that habitual sympathy of double organs with which we are so well acquainted. This observation is still more applicable to the remark of Wollaston, that by his theory "we clearly gain a step in the solution, if not a full explanation of the long agitated question of single vision with two eyes," because this great fact in vision can be perfectly explained, as we shall presently see, without any hypothesis whatever.

But not only is this theory of semi-decussation uncalled for, it is contradicted by numerous facts. It has been examined with great ability by Mr. Twining, of the Indian Medical Service,[1] who concludes "from anatomical observations respecting the structure of the optic nerves and thalami, and the effects of disease on those parts, that no decussation or semi-decussation of the optic nerves exists in the human subject. No anatomist, indeed, has pretended to say that there is any trace of semi-decussation; and it has been proved that the decussation or crossing at G H, *Fig.* 16, is only partial, the inner bundles decussating, while the outer bundles remain on the side on which they previously lay."[2]

There is no branch of physical science upon which such unsound views have prevailed as in that which relates to the optical functions of the eye; and in studying the speculations of modern metaphysicians and physiologists, we feel as if we were grappling with the chimeras of Aristotle or Descartes. While Dr. Reid maintains that objects appear single when their images are formed upon corresponding points of the retina,

[1] See *Transactions of the Medical and Physical Society of Calcutta*, vol. ii. p. 151; or *Edinburgh Journal of Science*, July 1828, vol. ix. p. 143.

[2] Wagner's *Handwörterbuch der Physiologie*, vol. iii. part ii. p. 297.

and double in all other circumstances, he gives *no explanation whatever* of single vision : he merely attaches the name of corresponding points to those upon which the image falls when it is seen single ! And when Dr. Brown tells us that it is from association alone we see objects single and erect, by means of double and inverted pictures, he merely asserts his ignorance of the cause ; and his assertion is contrary to the most notorious facts and to all experience, as Dr. Reid has shown.[1] Nor does Dr. Alison, one of the latest writers on the subject, bring us a single step nearer the truth. After controverting the views of Brown and Reid, he apprehends that he has established the following two facts, the one explaining single, and the other erect vision :[2]—

1. That images formed on corresponding points of the retinæ of the human eyes, and on those only, naturally affect our minds in the same manner as a single image formed on the retina of one eye ; and,

2. That impressions made on different points of the retina

[1] If by the sense of touch we could make the *two* images appear *one*, then we should also see an object single when it is doubled by looking either at a nearer or a more distant object, or when it is made 100 by a multiplying glass ; but if a man were to live 1000 years, he would still see the *two* or the hundred images, though he knew there was only one object. In order to illustrate his opinion, Dr. Brown says that the two English words *he conquered,* excite the same idea as the one Latin word *vicit.* In reply to this Dr. Whewell says, "that to make this pretended illustration of any value, it ought to be true that when a person has thoroughly learned the Latin language, he can no longer distinguish any separate meaning in *he* and in *conquered.*" With this assertion we cannot concur. The two words *he conquered,* undoubtedly convey the same meaning as *vicit.* If we unite the two words thus, *heconquered* or *conqueredhe,* we cannot doubt that the word *he* is as truly included in the termination *it* of *vicit,* as *he* is in the single word *heconquered,* unless it is alleged that *vicit* may also mean *she conquered.*

Dr. Brown's real mistake consists in not taking *two exactly similar words,* as *vicit, vicit,* like what he considers as the *two exactly* similar images. The two words pronounced in succession convey certainly only one idea, but the mind recognised the same in succession or its *duplicity,* just as it would do the two similar and united images, if one of them were slipped from its superposition on the other by pressing aside one of the eye-balls.

Dr. Brown's views are affected with another error, namely, in the assumption that the pictures in each eye are exactly similar.

[2] *Edinburgh Transactions,* vol. xiii. p. 479.

of the eye, are *naturally* followed by inferences as to the relative position of the objects producing these impressions, *exactly opposite* to those which follow impressions made on different points of the surface of the body.[1]

We are unable to controvert these two palpable facts. They are truisms which explain nothing ; and if Nature had been so perverse as to produce *three* pictures in place of *one* from two eyes, and had turned round an erect picture 90° in place of 180°, which it does in inverting it, that is, had represented a man upon the retina lying horizontally in place of vertically and inverted, the explanation of Dr. Alison would have been, that in the first case it was *natural*, and that in the other it was *naturally*, and *exactly half opposite* to other impressions on the surface of the body.[2]

From these speculations we venture to solicit the attention of the reader to the true explanation of single and erect vision, and of all the other normal visual phenomena with which we are acquainted, an explanation which has been overlooked by our most distinguished optical writers.

1. The retina[3] is the seat of visual sensation and of vision ; and there is a law of visual sensation as well as a law of vision, which can be determined only by experiment.

2. In order to determine the law of visual sensation, or the mental information given by the action of a physical point of

[1] There is no opposition between the impressions on the concave retina and on a concave surface of the body. If we hold up the hand vertically, and bend it into a concavity, an impression made on the *upper* part of the concavity, will be felt as coming from below, and an impression on the lower part of the concavity will be felt as coming from above, exactly as in the case of the concave retina.

[2] We have not noticed the additional explanation adopted by Dr. Alison, "that impressions on the *upper* part of the *retina* are impressions on the *lower* part of the *optic lobes, i.e.,* of the *sensorium* ;" because he has not told us what requires as much explanation as inverted vision, namely, why the lower part of the sensorium makes the object seem lower ! Is the sensorium a plane, or a convexity, or a concavity ? If it is a concavity, a physical impression on the *lower* part *will* correspond to the *top* of the object, and an impression on the *upper* part with the *bottom* of it.

[3] I omit all consideration of the question, whether the choroid coat or retina is the seat of vision, or whether the *foramen centrale* is or is not an opening in the retina.

light upon the retina, let us make a hole of the smallest size, that of the *minimum visibile*, for example, on a sheet of black paper, and let a ray of the sun's light pass through it and fall upon the eye. This cone of rays, with the pupil for its base, will be refracted by the humours of the eye into a smaller cone, the apex of which falls upon the retina. This apex, or point, is the image of the hole in the paper, and is formed by a cone of rays whose angle we may suppose to be 12°, so that the impression is made by rays falling at all angles on the retina from 0° to 6° on each side of the perpendicular or axis of the cone. If, while looking at the hole in the paper, we stop all the different rays in succession from 0° to 6°, we shall find that the hole is seen by them all *in one direction*, and that this direction is the axis of the cone, and, as nearly as can be ascertained, the real direction of the hole, or the axis of the incident cone of rays. Hence it follows, that the impression of a ray of light upon the retina, whatever be the angle of its incidence, gives the sensation of having proceeded in a direction perpendicular to the retina, a direction as will be afterwards seen coinciding nearly with the real direction of the hole from which it issues. This is the law of visible direction.

3. In order to determine the law of vision, look at the hole in the paper with both eyes, and it will be found, by opening and shutting each eye alternately, that a single image of the hole is seen, and always in the same place, namely, at the point where the optical axes of the two eyes meet, and consequently at the distance from the eye where these axes meet. The single image seen by both eyes is formed by the superposition or coincidence of the two images. This is the law of visible distance, and the law of single vision ; but the law of single vision is true only for visible points. If we had the *hundred* eyes of Argus in place of *two*, the hundred images of a point would coincide in *one* at the point where the hundred images converge.

4. The law of vision for visual *objects* is entirely different

from that for points. A visual object cannot be seen single at once. Let the object, for example, be a *line* $\frac{1}{10}$th of an inch long. The two images of it cannot be seen coincident by both eyes. When the right hand extremities of the images are coincident or single, the left hand extremities are not, and *vice versa.* When the object is a *lineal* space or *superficies*, only one point of it is seen single and distinct, the two eyes converging their optic axes on every point of it in succession, and thus obtaining the idea of space. When the object is a *solid*, such as a cube, only one point of it is seen single and distinct, the two eyes converging their optical axes to the near and remote parts of it in succession, and thus obtaining an idea of the different distances of its parts by the varying angle of the optic axes. This law of vision for solids, includes the theory of the stereoscope.[1]

We have stated that the law of sensation gives a visible direction, which is *nearly* coincident with the real direction of objects. The celebrated D'Alembert maintained that the action of light upon the retina is conformable to the laws of Mechanics,[2] and therefore that the visible direction of an object should be a line perpendicular to the curvature of the retina at the excited point; but he rejected this law as contrary to observation. By using, however, more correct refractive powers for the humours of the eye, and more accurate measures of its parts, we have shown that the visible and true direction of points nearly coincide.[3]

By means of these laws all the phenomena of erect vision from an inverted image,—of the single vision of points,—of the vision of plane surfaces and solids,—and of the conversion of

[1] See *Edinburgh Transactions*, vol. xv. p. 360; *North British Review*, vol. xvii. p. 165; and my Treatise on the Stereoscope.

[2] When a ray falls obliquely upon the retina (or any other surface of sensation) its action may be decomposed into two, the one lying in the surface of the membrane, and acting laterally upon the papillæ, and the other perpendicular, and acting in the direction of the axis of the papillæ, and therefore passing to the brain.

[3] See *Edinburgh Transactions*, vol. xv. pp. 350-353.

two plane pictures into solids or objects in relief, may be calculated with as much accuracy as we can compute the positions of the heavenly bodies.

Among the minor optical labours of Sir Isaac Newton, we must rank some curious observations on the action of strong light upon his own eyes, which have been only recently published by Lord King in his Life of Locke. In his work on Colours, Mr. Boyle has described a curious case, in which a gentleman "eminent for his profound skill in almost all kinds of philological learning, had injured his eyes by looking too fixedly upon the sun through a telescope, without any coloured glass to take off from the dazzling splendour of the object. The excess of light did so strongly affect his eye, that *ever since* when he turns it towards a window or any white object, he fancies he sees a globe of light of about the bigness the sun then appeared to him, to pass before his eyes; and having inquired of him how long he had been troubled with this indisposition, he replied, that it was already *nine* or *ten* years since the accident that occasioned it first befell him."[1] This remarkable case having attracted the attention of Locke, he requested Sir Isaac to give him his opinion on the subject. In his reply, dated Cambridge, June 30, 1691, Sir Isaac sent him the following very interesting observations, made by himself.[2]

"The observation you mention in Mr. Boyle's book of colours, I once made upon myself with the hazard of my eyes. The manner was this: I looked a very little while upon the sun in the looking-glass with my *right eye*, and then turned my eyes into a dark corner of my chamber, and winked, to observe the impression made, and the circles of colours which encompassed it, and how they decayed by degrees, and at last vanished. This I repeated a second and a third time. At the third time, when the phantasm of light and colours about it were almost vanished, intending my fancy upon them to see their last appearance, I

[1] *Experiments and Considerations touching Colours*, chap. ii. § 9, p. 19. Lond. 1664.
[2] King's *Life of Locke*, vol. i. pp. 404-408. Edit. 1830.

found, to my amazement, that they began to return, and by little and little to become as lively and vivid as when I had newly looked upon the sun. But when I ceased to intend my fancy upon them, they vanished again. After this, I found, that, as often as I went into the dark, and intended my mind upon them, as when a man looks earnestly to see anything which is difficult to be seen, I could make the phantasm return without looking any more upon the sun; and the oftener I made it return, the more easily I could make it return again. And at length, by repeating this without looking any more upon the sun, I made such an impression on my eye, that, if I looked upon the clouds, or a book, or any bright object, I saw upon it a round bright spot of light like the sun, and, which is still stranger, though I *looked upon the sun with my right eye only, and not with my left, yet my fancy began to make an impression upon my left eye, as well as upon my right.* For if I shut my right eye, or looked upon a book or the clouds *with my left eye, I could see the spectrum of the sun almost as plain as with my right eye*, if I did but intend my fancy a little while upon it; for at first, if I shut my right eye, and looked with my left, the spectrum of the sun did not appear till I intended my fancy upon it; but by repeating, this appeared every time more easily. And now, in a few hours' time, I had brought my eyes to such a pass, that I could look upon no bright object with either eye, but I saw the sun before me, so that I durst neither write nor read; but to recover the use of my eyes, shut myself up in my chamber made dark, for three days together, and used all means to divert my imagination from the sun. For if I thought upon him, I presently saw his picture, though I was in the dark. But by keeping in the dark, and employing my mind about other things, I began in three or four days to have some use of my eyes again; and, by forbearing to look upon bright objects, recovered them pretty well, though not so well, but that, for some months after, the spectrum of the sun began to return as often as I began to meditate upon the phenomena, even though I lay in bed at midnight with my curtains drawn. But now I have been very well *for many years*, though I am apt to think, if I durst venture my eyes, I could still make the phantasm return by the power of my fancy. This story I tell you, to let you understand, that in the observation related by Mr. Boyle, the man's fancy probably concurred with the impression made by the sun's light, to produce that phantasm of the sun which he constantly saw in bright objects. And so your question about the cause of this phantasm involves another about the power of fancy, which, I must confess, is too hard a knot for me to untie. To place this effect in a constant motion is hard, because the sun ought then to appear perpetually. It seems rather to consist in a disposition of the sensorium to move the imagination strongly, and to be easily moved, both by the imagination and by the light, as often as bright objects are looked upon."

These observations possess in many respects a high degree of interest. The fact of the transmission of the impression from the retina of the one eye to that of the other, or of its production in that eye merely by fancy, is particularly important; and it deserves to be remarked as a singular coincidence, that we had occasion to observe, and to describe the same phenomena above forty years ago,[1] and long before the observations of Sir Isaac were communicated to the scientific world. Æpinus of St. Petersburg observed the circles of colours described by Newton, when produced by looking at the setting sun for fifteen seconds. In the experiments alluded to, we looked at the brilliant image of the sun formed by a concave speculum of 30 inches focus with the right eye tied up, and upon turning the left eye to a white ground, we observed *six* successions of different colours with their complementary tints when the left eye was shut. Upon uncovering the right eye, and turning it to a white ground, we were surprised to observe the reverse spectra, as if the impression had been conveyed from the left to the right eye, as in Sir Isaac's case. A spectrum of a darkish

[1] Art. ACCIDENTAL COLOURS, in the *Edinburgh Encyclopædia*, vol. i. pp. 91, 92.

hue floated before the left eye for many hours, and this was succeeded by severe pains shooting through every part of the head. A slight inflammation, affecting both eyes, continued for several days, and it was not till several years had elapsed that our eyes had recovered their former power.

Among the inventions of Sir Isaac Newton, we may enumerate his reflecting sextant for observing the moon's distance from the fixed stars at sea. The description of this instrument was communicated to Dr. Halley in the year 1700; but, either

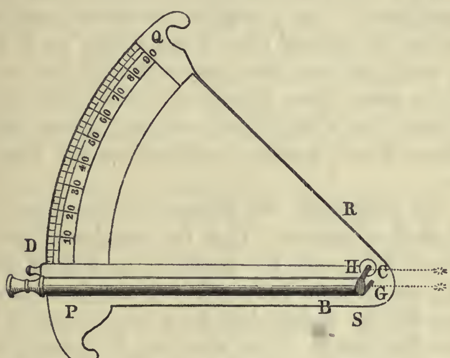


FIG. 17.

from having mislaid the manuscript, or from attaching no value to the invention, he never submitted it to the Royal Society, and it remained among his papers till after his death in 1742, when it was read on the 28th October. The following is Sir Isaac's own description of it, as copied from the original manuscript:[1]—

"In the annexed figure, PQRS denotes a plate of brass, accurately divided in the limb DQ into $\frac{1}{2}$ degrees, $\frac{1}{2}$ minutes, and $\frac{1}{12}$ minutes by a diagonal scale; and the $\frac{1}{2}$ degrees, and $\frac{1}{2}$

[1] See *Phil. Trans.* 1742-43, vol. xlii. p. 155.

minutes, and $\frac{1}{12}$ minutes, counted for degrees, minutes, and $\frac{1}{6}$ minutes. AB is a telescope three or four feet long, fixed on the edge of that brass plate. G is a speculum fixed on the brass plate perpendicularly as near as may be to the object-glass of the telescope, so as to be inclined forty-five degrees to the axis of the telescope, and intercept half the light which would otherwise come through the telescope to the eye. CD is a moveable index turning about the centre C, and, with its fiducial edge, showing the degrees, minutes, and $\frac{1}{6}$ minutes on the limb of the brass plate PQ; the centre C must be over against the middle of the speculum G. H is another speculum, parallel to the former, when the fiducial edge of index falls on $0° \ 0' \ 0''$; so that the same star may then appear through the telescope in one and the same place, both by the direct rays and by the reflexed ones; but if the index be turned, the star shall appear in two places, whose distance is showed on the brass limb by the index.

"By this instrument the distance of the moon from any fixed star is thus observed; view the star through the perspicil by the direct light, and the moon by the reflexed (or on the contrary); and turn the index till the star touch the limb of the moon, and the index shall show on the brass limb of the instrument the distance of the star from the moon's limb; and though the instrument shake by the motion of the ship at sea, yet the moon and star will move together as if they did really touch one another in the heavens; so that an observation may be made as exactly at sea as at land.

"And by the same instrument, may be observed exactly the altitudes of the moon and stars, by bringing them to the horizon; and thereby the latitude and times of observation may be determined more exactly than by the ways now in use.

"In the time of the observation, if the instrument move angularly about the axis of the telescope, the star will move in a tangent of the moon's limb, or of the horizon; but the observation may notwithstanding be made exactly, by noting when the line, described by the star, is a tangent to the moon's limb, or to the horizon.

"To make the instrument useful, the telescope ought to take in a large angle; and, to make the observation true, let the star touch the moon's limb, not on the outside, but on the inside."

This ingenious contrivance is obviously the very same as that which Mr. Hadley produced in 1731;[1] and which, under the name of Hadley's Quadrant, has been of so great service in navigation. But though the merit of this invention is thus transferred to Newton, we must not omit to state, that the germ of it, and something more, had been previously published by Hooke. In giving an account of the inventions of members of the Royal Society, Sprot mentions "a new instrument for taking angles by reflexion, by which means the eye at the same time sees the two objects both as touching on the same point, though distant almost to a semicircle, which is of great use in promoting exact observations at sea."[2] Hooke was the member who made this invention, and there is a drawing and description of it in his Posthumous Works.[3] About the end of the year 1730, Thomas Godfrey of Philadelphia invented an instrument similar to Hadley's; and the Royal Society, having found that Hadley's invention could be traced to the summer of 1730, decided that Hadley and Godfrey were independent inventors. The enlargement of this valuable instrument, so as to measure an angle of 120°, was first proposed by Captain Campbell in 1757.[4]

On the 6th February 1672, Sir Isaac communicated to Mr. Oldenburg his "design of a microscope by reflexion, which

[1] *Phil. Trans.* 1731, p. 147.

[2] Sprot's *Hist. of the Royal Society*, p. 246. Lond. 1667.

[3] *The Posthumous Works of* Robert Hooke, M.D., p. 503, tab. xi. fig. 2. Lond. 1705. In the description given of it by Waller, his biographer, the invention is mentioned as "an instrument for taking angles at one prospect, which he found described on a loose paper."

[4] Grant's *Hist. of Physical Astronomy*, p. 487; and *Nautical Mag.* vol. i. p. 351.

should have, instead of an object-glass, a reflecting piece of metal, and which seemed as capable of improvement as telescopes, and perhaps more so, because but one reflective piece of metal is requisite in them." This microscope is shown in the annexed diagram, copied from the original, where A B is the object-metal, C D the eye-glass, F their common focus, and O the other focus of the metal in which the object is placed. This ingenious idea has been greatly improved in modern times by Professor Amici, Professor Potter, and Dr. Goring,[1] who make A B a portion of an ellipsoid, whose foci are O and F, and who fix a small plain speculum between O and A B, in order to

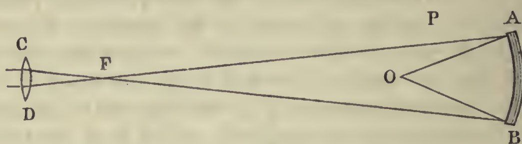


Fig. 18.

reflect into the speculum the object which is placed on one side at P, for the purpose of being illuminated.[2]

In another letter to Mr. Oldenburg, dated July 11th in the same year, he suggests an improvement of microscopes by refraction, "which I do," he says, "more willingly, because Mr. Hooke hath made such excellent use of that instrument; and I shall be glad to contribute any thing to your promotion of these his ingenious endeavours, or add to his inventions of that kind. The way is, by illuminating the object in a darkened room with light of any convenient colour not too much compounded; for by that means the microscope will, with distinctness, bear a deeper charge and larger aperture, especially if its construction be such as I may hereafter describe."[3] This happy

[1] See *Edinburgh Journal of Science*, vol. vi. p. 61; *Encyclopædia Brit.*, Art. *Microscope*, vol. xv. p. 41.

[2] Newtoni *Opera*, tom. iv. p. 300.

[3] Sir Isaac does not seem to have afterwards described this construction.

idea we have some years ago succeeded in realizing, by illuminating microscopic objects with the light of a monochromatic lamp, which discharges a copious flame of pure yellow light of definite refrangibility.[1] Since the time of Newton, the microscope has undergone the greatest improvement,—the single microscopes made of diamond and the other precious stones,—the microscopic doublets, and the magnificent compound microscopes of Ross, Powell, and Nachet fitted up as polarizing microscopes.

In order to remedy the evil of want of light in his reflecting telescope, arising from the weak reflecting power of speculum metal, and from its tarnishing by exposure to the air, Sir Isaac proposed to substitute for the small oval speculum a triangular prism of glass or crystal A B C, *Fig.* 19. Its side A B *b a* he supposes to perform the office of that metal, by reflecting towards the eye-glass the light which comes from the concave speculum D F, *Fig.* 20, the light reflected from which he supposes to enter into this prism at its side C B *b c*, and lest any colours should be produced by the refraction of these planes, it is requisite that the angles of the prism at A *a* and B *b* be precisely equal. This may be done most conveniently, by making them half right angles, and consequently the third angle at C *c* a right one. The plane A B *b a* will reflect all the light incident upon it, "especially if the prism be made of crystal;" but in order to exclude unnecessary light, it will be proper to cover it all over with some black substance, excepting two circular spaces of the planes A *c* and B *c*, through which the useful light may pass. The length of the prism should be such that its sides A *c* and B *c* may be

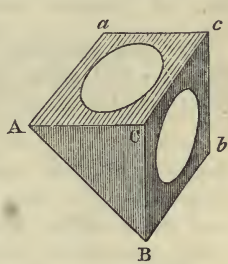


FIG. 19.

[1] See *Edinburgh Transactions*, vol. ix. p. 433; and the *Edinburgh Journal of Science*, July 1829, No. I. new series, p. 108.

"four-square," and so much of the angles B and *b* as are superfluous ought to be ground off, to give passage for as much light as is possible from the object to the speculum.

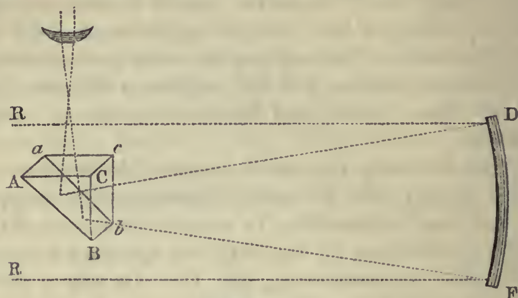


FIG. 20.

One great advantage of this prism, which cannot be obtained from the oval metal, is that, without using two glasses, the object may be erected, and the magnifying power of the telescope varied at pleasure, by merely varying the distances of the speculum, the prism, and the eye-glass. This will be understood from *Fig.* 21, where A I represents the great concave

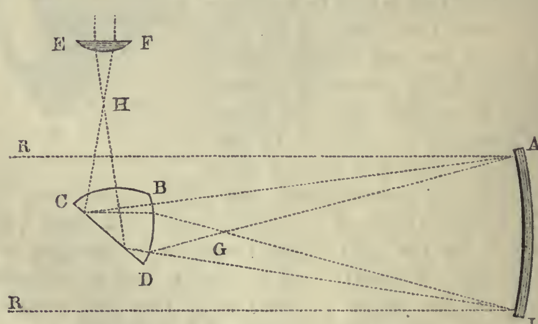


FIG. 21.

speculum, E F the eye-glass, and B C D the prism of glass, whose sides B C and B D are not flat, but spherically convex. The rays which come from G, the focus of the great speculum A I, will, by the refraction of the first side B D, be reduced to parallelism, and after reflexion from the base C D, will be made by the refraction of the next side B C to converge to the focus H of the eye-glass E F. If we now bring the prism B C D nearer the image at G, the point H will recede from B D, and the image formed there will be greater than that at G; and if we remove the prism B C D from G, the point H will approach to B C, and the image at H will be less than at G. The prism B C D performs the same part as a convex lens, G and H being its conjugate foci, and the relative size of the images formed at these points being proportional to their distance from the lens. These different contrivances were suggested by some criticisms upon his reflecting telescope by M. Auzout; and Newton does not seem to have executed them, as he recommends "that the first trials be made with prisms whose sides are all of them plane."[1] As more than one-half of the light is lost by reflexion from the small mirror, we have proposed to substitute for it an achromatic prism to refract the rays to the side of the tube.[2] An advantage would be gained by the use of a plane speculum of *silver*, which reflects much more light than speculum metal. The objection to reflecting prisms arises from the imperfection of the glass, and the difficulty of obtaining three perfectly flat surfaces, and two angles perfectly equal. This construction would be a good one for varying optically the angular distance of a pair of wires placed in the focus of the eye-glass E F; and by bisecting the lenticular prism B C D, and giving the halves a slight inclination, we should be able to separate and to close the two images or discs which the two halves would produce, and thus form a double image micrometer.

[1] See Newtoni *Opera*, tom. iv. p. 276.

[2] *Treatise on Optics*, edit. of 1853, p. 494.

In concluding our account of Newton's optical discoveries, some notice of the principal work which contains them will suitably terminate the present chapter. This work, entitled *Opticks, or a treatise on the Reflexions, Refractions, Inflexions, and Colours of Light*, was published in London, without a date, on the 16th February 1704. Newton, from the President's chair, presented it to the Royal Society. Dr. Halley was desired to peruse it and make an abstract of it, and the thanks of the Society were given to the author "for the book, and for being pleased to publish it." In the second edition, with the date of July 16, 1717, the date of April 1, 1704, is added to the advertisement of the first edition, a step of which, as Mr. Edleston observes, "the dispute with Leibnitz had probably taught our philosopher the importance."[1]

In the advertisement to the first edition, we are informed by the author, that "a part of the ensuing discourse about light was written at the desire of some gentlemen of the Royal Society in the year 1675, and then sent to their Secretary and read at their meetings, and the rest was added about twelve years after, to complete the theory, except the third book, and the last proposition of the second, which were since put together out of scattered papers. To avoid being engaged in disputes about these matters, I have hitherto delayed the printing, and should still have delayed it, had not the importunity of friends prevailed upon me. *If any other papers writ on this subject are got out of my hands, they are imperfect*, and were perhaps written before I had tried all the experiments here set down, and fully satisfied myself about the laws of refractions and compositions of colours. I have here published what I think proper to come abroad, wishing that it may not be translated into another language without my consent." In the advertisement to the second edition, which appeared in 1717, he mentions that he could have added at the end of the

[1] It is a curious fact, that "there is the same peculiarity about the preface to the Principia."—Edleston's *Correspondence*, &c. &c., pp. lviii. and lxxi.

third book some questions (namely the thirty-one celebrated queries); "and," he adds, "to show that I do not take gravity for an essential property of bodies, I have added one question concerning its cause, choosing to propose it by way of a question, because I am not yet satisfied about it for want of experiments."

At the request of Newton, Dr. Samuel Clark prepared a Latin edition of his Optics, which appeared in 1706, and he was generously presented by Sir Isaac with £500, or £100 for each of his five children, as a token of the approbation and gratitude of the author. Demoivre is said to have secured and taken charge of this translation, and to have spared neither time nor trouble in the task. Newton met him every evening at a coffee-house,[1] and when they had finished their work, he took Demoivre home with him to spend the evening in philosophical conversation.[2] Both the English and the Latin editions have been frequently reprinted, both in England and on the Continent, and perhaps there never was a work of profound science more widely circulated.[3]

The only other optical work by Newton was his *Lectiones Opticæ*, a course of lectures on optics, which he read as Lucasian Professor in the public schools of the University of Cambridge in the years 1669, 1670, and 1671. It was not published till after his death;—an English edition in 1728, in octavo,[4] and the Latin original in 1729 in quarto.

This valuable work is divided into two parts, and contains many beautiful propositions, and interesting and instructive experiments, which are not to be met with in any modern treatise on optics.

In the *first* part, which is entitled, *On the Refraction of the*

[1] "Probably Slaughters' Coffee-house in St. Martin's Lane."—Edleston's *Correspondence*, p. lxxiv.

[2] Eloge, by Fontenelle.—*Mém. Acad. Par.* 1727. Hist. p. 121.

[3] The English edition was reprinted at London in 1717, 1721, and 1730, and the Latin one at London in 1719, 1721, 1728, at Lausanne in 1740, and at Padua in 1773.

[4] *Biographia Brit.* Art. *Newton*, vol. vii. p. 779.

Rays of Light, he treats in *four* sections :—1. Of the different refrangibility of the rays of light ; 2. Of the measure of refractions ; 3. Of the refractions of plane surfaces ; and 4. Of the refractions of curved surfaces.

In the *second* part, which is entitled, *On the Origin of Colours*, he treats in five sections :—1. On the doctrine of colours, and its proof by experiments with the prism ; 2. On the various phenomena of colours, and on the phenomena of light thrown upon a wall by the prism ; 3. On the phenomena of light received in the eye from a prism ; 4. On the phenomena of light transmitted through a refracting medium terminated by parallel planes ; and, 5. On the phenomena of light transmitted through media terminated spherically, and on the rainbow.[1]

The manuscript from which the Latin edition was printed, was that which had been given by Newton himself to David Gregory, Savilian Professor of Astronomy at Oxford ; but after the edition had been printed, the editor learned that a more perfect manuscript, containing several corrections and emendations in Newton's own handwriting, had been preserved in the archives of the University of Cambridge. These emendations, occupying five quarto pages, were therefore printed at the end of the work, and we observe that Bishop Horsley has introduced them into the text in the third volume of his edition of Newton's works.

[1] An analysis of the *Lectiones Opticæ* has been given by the author of the Life of Newton in the *General Dictionary*, vol. ii. p. 779, note ; but it is by some mistake confined to the *first* Part, as if there were no *second* Part. The same mistake is committed in the *Biographia Britannica*, vol. v. p. 3215, note, where it is obvious that the author knew nothing of the *second* Part, as he calls the last portion of the *first* Part the "Last Section of these Lectures."

CHAPTER XI.

Astronomical Discoveries of Newton—Combined Exertion necessary for the Completion of great Discoveries—Sketch of the History of Astronomy previous to the Time of Newton—Discoveries of Nicolas Copernicus, born 1473, died 1553—He places the Sun in the centre of the System—His Work on the Revolutions of the Heavenly Bodies, printed at the expense of Cardinal Schonberg, and dedicated to Pope Paul III.—Tycho Brahe, born 1546, died 1601—His Observatory of Uraniburg—Is visited by James VI.—Is persecuted by the Danish Minister—Retires to Germany—His Discoveries and Instruments—The Tychonic System—John Kepler, born 1571, died 1631—His Speculation on the Six Regular Solids—Discovers the Ellipticity of Mars' Orbit—His Laws of the Planetary Motions—His Ideas of Gravitation—His Religious Character—Galileo, born 1564, died 1642—The First to apply a Telescope to the Heavens—Discovers the Four Satellites and Belts of Jupiter—His Researches in Mechanics—Is summoned before the Inquisition for Heresy—Retracts his Opinions, but persists in teaching the Doctrine of the Earth's Motion—Is again summoned before the Inquisition—His Sentence to Imprisonment for Life—Becomes Blind—His scientific Character—Labours of Bouillaud, and of Borelli—Suggestion of Dr. Hooke on Gravity—His Circular Pendulum—His Experiments with it—His Views respecting the Cause of the Planetary Motions.

From the optical researches of Newton, brilliant though they be, we turn with fresh wonder to the contemplation of his astronomical discoveries—those transcendent deductions of human reason, by which he has added to the scientific glory of his country, achieved for himself an immortal name, and vindicated the intellectual dignity of his species. Pre-eminent as his triumphs have been, it would be unjust to affirm that they were won by his single arm. The torch of many a preceding age had cast its light into the labyrinths of the material universe, and the grasp of many a powerful hand had thrown down the most impregnable of its barriers. An alliance indeed of many kindred spirits had been long struggling in the combat, and

Newton was but the leader of the mighty phalanx—the director of their combined genius—the general who won the victory, and wears its laurels.

The history of science presents us with no example of an individual mind throwing itself far in advance of its contemporaries. It is only in his career of crime and ambition that reckless man takes the start of his species, and, uncurbed by moral and religious ties, represses the claims of truth and justice, and founds an unholy empire upon the ruins of ancient and venerable institutions. The achievements of intellectual power, though frequently begun by one mind, and completed by another, have ever been the results of united labour. Slow in their growth, they gradually approximate to a more perfect condition: The variety in the objects and phenomena of nature, summons to research a variety of intellectual gifts: Observation collects her materials, and patiently plies her humble avocation: Experiment, with her quick eye and ready hand, develops new facts: The lofty powers of analysis and combination generalize insulated results, and establish physical laws; and in the ordeal of contending schools and rival inquirers, truth is finally purified from error. How different is it with those systems which the imagination rears—those theories of wild import which are directed against the liberties, the consciences, and the hopes of man! The fatal poison tree distils its virus in the spring as well as in the summer and the autumn of its growth; but the fruit which sustains life must have its bud prepared before the approach of winter, its blossom expanded in the spring, and its juice elaborated by the light and heat of a summer and an autumnal sun.

In the century which preceded the birth of Newton, the science of astronomy advanced with the most rapid pace. Emerging from the darkness of the middle ages, the human mind seemed to rejoice in its new-born strength, and to apply itself with elastic vigour to unfold the mechanism of the heavens. Ancient astronomers, indeed, had cleared and paved the way for the onward march of their science. A century and a half before Christ, Hipparchus, in his observatory at Rhodes, made the first catalogue of the stars, and representing the motions of the sun and moon by epicycles revolving upon circular orbits, he compiled tables for calculating their places in the heavens. Guided by the genius of Hipparchus, Claudius Ptolemy, a century and a half after Christ, though he placed the earth in the centre of the system, improved the theories of the sun, moon, and planets—discovered the principal inequality in the moon's orbit—gave a theory of astronomical refractions more complete than that of any astronomer before Cassini, and bequeathed to posterity the valuable legacy of his *Almagest*, and his Five Books of *Optics*.[1]

After centuries of darkness, Bagdad, the capital of Arabia, became the focus of science. The ancient astronomy was preserved and cultivated, but though new and more accurate observations were made, the science lay prostrate amid the cumbrous appendages of cycles and epicycles.

In the thirteenth century, the noble-minded Alphonso x., sovereign of Castile, published, at a great expense, new astronomical tables, computed by the most distinguished professors in the Moorish universities; and, as if he had obtained a glimpse of a simpler arrangement, he denounced the rude mechanism of epicycles in language less reverent in its expression than in its truth. Were the heavens thus constituted, he said, I could have given the deity good advice had he consulted me at their creation. Notwithstanding these obstructions, Astronomy advanced, though with faltering steps, unable to escape from the trammels of authority, and free itself from those vulgar prejudices which a false interpretation of Scripture had excited against a belief in the motion of the earth.

In this almost stationary condition, however, the science of the heavens was not suffered to remain. Nicolas Copernicus arose—a philosopher fitted to develop the true system of the

[1] See Art. OPTICS in *Edin. Encyclopædia*, vol. xv. p. 462.

universe, and a priest willing to give absolution for the sin of placing the great luminary in the centre of the system. This distinguished individual, a native of Thorn in Prussia, though of Bohemian origin, was born on the 19th January 1472. He at first followed his father's profession of medicine, but finding it uncongenial with his love of astronomy, he went to Bologna to study that science under Dominic Mario. In this situation he was less the disciple than the assistant and friend of Mario, and we find that he had made observations on the moon at that place in 1497. About the year 1500, he went to Rome, where he taught mathematics publicly to a large assemblage of youth, and of persons of distinction; and in the month of November of the same year, he observed an eclipse of the moon, and made other observations which formed the basis of his future researches. While thus occupied, the death of one of the Canons of the Cathedral Church of Ermeland, at Frauenburg, enabled his uncle, who was Bishop of that See, to nominate him to the vacant office. In this secluded spot,—in the residence of the Canons, situated on the brow of a hill, Copernicus carried on his astronomical observations. During his sojourn at Rome, the Bishop of Fossombrossa, who presided over the council for reforming the calendar, had requested his assistance in that important undertaking. Upon this congenial task he entered with youthful zeal. He charged himself with the duty of determining the length of the year, and the other elements which were required by the council; but the observations became irksome, and interfered with the completion of those interesting views which had already dawned upon his mind.

Convinced that the simplicity and harmony which appeared in the other works of creation should characterize the arrangements of the planetary system, he could not regard the hypothesis of Ptolemy as a representation of nature. This opinion was strengthened by actual observation. The variable appearance of the superior planets, of Mars, for example, in opposition and conjunction,—in the one case shining with the effulgence of Jupiter, and in the other with the light of a secondary star, was irreconcilable with the dogma that the planet moved round the earth. That it moved round the sun was the conclusion to which he was then led ; and the grand idea of the bright orb of day being the centre of the planetary system burst upon his mind, though perhaps with all the dimness of a dream—the first phase of every great discovery. In the opinions of the Egyptian sages,—in those of Pythagoras, Philolaus, Aristarchus, and Nicetas of Syracuse, he recognised his first conviction that the earth was not the centre of the universe ; and in the works of Martianus Capella, he found it to be the opinion of the Egyptians that Mercury and Venus revolved about the sun during his annual motion round the earth. Thus confirmed in his views, the difficulties which had previously surrounded them were gradually dispelled, and after thirty-six years of intense study, in which the labours of the observer, and the calculations of the mathematician, were combined with the sagacity of the philosopher, he was permitted to develop the true system of the heavens.

In his eye the sun stood immovable in the centre of the universe, while the earth revolved annually round him between the orbits of Venus and Mars, producing by its rotation upon its axis in twenty-four hours all the diurnal phenomena of the celestial sphere—Mercury and Venus moving round the sun within the earth's orbit, and all the rest of the planets without it, while the moon revolved monthly round the earth during its annual motion. In the system thus constituted, all the phenomena of the celestial motions received an immediate explanation. The alternation of day and night—the vicissitudes of the seasons—the varying brightness of the planets—their stations and retrogradations, and even the precession of the Equinoxes, became the necessary results of the Copernican System.

The circulation of these great truths, and of the principles on which they rest, became the leading object of Copernicus's life. The Canon of Ermeland, however, saw the difficulties of his position, and exhibited the most consummate prudence in surmounting them. Aware of the prejudice and even of the hostility with which his discoveries would be received, he resolved neither to startle the one nor provoke the other. He committed his opinions to the slow current of personal communication. The points of opposition which they presented to received doctrine were thus gradually worn down, and they insinuated themselves into ecclesiastical minds by the very reluctance of their author to bring them into notice. In 1536, Cardinal Nicolas Schonberg, Bishop of Capua,[1] and Tidemann Gyse, Bishop of Culm, exerted all their influence to induce Copernicus to lay his system before the world; but their entreaties were in vain, and it was not published till 1539, when an accidental circumstance contributed with other causes to alter his resolution.[2] Having heard of the system of Copernicus, George Rheticus, Professor of Mathematics at Wirtemberg, resigned his chair, and repaired to Frauenburg to make himself master of his discoveries. After studying and adopting them, this zealous disciple prevailed upon Copernicus to permit their publication; and they seemed to have arranged a plan for giving them to the world without alarming the vigilance of the Church. Under the disguise of a student of mathematics, Rheticus published in 1540 an account of the manuscript volume of Copernicus. The pamphlet was received without any expression of censure, and its author was thus encouraged to reprint it at Basle with his own name. The success of these publications, and the flattering manner in which the new astronomy was received, combined with the solicitations and even reproaches of his friends, overcame the scruples of Copernicus, and induced him to place his manuscript in the hands of Rheticus. It was accordingly printed at the expense of Cardinal Schonberg, and was published at Nuremberg in 1543,

[1] The Cardinal's letter is published in the work of Copernicus afterwards mentioned.

[2] These facts are recorded by Copernicus himself in the preface to his work.

under the title of "*On the Revolutions of the Celestial Bodies.*"[1] Its illustrious author, however, did not live to peruse it. A complete copy was handed to him on his dying day, and he saw and touched it a few hours before he expired.[2] In an introductory address "on the hypotheses of his work," Copernicus propitiates such of his readers as may be alarmed at their novelty, by assuring them that it is not necessary that astronomical hypotheses be either true or probable, and that they accomplish their object if they reconcile the calculus with observation.[3] With the same view he inscribed his preface to the Holy Pontiff himself,[4] and boldly alludes to the hostility to which his opinions will expose him. "I have preferred," says he, "dedicating my lucubrations to your Holiness rather than to any other person, because, in the very remote corner of the world in which I live, you are so distinguished by your rank and your love of learning and mathematics, that you will easily repress the virulence of slander, notwithstanding the proverb that there is no remedy against the wound of the sycophant." And "should there be any babblers who, ignorant of all mathematics, presume to judge of these things, on account of some passage of Scripture wrested to their own purpose, and dare to blame and cavil at my work, I will not scruple to hold their judgment in contempt. . . . . Mathematics are written for mathematicians, and I am much mistaken if such men will not regard my labours as conducive to the prosperity of the ecclesiastical republic over which your Holiness presides." Thus recommended to the sovereign authority of the Church, and vindicated against the charge of being hostile to Scripture, the

[1] Nicolai Copernici Torinensis *De Revolutionibus orbium cœlestium,* Lib. vi. Fol. A second edition in folio appeared at Basle in 1566, and a third edition in quarto was published at Amsterdam in 1617, with notes, by Nicolas Muler, under the title of *Astronomia Instaurata,* &c.

[2] Copernicus died in 1543, at the age of 70.

[3] Neque enim necesse est, eas hypotheses esse veras, imo ne verisimiles quidem, sed sufficit hoc unum, si calculum observationibus congruentem exhibeant."—*Ad Lectorem.*

[4] Paul III., a member of the Farnese family, who held the Pontificate from 1534 to 1550. The year in which this preface was written is not known.

Copernican system met with no ecclesiastical opposition, and gradually made its way in spite of the ignorance and prejudices of the age.

Although the true solar system was thus established, yet much remained to be done by the practical astronomer before the motions of the planets could be subjected to mechanical laws. Copernicus had not rejected the machinery of epicycles; and the distances of the planets and the form of their orbits were very imperfectly known. A skilful observer, therefore, expert in mechanism, and girt for nocturnal labour, was now required to prepare for Kepler distances and periods, and for Newton the raw material of his philosophy.

The astronomer thus required appeared in the person of Tycho Brahe, who was born at Knudstrup, in Scania, on the 14th December 1546, three years after the death of Copernicus. When a student at Copenhagen, the great solar eclipse of the 21st August 1560, arrested his attention, and having found that all its phases had been accurately predicted, he resolved to acquire the knowledge of a science so infallible in its results. Though destined for the profession of the law, he refused to enter upon its study; and when urged to it by the entreaties and reproaches of his friends, he escaped from their importunities by travelling into Germany. During his visit to Augsburg, he resided in the house of Peter Hainzell, the burgomaster, whom he inspired with such a love of astronomy, that he erected an excellent observatory at his own expense, and thus enabled his youthful instructor to commence that splendid career of observation which has placed him in the first rank of practical astronomers.

On his return to Copenhagen in 1570, he was welcomed by the King and the nobility as an honour to the nation, and his maternal uncle at Herritzvold, near his native place, offered him a retreat from the gaieties of the capital, and every accommodation for pursuing his astronomical studies. Love and alchemy, however, distracted his thoughts; and he found the peasant girl, whom he fancied, of easier attainment than the philosopher's stone. His noble relatives were deeply offended with the marriage, and it required all the influence of the King to allay the quarrel which it occasioned. In 1572 and 1573, he had observed the remarkable star in Cassiopeia, which rivalled Venus in her greatest brightness, and which, after being the wonder of astronomers for sixteen months, disappeared in March 1574; but he refused, for a long time, to publish his observations upon it, lest he should thus cast a stain upon his nobility!

Fickle in purpose, and discontented with Denmark, Tycho set out in search of a more suitable residence; but when the King heard of his plans, he resolved to detain him by acts of kindness and liberality. He was therefore presented to the canonry of Roschild, with an annual income of 2000 crowns, and an additional pension of 1000; and the island of Huen was offered to him as the site of an observatory, to be furnished with instruments of his own choice. The generous offer was instantly accepted. The celebrated observatory of Uraniburg—the city of the Heavens—was completed at the expense of £20,000, and from its hallowed towers Tycho continued for twenty-one years to enrich astronomy with the most valuable observations. From every kingdom in Europe admiring disciples repaired to this sanctuary of the sciences, to acquire a knowledge of the heavens; and kings and princes felt themselves honoured as the guests of the great astronomer.

Among the princes who visited Uraniburg, we are proud to enumerate James VI. of Scotland. In 1590, during his visit to Denmark to celebrate his marriage with the Princess Anne, he spent eight days with Tycho, accompanied by his counsellors and a large suite of nobility. He studied the construction and use of the astronomical instruments; he inspected the busts and pictures in the Museum, and when he found among them the portrait of his own distinguished preceptor, George Buchanan, he could not refrain from the strongest expressions of delight.

Upon quitting Uraniburg, James not only presented Tycho with a magnificent donation, but afterwards gave him his Royal license to publish his works in England.

The equanimity of Tycho was not disturbed by these marks of respect and admiration; but while they animated his zeal and stimulated his labours, they were destined to be the instruments of his ruin. By the death of Frederick II. in 1588, Tycho lost his most valued friend; and though his son and successor, Christian IV., visited Uraniburg, and seemed to take an interest in astronomy, his wishes to foster it, if he did cherish them, must have been overruled by the influence of his counsellors. The parasites of royalty found themselves eclipsed by the brightness of Tycho's reputation. They envied the munificent provision which Frederick had made for him; and instigated by a physician who was jealous of his reputation, as a successful practitioner of medicine, they succeeded in exciting against Tycho the hostility of the Court. Walchendorp, the President of the Council, was the tool of his enemies, and on the ground of an exhausted treasury, and the inutility of the studies of Tycho, he was deprived of his canonry, his pension, and his Norwegian estate.

Thus stripped of his income, and degraded from his office, Tycho, with his wife and family, sought for shelter in a foreign land. His friend, Count Henry Rantzau, offered him the hospitality of his Castle of Wandesberg, near Hamburg, and having embarked his family and his instruments on board a small vessel, the exiled patriarch left his ungrateful country never to return. In the Castle of Wandesberg he enjoyed the kindness and conversation of his accomplished host, by whom he was introduced to the Emperor Rodolph, who, to a love of science, added a passion for alchemy and astrology. The reputation of Tycho having already reached the Imperial ear, the recommendation of Rantzau was hardly necessary to insure him his warmest friendship. On the invitation of the Emperor, he repaired in 1599 to Prague, where he met with the kindest reception. A pension of three thousand crowns was immediately settled upon him, and a commodious observatory erected for his use. Here he renewed with delight his interrupted labours, and rejoiced in the resting-place which he had so unexpectedly found for his approaching infirmities. These prospects of returning prosperity were enhanced by the pleasure of receiving into his house two such pupils as Kepler and Longomontanus; but the fallacy of human anticipations was here, as in so many other cases, strikingly displayed. His toils and his disappointments had made severe inroads upon his constitution. Though surrounded with affectionate friends and admiring disciples, he was still an exile in a foreign land. Though his country had been base in its ingratitude, it was yet the land which he loved—the scene of his earliest affections—the theatre of his scientific glory. These feelings constantly preyed upon his mind, and his unsettled spirit was ever hovering among his native mountains. In this condition he was attacked with a disease of the most painful kind, and though the paroxysms of its agonies had lengthened intermissions, yet he saw that death was approaching him. He implored his pupils to persevere in their scientific labours. He conversed with Kepler on some of the profoundest questions in astronomy, and with these secular occupations he mingled frequent acts of piety and devotion. In this happy frame of mind he expired without pain on the 24th October 1601, at the age of fifty-five, the unquestionable victim of the councils of Christian IV.

Among the great discoveries of Tycho, his improvements of the lunar theory are perhaps the most important. He discovered the inequality, called the variation, amounting to thirty-seven minutes, and depending on the distance of the moon from the sun. He discovered also the annual inequality of the moon depending on the position of the earth in its orbit, and affecting also the place of her apogee and node. He determined likewise the greatest and the least inclination of the moon's orbit, and he represented this variation by the motion of the pole of the orbit in a small circle. Tycho had the merit, too, of being the first to correct by the refraction of the atmosphere the apparent places of the heavenly bodies; but, what is very unaccountable, he made the refraction which he found to be 34′ in the horizon, to vanish at 45°, and he maintained that the light of the moon and stars was refracted differently by the atmosphere! By his observations on the comet of 1577 he proved that it was three times as distant as the moon, and that since these bodies moved in all directions, the doctrine of solid orbs could not be true. By means of large and accurately divided instruments, some of which were altitude and azimuth ones, having their divided circles six and nine feet in diameter, and others mural quadrants, sextants, and armillary spheres, he made a vast collection of observations, which led Kepler to the discovery of his celebrated laws, and formed the basis of the Rudolphine Tables. But the most laborious of his undertakings was his catalogue of 777 stars, for the epoch of 1600, A.D.[1]—a catalogue afterwards enlarged by Kepler from Tycho's observations, and published in 1627.[2] The skill of Tycho in observing phenomena, surpassed his genius for discovering their cause, and it was perhaps from a mistaken veneration for the Scriptures, rather than from the vanity of giving his name to a new system, that he rejected the Copernican hypothesis. In the system which bears his name, the earth is stationary in the centre of the universe, while the sun, with all the other planets and comets revolving around him, performs his daily revolution about the earth.

Notwithstanding the great accessions which astronomy had received from Copernicus and Tycho, yet no progress had been made in developing the general laws of the Solar System, and scarcely an idea had been formed of the invisible power by which the planets were retained in their orbits. The

[1] *Astronomiæ Instauratæ Progymnasmata.* 1602.

[2] Published at the end of the *Rudolphine Tables.*

materials, however, were prepared, and Kepler arose to lay the foundations of a structure which Newton was destined to complete.

John Kepler was born at the imperial city of Wiel, in Wirtemberg, on the 21st December 1571. Although his early education was neglected, he made considerable progress in his studies at the preparatory school of Maulbronn, and when he took his degree of Master of Arts at the University of Tübingen in 1591, he held the second place at the examination. While he was the mathematical pupil of Mæstlin, he not only adopted his views of the Copernican System, but wrote an essay on the "Primary Motion," as produced by the earth's daily rotation. When the astronomical chair at Gratz, in Styria, fell vacant in 1594, Kepler accepted the appointment, although he knew little of mathematics. His attention, however, was necessarily turned to astronomy, and in 1595, when he enjoyed some professional leisure, he directed the whole energy of his mind to the number, the dimensions, and the motions of the orbits of the planets. After various fruitless attempts to discover some relation between the distances and magnitude of the planets, by assuming the existence of new planets in the wider spaces, he at last conceived the extraordinary idea that the distances of the planets were regulated by the six regular geometrical solids. "The *Earth's* orbit," says he, "is the *sphere*, the measurer of all. Round it describe a *dodecahedron*, the circle including this will be (the orbit of) *Mars*. Round Mars describe a *tetrahedron*, the circle including this will be *Jupiter*. Describe a *cube* round Jupiter, the circle including this will be *Saturn*. Then inscribe in the (orbit of the) Earth an *icosahedron*, the circle described in it will be *Venus*. Describe an *octohedron* round Venus, the circle inscribed in it will be *Mercury*." This singular law, rudely harmonizing with some of Copernicus's measures, would have failed, for want of solids, in its application to *Uranus* and *Neptune;* but it took possession of Kepler's mind, and he declared that he "would not barter the glory of its invention for the whole Electorate of Saxony."[1] When Galileo's opinion of this hypothesis was requested by Kepler, he praised the ingenuity which it displayed; but when a copy of the *Prodromus* was presented to Tycho, he advised his young friend "first to lay a solid foundation for his views by actual observation, and by ascending from these to strive to reach the cause of things;" and there is reason to believe, that by the magic of the whole Baconian philosophy thus compressed by anticipation into a nutshell, Kepler abandoned for a while his visionary speculations.

When driven by religious persecution from the states of Styria, he accepted an invitation from Tycho to settle at Prague as his assistant. Here he was introduced to the Emperor Rodolph, and upon Tycho's death in 1601, he was appointed mathematician to the Emperor, a situation which he held during the successive reigns of Matthias and Ferdinand.

After devoting much of his time to the subjects of refraction and vision, and adding largely to our knowledge of both these branches of Optics,[2] he resumed his inquiries respecting the orbits of the planets. Possessed of the numerous and valuable observations of Tycho, he endeavoured to represent them by the hypothesis of a uniform motion in circular orbits; but in examining the orbit of Mars, he found the deviations from a circle too great to be owing to errors of observation. He therefore compared the observations with various other curves, and was led to the fine discovery that *Mars revolved round the sun in an elliptical orbit in one of the foci of which the sun himself was placed.* By means of the same observations he computed the dimensions of the planet's orbit, and by comparing the times in which Mars passed over different parts of it, he found that they were to one another as the areas de-

[1] These researches were published in his *Prodromus Dissertationum Cosmographicorum*, &c. Tubingæ, 1596, 4to.

[2] Kepler was foiled in his attempt to find out the law of refraction, afterwards discovered by Snellius. His optical discoveries will be found in his *Paralipomena ad Vitellionem*, Francof. 1604; and in his admirable *Dioptrica*, Franc. 1611.

scribed by the lines drawn from the centre of the planet to the centre of the sun, or, in more technical language, *that the radius vector, or line joining the sun and planet, describes equal areas in equal times.* These two brilliant discoveries, the first ever made in physical astronomy, were extended to all the other planets of the system, and were given to the world in his Commentaries on the Motions of the Planet Mars.[1]

Thus successful in his researches, and overjoyed with the result of them, Kepler renewed his attempts to discover the mysterious relation which he believed to exist between the mean distances of the planets from the sun. Distrusting his original hypothesis of the geometrical solids, he compared the planetary distances with the intervals of musical notes, but though he was supported in this notion by the opinions of Pythagoras, and even of Archimedes, his comparisons were fruitless, and he was about to abandon an inquiry which had more or less occupied his mind during seventeen years of his life.

After Kepler had refused to accept the mathematical chair at Bologna, which was offered to him in 1617, he seems to have resumed his speculations "on the exquisite harmonies of the celestial motions." On the 8th March 1618, he conceived the idea of comparing the powers of the different numbers which express the distances of the planets, with the powers of the different numbers which express their periods round the sun. He compared, for example, the squares and the cubes of the distances with the same powers of the periodic times, and he even made the comparison between the squares of the periodic times and the cubes of the distances; but having, in the hurry and impatience of research, been led into an error of calculation, he rejected the last of these relations,—the relation that was true,—as having no existence in nature. Before a week, however, had elapsed, his mind reverted to the law which he had

[1] *Nova Astronomia seu Physica Cœlestis tradita Commentariis de Motibus Stellæ Martis.* Pragæ, 1609, fol.

rejected, and, upon repeating his calculations, and discovering his error, he recognised with rapture the great truth of which he had for seventeen years been in search, *that the periodic times of any two planets in the system are to one another as the cubes of their distances from the sun.* This great discovery was published in 1619 in his "Harmony of the World,"[1] which was dedicated to James VI. of Scotland, and which is marked with all the peculiarities of the author. The passage which describes the feelings under which he recognised the truth of his third law, is too instructive to be omitted from his history:—"What sixteen years ago I urged as a thing to be sought—that for which I joined Tycho Brahe—for which I settled in Prague—for which I have devoted the best part of my life to astronomical contemplations—at length I have brought to light, and have recognised its truth beyond my most sanguine expectations. . . . . It is now eighteen months since I got the first glimpse of light, three months since the dawn; a very few days since the unveiled sun, most admirable to gaze on, burst out upon me . . . . the die is cast—the book is written, to be read either now or by posterity, I care not which. It may well wait a century for a reader, as God has waited six thousand years for an interpreter of his works."[2]

As the planes of the orbits of all the planets, as well as the line of their apsides passed through the sun, Kepler could not fail to suspect that some power resided in that luminary, by which the motions of the planets were produced, and he went so far as to conjecture that this power diminishes as the square of the distance of the body on which it was exerted; but he immediately rejects this law in favour of that of the simple distances. In the Introduction to his Commentaries on Mars, he distinctly recognises the mutual gravitation of matter, in the descent of heavy bodies to the centre of the earth, as the centre of a round body of the same nature with themselves. He

[1] *Harmonia Mundi*, lib. v. Linzii, 1619, fol.
[2] Ibid. p. 178.

maintained, that two stones situated beyond the influence of a third body would approach like two magnets, and meet at a point, each describing a space proportional to the mass of the other. He maintained also, that the tides were occasioned by the moon's attraction, and that the lunar inequalities were owing to the joint action of the sun and earth. Our countryman, Dr. Gilbert, in his celebrated book *De Magnete*, published in 1600, had about the same time announced similar opinions on gravitation. He compares the earth's action upon the moon to that of a great loadstone; and in his posthumous work which appeared half a century afterwards, he maintains that the earth and moon act upon each other like two magnets, the influence of the earth being the greater on account of its superior mass. But though these opinions were a step in celestial physics, yet the identity of the gravity which is exhibited on the earth's surface by falling bodies, with that which guided the planets in their orbits, was not revealed either to the English or the German philosopher. It required more patience and thought than either could command, and its discovery was reserved for the exercise of higher powers.

The misery in which Kepler lived, stands in painful contrast with his arduous labours as an author, and his noble services to science. His small pension was ever in arrears, and when he retired to Silesia to spend the remainder of his days in retirement, his pecuniary difficulties became more embarrassing than before. He was compelled to apply personally for his arrears; and, in consequence of the great fatigue which he suffered in his long journey to Ratisbon on horseback, he was seized with a fever which carried him off on the 30th November 1630, in the fifty-ninth year of his age. Thus perished one of the noblest of his race, a victim of poverty, and a martyr to science.

In a work which is to record the religious character of Newton, it would be unjust to withhold from Kepler the credit which is due to his piety and faith. The harmony of the universe, which he strove to expound, excited in him not only admiration, but love. He felt his own humility the farther he penetrated into the mysteries of the universe, and sensible of the incompetency of his unaided powers for such transcendent researches, and recognising himself as but the instrument of the Almighty in making known his wonders, he never entered upon an inquiry without praying for assistance from above. Nor was this frame of mind inconsistent with the tumultuous delight with which he surveyed his discoveries. His was the unpretending ovation of success, not the ostentatious triumph of ambition; and if a noble pride occasionally mingled with his feelings, it was the pride of being the chosen messenger of physical truth, not the vanity of being the favoured possessor of superior genius. With such a frame of mind, Kepler was necessarily a Christian. The afflictions with which he was tried confirmed his faith and brightened his hopes. He bore them in all their variety and severity with Christian patience; and though he knew that this world was to be the theatre of his glory, yet he felt that his rest and his reward could be found only in another.

It is a remarkable fact in the history of astronomy, that three of its most distinguished cultivators were contemporaries. Galileo was the contemporary of Tycho during thirty-seven years, and of Kepler during the fifty-nine years of his life. Galileo was born seven years before Kepler, and survived him nearly the same time. We have not learned that the intellectual triumvirate of the age enjoyed any opportunity for mutual congratulation. What a privilege would it have been to have contrasted the aristocratic dignity of Tycho with the reckless ease of Kepler, and the manly and impetuous mien of the Italian sage!

While his two predecessors were laying deeply and surely the foundations of physical astronomy, Galileo was preparing himself for extending widely the limits of the Solar system, and exploring the structure of the bodies that compose it. He was born at Pisa on the 15th February 1564, and was descended from the noble family of Bonajuti. Although he exhibited an early passion for geometry, and had studied without a master the writings of Euclid and Archimedes, yet even after he was called to the mathematical chair at Pisa in the twenty-fifth year of his age, he was more distinguished for his hostility to the Aristotelian philosophy than for his progress in original inquiry. In 1592 he was promoted to the same chair in Padua, where he remained for eighteen years, adorning the university by his talents, and diffusing around him a taste for science. With the exception of some minor contrivances, Galileo had made no discovery till he entered his forty-fifth year, *an age at which Newton had completed all his discoveries.* In 1609, the memorable year in which Kepler published his "New Astronomy," Galileo paid that visit to Venice during which he heard of the telescope of Lippershey.[1] The idea of so extraordinary an instrument at once filled his mind, and when he learned from Paris that it had an existence, he resolved instantly to realize it. The simple idea, indeed, was the invention, and Galileo's knowledge of optics was sufficient to satisfy him that a convex lens at one end of a tube, with a concave one at the other, would bring objects nearer to his eye. The lenses were placed in the tube, the astronomer looked into the concave lens, and saw the objects before it "pretty large and pretty near him." This little toy, which magnified only *three* times lineally, and *nine* times superficially, he carried in triumph to Venice, where the chief magistrate obtained it in barter for the life possession of his professorship, and 480 florins as an increase of salary. The excitement produced on this occasion at Venice was of the most extraordinary kind; and, on a subsequent occasion, when Sirturi[2] had made one of the instruments, the populace followed him with eager curiosity, and at last took possession of the tube, till they had each

[1] Professor Moll, *Journal of Royal Institution*, 1831, vol. i. p. 496.

[2] Sirturus, *De Telescopio*. Francofurtæ, 1618.

witnessed its wondrous effects. Galileo lost no time in availing himself of his new power. He made another telescope which magnified about eight or nine times, and, sparing neither labour nor expense, he finally constructed an instrument so excellent, as "to show things almost a thousand times larger (in surface), and above thirty times nearer to the eye."

There is, perhaps, no invention in science so extraordinary in its nature, and so boundless in its influence, as that of the telescope. To the uneducated man the power of bringing distant objects near to the eye must seem almost miraculous; and to the philosopher even who comprehends the principles upon which it acts, it must ever appear one of the most elegant applications of science. To have been the first astronomer in whose hands such a power was placed, was a preference to which Galileo owed much of his reputation.

Before the telescope was directed to the heavens, it was impossible to distinguish a planet from a star. Even with his first instrument, Galileo saw that Jupiter had a round appearance like the sun and moon; but, on the 7th January 1610, when he used a telescope of superior power, he saw three little bright stars very near him, *two* to the *right*, and *one* to the *left* of his disc. Though ranged in a line parallel to the ecliptic, he regarded them as ordinary stars; but having, on the 8th of January, accidentally[1] directed his telescope to Jupiter, he was surprised to see the three stars to the west of the planet, and nearer one another than before,—a proof that they had a motion of their own. This fact did not excite his notice; and it was only after observing various changes in their relative position, and discovering a fourth on the 13th of January, that he was enabled to announce the discovery of the four satellites of Jupiter.[2]

In continuing his observations with the telescope, Galileo

[1] "Nescio quo fato ductus."—*Sidereus Nuncius*, p. 20.

[2] The satellites were observed by our celebrated countryman, Harriot, on the 17th October 1610.—See *Martyrs of Science*, Life of Galileo, pp. 40, 41.

discovered that Venus had the same crescent phases as the waxing and the waning moon ;—that the sun had spots on his surface which proved that he revolved round his axis ;—that Saturn was not round, but had handles attached to his disc ;—that the surface of the moon was covered with mountains and valleys, and that parts of the margin of her disc occasionally appeared and disappeared ;—that the milky way consisted of numerous stars, which the unassisted eye was unable to perceive ; and that the apparent size of the stars arose from irradiation, or a spurious light, in consequence of which they were not magnified by the telescope. These various discoveries furnished new arguments in support of the hypothesis of Copernicus ; and we may now consider it as established by incontrovertible evidence, which ignorance or fanaticism only could resist, that the sun is placed in the centre of the System, in the focus of the elliptical, or in the centre of the nearly circular, orbits of the planets, and that by some power, yet to be discovered, he guides them in their course, while the Earth and Jupiter exercise a similar influence over the satellites which accompany them.

But it is not merely from his astronomical discoveries, brilliant as they are, that Galileo claims a high place in the history of Newton's discoveries. His profound researches on mechanical science—his determination of the law of acceleration in falling bodies—and his researches respecting the resistance and cohesion of solid bodies, the motion of projectiles, and the centre of gravity of solids, have ranked him among the most distinguished of our mechanical philosophers. The great step, however, which he made in mechanics, was his discovery of the general laws of motion uniformly accelerated, which may be regarded as the basis of the theory of universal gravitation.[1]

The current of Galileo's life had hitherto flowed in a smooth and undisturbed channel. His discoveries had placed him at

[1] See *Edinburgh Encyclopædia*, Art. MECHANICS, vol. xiii. p. 502, where we have given a copious abstract of the mechanical discoveries of Galileo.

the head of the great men of the age, and with an income above his wants, he possessed both the means and the leisure for prosecuting his studies. Anxious, however, to propagate the great truths which he discovered, and by force of reason to make proselytes of his enemies, he involved himself in disputes which tried his temper and disturbed his peace. When argument failed to convince his opponents, he wielded against them the powerful weapons of ridicule and sarcasm, and he thus marshalled against himself and his opinions the Aristotelian professors, the temporizing Jesuits, the political churchmen, and that timid section of the community who tremble at innovation, whether it be in religion or in science. The party of Galileo who abetted him in his crusade against error, though weak in numbers, were strong in position and in zeal. His numerous pupils occupying the principal chairs in the Italian universities, formed a devoted band who cherished his doctrines and idolized his genius. The enemies of religion followed the intellectual banner, and many princes and nobles, who had smarted under ecclesiastical jurisdiction, were willing to see it shorn of its power.

While these two parties were standing on the defensive, Galileo hoisted the first signal for war. In a letter to his friend and pupil, the Abbé Castelli, he proved that the Scriptures were not intended to teach us science and philosophy, and that the expressions in the Bible were as irreconcilable with the Ptolemaic as with the Copernican system. In reply to this letter, Caccini, a Dominican friar, attacked Galileo from the pulpit, and so violent was his language, that Maraffi, the general of the Dominicans, expressed his regret that he should be implicated "in the brutal conduct of thirty or forty thousand monks." Encouraged by this apology, Galileo launched another pamphlet, addressed to the Grand Duchess of Tuscany, in which he supports his views by quotations from the Fathers, and by the conduct of the Roman Pontiff himself, Paul III., in accepting the dedication of Copernicus's work. It was in vain to meet such arguments by any other weapon than that of the civil power. It was deemed necessary either to crush the heresy, or retire from the contest ; and the Church party determined to appeal to the Inquisition.

Various circumstances concurred to excite the suspicions of Galileo, and, about the end of 1615, he set off for Rome, where he was lodged in the palace of the Tuscan ambassador. While Galileo was enjoying the hospitality of his friend, Caccini was preparing the evidence of his heresy, and in due time he was charged by the Inquisition with maintaining the motion of the earth and the stability of the sun,—with teaching and publishing this heretical doctrine, and with attempting to reconcile it to Scripture. On the 25th February 1615, the Inquisition assembled to take these charges into consideration, and having no doubt of their truth, they desired that Galileo should be enjoined by Cardinal Bellarmine to renounce the obnoxious doctrines, and to pledge himself that he would neither teach, publish, nor defend them in future. In the event of his refusing to obey this injunction, it was decreed that he should be thrown into prison. Galileo acquiesced in the sentence, and on the following day he renounced before the Cardinal his heretical opinions, abandoning the doctrine of the earth's motion, and pledging himself neither to defend nor teach it either in his writings or his conversation.

Although Galileo had made a narrow escape from the grasp of the Inquisition, he left Rome in 1616 with a suppressed hostility against the Church ; and his resolution to propagate the heresy seems to have been coeval with the vow by which he renounced it. Although he affected to bow to the decisions of theology, he never scrupled, either in his writings or in his conversation, to denounce them with the severest invective. The Lyncean Academy, ever hostile to the Church, encouraged him in this unwise procedure, and it was doubtless at their instigation that he took the daring step which brought him a second time to the bar of the Inquisition. Forgetting the pledges under which he lay,—the personal kindness of the Pope,—and the pecuniary obligations which he owed him, he resolved to compose a work in which the Copernican system should be indirectly demonstrated. This work, entitled, *The System of the World of Galileo Galilei*, &c., was completed in 1630, but was not published till 1632, owing to the difficulty of obtaining a license to print it. It was dedicated to the Grand Duke of Tuscany; and while the decree of the Inquisition was referred to in insulting and ironical language, the Ptolemaic system, the doctrine of the Church, was assailed by arguments which admitted of no reply. The Copernican doctrines, thus eloquently maintained, were eagerly received and widely disseminated, and the Church of Rome felt the shock thus given to its intellectual supremacy. Pope Urban VIII., though attached to Galileo, and friendly to science, was driven into a position from which he could not recede. The guardian of its faith, he mounted the ramparts of the Church to defend the weakest of its bastions, and, with the artillery of the Inquisition, he silenced the batteries of its assailants. The Pope brought the obnoxious work under the eye of the Inquisition, and Galileo, advanced in years, and infirm in health, was summoned before its stern tribunal. He arrived in Rome on the 14th of February 1663, and soon after his arrival he was kindly visited by Cardinal Barberino, the Pope's nephew, and other friends of the Church, who, though they felt the necessity of its interference, were yet anxious that it should be done with the least injury to Galileo and to science.

Early in April, when his examination in person took place, he was provided with apartments in the house of the Fiscal of the Inquisition; and to make this nominal confinement as agreeable as possible, his table was provided by the Tuscan ambassador, and his servant was allowed to sleep in an adjoining apartment. Even with these indulgences, however, Galileo could not brook the degradation under which he lay. A return of his complaint ruffled his temper, and made him impatient for his release ; and the Cardinal Barberino having been made acquainted with his feelings, liberated the philosopher on his own responsibility, and on the 30th of April, after ten days' confinement, restored him to the hospitable roof of the Tuscan ambassador.

It has been stated on authority which is considered unquestionable, that during his personal examination Galileo was put to the torture, and that confessions were thus extorted which he had been unwilling to make. He acknowledged that the obnoxious dialogues were written by himself ;—that he had obtained a license to print them without informing the functionary who gave it—and that he had been prohibited from publishing such opinions ; and in order to excuse himself, he alleged that he had forgotten the injunction under which he lay not to teach, in any manner, the Copernican doctrines. After duly considering the confessions and excuses of their prisoner, the Inquisition appointed the 22d of June as the day on which their sentence was to be pronounced. In obedience to the summons, Galileo repaired to the Holy Office on the morning of the 21st. Clothed in a penitential dress, he was conducted, on the 22d, to the convent of Minerva, where the Inquisition was assembled, and where an elaborate sentence was pronounced, which will ever be memorable in the history of science. Invoking the name of our Saviour and of the Holy Virgin, Galileo is declared to be a heretic, in consequence of believing that the sun was the centre of the earth's orbit, and did not move from east to west, and defending the opinion that the earth moved and was not the centre of the world. He is therefore charged with having incurred all the censures and penalties enacted against such offences ; but from all these he is to be absolved, provided that with a sincere heart, and faith unfeigned, he abjures and curses the heresies he has maintained, as well as every other heresy against the Catholic Church. In order to prevent the recurrence of such crimes, it was also decreed that his work should be prohibited by a formal edict,—

that he should be imprisoned during the pleasure of the Inquisition,—and that during the next three years he should recite weekly the seven penitential psalms. This sentence was subscribed by seven cardinals, and on the same day Galileo signed the abjuration which the sentence imposed.

Clothed in the sackcloth of a repentant criminal, Galileo, at the age of seventy, fell upon his knees before the assembled cardinals, and laying his right hand on the Holy Evangelists, he invoked the Divine assistance, in abjuring and detesting and vowing never again to teach the doctrine of the earth's motion and of the sun's stability. He pledged himself never again to propagate such heresies either in his conversation or in his writings, and he vowed that he would observe all the penances which had been inflicted upon him. What a mortifying picture does this scene present to us of moral infirmity and intellectual weakness! If we brand with infamy the unholy zeal of the inquisitorial conclave, what must we think when we behold the venerable sage, whose grey hairs were entwined with the chaplet of immortality, quailing under the fear of man, and sacrificing the convictions of his conscience, and the deductions of his reason, at the altar of a base superstition? Had Galileo added the courage of the martyr to the wisdom of the sage,—had he carried the glance of his eye round the circle of his judges, and with uplifted hands called upon the living God to witness the truth and immutability of his opinions, he might have disarmed the bigotry of his enemies, and science would have achieved a memorable triumph.

The sentence of abjuration was publicly read at several universities. At Florence it was promulgated in the Church of Santa Croce, and the friends and disciples of Galileo were summoned to the ceremonial, in order to witness the degradation of their master. But though the Church was thus anxious to maintain its authority, Galileo was personally treated with consideration, and even kindness. After remaining only four days in the dungeons of the Inquisition, he was, at the request of the

Tuscan ambassador, allowed to reside with him in his palace, and when his health began to suffer, he was permitted to leave Rome and to reside with his friend Piccolomini, Archbishop of Sienna, under whose hospitable roof he completed his investigations respecting the resistance of solids. At the end of six months he was allowed to return to Florence, and before the close of the year he re-entered his house at Arcetri, where he spent the remainder of his days.

Although still a prisoner, Galileo had the happiness of being with his family and living under his own roof; but like the other "spots of azure in his cloudy sky," it was ordained to be of short duration. It was now that he was justly characterized by the poet as "the starry Galileo with his woes." His favourite daughter Maria, who, along with her sister, had joined the convent of St. Matthew, near Arcetri, hastened to the filial duties which she had so long been prevented from discharging. She assumed the task of reciting weekly the seven penitential psalms which formed part of her father's sentence; but she had scarcely commenced her domestic toils when she was seized with a dangerous illness, which in a few weeks proved fatal. Galileo was laid prostrate by this heavy and unexpected blow. He was inconsolable for the loss of his daughter, and disease in various forms shook the frail tenement which philosophy had abandoned. Time, however, the only anodyne of sorrow, produced its usual effects, and Galileo felt himself able to travel to Florence for medical advice. The Pope refused him permission, and he remained at Arcetri from 1634 to 1638, preparing for the press his "Dialogues on Motion," and corresponding with the Dutch government on his proposal to find the longitude by the eclipses of Jupiter's satellites. Galileo, whose eyes had been gradually failing him since 1636, was struck with total blindness in 1638. "The noblest eye," as his friend Father Castelli expressed it, "was darkened—an eye so privileged and gifted with such rare powers, that it may truly be said to have seen more than the eyes of all that are gone, and have opened the eyes of all that were to come." To the want of sight was soon added the want of hearing, and in consequence of the mental labour to which he had been subjected, "his head," as he himself said, "became too busy for his body;" and hypochondriacal attacks, want of sleep, acute rheumatism, and palpitation of the heart, broke down his constitution. His last illness, after two months' continuance, terminated fatally on the 8th January 1642, when he was in the 78th year of his age.

"The scientific character of Galileo," as we have elsewhere[1] had occasion to remark, "and his method of investigating truth, demand our warmest admiration. The number and ingenuity of his inventions, the brilliant discoveries which he made in the heavens, and the depth and beauty of his researches respecting the laws of motion, have gained him the applause of every succeeding age, and have placed him next to Newton in the lists of original and inventive genius. To this high rank he was doubtless elevated by the inductive processes which he followed in all his inquiries. Under the sure guidance of observation and experiment, he advanced to general laws; and if Bacon had never lived, the student of nature would have found in the writings and labours of Galileo not only the boasted principles of the Inductive philosophy, but also their practical application to the highest efforts of invention and discovery."

Among the astronomers who preceded Newton in astronomical inquiries, and contributed some ideas to the establishment of the true system of the planets, we must place the names of Bouilland,[2] Borelli, Hooke, Huygens, Wren, and Halley. After refuting the magnetic notions of Kepler, Bouillaud maintained that the force of attraction must vary reciprocally as the square, and not, as Kepler asserted, in the simple ratio of the distance;

[1] *Life of Galileo*, chap. vi. in the *Martyrs of Science.*

[2] Ismaelis Bullialdi *Astronomia Philolaica.*—Paris, 1645, p. 23. Sir Isaac admitted that Bullialdus here gives the true "proportion on gravity."—*Letter to Halley*, June 20, 1686, postscript.

but Delambre does not allow him any credit in this respect, and remarks that he has done nothing more for astronomy than to introduce the word evection into its language.

The influence of gravity as a central force in the planetary motions has been very distinctly described by Borelli, Professor of Mathematics at Pisa, in his work on the theory of Jupiter's Satellites.[1] He considers the motions of the planets round the sun, and of the satellites round their primaries, as produced by some virtue residing in the central body. In speaking of the motion of bodies in circular orbits, he compares the tendency of the body to recede from the centre of motion to that of a stone whirled in a sling. When this force of recession is equal to the tendency of the body to the centre, a balance is effected between these tendencies, and the body will continually revolve round the centre, and at a determinate distance from it. Delambre attaches no value to these speculations of Borelli. He has in his opinion pointed out no physical cause,[2] and has merely made a series of reflections which every astronomer would necessarily make who was studying the theory of the satellites. He gives him the credit, however, of being one of the first who conjectured that the comets described round the sun elliptical or parabolic orbits.[3]

The speculations of our distinguished countryman, Dr. Hooke, respecting the cause of the planetary motions, exceeded greatly

[1] *Theoricæ Medicæorum Planetarum ex causis physicis deductæ.* A Alphonso Borellio.—Florentiæ, 1666.

[2] Newton (in his posthumous work, *De Systemate Mundi*, § 2, *Opera*, tom. iii. p. 180, and in his postscript in his letter to Halley, June 26, 1688, where he says "that Borelli did something") and Huygens have attached greater value to the views of Borelli. The last of these philosophers thus speaks of them :—"Refert Plutarchus in libro supramemorato de Facie in Orbe Lunæ, fuisse jam olim qui putaret ideo manere lunam in orbe suo, quod vis recedendi a terra, ob motum circularem, inhiberetur pari vi gravitatis, qua ad terram accedere conaretur. Idemque ævo nostro, non de luna tantum sed et planetis ceteris statuit Alphonsus Borellius, ut nempe primariis eorum gravitas esset solem versus; lunis vero ad Terram Jovem et Saturnum quos comitantur. Multoque diligentius, subtiliusque idem nuper explicuit Isaacus Newtonus, et quomodo ex his causis nascantur Planetarum orbes Elliptici, quos Keplerus excogitaverat; in quorum foco altero Sol ponitur. Christiani Hugenii *Cosmotheoros*, lib. ii. *ad finem*. OPERA, tom. ii. p. 720.

[3] Angelo Fabroni, *Lettere inedite d'uomini illustri*, tom. i. p. 173.

in originality and value the crude views of Borelli, and form a decided step in physical astronomy. On the 21st of March 1666, he communicated to the Royal Society an account of a series of experiments to determine if bodies experienced any change in their weight at different distances from the surface of the earth "either upwards or downwards." Kepler had maintained that this force, namely, that of gravity, was a property inherent in all celestial bodies, and Hooke proposed "to consider whether this gravitating or attractive power be inherent in the parts of the earth ; and, if so, whether it be magnetical, electrical, or of some other nature distinct from either." The experiments which he made with the instrument described in this communication, were far from being satisfactory, and he was therefore led to the ingenious idea of measuring the force of gravity "by the motion of a swing clock," which would go slower at the top of a hill than at the bottom.[1]

About two months afterwards, namely, on the 22d May 1666, Hooke communicated to the Society a paper "On the inflexion of a direct motion into a curve by a supervening attractive principle."[2] After maintaining that the celestial bodies moving in circular and elliptical orbits "must have some other cause beside the first impressed impulse to bend their motion into these curves," he considers the only two causes which appear to him capable of producing such an effect. The *first* of these causes, which he considers an improbable one, is that the tendency to a centre is produced by a greater density of the ether in approaching to the sun. "But the *second* cause," he adds, "of inflecting a direct motion into a curve may be from an attractive property of the body placed in the centre, whereby it continually endeavours to attract or draw it to itself. For if such a principle be supposed, all the phenomena of the planets seem possible to be explained by the common principle of mechanic motions ; and possibly the prosecuting this speculation, may give us a true hypothesis of their motion, and from some few observations their

[1] Birch's *Hist. of Royal Society*, vol. ii. pp. 69-72. [2] *Ibid.* vol. ii. pp. 90-92.

motions may be so far brought to a certainty that we may be able to calculate them to the greatest exactness and certainty that can be desired." After describing the circular pendulum[1] for illustrating these views, he adds that "by this hypothesis the phenomena of the comets, as well as of the planets, may be solved; and the motions of the secondary as well as of the primary planets. The motions also of the progression of the apsides are very evident, but as for the motion of libration or latitude that cannot be so well made out by this way of pendulum; but by the motion of a wheel upon a point is most easy."

By means of the circular pendulum already mentioned, it was found that "if the impetus of the *endeavour by the tangent* at the first setting out was stronger *than the endeavour to the centre*, there was then generated an elliptical motion whose *longest* diameter was parallel to the direct endeavour of the body in the first point of impulse. But if that impetus was weaker than the endeavour to the centre, there was generated such an elliptical motion whose *shorter* diameter was parallel to the direct endeavour of the body in the first point of impulse." Another experiment was made by fastening a small pendulous body by a shorter string on the lower part of the wire which suspended the larger ball, "that it might freely make a circular or elliptical motion round about the bigger, whilst the bigger moved circularly or elliptically about another centre." The object of this arrangement was to explain the manner of the moon's motion about the earth; but neither of the balls moved in such perfect circles and ellipses as when they were suspended singly. "A certain point, however, which seemed to be the centre of gravity of the two bodies, however pointed (considered as one), seemed to be regularly moved in such a circle or ellipsis, the two balls having other peculiar motions in small epicycles about the same point."[2]

[1] This pendulum consisted of a wire fastened to the roof of the room, with a large wooden ball of *lignum vitæ* at the end of it.—Waller's *Life of Hooke*, p. xii.

[2] Waller's *Life of Hooke*, p. xii.; and Birch's *Hist.*, vol. ii. p. 92.

At a later period of his life, Hooke resumed the consideration of the subject of the planetary motions, and, in a work which appeared in 1674,[1] he published some interesting observations on gravity, which we shall give in his own words.—"I shall hereafter," he says, "explain a system of the world differing in many particulars from any yet known, but answering in all things to the common rules of mechanical motions. This depends upon three suppositions: *First*, That all celestial bodies whatsoever have an attraction or gravitating power towards their own centres, whereby they attract not only their own parts, and keep them from flying from them, as we may observe the Earth to do, but that they also do attract all the other celestial bodies that are within the sphere of their activity, and consequently that not only the Sun and Moon have an influence upon the body and motion of the Earth, and the Earth upon them, but that Mercury, Venus, Mars, Jupiter, and Saturn also, by their attractive powers, have a considerable influence upon its motion, as in the same manner the corresponding attractive power of the Earth hath a considerable influence upon every one of their motions also. The *second* supposition is this, that all bodies whatsoever that are put into a direct and simple motion, will so continue to move forward in a straight line till they are, by some other effectual powers, deflected, and sent into a motion describing a circle, ellipsis, or some other more compound curve line. The *third* supposition is, that these attractive powers are so much the more powerful in operating by how much the nearer the body wrought upon is to their own centres. Now, what these several degrees are, I have not yet experimentally verified, but it is a motion which, if fully prosecuted, as it ought to be, will mightily assist the astronomers to reduce all the celestial motions to a certain rule, which I doubt will never be done without it. He that understands the nature of the circular pendulum, and of circular

[1] *An Attempt to prove the Motion of the Earth, from Observations made by* Robert Hooke, 4to. See *Phil. Trans.* No. 101, p. 12.

motion, will easily understand the whole of this principle, and will know where to find directions in nature for the true stating thereof. This I only hint at present to such as have ability and opportunity of prosecuting this inquiry, and are not wanting of industry for observing and calculating, wishing heartily such may be found, having myself many other things in hand which I would first complete, and therefore cannot so well attend it. But this I durst promise the undertaker, that he will find all the great motions of the world to be influenced by this principle, and that the true understanding thereof will be the true perfection of astronomy."[1]

In this remarkable passage, the doctrine of universal gravitation, and the general law of the planetary motions, are clearly laid down. The diminution of gravity as the square of the distance, is alone wanting to complete the basis of the Newtonian philosophy; but even this desideratum was in the course of a few years supplied by Dr. Hooke. In a letter which he addressed to Newton in 1679, relative to the curve described by a projectile influenced by the Earth's daily motion, he asserted, that if the force of gravity decreased as the square of the distance, the curve described by a projectile would be an ellipse, whose focus was the centre of the earth. But however great be the merit which we may assign to Hooke's experimental results and sagacious views, they cannot be regarded either as anticipating the discoveries of Newton, or diminishing his fame. Newton had made the same discoveries by independent researches, and there is no reason to believe that he derived any ideas from his contemporaries.

[1] In quoting this passage, which Delambre admits to be very curious, we think he scarcely does justice to Hooke, when he says that what it contains is found expressly in Kepler. It is quite true that Kepler mentioned as probable the law of the squares of the distances, but he afterwards, as Delambre admits, rejected it for that of the simple distances. Hooke, on the contrary, announces it as a truth.—See *Astronomie du 18me Siècle*, pp. 9, 10. Clairaut has justly remarked, that the example of Hooke and Kepler shows how great is the difference between a truth conjectured or asserted, and a truth demonstrated.

CHAPTER XII.

The First Idea of Gravity occurs to Newton in 1665—His first Speculations upon it—He abandons the subject from having employed an erroneous measure of the Earth's Radius—He resumes the subject in consequence of a discussion with Dr. Hooke, but lays it aside, being occupied with his Optical Experiments—By adopting Picard's Measure of the Earth he discovers the Law of Gravity, and the cause of the Planetary Motions—Dr. Halley goes to Cambridge, and urges him to publish his Treatise on Motion—The germ of the Principia which was composed in 1685 and 1686—Correspondence with Flamsteed—Manuscript of Principia sent to the Royal Society—Halley undertakes to publish it at his own expense—Dispute with Hooke, who claims the discovery of the Law of Gravity—The Principia published in 1687—The New Edition of it by Cotes begun in 1709, and published in 1713—Character and Contents of the Work—General account of the discoveries it contains—They meet with opposition from the followers of Descartes—Their reception in Foreign Countries—Progress of the Newtonian Philosophy in England and Scotland.

SUCH is a brief and general view of the labours and lives of those illustrious men who prepared the science of Astronomy for the application of Newton's genius. Copernicus had determined the form of the Solar System, and the relative position and movements of the bodies that composed it. Kepler had proved that the planets revolve in elliptical orbits ; that their *radii vectores* describe areas proportional to the times ; and that the squares of their periodic times are as the cubes of their distances from the sun. Galileo had added to the universe a whole system of secondary planets. Huygens had given to Saturn a satellite, and the strange appendage of a ring ; and while some astronomers had maintained the doctrine of universal gravitation, others had referred the motions to an attractive force, diminishing with the square of the distance, and producing a curvilineal motion from one in a straight line.[1]

We have already seen that, in the autumn of 1665, Newton was led to the opinion that the same power by which an apple falls from a tree extends to the moon, and retains her in her orbit; but upon making the calculation, he found such a discrepancy between the two forces that he abandoned the subject, suspecting that the power which retained the moon in her orbit might be partly that of gravity, and partly that of the vortices of Descartes.[2] This discrepancy arose from the adoption of an erroneous measure of the semi-diameter of the earth, of which the moon's distance was taken as a multiple. Unacquainted with the more accurate determinations of Snellius[3] and Norwood,[4] the last of which would have given Newton the exact quantity which he required, he adopted the measure of sixty miles for a degree of latitude, which had been employed by the old geographers and seamen, and in which, as Mr. Rigaud conjectures, he may have placed the more confidence, as it agreed with the result of the observations which Edward Wright, a Cambridge mathematician, had published in 1610.

It does not distinctly appear at what time Newton became acquainted with the more accurate measurement of the earth, executed by Picard in 1670, and was thus led to resume his investigations. Picard's method of measuring his degree, and the precise result which he obtained, were communicated to the Royal Society on the 11th January 1672,[5] and the result of his

[1] In 1673, Huygens had announced the relations between attractive force and velocity in circular motion.

[2] Whiston's *Memoirs of his own Life*, p. 37.

[3] *Eratosthenes Batavus*, 1617.

[4] *Seaman's Practice*, 1636.

[5] Mr. Rigaud remarks, that "we do not know when Norwood's determination became known to Newton, but we are certain that he was well aware of Snellius's measures quite as soon as he was of Picard's,—probably much sooner, since the specific mention of them is made in Varenius's *Geography* (cap. iv. pp. 24-26, 1672), of which he edited a new edition at Cambridge in 1672."—*Historical Essay*, p. 12. "Had he adopted," as Mr. Rigaud adds, "28,500 Rhinland perches, the length of a degree given by Snellius, he would have obtained for the moon's deflexion, in a minute, 15·5 feet."

observations and calculations were published in the Philosophical Transactions for 1675. But whatever was the time when Newton became acquainted with Picard's measurement, it seems to be quite certain that he did not "resume his former thoughts concerning the moon" till 1684. Pemberton tells us, that "some years after he laid aside" his former thoughts, "a letter from Dr. Hooke put him on inquiring what was the real figure in which a body, let fall from an high place, descends, taking the motion of the earth round its axis into consideration;" and that this gave occasion to his resuming his former thoughts concerning the moon, and determining from Picard's recent measures, that "the moon appeared to be kept in her orbit purely by the power of gravity."[1] But though Hooke's letter of 1679 was the occasion of Newton's resuming his in-

[1] Among the manuscripts of Conduitt, I found the following statement regarding Newton's "resuming his former thoughts concerning the moon:"—

"In 1673, Dr. Hooke wrote to him to send him something new for the *Transactions*, whereupon he sent him a little dissertation to confute the common objection that if it were true that the earth moved from east to west, all falling bodies would be left to the west; and maintained that, on the contrary, they would fall a little eastward, *and having described a curve with his hand to represent the motion of a falling body, he drew a negligent stroke with his pen, from whence Dr. Hooke took occasion to imagine that he meant the curve would be a spiral*, whereupon the Doctor wrote to him that the curve would be an ellipsis, and that the body would move according to Kepler's notion, which gave Sir Isaac Newton an occasion to examine the thing thoroughly; and for the foundation of the calculus he intended, he laid down this proposition, that the areas described in equal times were equal, which, though assumed by Kepler, was not by him demonstrated, of which demonstration the first glory is due to Newton."

Immediately after this statement, Conduitt adds: "Pemberton, in his preface, mentions this in another manner," and he quotes part of that preface.

The above extraordinary story of Hooke's having considered a negligent stroke of Newton's pen as a spiral, and on that ground having charged him with maintaining that falling bodies would describe such a curve, could not have been given on Newton's authority, but must have been invented by an enemy of Hooke's. Newton himself admits, in his letter to Halley, July 27, 1686, that Hooke's "correcting his spiral occasioned his finding the theorem by which he afterwards examined the ellipsis."

In the preceding extract, the date 1673 is obviously erroneous. The document was copied for me by the late Henry Arthur Wallop Fellowes, the elder brother of the Earl of Portsmouth, who kindly assisted me in the examination of Newton's papers, and who placed at the top of the document the words (P. 49 in Jones), which I cannot explain.

quiries, it does not fix the time when he employed the measures of Picard. In a letter from Newton to Halley in 1686, he tells him that Hooke's letters in 1679 were the cause of his "finding the method of determining the figures, which, when I had tried in the ellipsis, I threw the calculations by, being upon other studies; and so it rested for about five years, till, upon your request, I sought for the papers." Hence Mr. Rigaud considers it clear, that the figures here alluded to were the paths of bodies acted upon by a central force, and that the same occasion induced him to resume his former thoughts concerning the moon, and to avail himself of Picard's measures to correct his calculations. It was, therefore, in 1684, that Newton discovered that the moon's deflexion in a minute was sixteen feet, the space through which a body falls in a second at the surface of the earth. As his calculations drew to a close, he is said to have been so much agitated that he was obliged to desire a friend to finish them.[1]

Sir Christopher Wren and Hooke and Halley had each of them, from independent considerations, concluded that "the centripetal force decreased in the proportion of the squares of the distances reciprocally."[2] Halley had in 1683-4 derived this law "from the consideration of the sesquialterate proportion of Kepler," but was unsuccessful in his attempts to demonstrate by it the laws of the celestial motions. Sir Christopher Wren had, "very many years" before 1686, attempted by the same law "to make out the planet's motion by a descent towards the sun, and an impressed motion," but had "given it over, not finding the means of doing it;" and Dr. Hooke, as we have already seen, though he adopted the law of the squares, never fulfilled his promise of proving that it could be applied to the motions of the planets.[3] It is therefore to Newton alone

[1] Robison's *Works*, vol. ii. p. 94, 1822. Tradition is, we believe, the only authority for this anecdote. It is not supported by what is known of Newton's character.

[2] *Principia*, lib. i. Prop. iv. Schol.

[3] These various facts are stated in a letter from Halley to Newton, dated June 29.

that we owe the demonstration of the great truth, that the moon is kept in her orbit by the same power by which bodies fall on the earth's surface.

The influence of such a result upon such a mind, may be more easily conceived than described. If the force of the earth's gravity bends the moon into her orbit, the satellites of the other planets must be guided by the same power in their primaries, and the attractive force of the sun must in like manner control the movements of the comets and the planets which surround him. In the application of this grand truth to the motions of the Solar System, and to the perturbations arising from the mutual action of the bodies that compose it, Newton must have rejoiced in the privilege of laying the foundation of so magnificent a work, while he could not fail to see that the completion of it would be the achievement of other minds, and the glory of another age. But, however fascinating must have been the picture thus presented to his mind, it was still one of limited extent. He knew not of the existence of binary and multiple systems of stars, to which the theory of

1686. "According to your desire in your former, I waited upon Sir Christopher Wren, to inquire of him if he had the first notion of the reciprocal duplicate proportion from Mr. Hooke. His answer was, that he himself very many years since had had his thoughts upon making out the planet's motions by a composition of a descent towards the sun and an impressed motion; but that at length he gave over, not finding the means of doing it. Since which time Mr. Hooke had frequently told him that he had done it, and attempted to make it out to him, but that he never was satisfied that his demonstrations were cogent. And this I know to be true, that in January 1683-4, I, having from the consideration of the sesquialterate proportion of Kepler, concluded that the centripetal force decreased in the proportion of the squares of the distances reciprocally, came on Wednesday to town (from Islington) where I met with Sir Christopher Wren and Mr. Hooke, and falling in discourse about it, Mr. Hooke affirmed that upon that principle all the laws of the celestial motions were to be demonstrated, and that he himself had done it. I declared the ill success of my attempts, and Sir Christopher, to encourage the inquiry, said that he would give Mr. Hooke some two months' time to bring him a convincing demonstration thereof, and besides the honour he of us that did it should have from him the present of a book of forty shillings. Mr. Hooke then said he had it, but that he would conceal it for some time, that others trying and failing might know how to value it when he should make it public. However, I remember that Sir Christopher was little satisfied that he could do it, and though Mr. Hooke then promised to show it him. I do not find that in that particular he has been so good as his word."

universal gravitation would be extended. He could not have anticipated that Adams and Leverrier would have tracked an unseen planet to its place by the perturbations it occasioned: Nor could he have conjectured that his own theory of gravitation might detect the origin and history of nearly thirty planetary bodies, revolving within a sphere apparently destined for one. It was enough for one man to see what Newton saw. The service in the Temple of Science must be performed by many priests; and fortunate is he who is called to the humblest task at its altar. The revelations of infinite wisdom are not vouchsafed to man in a day. A light so effulgent would paralyse the noblest intellect. It must break in upon it by degrees; and even each separate ray must be submitted to the ordeal of various minds,—to the apprentice skill of one age, and to the master genius of another.

It is not easy to determine the exact time when Newton first adopted the great truth, "that the forces of the planets from the sun are reciprocally duplicate of their distances from him," but there is sufficient evidence to show that it must have been as early as 1666, and therefore contemporaneous with his speculations on Gravity in his garden at Woolsthorpe. "In one of my papers," says he,[1] "writ (I cannot say in what year), but I am sure some time before I had any correspondence with Oldenburg,[2] and that's *above fifteen years ago* (1671), the proportion of the forces of the planets from the sun, reciprocally duplicate of their distances from him, is expressed, and the proportion of our gravity to the moon's *conatus recedendi a centro terræ*, is calculated, though not accurately enough. That when Hugenius put out his Horologium Oscillatorium, a copy being presented to me, in my letter of thanks to him I gave those rules in the end thereof a particular commendation for their usefulness in philosophy, and added, *out of my aforesaid* paper,

[1] Letter to Halley, June 20, 1686. See also Rigaud's *Hist. Essay*, pp. 51, 52.

[2] It appears from Birch, in his *Hist. of the Royal Society*, vol. iii. p. 1, that Newton had written to Oldenburg a letter, dated January 6, 1673.

an instance of their usefulness in comparing the forces of the moon from the earth, and the earth from the sun; in determining a problem about the moon's phase, and putting a limit to the sun's parallax, which shows that I had then my eye upon comparing the forces of the planets arising from their circular motion, and understood it; so that a while after when Mr. Hooke propounded the problem solemnly in the end of his attempt to prove the motion of the earth, if I had not known the duplicate proportion before, I could not but have found it now." In another letter to Halley, written about three weeks afterwards,[1] he distinctly states, that "for the duplicate proportion I can affirm that I gathered it from Kepler's theorem *about twenty* years ago," that is, in 1666. Hence it is obvious that the written paper referred to by Newton was, as Mr. Rigaud says, "the result of his early speculations at Woolsthorpe," and that "the deduction from Kepler, which is said to have preceded the calculation[2] by a twelvemonth, took place in 1665."

Such was the state of Newton's knowledge regarding the law of gravity, when, in January 1684, Halley, Wren, and Hooke were discussing together the subject in London. Halley had learned from this interview that neither of his friends possessed a "convincing demonstration" of this law, and finding, after a delay of some months, that Hooke "had not been so good as his word," in showing his demonstration to Wren, he set out for Cambridge in the month of August 1684, to consult Newton on the subject.[3] Without mentioning either his own specula-

[1] July 14, 1686. Rigaud's *Hist. Ess.* App. pp. 39, 40.

[2] The erroneous calculations from his having used an incorrect measure of the earth's diameter.

[3] In both the editions of the *Commercium Epistolicum*, drawn up by a committee of Newton's best friends, there occurs the following passage, which has misled several of Newton's biographers. "Anno . . . 1683, in . . . Actis Lipsicis pro mense Octobri, calculi differentialis elementa primum edidit D. Leibnitius, literis A. G. L. designatus. Anno autem 1683 ad finem vergente, D. Newtonus propositiones principales, earum quæ in Philosophiæ Principiis Mathematicis habentur Londinum misit," &c., No. LXXI. It is certain that 1684 should have been substituted for 1683. Mr. Rigaud, who justly

tions, or those of Hooke and Wren, he at once indicated the object of his visit by asking Newton what would be the curve described by the planets on the supposition that gravity diminished at the square of the distance. Newton immediately answered, *an Ellipse.* Struck with joy and amazement, Halley asked him how he knew it? Why, replied he, I have calculated it; and being asked for the calculation, he could not find it, but promised to send it to him. After Halley left Cambridge, Newton endeavoured to reproduce the calculation, but did not succeed in obtaining the same result. Upon examining carefully his diagram and calculation, he found that in describing an ellipse coarsely with his own hand, he had drawn the two axes of the curve instead of two conjugate diameters somewhat inclined to one another. When this mistake was corrected he obtained the result which he had announced to Halley.[1]

remarks that this could not have been an error of the press, as "the argument with reference to Leibnitz would fall to the ground if 1684 were substituted for it," has endeavoured successfully to find out the cause of the mistake. In the Macclesfield Collection he found *two* Memoranda on the first communication of the Principia to the Royal Society, said to be "from an original paper of Newton," which we presume means in Newton's handwriting. In the first the date of 1683 is given, and in the second the correct date of 1684, "the 3 having been evidently altered to 4," by Newton himself, so that the editors of the *Commercium Epistolicum* made a grave mistake in adopting the date 1683.

Since the publication of Mr. Rigaud's Historical Essay, Mr. Edleston has thrown a new light on this subject. The two Memoranda mentioned by Mr. Rigaud are the commencement of a critique by Newton himself on three papers by Leibnitz in the Leipsic Acts for January and February 1689. The critique, which Mr. Edleston thinks was probably written in 1712, occupied nearly six pages, and is preserved among the Lucasian Papers. The first sentence is given in *four* different forms. In the two first the date 1684 is used, and in the two last 1683. "Newton," says Mr. Edleston, "first of all clearly wrote 1684, then altered the 4 to a 3, afterwards crossed all the figures out, *and wrote distinctly* 1683. . . . . Newton, therefore, after endeavouring to recollect the exact year in which he sent up the fundamental proposition of the *Principia* to London, antedated the event by a twelvemonth," so that no blame can be cast upon the editors of the *Commercium Epistolicum*, for the erroneous date which they adopted. The critique is given by Mr. Edleston in his APPENDIX, p. 307. See Rigaud's *Hist. Essay*, pp. 16-18, and his APPENDIX, No. xix.

[1] We have given this account of Halley's interview with Newton, nearly as we find it in Conduit's manuscript, in which *May* is erroneously mentioned as the time of Halley's visit. Halley's own account is more brief:—"The August following when I did myself

Halley returned to London with the double satisfaction that a grand truth had been demonstrated which he himself had anticipated, and that he had the honour of bringing it to light. He was indeed proud of the success of his mission, and after the Principia had excited the admiration of Europe, he used frequently to boast to Conduit that he had been the Ulysses who produced this Achilles.[1] In the month of November, Newton fulfilled the promise he made to Halley, by sending him through Mr. Paget[2] a copy of the demonstration which he had brought to perfection ; and very soon after receiving it, Halley took another journey to Cambridge, " to confer with Newton about it." Immediately after his return to London, namely, on the 10th December, he informed the Royal Society " that he had lately seen Mr. Newton at Cambridge, who had showed him a curious treatise *De Motu*," which at Dr. Halley's desire he promised to send to the Society to be entered upon their register. " Mr. Halley was desired to put Mr. Newton in mind of his promise for the securing this invention to himself, till such time as he could be at leisure to publish it," and Mr. Paget was desired to join with Mr. Halley.

the honour to visit you, I then learned the good news that you had brought the demonstration to perfection, and you were pleased to promise me a copy thereof, which I received with a great deal of satisfaction from Mr. Paget."—*Letter to Newton*, June 29, 1686.

1 " Dr. Halley has often valued himself to me," says Conduitt, " for being the Ulysses which produced this Achilles."

2 Mr. Paget was Mathematical Master in Christ's Hospital. He was a friend of Newton's, and was recommended by him to Flamsteed on the 3d April 1682, as a competitor for the Mastership. Flamsteed joined in the recommendation, and after his appointment found him " an able mathematician." He gave such satisfaction to the Governors, indeed, that they sent Flamsteed " a staff," and made him one of their number. Flamsteed has left it on record that this accomplished young man, before seven years had expired, became a drunkard, neglected his duties, lost his character, and banished himself to India. What a lesson to the young who are accidentally associated with great men after whom posterity inquires! As the bearer of the germ of the Principia to Halley, Paget's name has for nearly two centuries been mentioned with honour. As a protégé of Newton and Flamsteed, who failed in justifying their recommendation, a blot has been left upon his name, which but for that honour would never have been known. See Baily's *Flamsteed*, p. 125.

That Halley and Paget would, without delay, remind Newton of his promise, and that Newton would fulfil it, there can be no doubt ; and we accordingly find that about the middle of February he had sent to Mr. Aston, one of the Secretaries of the Royal Society, his "notions about motion." Mr. Aston, as a matter of course, would thank Newton for the communication, and mention the fact of its being registered ; and that all this was done, appears from a letter of Newton's to Aston of the 23d February 1685, written on another subject, but thanking him for "having entered on the register his notions about motion." Newton added, "I designed them for you before now, but the examining several things has taken a greater part of my time than I expected, and a great deal of it to no purpose. And now I am to go into Lincolnshire for a month or six weeks. Afterwards I intend to finish it as soon as I can conveniently."

The treatise *De Motu*, thus registered in the books of the Royal Society, was the germ of the Principia, and was obviously intended to be a brief exposition of the system which that work was to establish. It occupies twenty-four octavo pages, and consists of four theorems and seven problems, four of the theorems and four of the problems containing the more important truths which are demonstrated in the second and third sections of the First Book of the Principia.[1]

[1] Mr. Rigaud has published it in his Historical Essay. He is of opinion that it is not the same paper, a copy of which was brought to Halley by Mr. Paget in November 1684, on the ground that that paper was never mentioned to the Royal Society by Halley, and that Halley did not see the "curious treatise *De Motu* till his second visit to Cambridge, in November or December 1684." Mr. Edleston, however, is of opinion that the treatise De Motu was part of the lectures delivered by Newton as Lucasian Professor, which commenced in October 1684, and a copy of which is preserved in the University library ; and that the paper sent to Halley in November was the germ of this treatise, and the one registered by Mr. Aston. In a letter from Cotes to Jones, published in Edleston's *Correspondence*, p. 209, it is stated that the manuscript at Cambridge was "the first draught of the Principia," as Newton read it in his lectures,—a statement to which Mr. Edleston refers in support of his opinion. There are certainly expressions in the letters both of Newton and Halley unfavourable to both these opinions, but we think that the following view of the question is the most probable. Halley went to Cambridge to learn

The years 1685 and 1686 will ever be memorable in the life of Newton, and in the history of science. It was in these two years, and in the early months of 1687, that he composed the Principia and gave it to the world, and all the details connected with this great event have been carefully preserved for the instruction and gratification of posterity. The personal history of the philosopher, therefore, during this period, the nature of his correspondence and inquiries, and all the mechanical and even commercial circumstances under which his great work was written, and printed and published, are subjects which cannot be overlooked in any extended account of his life and writings. Although Newton had identified the law of gravity on the earth with the same law at the moon, yet he required the aid of the practical astronomer in enabling him to apply his theory to the motions of the planets and comets of the system. Fortunately for Newton, Flamsteed was the Astronomer-Royal at Greenwich.

In November and December 1680, when the great comet appeared, Flamsteed observed it with peculiar care, and, before it had ceased to be visible, he put all its observed places into

if Newton had a demonstration of a proposition that a force varying reciprocally with the square of the distance would produce a motion in an ellipsis. Newton told him that he "had brought this demonstration to perfection," but that having mislaid it, he would send him "a copy thereof." This copy was sent to Halley in November obviously for his own information. Halley does not lay it before the Society, but is so pleased with *it*, that he goes again to Cambridge in order to "confer with Newton about *it*." He now saw the treatise De Motu which Newton promised to send to the Society, and which was registered. Now when Halley says (letter to Newton, June 29. 1686) that he went to Newton to confer with him about *it*, that is, the demonstration, and adds immediately, "since which time *it* has been entered upon the register books of the Society," he can only mean that the demonstration was entered *as part* of the treatise De Motu, of which it was certainly the leading feature. If the two *its* mean the same thing, then Halley received in November the same treatise that was afterwards sent to Aston in the following February, which is scarcely admissible even upon Mr. Edleston's conjecture that Halley did produce the paper on the 10th December, though the fact is not recorded in the journal book. In Newton's letter to Halley, July 14, 1686, he says, that having tried the calculation *in the Ellipsis*, he had thrown them by for about five years, till upon Dr. Halley's request "he sought for that paper (namely, the calculation in the Ellipsis), and not finding it, did *it* again, and reduced it into the propositions (we read proposition) showed you by Mr. Paget."—See Rigaud's *Hist. Essay*, p. 14, and Edleston's *Correspondence*, pp. lv. and 209.

a little table, which, with his thoughts on the subject of comets, he communicated to Mr. Crompton, Fellow of Jesus College, Cambridge. In this letter, Flamsteed asserted that "the *two* comets (as they were generally thought) were only one and the same; and he described the line of their motions before and after it passed the sun." Mr. Crompton showed this letter to Newton, who, in return, addressed a long letter to him, to be sent to Flamsteed, containing observations on Flamsteed's "hypothetical notions," and endeavouring to prove "that the comets of November and December were different comets. The commencement of Newton's letter is very characteristic, and though it is intended to be kind in its expressions, we can conceive a mind like that of Flamsteed regarding it, as he did many years afterwards, as "magisterially ridiculing the opinion for which he thought the arguments convincing and unanswerable."[1] "I thank Mr. Flamsteed," says Newton, "for this kind mention of me in his letters to Mr. Crompton, and, as I commend his wisdom in deferring to publish his hypothetical notions till they have been well considered both by his friends and himself, so I shall act the part of a friend in this paper, not objecting against it by way of opposition, but in describing what I imagine might be objected by others, and so leaving it to his consideration. If hereafter he shall please to publish his theory, and think any of the objections I propound need an answer, to prevent their being objected by others, he may describe the objections as raised by himself or his friends in general, without taking any notice of me." After this kind introduction, Newton proceeds, in a long and elaborate letter, to controvert Flamsteed's opinions, and, from the evidence of several Cambridge scholars, to show that there were *two* comets, and not *one*; and also in opposition to Flamsteed, that "more comets go northward than southward." Flamsteed replied to this letter on the 7th March 1681, in such complimentary terms, that he could not have taken any offence at

[1] See Baily's *Flamsteed*, p. 50, *note*.

Newton's remarks upon his views.[1] He seems to have answered several of Newton's objections, and removed some of his difficulties, but to have failed in satisfying him that there was only one comet in 1680. Newton had been on a visit in the country during almost the whole of March, and, after his return to Cambridge, was prevented, "by some indisposition and other impediments," from replying to Flamsteed till the 16th of April. In this letter "he forbears to urge further" any objections to Flamsteed's hypothesis, and confines himself "to the question of two comets," which he discusses at great length, pertinaciously maintaining an opinion, which, a few years afterwards, he was obliged to abandon.[2]

When, after his return from Lincolnshire to Cambridge, Newton was occupied with the composition of the Principia, he renewed his correspondence with Flamsteed. Some of their

[1] Mr. Baily, whose views respecting the quarrel which subsequently arose between Newton and Flamsteed, we shall afterwards have occasion to controvert, acknowledges that he cannot find in these two letters of Newton "any fonndation for Flamsteed's censure." It is very obvious, indeed, from the highly complimentary terms in which Flamsteed at this time wrote to Newton, that he did not consider Newton as "magisterially ridiculing his opinions."

[2] At this time, and even in 1684, when he wrote his treatise *De Motu*, Newton had very erroneous views regarding the motions of comets; and it was not till September 19, 1685, that he acknowledged, in a letter to Flamsteed, that "it seemed very probable that the comets of November and December were the same comet." In the first edition of the *Principia*, p. 494, he went farther, and acknowledged that Flamsteed was right. In giving an account of the treatise *De Motu*, Mr. Rigaud thus speaks of Newton's views respecting the motions of comets:—"He certainly at this time had not resolved the difficult question of the paths of comets. In the *Arithmetica Universalis* (Prob. 56), he had proceeded on their supposed uniform rectilinear motion, and, in the present case, he still holds expressly to that earlier theory. How, under such conditions (if strictly adhered to), they could return, is not easy to understand; but waiving this question, his reasoning seems to show that if they did, they might be recognised by a similarity in their motions. To determine this, he proposes to reduce the places of the comet to analogous points in an imaginary ellipse, of which the focus is occupied by the sun; and these places having been calculated by means of the auxiliary curve, were to be verified by their application to the rectilinear path. It seems wonderful, when we consider his extraordinary acuteness, that such an hypothesis did not immediately lead him to the truth; but as he so repeatedly and so distinctly describes the supposed motion of the comet to be in a straight line, it is impossible not to conclude, that even his most powerful mind required the assistance of time to emancipate itself from preconceived opinions."—Rigaud's *Hist. Essay*, p. 29.

letters are lost;[1] but it is obvious, from one of Newton's, dated September 19, 1685, that he had received many useful communications from Flamsteed, and especially regarding Saturn, "whose orbit, as defined by Kepler," Newton "found too little for the sesquialterate proportions." In the other letters written in 1685 and 1686, he applies to Flamsteed for information respecting the orbits of the satellites of Jupiter and Saturn;—respecting the rise and fall of the spring and neap tides at the solstices and the equinoxes;—respecting the flattening of Jupiter at the poles, which, if certain, he says, would conduce much to the stating the reasons of the procession of the equinoxes;—and respecting the differences between the observed places of Saturn and those computed from Kepler's tables about the time of his conjunction with Jupiter. On this last point the information supplied by Flamsteed was peculiarly gratifying to Newton; and it is obvious from the language of this part of his letter, that he had still doubts of the universal application of the sesquialteral proportion. "Your information," says he, "about the errors of Kepler's tables for Jupiter and Saturn, has eased me of several scruples. I was apt to suspect there might be some cause or other unknown to me which might disturb the sesquialteral proportions, for the influences of the planets one upon another seemed not great enough, though I imagined Jupiter's influence greater than your numbers determine it. It would add to my satisfaction if you would be pleased to let me know the long diameters of the orbits of Jupiter and Saturn, assigned by yourself and Mr. Halley in your new tables, *that I may see how the sesquialteral proportion fills the heavens, together with another small proportion which must be allowed for.*"[2]

[1] The dates of these letters, which are published in the *General Dictionary*, vol. vii. pp. 793-797, are September 19, 1685; September 25, 1685; October 14, 1685; December 30, 1685 (?); January (?) 1686; September 3, 1686. Excepting the second, which is from Flamsteed, they are all from Newton. See Vol. II. Chap XVIII.

[2] This letter has no date, but Flamsteed says that it was written about 1685, or January 1685-86.

Upon Newton's return from Lincolnshire in the beginning of April 1685, he seems to have devoted himself to the preparation of his work, and to fulfil his intention, as expressed to Aston, "of finishing it as soon as he conveniently could." In the spring he had determined the attractions of masses, and thus completed the demonstration of the law of universal gravitation; and in summer he had finished the *Second* Book of the Principia,[1] the *First* Book being the Treatise *De Motu*, which he had already enlarged and completed. Excepting in the correspondence with Flamsteed, to which we have already referred, we hear nothing more of the preparation of the Principia till the 21st of April 1686, when Halley read to the Royal Society his "Discourse concerning Gravity, and its Properties," in which he states "that his worthy countryman, Mr. Isaac Newton, has an incomparable *Treatise of Motion almost ready* for the press;" and that the reciprocal law of the squares "is the principle on which Mr. Newton has made out all the phenomena of the celestial motions so easily and naturally, that its truth is past dispute."[2] The intelligence thus given by Halley was speedily confirmed. At the very next meeting of the Society on the 28th of April, "Dr. Vincent presented to the Society a manuscript treatise entitled *Philosophiæ Naturalis Principia Mathematica*, and dedicated to the Society by Mr. Isaac Newton." Although this manuscript contained only the First Book, yet such was the confidence which the Society placed in its author, that an order was given "that a letter of thanks be written to Mr. Newton; that the printing of his book be referred to the consideration of the Council; and that in the meantime the book be put into the hands of Mr. Halley, to make a report thereof to the Council." Although there could be no doubt of the meaning of this report, yet no progress was made in the publication of the work. At the next meeting of the Society on the 19th May, some dissatisfaction seems

[1] Edleston's *Correspondence*, &c., p. xxix.; and Newton's letter to Halley, June 20, 1686.
[2] *Phil. Trans.* 1686, pp. 6-8.

to have been expressed at the delay, as it was ordered "that Mr. Newton's work should be printed *forthwith* in quarto, and that a letter should be written to him to signify the Society's resolutions, and to desire his opinion as to the print, volume, cuts, and so forth." Three days afterwards, namely, on the 22d of May, Halley communicated the resolution to Newton, and stated to him that the printing was to be at the charge of the Society. As the manuscript, however, had not been referred to the consideration of the Council, as previously ordered, and as no sum exceeding five pounds could be paid without its authority, a farther delay took place. At the next meeting of the Council on the 2d of June, it was again ordered "that Mr. Newton's book be printed ;" but instead of sanctioning the resolution of the general meeting to print it at their charge, they added, "that Mr. Halley undertake the business of looking after it, and printing it at his own charge, which he engaged to do."

In order to explain to Newton the cause of the delay, Halley, in his letter of the 22d May, alleges that it arose from "the President's attendance upon the King, and the absence of the Vice-Presidents, whom the good weather had drawn out of town ;" but there is reason to believe that this was not the real cause, and that the delay arose from the unwillingness of the Council to undertake the publication in the present state of their finances.[1]

Such was the emergency in which Halley undertook the labour of editing, and the expense of printing, the *Principia*, and thus earned the gratitude of Newton and of posterity. We cannot admit that the low state of their funds was any apology for the conduct of the Council in refusing to carry into effect the resolution of a general meeting of the Society. Why did

[1] We here express the opinion of Mr. Rigaud, who, after a careful and repeated examination of the Royal Society's minutes, from 1686 to 1699, "ventures to say," that "there is no notice of any pecuniary aid having been extended to the *Principia*." Halley was a married man with a family, and at "a considerable pecuniary risk provided for the disbursement, precisely at that period of his life when he could least afford it."—Rigaud's *Hist. Essay*, pp. 33-37.

they not borrow the necessary sum on the security of their future income, or subscribe individually to fulfil an honourable obligation, and discharge an important duty? If the nobility and gentry who then composed the Royal Society devolved upon their secretary the payment of expenses which, as a body, they had agreed to defray, let it not be said that it was to the Royal Society that we are indebted for the publication of the *Principia.* It is to Halley alone that science owes this debt of gratitude: It was he who tracked Newton to his College, who drew from him his great discoveries, and who generously gave them to the world.

In Halley's letter of the 22d May, announcing to Newton the resolution of the Society, he found it necessary to inform him of the conduct of Hooke when the manuscript of the Principia was presented to the Society. Sir John Hoskyns, the particular friend of Hooke, was in the chair, when Dr. Vincent presented the manuscript, and passed a high encomium on the novelty and dignity of the subject. Another member remarked that Newton had carried the thing so far that there was no more to be added, to which Sir John replied, that it was so much the more to be prized, as it was both invented and perfected at the same time. Mr. Hooke was offended because Sir John did not mention what he had told him of his own discovery; and the consequence was, as Halley says, "that these two, who, till then, were inseparable cronies, have since scarce seen one another, and are utterly fallen out." After the meeting broke up and adjourned to the coffee-house, Mr. Hooke endeavoured to persuade the members "that he had some such thing by him, and that he gave Newton the first hint of this invention." Although this scene passed at the Royal Society, Halley only communicated to Newton the fact, "that Hooke had some pretensions to the invention of the rule for the decrease of gravity being reciprocally as the squares of the distances from the centre," acknowledging at the same time, that though Newton had the notion from him, "yet the demonstration of the curves generated thereby belonged wholly to Newton." "How much of this," Halley adds, "is so, you know best, as likewise what you have to do in this matter; only Mr. Hooke seems to expect you should make some mention of him in the preface which 'tis possible you may see reason to prefix. I must beg your pardon that 'tis I that send you this ungrateful account; but I thought it my duty to let you know it, that so you might act accordingly, being in myself fully satisfied that nothing but the greatest candour imaginable is to be expected from a person who has of all men the least need to borrow reputation." In thus appealing to Newton's candour, Halley obviously wished that some acknowledgment of Hooke should be made. He knew indeed, that before Newton had announced the inverse law, Hooke and Wren and himself had spoken of it and discussed it, and therefore justice demanded that though none of them had given a demonstration of the law, Hooke especially should receive credit for having maintained it as a truth of which he was seeking the demonstration.

Newton's reply to Halley,[1] written after a month's delay, is a remarkable production. He acknowledges that Hooke told him of the duplicate proportion, but that his views were erroneous, as he conceived it "to reach down from hence to the centre of the earth." He confesses "that he himself *had never extended the duplicate proportion lower than to the superficies of the earth*," and that "*before a certain demonstration he found last year* (1685) *he suspected it did not reach accurately enough down so low.*" In the rest of the letter he shows very satisfactorily, from letters to Oldenburg and Huygens, and even from this theory of gravity, that he must have been acquainted with the duplicate proportion before his conversation with Hooke.

When Newton had finished this letter, he was informed "by one who had it from another lately present at one of the

[1] June 20, 1686. APPENDIX, No. IX.

Society's meetings, that Mr. Hooke had there made a great stir, pretending that Newton had all from him, and desiring they would see that he had justice done him." Roused by what he considered " a very strange and undeserved carriage towards him," he writes an angry postscript to his letter, putting forward the claims of Borelli and Bullialdus to the duplicate proportion, and ungenerously charging Hooke with having derived his knowledge of it from them, and even with having been led to it by perusing his own letter to Huygens, which might have come into his possession after the death of Oldenburg. " My letter," says he, " to Huygens was directed to Mr. Oldenburg, who used to keep the originals. His papers came into Mr. Hooke's possession. Mr. Hooke, knowing my hand, might have the curiosity to look into that letter, and thence take the notion of comparing the forces of the planets from their circular motion; and so what he wrote to me afterwards about the rate of gravity might be nothing but the fruit of my own garden. And it's more than I can affirm, that the duplicate proportion was not expressed in that letter."[1] This reasoning is certainly far from being sound. If Hooke had the law of gravity from Borelli and Bullialdus, Newton might have had it from them also; and if Hooke obtained it by the process indicated in the letter to Huygens, which he probably never saw, it follows that Hooke's views were as sound as those expressed in that letter, and that he then knew as much about the law as Newton did. But there is no evidence whatever that Hooke saw the letter.

Halley was much annoyed with the contents of this postscript, and lost no time in replying to it. He gives Newton an account of the interview between Hooke, Wren, and himself, previously described, and which led him to go to Cambridge. He tells him that Hooke's manner of claiming the discovery has been represented in worse colours than it ought, " for he

[1] It was not expressed in the letter, as Newton afterwards admits. See APPENDIX, No. X. Letter, July 27, 1686.

neither made application to the Society for justice, nor pretended you had all from him;" and he gives "the truth," by telling what really happened at the meeting of the Society, and of the little quarrel between Hooke and his friend Sir John Hoskyns. Halley concludes his letter by begging Newton "not to let his resentments run so high" as to deprive the world of his Third Book, on the theory of comets.

Though ruffled for a moment, Newton's excellent temper soon recovered its serenity. When he understood from Halley that Hooke had been in some respects misrepresented to him, he "wished that he had spared the postscript in his last;" and he goes on to acknowledge that Hooke's "letters occasioned his finding the method of determining figures which he tried in the ellipsis;"—that Hooke told him of the experiment with "Halley's pendulum clock at St. Helena, as an argument that gravity was lessened at the equator by the diurnal motion;"—and that he also told him a third thing which was new to him, and which he would acknowledge if he made use of it, namely, "the deflexion of fallen bodies to the south-east in our latitude." Having thus sincerely told Mr. Halley the case between him and Mr. Hooke, "he considered how best to compose the present dispute," which he thought might be done by the enclosed scholium to the fourth proposition. "The inverse law of gravity holds in all the celestial motions, as was discovered also independently by my countrymen, Wren, Hooke, and Halley."

On the 30th June, the President was desired by the Council to license Mr. Newton's book, entitled *Philosophiæ Naturalis Principia Mathematica*, and after Halley had obtained the author's leave about the middle of July to substitute wooden cuts for copperplates, the printing of it was commenced and went on with considerable regularity. The Second Book, though ready for the press in autumn, was not sent till March 1687. The Third Book was presented to the Society on the 6th of April, and the whole work published about midsummer of 1687.[1] It was dedicated to the Royal Society as flourishing under his august Majesty James VII.,[2] and there was prefixed to it a set of beautiful Latin hexameters, addressed by Halley to its immortal author.[3] They began thus—

En tibi norma poli, et divæ libramina molis,
Computus atque Jovis; quas, dum primordia rerum
Pangeret, omniparens leges violare creator
Noluit, æternique operis fundamina fixit,

and ended with the following lines—

Talia monstrantem mecum celebrate camœnis,
Vos qui cœlesti gaudetis nectare vesci,
NEWTONUM clausi reserantem scrinia veri;
NEWTONUM Musis charum, cui pectore puro
Phœbus adest, totoque incessit numine mentem,
Nec fas est propius mortali attingere divos.

This great work, as might have been expected, excited a warm interest in every part of Europe. The impression was

[1] The manuscript of the Principia, without the preface, bound in one volume, is in the possession of the Royal Society. Mr. Edleston is of opinion that the manuscript is not in Newton's autograph, and he believes it to be of the same hand as the first draught of the Principia in the University library, the author's own handwriting being easily recognised in the additions and alterations in both manuscripts. Edleston's *Correspondence*, &c., pp. lvii. lviii. In a very interesting letter from Dr. Humphrey Newton to Conduitt, which is printed in our second volume, he informs him, that "he copied out the Principia before it went to press." Pemberton states that the Principia was written in a year and a half. In reference to this point I found the following memorandum in Sir Isaac's handwriting:—

"In the tenth proposition of the second book, there was a mistake in the first edition, by drawing the tangent of the arch GH from the wrong end of the arch, which caused an error in the conclusion; but in the second edition I rectified the mistake. And there may have been some other mistakes occasioned by the shortness of the time in which the book was written, and by its being copied by an amanuensis who understood not what he copied, besides the press faults; for I wrote it in seventeen or eighteen months, beginning in the end of December 1684, and sending it to the Royal Society in May 1686, excepting that about ten or twelve of the propositions were composed before, viz., the 1st and 11th in December 1679, the 6th, 7th, 8th, 9th, 10th, 12th, 13th, and 17th, Lib. I., and the 1st, 2d, 3d, and 4th, Lib. II., in June and July 1684."

[2] A copy of the Principia was presented to the King by Halley, accompanied with a paper giving a general account of the Book, and more especially an explanation of the tides, a subject in which the King was likely to take a deep interest, from his having as Lord High Admiral commanded the British fleet in the war with the United Provinces. See *Phil. Trans.* vol. xix. p. 445, and Rigaud's *Hist. Essay*, APP. p. 77.

[3] See APPENDIX, No. XI.

quickly sold.[1] A copy of the Principia could scarcely be procured in 1691, and at that time an improved edition was in contemplation. Newton himself, though pressed by his friends, had refused to undertake it, and M. Facio D'Huillier, who had studied it with the most minute attention, had intimated to Huygens his design of publishing a new edition.[2] In 1694, Newton resumed the study of the lunar and planetary theories, with the view of rendering more perfect a new edition of his book; but the difficulty which he experienced in getting the necessary observations from the Astronomer-Royal, interfered with his investigations, and contributed more than any other cause to prevent him from bringing them to a close. Flamsteed did not sufficiently appreciate the importance of Newton's labours; but while we deeply regret that he should have treated so ungraciously the importunities of his friend, we are disposed to find some apology for his conduct in the infirmities of his health and of his temper.

Mr. Edleston has stated, with much appearance of truth, that the steps taken by Newton's friends at the close of 1695, may have interfered as much as the infirmities of Flamsteed with the completion of the lunar theory;[3] but whether or not this was the case, there can be no doubt that his appointment to the Wardenship of the Mint in 1696, and to the Mastership in 1699, deferred to a distant day the appearance of a new edition of the Principia. Even in November 1702, when he was visited by Bd. Greves, who saw in his hands an interleaved and corrected copy of the Principia, he would not acknowledge that he had any intention to reprint it.[4] The preparation of his Optics, which was published in April 1704, must have interfered with his revision of the Principia, and it appears, from his letter to Flamsteed, in November 1694, that he was then

[1] The number of copies printed is not known. The original price seems to have been ten shillings.

[2] See Rigaud's *Hist. Essay*, pp. 89-95.

[3] *Correspondence*, &c., *Præf.* p. xi.

[4] Ibid. *Præf.* p. xiv.

occupied in preparing a new edition of his great work.[1] His duties at the Mint allowed him but little time for the performance of so laborious a task; and when his consent was at last obtained to put the work to press, they greatly interrupted its progress.

Dr. Bentley, the distinguished Master of Trinity College, had for a long time solicited and even urged Newton to give his consent to the re-publication of the Principia.[2] In the middle of 1708 he succeeded in removing his scruples, but it was not till the spring of 1709 that he prevailed upon him to intrust the superintendence of it to a young mathematician of great promise, Roger Cotes, Fellow of Trinity College, who had been recently appointed Professor of Astronomy and Experimental Philosophy. On the 21st May 1709, after having been that day with Newton, Bentley announced this arrangement to Cotes. "Sir Isaac Newton," he said, "will be glad to see you in June, and then put into your hands one part of his Book corrected for the press." About the middle of July, Cotes went to London, in the expectation doubtless to bring down with him to Cambridge the corrected portion of the Principia. Newton, however, had some farther improvements to make upon it, and promised to send it down in about a fortnight. Cotes was impatient to begin his work, and when a whole month had passed without any intelligence from Newton, he addressed to him the following letter:—

"CAMBRIDGE, *August* 18*th*, 1709.

"S^{R}.—The earnest desire I have to see a new Edition of y^{r} Princip. makes me somewhat impatient till we receive your Copy of it which You was pleased to promise me about the

[1] Baily's *Flamsteed*, p. 138.

[2] It would appear from a conversation between Sir Isaac and Conduitt, that Bentley was at the expense of printing the second edition of the Principia, and received the profits of the work. "I asking him (Newton)," says Conduitt, "how he came to let Bentley print his Principia, which he did not understand—'Why,' said he, 'he was covetous, and I let him do it to get money.'"—Conduitt's MS. See Vol. II. ch. xxi.

middle of the last Month, You would send down in about a Fourtnights time. I hope you will pardon me for this uneasiness from which I cannot free myself & for giving You this Trouble to let You know it. I have been so much obliged to You by Yrself & by Yr Book yt (I desire you to believe me) I think myself bound in gratitude to take all the Care I possibly can that it shall be correct . . . . . I take this Opportunity to return You my most hearty thanks for Yr many Favours and Civilitys to me who am

Your most obliged humble Servant,

ROGER COTES.

"For SIR ISAAC NEWTON at His House in
Jermin Street near St. James's Church
Westminster."

No answer was returned to this letter from Cotes, and a long month had passed away when one evening his next-door neighbour, William Whiston, about the end of September, put into his hands "the greatest part of the copy of the Principia," ending with the thirty-second Proposition of the Second Book. In a letter dated October 11, Newton intimated to Cotes that he had sent him by Mr. Whiston "the greatest part of the copy of his Principia, in order to a new edition," thanked him for his letter of the 18th of August, and requested him not to be at the trouble of examining all the Demonstrations, but "to print by the copy sent him, correcting only such faults as occur in reading over the sheets," which would entail upon him "more labour than it was fit to give him." These were the two first letters of that celebrated correspondence between Newton and Cotes, which has lain in Trinity College Library for nearly a century and a half, in spite of the wishes expressed by Dr. Monk,[1] and felt by other admirers of the Principia, "that one of the many accomplished Newtonians who are resident in that society would favour the world by publishing

[1] Monk's *Life of Bentley*, p. 180.

the whole collection." Through the liberality of the present Master and Seniors of Trinity College, this has at last been done, and in a manner highly creditable to the learning and talents of Mr. Edleston, by whom the correspondence is edited.[1] The printing of the Principia went on very slowly, and was not finished till the first week of March 1713. Cotes expressed a wish that Dr. Bentley should write the preface to it, but it was the opinion of Sir Isaac and the Master of Trinity, that the preface should come from the pen of Cotes himself. This he readily undertook, but previous to writing it he addressed the following letter to Dr. Bentley, in order to learn "with what view he thought proper to have it written."

TO DR. BENTLEY.

"*March* 10*th*,1712-13.

"SR,—I received what you wrote to me in Sr Isaac's letter. I will set about the Index in a day or two. As to the Preface, I should be glad to know from Sr Isaac with what view he thinks proper to have it written. You know the book has been received abroad with some disadvantage, and the cause of it may easily be guessed at. The Commercium Epistolicum, lately published by order of the Royal Society, gives such indubitable proofs of Mr. Leibnitz's want of candour, that I shall not scruple in the least to speak out the full truth of the matter if it be thought convenient. There are some pieces of his looking this way which deserve a censure, as his *Tentamen de Motuum Cœlestium causis*.[2] If Sr Isaac is willing that some-

[1] These letters, relating to questions connected with the new edition of the Principia, are *seventy-two* in number, and extend from May 21, 1709, to March 31, 1713. Mr. Edleston has added other *fifty*, connected with the Principia, from Newton, Cotes, Keill, Jones, Brook Taylor, and others, and in an Appendix he has published *thirty-four* letters, chiefly from Newton, and collected principally from original sources. Mr. Edleston has enriched this valuable work with an excellent synoptical view of Newton's life, and a large number of notes of the highest interest.

[2] The critique by Newton, already mentioned, bore upon this paper by Leibnitz; see p. 258, Note.

thing of this nature may be done, I should be glad of it, whilst I am making the Index, he would be pleased to consider of it, and put down a few notes of what he thinks most material to be insisted on. This I say upon supposition that I write the Preface myself. But I think it would be much more adviseable that you or he or both of you should write it whilst you are in town. You may depend upon it that I will own it, and defend it as well as I can, if hereafter there be occasion.—I am, Sr. " &c.

Immediately after the arrival of this letter on the 12th, Sir Isaac happened to call upon Dr. Bentley, and they agreed to meet in the evening at Sir Isaac's house, to write a reply to it. They objected to any joint preface " to be fathered by Cotes :" they suggested as the subject of the Preface an account of the work itself, and of the improvements of the new edition, and they answered that he has Sir Isaac's consent " to add what he thought proper about the controversy of the first invention, you yourself being full master of it, and want no hints to be given you." Cotes was also instructed " to spare the *name* of M. Leibnitz, and abstain from all words and epithets of reproach." In reply to this letter on the 18th March, Cotes sketched the plan of the Preface in conformity with the directions already given him, and asks Newton for permission to appeal to the judgment of the Society in the Commercium Epistolicum. To this Newton answers, that if any farther Preface is written, " he must not see it, as he finds he shall be examined about it." The plan of the Preface is therefore altered, and the proposed notice of the dispute respecting the discovery of fluxions is abandoned. Cotes confines himself to an exposition of " the manner of philosophizing made use of " in the Principia, and to an examination of the objections of Leibnitz and of the theory of vortices.

The general Preface thus drawn up by Cotes, is dated 13th May 1713, and in a subsidiary Preface, dated March 2d, Sir Isaac himself mentions the leading alterations which have been made in the New Edition. "In the second section of the First Book," he says, "the determination of the force by which bodies may revolve in given orbits, is simplified and enlarged. In the seventh section of the Second Book, the theory of the resistance of fluids is more accurately investigated, and confirmed by new experiments; and in the Third Book the theory of the moon, and the precession of the equinoxes, are more fully deduced from their principles, and the theory of comets is confirmed by several examples, and their orbits more accurately computed."

On the 25th of June, Cotes[1] announces to its author, through Dr. Samuel Clarke, "that the book is finished," and on the 27th of July, Newton waited on the Queen to present a copy of the Principia to her Majesty.

Such is a brief notice of the composition and printing of the first and second editions of a work which will be memorable not only in the annals of one science or of one country, but which will form an epoch in the history of the world, and will ever be regarded as the brightest page in the records of human reason,—a work, may we not add, which would be read with delight in every planet of our system,—in every system of the universe. What a glorious privilege was it to have been the author of the Principia! There was but one earth upon whose form and tides and movements the philosopher could exercise his genius,—one moon, whose perturbations and inequalities and actions he could study,—one sun, whose controlling force and apparent motions he could calculate and determine,—one system of planets, whose mutual disturbances could tax his highest reason,[2]—one system of comets, whose eccentric paths he could explore and rectify,—and one universe of stars, to

[1] Some account of this interesting and distinguished person, whose name is so indissolubly associated with that of Newton, and with the Principia, will be found in APPENDIX, No. XII.

[2] The celebrated Lagrange, who frequently asserted that Newton was the greatest genius that ever existed, used to add—and the most fortunate, for we cannot find *more than once* a system of the world to establish.—Delambre, *Notice sur la Vie de Lagrange, Mém. de l'Institut.* 1812, p. lxv.

whose binary and multiple combinations he could extend the law of terrestrial gravity. To have been the chosen sage summoned to the study of that earth, these systems, and that universe,—the favoured lawgiver to worlds unnumbered, the high-priest in the temple of boundless space,—was a privilege that could be granted but to one member of the human family;—and to have executed the task was an achievement which in its magnitude can be measured only by the infinite in space, and in the duration of its triumphs by the infinite in time. That Sage—that Lawgiver—that High-priest was Newton. Let us endeavour to convey to the reader some idea of the revelations which he made, and of the brilliant discoveries to which they conducted his successors.

The Principia consists of three Books. The *First* and *Second*, which occupy three-fourths of the work, are entitled, *On the Motion of Bodies;* the First treating of their motions in free space, and the Second of their motions in a resisting medium. The *Third* bears the title, *On the System of the World.*

The *First* Book, besides the definition and axioms, or laws of motion, with which it begins, consists of *fourteen* sections, in the first of which the author explains the method of prime and ultimate ratios, used in his investigations, and which is similar to the method of fluxions, more fully explained in the Second Book. The other sections treat of centripetal forces, and motions in fixed and moveable orbits.

The *Second* Book consists of nine sections, and treats of bodies moving in resisting media, or oscillating as pendulums.

The *Third* Book is introduced by the "Rules of Philosophizing." It consists of *five* sections, on the Causes of the System of the World,—on the Quantity of Lunar Errors,—on the Quantity of the Tides,—on the Precession of the Equinoxes,—and on Comets; and it concludes with a general scholium, containing reflections on the constitution of the universe, and on the "Eternal, Infinite, and perfect Being" by whom it is governed.

The great discovery which characterizes the Principia, is that of the principle of universal gravitation, *that every particle of matter in the universe is attracted by, or gravitates to every other particle of matter, with a force inversely proportional to the squares of their distances.* In order to establish this principle, Newton begins by considering the curves, which are generated by the composition of a direct impressed motion with a gravitation or tendency towards a centre; and having demonstrated, that in all cases the areas described by the revolving body are proportional to the times of their description, he shows how to find, from the curves described, the law of the force. In the case of a circular orbit passing through the centre of tendency, the force or tendency towards the centre will be in every point as the fifth power of the distance. If the orbit is the proportional spiral, the force will be reciprocally as the cube of the distance. If it is an ellipse, the force towards the centre of it will be directly as the distance. If it is any of the conic sections, the centripetal force, or tendency towards the focus, will, in all points, be reciprocally as the square of the distance from the focus. If the velocity of the impressed motion is of a certain magnitude, the curve described will be a hyperbola,—if different to a certain degree, it will be a parabola,—and if slower, an ellipse, or a circle in one case.

In order to determine whether the force of gravity resided in the centres of the sun and planets, or in each individual particle of which they are composed, Newton demonstrated that if a spherical body acts upon a distant body with a force varying as the distance of this body from the centre of the sphere, the same effect will be produced as if each of its particles acted upon the distant body according to the same law. And hence it follows, that the spheres, whether they are of uniform density, or consist of concentric layers, with densities varying according to any law whatever, will act upon each other in the same manner as if their force resided in their centre alone. But as the bodies of the solar system are very nearly spherical, they will all act upon one another, and upon bodies placed on their surface, as if they were so many centres of attraction; and therefore we obtain the law of gravity which subsists between spherical bodies, namely, that one sphere will act upon another with a force directly proportional to their quantities of matter, and inversely, as the square of the distance between the centres of the spheres. From the equality of action and re-action, to which no exception can be found, Newton concluded that the sun gravitated to the planets, and the planets to their satellites, and the earth itself to the stone which falls upon its surface; and consequently that the two mutually gravitating bodies approached to one another with velocities inversely proportional to their quantities of matter.

Having established this universal law, Newton was enabled not only to determine the weight which the same body would have at the surface of the sun and the planets, but even to calculate the quantity of matter in the sun and in all the planets that had satellites, and even to determine the density or specific gravity of the matter of which they were composed,—results which Adam Smith pronounced to be "above the reach of human reason and experience." In this way he found that the weight of the same body would be twenty-three times greater at the surface of the sun than at the surface of the earth, and that the density of the earth was four times greater than that of the sun, the planets increasing in density as they are nearer the centre of the system.

If the peculiar genius of Newton has been displayed in his investigation of the law of universal gravitation, it shines with no less lustre in the patience and sagacity with which he traced the consequences of this fertile principle.

The discovery of the spheroidal form of Jupiter by Cassini had probably directed the attention of Newton to the determination of its cause, and consequently to the investigation of the true figure of the earth. The spherical form of the planets had been ascribed by Copernicus to the gravity or natural appetency of their parts ; but upon considering the earth as a body revolving upon its axis, Newton quickly saw that the figure arising from the mutual attraction of its parts must be modified by another force arising from its rotation. When a body revolves upon an axis, the velocity of rotation increases from the poles where it is nothing, to the equator where it is a maximum. In consequence of this velocity the bodies on the earth's surface have a tendency to fly off from it, and this tendency increases with the velocity. Hence arises a centrifugal force, which acts in combination with the force of gravity, and which Newton found to be the 289th part of the force of gravity at the equator, and decreasing as the cosine of the latitude, from the equator to the poles. The great predominance of gravity over the centrifugal force prevents the latter from carrying off any bodies from the earth's surface, but the weight of all bodies is diminished by the centrifugal force, so that the weight of any body is greater at the poles than it is at the equator. If we now suppose the waters at the pole to communicate with those at the equator by means of a canal, one branch of which goes from the pole to the centre of the earth, and the other from the centre of the earth to the equator, then the polar branch of the canal will be heavier than the equatorial branch, in consequence of its weight not being diminished by the centrifugal force ; and, therefore, in order that the two columns may be in equilibrio, the equatorial one must be lengthened. Newton found that the length of the polar must be to that of the equatorial canal as 229 to 230, or that the earth's polar radius must be seventeen miles less than its equatorial radius ; that is, that the figure of the earth is an oblate spheroid, formed by the revolution of an ellipse round its lesser axis. Hence it follows, that the intensity of gravity at any point of the earth's surface is in the inverse ratio of the distance of that point from the centre, and consequently that it diminishes from the equator to the poles,—a result which he confirmed by the fact, that clocks required to have their pendulums shortened, in order to beat true time, when carried from Europe towards the equator.[1]

The next subject to which Newton applied the principle of gravity, was the tides of the ocean. The philosophers of all ages had recognised the connexion between the phenomena of the tides and the position of the moon. The College of Jesuits at Coimbra, and subsequently Antonio de Dominis and Kepler, distinctly referred the tides to the attraction of the waters of the earth by the moon, but so imperfect was the explanation which was thus given of the phenomena, that Galileo ridiculed the idea of lunar attraction, and substituted for it a fallacious explanation of his own. That the moon is the principal cause of the tides is obvious from the well-known fact, that it is high water at any given place a short time after she is in the meridian of that place ; and that the sun performs a secondary part in their production, may be proved from the circumstance, that the highest tides take place when the sun, the moon, and the earth are in the same straight line, that is, when the force of the sun conspires with that of the moon ; and that the lowest tides take place when the lines drawn from the sun and moon to the earth are at right angles to each other, that is, when the force of the sun acts in opposition to that of the moon. The most perplexing phenomenon in the tides of the ocean, and one which is still a stumbling-block to persons slightly acquainted with the theory of attraction, is the existence of high water on the side of the earth opposite to the moon, as well as on the side next the earth. To maintain that the attraction of the moon at the same instant draws the waters of the ocean towards herself, and also draws them from the earth in an opposite direction, seems at first sight paradoxical ; but the difficulty vanishes when we consider the earth, or rather the centre of the earth, and the water on each side of it, as three distinct bodies,

[1] This was first observed by Richer, who found that a clock regulated to mean time at Paris lost 2′ 28″ daily at Cayenne.

placed at different distances from the moon, and consequently attracted with forces inversely proportional to the squares of their distances. The water nearest the moon will be much more powerfully attracted than the centre of the earth, and the centre of the earth more powerfully than the water farthest from the moon. The consequence of this must be, that the waters nearest the moon will be drawn away from the centre of the earth, and will consequently rise from their level, while the centre of the earth will be drawn away from the waters opposite the moon, which will, as it were, be left behind, and consequently be in the same situation as if they were raised from the earth in a direction opposite to that in which they are attracted by the moon. Hence the effect of the moon's action upon the earth is to draw its fluid parts into the form of an oblong spheroid, the axis of which passes through the moon. As the action of the sun will produce the very same effect, though in a smaller degree, the tide at any place will depend on the relative position of these two spheroids, and will be always equal either to the sum, or to the difference of the effects of the two luminaries. At the time of new and full moon, the two spheroids will have their axes coincident; and the height of the tide, which then will be a *spring* one, will be equal to the sum of the elevations produced in each spheroid considered separately, while at the first and third quarters the axes of the spheroids will be at right angles to each other, and the height of the tide, which will then be a *neap* one, will be equal to the difference of the elevations produced in each separate spheroid. By comparing the spring and neap tides, Newton found that the force with which the moon acted upon the waters of the earth, was to that with which the sun acted upon them as 4·48 to 1;—that the force of the moon produced a tide of 8·63 feet;—that of the sun one of 1·93 feet;—and both combined, one of $10\frac{1}{2}$ feet,—a result which, in the open sea, does not deviate much from observation. Having thus ascertained the force of the moon on the waters of our globe, he found that the quantity of water in the moon was to that in the earth as 1 to 40, and the density of the moon to that of the earth as 11 to 9.

The motions of the moon, so much within the reach of our own observation, presented a fine field for the application of the theory of universal gravitation. The irregularities exhibited in the lunar motions had been known in the time of Hipparchus and Ptolemy. Tycho had discovered the great inequality called the *variation*, amounting to 37′, and depending on the alternate acceleration and retardation of the moon by the action of the sun in every quarter of a revolution; and he had also ascertained the existence of the annual equation. Of these two inequalities, Newton gave a most satisfactory explanation, making the first 36′ 10″, and the other 11′ 51″, differing only a few seconds from the numbers adopted by Tobias Mayer in his celebrated Lunar Tables. The force exerted by the sun upon the moon may be always resolved into two forces, one acting in the direction of the line joining the moon and the earth, and consequently tending to increase or diminish the moon's gravity to the earth; and the other in a direction at right angles to this, and consequently tending to accelerate or retard the motion in her orbit. Now, it was found by Newton that this last force was reduced to nothing, or vanished at the syzygies or quadratures, so that at these four points the described areas are proportional to the times. The instant, however, that the moon quits these positions, the force under consideration, which we may call the tangential force, begins, and it reaches its maximum in the four octants. The force, therefore, compounded of these two elements of the solar force, or the diagonal of the parallelogram which they form, is no longer directed to the earth's centre, but deviates from it at a maximum about thirty minutes, and therefore affects the angular motion of the moon, the motion being accelerated in passing from the quadratures to the syzygies, and retarded in passing from the syzygies to the quadratures. Hence the velocity is in its mean state in the octants, a maximum in the syzygies, and a minimum in the quadratures.

Upon considering the influence of the solar force in diminishing or increasing the moon's gravity to the earth, Newton saw that her distance and periodic time must, from this cause, be subject to change, and in this way he accounted for the annual equation observed by Tycho. By the application of similar principles, he explained the cause of the motion of the apsides, or of the greater axis of the moon's orbit, which was an angular progressive motion of 3° 4′ nearly in the course of one lunation ;[1] and he showed that the retrogradation of the nodes, amounting to 3′ 10″ daily, arose from one of the elements of the solar force being exerted in the plane of the ecliptic, and not in the plane of the moon's orbit,—the effect of which was to draw the moon down to the plane of the ecliptic, and thus cause the line of the nodes, or the intersection of these two planes, to move in a direction opposite to that of the moon.

The lunar theory thus sketched by Newton, required for its completion the labours of another century. The imperfections of the fluxionary calculus prevented him from explaining the other inequalities of the moon's motions, and it was reserved to Euler, D'Alembert, Clairaut, Mayer, and Laplace, to bring the lunar tables to a high degree of perfection, and to enable the navigator to determine his longitude at sea with a degree of precision which the most sanguine astronomer could scarcely have anticipated.

By the consideration of the retrograde motion of the moon's nodes, Newton was led to one of the most striking of all his discoveries, namely, the cause of the remarkable phenomenon of the precession of the equinoctial points, which moved 50″ annually, and completed the circuit of the heavens in 25,920

[1] Newton made it only 1° 31′ 28″, just one-half of its real value. Clairaut obtained the same result, but afterwards, by a more accurate calculation, found it to be 3° 4′, agreeing exactly with observation.

years. Kepler had declared himself incapable of assigning any cause for this motion, and we do not believe that any other astronomer ever made the attempt. From the spheroidal form of the earth, it may be regarded as a sphere with a spheroidal ring surrounding its equator, one half of the ring being above the plane of the ecliptic, and the other half below it. Considering this excess of matter as a system of satellites adhering to the earth's surface, Newton now saw that the combined actions of the sun and the moon upon these satellites tended to produce a retrogradation in the nodes of the circles which they described in their diurnal rotation, and that the sum of all the tendencies being communicated to the whole mass of the planet, ought to produce a slow retrogradation of the equinoctial points. The effect produced by the motion of the sun he found to be *forty* seconds, and that produced by the action of the moon *ten* seconds.

Although there could be little doubt that the comets were retained in their orbits by the same laws which regulated the motions of the planets, yet it was not easy to put this opinion to the test of observation. The visibility of comets only in a small part of their orbits rendered it difficult to ascertain their distance and periodic times, and as their periods were probably of great length, it was impossible to obtain approximate results by repeated observation. Newton, however, though he at first imagined that comets moved in straight lines, removed this difficulty, by showing how to determine the orbit of a comet, namely, the form and position of the orbit, and the periodic time, by three observations. This method consists of an easy geometrical construction, founded on the supposition that the paths of comets are so nearly parabolic, that the parabola may be used without any sensible error, although he considers it more probable that their orbits are elliptical, and that after a long period they may return. By applying this method to the comet of 1680, he calculated the elements of its orbit, and from the agreement of the computed places with those which were observed, he justly inferred that the motions of comets were regulated by the same laws as those of the planetary bodies. This result was one of great importance ; for as the comets enter our system in every possible direction, and at all angles with the ecliptic, and as a great part of their orbits extends far beyond the limits of the solar system, it demonstrated the existence of gravity in spaces beyond the planets, and proved that the law of the inverse ratio of the squares of the distance was true in every possible direction, and at very remote distances from the centre of our system.

Such is a brief view of the leading discoveries which the *Principia* first announced to the world. The grandeur of the subjects of which it treats,—the beautiful simplicity of the system which it unfolds,—the clear and concise reasoning by which that system is explained,—and the irresistible evidence by which it is supported, might have insured it the warmest admiration of contemporary mathematicians, and the most welcome reception in all the schools of philosophy throughout Europe. This, however, is not the way in which great truths are generally received. Though the astronomical discoveries of Newton were not assailed by the class of ignorant pretenders who attacked his optical writings, yet they were everywhere resisted by the errors and prejudices which had taken a deep hold even of the strongest minds. The philosophy of Descartes was predominant throughout Europe. Appealing to the imagination more than to reason, it was quickly received into popular favour, and the same causes which facilitated its introduction, extended its influence, and completed its dominion over the human mind. In explaining all the movements of the heavenly bodies by a system of vortices in a fluid medium diffused through the universe, Descartes had seized upon an analogy of the most alluring and deceitful kind. Those who had seen heavy bodies revolving in the eddies of a whirlpool, or in the gyrations of a vessel of water thrown into a circular motion, had no difficulty in conceiving how the planets might revolve round the sun by analogous movements. The mind instantly grasped at an explanation of so palpable a character, and which required for its development neither the exercise of patient thought, nor the aid of mathematical skill. The talent and perspicuity with which the Cartesian system was expounded, and the show of experiments with which it was sustained, contributed powerfully to its adoption, while it derived a still higher sanction from the excellent character and the unaffected piety of its author.

Thus entrenched as the Cartesian system was, in the strongholds of the human mind, and fortified by its most obstinate prejudices, it was not to be wondered at that the pure and sublime doctrines of the Principia were distrustfully received and perseveringly resisted. The uninstructed mind could not readily admit the idea, that the great masses of the planets were suspended in empty space, and retained in their orbits by an invisible influence residing in the sun; and even those philosophers who had been accustomed to the rigour of true scientific research, and who possessed sufficient mathematical skill for the examination of the Newtonian doctrines, viewed them at first as reviving the occult qualities of the ancient physics, and resisted their introduction with a pertinacity which it is not easy to explain. Prejudiced, no doubt, in favour of his own metaphysical views, Leibnitz himself misapprehended the principles of the Newtonian philosophy, and endeavoured to demonstrate the truths in the Principia by the application of different principles. Even two years after the publication of the Principia, he published a dissertation in which he explained the motions of the planets by an ethereal fluid. Huygens, who above all other men was qualified to appreciate the new philosophy, rejected the doctrine of gravitation as existing between the individual particles of matter, and received it only as an attribute of the planetary masses. John Bernouilli, also, one of the first mathematicians of the age, opposed the philosophy of Newton. Mairan, in the early part of his life, was a strenuous defender of the system of vortices. Cassini and Maraldi were quite ignorant of the Principia, and occupied themselves with the most absurd methods of calculating the orbits of the comets long after the Newtonian method had been established on the most impregnable basis ; and even Fontenelle, a man of liberal views and extensive information, continued throughout the whole of his life to maintain the doctrines of Descartes.

The Chevalier Louville, in his memoir "On the construction and Theory of Tables of the Sun," had applied the doctrine of central force to the motions of the planets, so early as 1720.[1] S'Gravesande had introduced it into the Dutch universities at a somewhat earlier period ; and Maupertuis, in consequence of a visit which he paid to England in 1728, zealously defended it in his Treatise on the Figures of the Celestial Bodies. But notwithstanding these and some other examples that might be quoted, we must admit the truth of the remark of Voltaire, that though Newton survived the publication of the Principia more than forty years, yet at the time of his death he had not above twenty followers in England.[2] With regard to the progress of the Newtonian philosophy in England, some difference of opinion has been entertained. Professor Playfair gives the following account of it :—"In the universities of England, though the Aristotelian physics had made an obstinate resistance, they had been supplanted by the Cartesian, which became firmly established about the time when their foundation began to be sapped by the general progress of science, and particularly by the discoveries of Newton. For more than thirty years after the publication of these discoveries, the system of vortices kept its ground, and a translation from French into Latin of the Physics of Rohault, a work entirely Cartesian, continued at Cambridge to be the text for philosophical instruction. About the year 1718, a new and more elegant translation of

[1] *Mém. Acad. Par.* 1720.

[2] In 1738, Voltaire published a popular exposition of Newton's discoveries, which contributed greatly to their reception on the Continent.

the same book was published by Dr. Samuel Clarke, with the addition of notes, in which that profound and ingenious writer explained the views of Newton on the principal subjects of discussion, so that the notes contained virtually a refutation of the text; they did so, however, only virtually, all appearance of argument and controversy being carefully avoided. Whether this escaped the notice of the learned Doctor or not is uncertain, but the new translation, from its better Latinity and the name of the editor, was readily admitted to all the academical honours which the old one had enjoyed. Thus the stratagem of Dr. Clarke completely succeeded; the tutor might prelect from the text, but the pupil would sometimes look into the notes; and error is never so sure of being exposed, as when the truth is placed close to it, side by side, without anything to alarm prejudice, or awaken from its lethargy the dread of innovation. Thus, therefore, the Newtonian philosophy first entered the University of Cambridge, under the protection of the Cartesian." To this passage Professor Playfair adds the following as a note.

"The Universities of St. Andrews and Edinburgh were, I believe, the first in Britain where the Newtonian philosophy was made the subject of the academical prelections. For this distinction they are indebted to James and David Gregory, the first in some respects the rival, but both the friends of Newton. Whiston bewails, in the anguish of his heart, the difference in this respect between those universities and his own. David Gregory taught in Edinburgh for several years prior to 1690, when he removed to Oxford; and Whiston says[1] 'he had already caused several of his scholars to keep Acts, as we call them, upon several branches of the Newtonian philosophy, while we at Cambridge, poor wretches, were ignominiously studying the fictitious hypotheses of the Cartesians.'[2] I do

[1] "Whiston's *Memoirs of his own Life*," p. 36.

[2] It does not appear at what time the Newtonian Philosophy was received at Oxford. Judging from Addison's "Oration in Defence of the New Philosophy," spoken in the

not, however, mean to say that from this date the Cartesian philosophy was expelled from those universities; the Physics of Rohault were still in use as a text-book,—at least occasionally, to a much later period than this, and a great deal, no doubt, depended on the character of the individual. Professor Keill introduced the Newtonian philosophy in his lectures at Oxford in 1697; but the instructions of the tutors, which constitute the real and efficient system of the University, were not cast in that mould till long afterwards." Adopting the same view of the subject, Mr. Dugald Stewart has stated, "that the philosophy of Newton was taught by David Gregory at Edinburgh, and by his brother, James Gregory, at St. Andrews,[1] before it was able to supplant the vortices of Descartes in that very university of which Newton was a member. It was in the

Theatre at Oxford, July 7, 1693, six years after the publication of the Principia, we have no doubt that the Cartesian Philosophy, which is obviously the "New Philosophy," defended by Addison, was in full force at that date. This oration, "done from the Latin original," is appended to the English translation of Fontenelle on the Plurality of Worlds; and on the title-page to that work it is called "*Mr. Addison's Defence on the Newtonian Philosophy.*" Our readers will decide from the following extract whether the New Philosophy means the Newtonian or the Cartesian Philosophy:—

"How long, gentlemen of the University, shall we slavishly tread in the steps of the ancients, and be afraid of being wiser than our ancestors? How long shall we religiously worship the triflings of antiquity as some do old wives' stories? It is indeed shameful, when we survey the *great* ornament of the present age (NEWTON), to transfer our applauses to the ancients, and to take pains to search into ages past for persons fit for panegyrick." So far the New Philosophy may mean that of Newton, but the following passage contradicts any such inference:—"The ancient philosophy has had more allowed than it could reasonably pretend to; how often has Sheldon's Theatre rung with encomia on the Stagyrite, who, greater than his own Alexander, has long, unopposed, triumphed in our school desks, and had the whole world for his pupils? At length rose CARTESIUS, a happier genius, who has bravely asserted the truth against the united force of all opposers, and has brought on the stage a *new method of philosophizing.* But shall we stigmatize with the name of *novelty, that philosophy* which, though but lately revived, is more ancient than the peripatetic, and as old as the mother from whence it is derived? A great man indeed he was, and the only one we envy France (*Descartes*). He solved the difficulties of the universe almost as well as if he had been its architect." The name of Newton or his philosophy is never again mentioned.—*Author.*

[1] Dr. Reid states, that James Gregory, Professor of Philosophy at St Andrews, printed a Thesis at Edinburgh in 1690, containing twenty-five positions, of which twenty-two were a compend of Newton's Principia.

Scottish universities that the philosophy of Locke, as well as that of Newton, was first adopted as a branch of academical education."

Anxious as we should have been to have awarded to Scotland the honour of having first adopted the Newtonian philosophy, yet a regard for historical truth compels us to take a different view of the subject. It is well known that Sir Isaac Newton delivered lectures on his own philosophy from the Lucasian chair before the publication of the Principia ; and in the very page of Whiston's life quoted by Professor Playfair, he informs us that he had heard him read such lectures in the public schools, though at that time he did not at all understand them. Newton continued to lecture till 1699, and occasionally, we presume, till 1703, when Whiston became his successor, having been appointed his deputy in 1699. In both of these capacities, Whiston delivered in the public schools a course of lectures on astronomy, and a course of physico-mathematical lectures, in which the mathematical philosophy of Newton was explained and demonstrated, and both these courses were published,—the one in 1707, and the other in 1710,—for the use of the young men in the University. In 1707, the celebrated blind mathematician, Nicholas Saunderson, took up his residence in Christ's College, without being admitted a member of that body. The society not only allotted to him apartments, but gave him the free use of their library. With the concurrence of Whiston, he delivered a course "On the Principia, Optics, and Universal Arithmetic of Newton," and the popularity of these lectures was so great, that Sir Isaac corresponded on the subject of them with their author ; and on the ejection of Whiston from the Lucasian chair in 1711, Saunderson was appointed his successor. In this important office he continued to teach the Newtonian philosophy till the time of his death, which took place in 1739.

But while the Newtonian philosophy was thus regularly taught in Cambridge, after the publication of the Principia, there were not wanting other exertions for accelerating its progress. About 1694, the celebrated Dr. Samuel Clarke, while an under-graduate, defended in the public schools, a question taken from the Newtonian philosophy; and his translation of Rohault's Physics, which contains references in the notes to the Principia, and which was published in 1697 (and not in 1718, as stated by Professor Playfair), shows how early the Cartesian system was attacked by the disciples of Newton. The author of the life of Saunderson informs us, that public exercises or acts, founded on every part of the Newtonian system, were very common about 1707, and so general were such studies in the University, that the Principia rose to four times its original price.[1] One of the most ardent votaries of the Newtonian philosophy was Dr. Laughton, who had been tutor in Clare Hall from 1694, and it is probable that, during the whole, or at least a greater part of his tutorship, he had inculcated the same doctrines. In 1709-10, when he was proctor in that college, instead of appointing a moderator, he discharged the office himself, and devoted his most active exertions to the promotion of mathematical knowledge. Previous to this, he had even published a paper of questions on the Newtonian philosophy, which appear to have been used as theses for disputations; and such was his ardour and learning, that they powerfully contributed to the popularity of his college.[2] About the same time the learned Dr. Bentley, who first made known the philosophy of his friend to general readers, filled the high office of Master of Trinity College, and could not fail to have exerted his influence in propagating doctrines which he so greatly admired. Had any opposition been offered to the introduction of the true system

[1] Cotes states, in his preface to the second edition of the Principia, that copies of the first edition were scarce, and could only be obtained at an immense price. Sir William Brown, when at college, gave more than two guineas for a copy, and owing to the difficulty of procuring one at a reasonable price, the father of Dr. John Moore of Glasgow transcribed the whole work with his own hand. See Nichol's *Literary Anecdotes*, vol. iii. p. 322, and *Encyc. Brit.* Art. MOORE.

[2] See the *Museum Criticum*, vol. ii. p. 514.

of the universe, the talents and influence of these individuals would have immediately suppressed it, but no such opposition seems to have been made ; and though there may have been individuals at Cambridge ignorant of mathematical science, who adhered to the system of Descartes, and patronized the study of the Physics of Rohault, yet it is probable that similar persons existed in the Universities of Edinburgh and St. Andrews ; and we cannot regard their adherence to error as disproving the general fact, that the philosophy of Newton was quickly introduced into all the universities of Great Britain.[1]

But while the mathematical principles of the Newtonian system were ably expounded in our seats of learning, its physi-

[1] The following passage in Whiston's Life of Dr. Clarke, is not in accordance with some of the preceding statements. "About the year 1697, while I was chaplain to Dr. John Moor, then Bishop of Norwich, I met at one of the coffee-houses in the market-place at Norwich, a young man, to me then wholly unknown; his name was Clarke, pupil to that eminent and careful tutor, Mr. Ellis, of Gonvil and Caius College in Cambridge. Mr. Clarke knew me so far at the university, I being about eight years elder than himself, and so far knew the nature and success of my studies, as to enter into a conversation with me about that system of Cartesian philosophy his tutor had put him to translate,—I mean Rohault's Physics; and to ask my opinion about the fitness of such a translation. I well remember the answer I made him, that, 'since the youth of the university must have, at present, some system of Natural Philosophy for their studies and exercises; and since the true system of Sir Isaac Newton's was not yet made easy enough for the purpose; it was not improper, for their sakes, yet to translate and use the system of Rohault (who was esteemed the best expositor of Descartes), but that as soon as Sir Isaac Newton's philosophy came to be better known, that only ought to be taught, and the other dropped.' Which last part of my advice, by the way, has not been followed, as it ought to have been, in that university. But, as Bishop Hoadley truly observes, Dr. Clarke's Rohault is still the principal book for the young students there. Though such an observation be no way to the honour of the tutors in that university, who, in reading Rohault, do only read a philosophical romance to their pupils, almost perpetually contradicted by the better notes thereto belonging. And certainly to use Cartesian fictitious hypotheses at this time of day, after the principal parts of Sir Isaac Newton's certain system have been made easy enough for the understanding of ordinary mathematicians, is like the continuing to eat old acorns after the discovery of new wheat, for the food of mankind. However, upon this occasion, Mr. Clarke and I fell into a discourse about the wonderful discoveries made in Sir Isaac Newton's philosophy;—and the result of that discourse was, that I was greatly surprised that so young a man as Mr. Clarke then was, not much, I think, above twenty-two years of age, should know so much of those sublime discoveries which were then almost a secret to all, but to a few particular mathematicians."

cal truths had been studied by some of the most distinguished scholars and philosophers of the times, and were subsequently explained and communicated to the public by various lecturers on experimental philosophy. The celebrated Locke, who was incapable of understanding the Principia from his want of geometrical knowledge, inquired of Huygens if all the mathematical propositions in that work were true. When he was assured that he might depend upon their certainty, he took them for granted, and carefully examined the reasonings and corollaries deduced from them. In this manner he acquired a knowledge of the physical truths in the Principia, and became a firm believer in the discoveries which it contained. In the same manner he studied the treatise on Optics, and made himself master of every part of it which was not mathematical.[1] From a manuscript of Sir Isaac Newton's, entitled, "A Demonstration that the Planets, by their gravity towards the Sun, may move in Ellipses,[2] found among the papers of Mr. Locke, and published by Lord King," it would appear that he himself had been at considerable trouble in explaining to his friend that interesting proposition. This manuscript is endorsed, "Mr. Newton, March, 1689." It begins with three hypotheses (the two first being the two laws of motion, and the third the parallelogram of motion), which introduce the proposition of the proportionality of the areas to the times in motions round an immovable centre of attraction.[3] Three lemmas, containing the properties of the ellipse, then prepare the reader for the celebrated proposition, that when a body moves in an ellipse,[4] the attraction is reciprocally as the square of the distance of the body from the focus to which it is attracted. These propositions are demonstrated in a more popular manner than in the Principia, but there can be no doubt that, even in

[1] Preface to Desaguliers' *Course of Experimental Philosophy*, vol. i. p. viii. Dr. Desaguliers says that he was told this anecdote several times by Sir Isaac Newton himself.

[2] The *Life of John Locke*. Edit. 1830, vol. i. pp. 389-400.

[3] *Principia*, lib. i. prop. i.

[4] *Ibid.* lib. i. prop. xi.

their present modified form, they were beyond the capacity of Mr. Locke.

Among the learned men who were desirous of understanding the truths revealed in the Principia, Richard Bentley was one of the most distinguished. In 1691, when only thirty years of age, he applied to John Craige, a mathematician of some eminence, and a friend of Newton, for a list of works which would enable him to study the Principia. Alarmed at the list which Craige sent him, he was induced to apply to Newton himself, who drew up the directions which, along with those of Craige, we have given in the Appendix.[1] When Bentley was appointed, in 1692, the first Lecturer on Robert Boyle's Foundation, he chose as the subject of his discourse, "A Confutation of Atheism." The insidious doctrines of Spinoza and Hobbes had at that time made considerable progress among the upper ranks of society, and as these authors denied a Divine Providence, and considered the existence of the universe as the result of necessity, Bentley proposed to conclude his course of lectures with the demonstration of a Divine Providence from the physical constitution of the universe, as demonstrated by Newton. Before printing his discourses, he consulted Newton on some points which required elucidation, and it was in reply to the Queries thus addressed to him, that Newton wrote the five remarkable letters already alluded to. By this means some of the great truths of the Newtonian philosophy were promulgated among a class of readers who would not otherwise have heard of them.[2]

[1] See APPENDIX, No. XIII. The original of these directions was given by Richard Cumberland, the relation of Bentley, to Trinity College, along with the originals of the four celebrated letters from Newton to Bentley, to which our attention will be afterwards directed.

[2] Lord Aston, "a great lover of the mathematics, who would gladly be satisfied in a difficulty or two on that science," requested Mr. Greves and Sir E. Southcote to submit these difficulties to Sir Isaac Newton. Mr. Greves accordingly went on Monday, the 30th November 1702, and gives the following account of the conversation. "He owns there are a great many faults in his book, and has crossed it and interleaved it, and writ in the margin of it, in a great many places. It is talked he designs to reprint it, though he would not own it I asked him about his proof of a vacuum, and said that if there is

About the year 1718, Isaac Watts speaks of the exploded Physics of Descartes, and the noble inventions of Sir Isaac Newton, in his "hypotheses of the heavenly bodies and their motions;" and he refers to previous writers who have explained Nature and its operations in a more sensible and geometrical manner than Aristotle, especially those who have followed the principles of that wonder of our age and nation, Sir Isaac Newton.[1]

Dr. John Keill was the first person who publicly taught natural philosophy "by experiments in a mathematical manner." Desaguliers informs us that this author "laid down very simple propositions, which he proved by experiments, and from these he deduced others more compound, which he still confirmed by experiments, till he had instructed his auditors in the laws of motion, the principles of hydrostatics and optics, and some of the chief propositions of Sir Isaac Newton, concerning light and colours. He began these courses in Oxford about the year 1704 or 1705, and in that way introduced the love of the Newtonian philosophy."[2] When Dr. Keill left the University, Desaguliers began to teach the new philosophy by experiments. He commenced his lectures at Harthall, in Oxford, in 1710, and delivered more than a hundred and twenty discourses; and when he went to settle in London in 1713, he informs us that he found "the Newtonian philosophy generally received among persons of all ranks and professions, and even among the ladies by the help of experiments."[3]

such a matter as escapes through the pores of all sensible bodies, this could not be weighed. . . . I find he designs to alter that part, for he has writ on the margin, *Materia sensibilis;* perceiving his reasons do not conclude in all matter whatsoever."—Edleston's *Correspondence*, Pref. p. xiv., and Tixall's *Letters*, II. 152, quoted there.

[1] *Improvement of the Mind*, Part I. chap. xx. Art. vi. and xvi., or his *Works*, vol. v. pp. 301, 306.

[2] These lectures were first published in Latin in 1718, and afterwards in English in 1721 and 1739, under the title of *An Introduction to the true Astronomy*, or *Astronomical Lectures read in the Astronomical School of the University of Oxford.* By John Keill, M.D. F.R.S.

[2] Desaguliers, *ut supra*, Preface, pp. viii. x.

Such were the steps by which the philosophy of Newton was established in Great Britain. From the time of the publication of the Principia, its mathematical doctrines formed a regular part of academical education, and before twenty years had elapsed, its physical truths were communicated to the public in popular lectures, illustrated by experiments, and accommodated to the capacities of those who were not versed in mathematical knowledge. The Cartesian system, though it may have lingered for a while in the recesses of our universities, was soon overturned; and long before his death, Newton enjoyed the high satisfaction of seeing his philosophy triumphant in his native land.

In closing our account of the Principia, and in justification of the high eulogium we have pronounced upon it, we may quote the opinions of two of the most distinguished men of the past or the present age. "It may be justly said," observes Halley, "that so many and so valuable philosophical truths, as are herein discovered, and put past dispute, were never yet owing to the capacity and industry of any one man."[1] "The importance and generality of the discoveries," says Laplace, "and the immense number of original and profound views which has been the germ of the most brilliant theories of the philosophers of this century, and all presented with much elegance, will insure to the work, on the *Mathematical Principles of Natural Philosophy*, a pre-eminence above all the other productions of human genius."[2]

[1] *Phil. Trans.* vol. xvi. p. 296.
[2] *Système du Monde,* Edit. 2de, 1799, p. 336.

CHAPTER XIII.

The Newtonian Philosophy stationary for half a Century, owing to the imperfect state of Mechanics, Optics, and Analysis—Developed and extended by the French Mathematicians—Influence of the Academy of Sciences—Improvements in the Infinitesimal Calculus—Christian Mayer of the Arithmetic of Sines—D'Alembert's Calculus of Partial Differences—Lagrange's Calculus of Variations—The Problem of Three Bodies—Importance of the Lunar Theory—Lunar Tables of Clairaut, D'Alembert, and Euler—The Superior Tables of Tobias Mayer gains the Prize offered by the English Board of Longitude—Euler receives part of the English Reward, and also a Reward from the French Board—Laplace discovers the cause of the Moon's Acceleration, and completes the Lunar Theory—Lagrange's Solution of the Problem of Three Bodies as applied to the Planets—Inequalities of Jupiter and Saturn explained by Laplace—Stability of the Solar System the proof of Design—Maclaurin, Laplace, and others, on the Figure of the Earth—Researches of Laplace on the Tides, and the stable Equilibrium of the Ocean—Theoretical Discovery of Neptune by Adams and Leverrier—New Satellites of Saturn and Neptune—Extension of Saturn's Ring and its partial fluidity—Twenty-seven Asteroids Discovered—Leverrier's Theory of them—Comets with Elliptic Orbits within our System—Law of Gravity applied to Double Stars—Spiral Nebulæ—Motion of the Solar System in Space.

When Halley remarked that the author of the *Principia* "seemed to have exhausted his argument, and left little to be done by those who should succeed him," he committed a mistake which, though it had a tendency to check the progress of inquiry, was yet one into which philosophers are apt to fall when their science has made a great start by the discovery of some general and comprehensive law. Had Halley ventured to make this remark at the close of his life, rather than in 1687, he might have found some justification of it in the long interval which elapsed before any brilliant addition had been made to physical astronomy. During the half century which had passed away since the discovery of universal gravitation, no application of it of any importance had been made, and, as

Laplace has observed, "all this interval was required for this great truth to be generally comprehended, and for surmounting the opposition which it encountered from the system of vortices, and from the prejudices of contemporaneous mathematicians." The infinitesimal analysis, as it was left by Newton and Leibnitz, was incapable of conducting the physical astronomer to any higher results than those which were consigned in the Principia ; and it is a remarkable fact in the history of science, that the very men who spurned the new philosophy of gravitation, were strenuously engaged in improving that very calculus which was destined to establish and extend those great truths which they had so rashly denounced.

It has been remarked by Laplace, that "with the exception of his researches on the elliptical motion of the planets and comets, of the attraction of spherical bodies, and of the intensity of gravity at the surface of the sun, and of the planets that are accompanied by satellites, all the other discoveries which we have described were only blocked out by Newton. His theory of the figures of the planets was limited by the supposition of their homogeneity. His solution of the problem of the precession of the equinoxes, though very ingenious and accordant with observations, is in many respects defective. In the great number of perturbations in the celestial motions, he has considered only those of the lunar motions, the most important of which, namely, the evection, had escaped his researches. He has completely established the existence of the principle which he discovered, but the development of its consequences and of its advantages has been the work of the successors of this great geometer."[1]

In thus completing the great work of which Newton laid the foundation, it was necessary, as Laplace observes, "to bring to perfection at once the sciences of mechanics, optics, and analysis ; and though physical astronomy may still be improved and simplified, yet posterity will gratefully acknowledge that the

[1] *Système du Monde*, p. 336.

geometers of the eighteenth century have not transmitted to us a single astronomical phenomenon of which they have not determined the cause and the law. We owe to France the justice of observing, that if England had the advantage of giving birth to the discovery of universal gravitation, it is principally to the French geometers, and to the encouragement held out by the Academy of Sciences, that we owe the numerous developments of this discovery, and the revolution which it has produced in astronomy."[1]

In submitting to our readers a brief history of these developments, and of that revolution, we shall gather fresh laurels for the author of the Principia. It is from what he left undone, and what he enabled others to do, that we can rightly estimate the magnitude and appreciate the value of his achievement. The importance of a great discovery does not lie in its intrinsic novelty and beauty: It is the number of its applications, and the ubiquity of its range, that stamps its value; and when we proclaim Newton the Father of the Philosophy of the Universe, we must regard the Eulers, the Clairauts, the D'Alemberts, the Lagranges, and the Laplaces of another age, as the intellectual progeny which he educated and reared. A distinguished philosopher has asked the question, why no British name is ever mentioned in the list of mathematicians who followed Newton in his brilliant career, and completed the magnificent edifice of which he laid the foundation?[2] May we not make the question more special by asking why the University which he instructed and adorned, which possessed such noble endowments, and which claims the honour of having first adopted and taught his philosophy, did not rear a younger son, or even a sickly child, that could be ranked in the great family we have named? Scotland contributed

[1] Laplace, *Système du Monde*, p. 340.

Professor Playfair adds, that this was "the more remarkable, as the interests of navigation were deeply involved in the question of the lunar theory, so that no motive which a regard to reputation or to interest could create was wanting to engage the mathematicians of England in the inquiry."—*Edinburgh Review*, vol. xi. p. 280. Jan. 1808.

her Maclaurin, but England no European name ; and a century and a half passed away till Airey and Adams adorned the birth-place of Newton's genius. In the same spirit in which we have asked these questions, M. Arago, equally jealous of the glory of his country, has freely confessed, "that no Frenchman can reflect, without an aching heart, on the small participation of his own country in the memorable achievement of the discovery of universal gravitation ;" and Mr. Grant, the latest historian of physical science,[1] in responding to this liberal sentiment, has added, in the language of just severity, that "*if an Englishman could be supposed to be equally sensitive*, he has ample reason to regret the inglorious part his country played during the long period which marked the development of the Newtonian theory."[2]

In the imperfect state in which the differential calculus was left by Newton and Leibnitz, its inventors, it was not fitted to

[1] *History of Physical Astronomy*, &c. p. 108. London, 1852. Mr. Grant also remarks, "that with the exception of Maclaurin and Thomas Simpson, hardly any individual of these islands deserves even to be mentioned in connexion with the history of physical astronomy during that period ;" and that, at the beginning of the present century, "there was hardly an individual in this country who possessed an intimate acquaintance with the methods of investigation which had conducted the foreign mathematicians to so many sublime results."

[2] Referring our readers to the statements at the end of Chapter IV., as showing the probable cause of the success of the French mathematicians, and of the inglorious failure of our own, we beg their attention to the following confirmation of our views by one of the wisest and most eminent of our Scottish mathematicians. In a review of Laplace's *Système du Monde*, Professor Playfair makes the following observations.

"The literary institution which has most completely produced its effect of any in modern times, and that has been most successful in promoting the interests of science, is that of the Royal Academy of Sciences of Paris, where *small pensions and great honours*, bestowed on a few men for devoting themselves exclusively to works of invention and discovery, have been the means of advancing the mathematical sciences in France to a state of unexampled prosperity.

"In England, *where such an institution as that just mentioned was wanting*, and where the public is perpetually prepared, with the question, *cui bono*, to repress what seems the luxury of science, the same progress has not been made ; and our mercantile prejudices have so far defeated our own purpose, that if the matter had been left to us, the theory of the moon's motion would still have been extremely imperfect, and the great nautical problem of finding the longitude could have received nothing like an accurate solution."—*Edinburgh Review*, vol. xv. p. 39. Jan. 1810.

grapple with the higher problems in physical astronomy which still remained to be solved; and it was fortunate for the future progress of the science, that distinguished mathematicians directed themselves to the improvement of the infinitesimal calculus, and to the discovery of new mechanical principles, or extended applications of those already known.

In 1727, the very year in which Newton died, Christian Mayer published in the Petersburg Commentaries, a valuable memoir on the application of algebra to geometry; and the geometrical theorems which he demonstrated, formed the basis of the *Arithmetic of Sines*, for which Euler provided a notation and an algorithm, which has rendered it one of the most simple and valuable instruments of astronomical research. The invention of the calculus of *Partial Differences* by D'Alembert, which he first made known in 1747, was particularly applicable to the more difficult problems in physical astronomy, and when improved and extended by Euler, it became an invaluable instrument in every inquiry which demanded the aid of the pure or mixed mathematics.

But however valuable were these instruments of analysis, the *calculus of variations* discovered by Lagrange in 1760, was the greatest step in the improvement of the infinitesimal calculus which was made in the last century. It not only afforded the most complete solution of the problems that gave rise to it, but had an application of the most extensive kind, exceeding even the expectations of its inventor. Euler, who had made some progress in the same subject, at once acknowledged the superiority of his youthful rival, and with a nobility of mind not frequently displayed even by the greatest men, he renounced his own less perfect methods, and devoted himself to the study and extension of the new calculus.[1]

Nearly twenty years after the death of Newton, Euler, Clairaut, and D'Alembert were engaged in solving what has

[1] See the Article MATHEMATICS in the *Edinburgh Encyclopædia*, vol. xiii. p. 380, where Sir John Herschel pronounces a beautiful eulogy on the conduct of Euler.

been called the *problem of three bodies*,—that is, the determination of the motion of *one* body revolving round a *second* body, such as the *moon* round the *earth*, and disturbed by the attractions of a *third* body, such as the *sun*. The rigorous solution of this problem is beyond the reach of human genius, and the imperfect solution which has been obtained is only an approximate one depending for its accuracy on the more or less advanced state of the infinitesimal calculus. But even if the problem of three bodies had been susceptible of an accurate solution, it would not have diminished the difficulty of solving the more general problem of finding the motion of a planet, when simultaneously acted upon by all the other planets of the system. In this case the disturbances are very small, and when the separate action of each planet upon the disturbed body is determined, the sum of the perturbations, when applied to the place of the planet in its elliptic orbit, will give its true place in the heavens as seen from the centre of the sun.

When the three bodies are the sun, the moon, and the earth, the disturbance of the moon's motions by the action of the sun is very considerable, and hence the theory of the moon was the first subject to which the continental mathematicians directed their attention. The determination of the longitude at sea by observing the distance of the moon from the stars, had given a peculiar interest to the construction of accurate tables for computing the moon's place, and the Board of Longitude in England had offered a high reward. Mathematicians were urged to the inquiry by the united motives of wealth and fame. Newton had explained only five of the principal equations of the moon's orbit, and it was obvious that there were many other irregularities which observation alone was incapable of detecting. Clairaut seems to have been the first of the three mathematicians who undertook this inquiry; but however this may be, the competitors arrived at the same goal with nearly equal success. Clairaut had at first endeavoured to compute the lunar inequalities by the method of Newton, but he was obliged to abandon it, and appeal to the higher powers of analysis. In 1746, Euler drew up a set of lunar tables, founded on the results of his researches, but they were not found to be very superior to those in common use. In 1754, Clairaut and D'Alembert published lunar tables, embodying the results of their theory. Those of Clairaut were far superior to any that had hitherto been published, while those of D'Alembert were very inferior in accuracy. Encouraged by the failure of his rivals, Euler resumed his investigations in 1755, and published a more complete set of lunar tables, along with his researches on the lunar theory; but though more conformable with observation than his former set, they had not that degree of accuracy which was required for the determination of the longitude.

While the mathematicians, trusting too much to theory, were thus baffled in the useful application of their own results, a sagacious practical astronomer directed his attention to the improvement of the lunar tables, and carried off the prize. Tobias Mayer of Göttingen, comparing the results obtained by Euler with a number of accurate observations made by himself and others, produced a set of tables which, when compared with the observations of Bradley, gave the moon's place within *thirty seconds*. These tables were sent to the English Board of Longitude in 1755, in competition for the prize; but they did not possess that degree of precision which was required. Mayer, however, continued till the day of his death to add to their accuracy, and he left behind him a complete set of solar and lunar tables, for which the Board of Longitude awarded his widow the sum of *three thousand pounds*, a portion of the reward which they had offered for the discovery of the longitude. These tables were first published in 1770, and their greatest error was found never to exceed *one minute and a quarter*. As these tables were founded on Euler's theorems, the Board presented this distinguished mathematician with the sum of *three hundred pounds*. Though advanced in years, Euler was full of intellectual life, and having continued to labour at the lunar theory, he constructed a new set of tables, which were published in 1771, and were rewarded by the Board of Longitude in France.

Notwithstanding the accuracy of Mayer's tables, an irregularity had been discovered by observation which was not indicated by the theory of gravity. Halley and other astronomers had placed it beyond a doubt that the moon performed her monthly revolution round the earth in a shorter time than formerly. This acceleration of the moon, as it was called, amounted to nearly *ten seconds* in a century, and various hypotheses were framed to account for it. The most plausible of these was, that all space was filled with an ethereal medium which opposed such a resistance to the motions of the planets, that the force which kept them in their orbit would gradually overpower their diminished velocity, and thus shorten their period round the central body. This hypothesis was supported by Euler, and by the abettors of the undulatory theory, who required the existence of a medium for the propagation of light, and it was adopted with equal eagerness by another class of theorists, who saw in the acceleration of the celestial motions the process by which the Almighty was to destroy the solar system, by precipitating the secondary planets upon their primaries, and the primary planets upon the sun. Laplace admitted the sufficiency of the hypothesis, but as he saw no reason for admitting the existence of a resisting medium, he did not consider himself warranted in adopting such an hypothesis till it was found that gravitation was incapable of accounting for the fact. Another theory of the moon's acceleration was founded on the supposition that the daily motion of the earth was retarded by the continued blowing of the easterly winds of the tropics against the mountain ranges which extend from the equator to the poles ; but Laplace satisfied himself, from a rigorous examination of this supposition, that no retardation of the earth's motion could be thus produced. Another hypothesis still remained to which astronomers might appeal not only for the explanation of the moon's acceleration, but also of some considerable inequalities in the motions of Jupiter and Saturn, which appeared not to have a periodical character, and therefore to be in the same category with the moon's acceleration. Newton and every other philosopher had taken it for granted that the force of gravity was propagated instantaneously from bodies, and not in time like the rays of light ; but it occurred to Laplace that if time was required for the transmission of gravity, it would affect the intensity of the force. He therefore computed the velocity of gravity that would be required to produce the observed acceleration, and he found it to be *eight millions* of times greater than the velocity of light, that is 192,500 miles multiplied by 8,000,000, or 1,540,000,000,000 miles in a second—a velocity which no language can express. After arriving at this result, Laplace found that if the acceleration is produced by another cause, then the effect of the successive transmissions would be insensible, and consequently the velocity of gravity, if it is not instantaneous, must at least be *fifty millions* of times greater than that of light, that is, must be at least 9,625,000,000,000 miles in a second.

In the course of these investigations it had been placed beyond a doubt that every inequality in the motion of the planets, and in the form of their orbits produced by their mutual gravitation, must be periodical, that is, that the inequality, after reaching its maximum, will diminish according to the same law by which it increased, and hence it became doubly interesting to discover the cause of phenomena which had this character. Although foiled in so many attempts to refer the moon's acceleration to the action of gravity, Laplace returned to the inquiry with fresh zeal, and about the end of 1787, his labours were crowned with success. It was well known to Lagrange and to himself, that the eccentricities of the planetary orbits underwent extremely slow changes, which had a very long period. To such a change the eccentricity of the earth's orbit is subject from the action of the planets. The mean action of the sun must therefore vary with the earth's eccentricity, and the earth, thus exerting a greater or a less force over the moon, will accelerate or retard her, and thus produce the secular inequality which has been observed in her mean motion. When the eccentricity is diminishing, which it has been doing since the date of the earliest astronomical observations, the moon's mean motion will be accelerated ; but when the diminution ceases, and the orbit returns to its former ellipticity, the sun's action will increase, and the moon's mean motion will be retarded.[1] Laplace found the acceleration to be *ten seconds* during a century,—a rate which, notwithstanding its variable character, may be considered as uniform for two thousand years.

Although Halley suspected the existence of this inequality so early as 1693, yet it is to Mr. Dunthorne that we owe the first accurate determination of its magnitude. By means of lunar eclipses observed at Babylon in 721 B.C., and at Alexandria in 201 B.C.,—a solar eclipse observed by Theon, A.D. 364, and other two by Ibyn Jounis at Cairo, about the end of the tenth century, he found the acceleration to be *ten* seconds in a hundred years.[2] The consequence of this inequality is, that the moon is about *two hours* later in coming to the meridian than she would have been had she performed her monthly revolution in the same time that she did when the earliest Chaldean observations were made. "It is indeed a wonderful fact in the history of science," as Mr. Grant remarks, "that these rude notes of the priests of Babylon should escape the ruins of successive empires, and finally, after the lapse of three thousand years, should become subservient in establishing a phenomenon of so refined and complicated a character as the inequality we

[1] M. Leverrier has recently shown that the earth's eccentricity will diminish during the period of *twenty-four thousand years!*

[2] Mr. Airy has more recently found by discussing three ancient total eclipses (Aug. 15, B.C. 309 ; May 19, B.C. 556 ; May 28, B.C. 584), that the secular acceleration is at least 12′ 12″, as adopted by Hansen in his Lunar Tables. See *Memoirs of the Astronomical Society*, vol. xxvi.

have just been considering."[1] And in referring to the long period of the same inequality, Professor Playfair remarks, that "two thousand years are little more than an infinitesimal in this reckoning; and as an astronomer thinks that he commits no error when he considers the rate of the sun's motion as uniform for twenty-four hours, so he commits none when he regards the rate of this equation as continuing the same for twenty centuries. That man, whose life, nay, the history of whose species occupies such a mere point in the duration of the world, should come to the knowledge of laws that embrace myriads of ages in their revolution, is perhaps the most astonishing fact that the history of science exhibits."[2]

By this great discovery, which had eluded the grasp of Euler and Lagrange,[3] Laplace may be regarded as having completed the lunar theory exactly one hundred years after it had been sketched out in the first edition of the Principia.

The curious subject of the moon's acceleration has recently excited much interest in consequence of Professor Adams[4] having proposed an important correction upon the theory of Laplace, by which the secular acceleration was reduced to 6″·11. M. Delaunay, by a different process, has since found it to be 6″·11, exactly the same as that obtained by Mr. Adams.[5] M. Plana, the distinguished Sardinian mathematician, and M. Pontecoulant,[6] still adhere to the theory of Laplace, on grounds which are not yet published. M. Delaunay is of opinion that

[1] *History of Physical Astronomy*, pp. 63, 64.

[2] *Edinburgh Review*, vol xi. p. 261

[3] The Academy of Sciences proposed the moon's acceleration as the subject of their prize for 1770. Euler gained it, but came to the conclusion that it was not produced by the force of gravity. The same subject was again proposed in 1772, and the prize was divided between Euler and Lagrange. Euler ascribed the acceleration to a resisting medium, and Lagrange evaded the difficulty. The prize was again offered in 1774, and was gained by Lagrange, and he now doubted the existence of the inequality. It was under these circumstances that Laplace took up the subject, and obtained the results which we have mentioned.

[4] *Phil. Trans.* 1853, p. 398.

[5] *Comptes Rendus*, tom. xlviii. pp. 137, 249, 817, 1031; tom. xlix. p. 309.

[6] *Id.* tom. xlviii. p. 1023.

this difference has arisen from M. Plana having regarded the equal description of areas as constant, whereas it is variable. The difference between the results of MM. Adams and Delaunay, if correct, and those obtained from ancient eclipses, is very remarkable, and indicates the operation of some cause which remains to be discovered.

The theory of the lunar motions being thus completed, Euler, Lagrange, and Laplace directed all the powers of their mind, and all the refinements of analysis, to the determination of the mutual action of the primary planets. In this case the three bodies were the sun, the disturbed and the disturbing planet. In 1748, the Academy of Sciences proposed the Inequalities of Jupiter and Saturn as the subject of their prize. In his Memoir, which gained the prize, Euler proved that both Jupiter and Saturn were subject to considerable inequalities, arising from their mutual action, but all of them periodical, and returning nearly in the same order after short intervals of not much more than twenty or thirty years. But though these results accorded with observation, they afforded no explanation of the great secular inequalities which in twenty centuries had produced in Jupiter an acceleration of 3° 33′, and in Saturn a retardation of 5° 13′. The Academy, therefore, again offered their prize of 1752 for the best Memoir on the same subject. Euler a second time carried off the prize; but though he found two inequalities of long periods depending on the angle formed by the line of the apsides of each planet, yet he made them equal and additive, contrary to observation. Lagrange failed in the same inquiry; and Laplace, after carrying his approximation farther than either of his rivals, came to the conclusion that no change in the mean motion of Jupiter and Saturn could be produced by their mutual action. Under this grave embarrassment, apparently threatening the truth or accuracy of the law of gravity, but really heralding a great discovery, Lagrange appeared with a new solution of the problem of three bodies. At the age of twenty-seven he published this solution in the Turin Memoirs for 1763, and, in applying it to the motions of Jupiter and Saturn, he obtained for the former an additive secular equation of nearly *three* seconds, and for the latter a subtractive one of *fourteen* seconds; but though this result was in its general character superior to that of Euler, it yet afforded no explanation of the great inequalities we have mentioned. Having observed that the calculus had never given any inequalities but periodical ones, Lagrange now set himself to inquire, whether in the planetary system, continually increasing or continually diminishing inequalities, affecting the mean motions, could be produced by the mutual action of the two planets. Independently of any approximation, and by a method peculiarly his own, he found that all inequalities produced by gravity must be periodical, and that amid all the changes arising from the mutual action of the planets, two elements are unchangeable—the length of the major axis of the planet's elliptical orbit, and the time in which that orbit is described. The inclination of the orbit to the ecliptic changes, the ellipse and its eccentricity change, but its greater axis and the time of the planet's revolution are unalterable. This grand discovery, excluding every source of disorder, and securing the stability of the system, is doubtless one of the noblest in physical astronomy, and more than any other displays the wisdom of the Creator.

But though Lagrange had made this great step in celestial physics, he failed in discovering the cause of the inequalities of Jupiter and Saturn, and left to Laplace the honour of solving this perplexing problem. By a more rigorous inquiry into the effects of their mutual action, Laplace found that the mean motion of Jupiter would be accelerated, while that of Saturn would be retarded, and that the relative derangement of the two planets would be as *five* to *ten*, the ratio of their mean motion, or as 3° 58′ to 5° 16′, the result for Jupiter differing only *nine* minutes from that given by Halley. In continuing the inquiry, he found that each planet was subject to an inequality whose period was 969 years;—that of Saturn, when a maximum, being 48′ 44″, and that of Jupiter 20′ 49″, with an opposite sign. These inequalities were a maximum in 1560, and from that epoch the apparent mean motions of the two planets have been approaching to their true mean motions, and became the same in 1790. By a comparison of these results with forty-three observed oppositions of Saturn, Laplace found them generally correct, and the error never exceeding *two minutes* of a degree. This difference he afterwards reduced in the case of both planets to *twelve seconds*, although the best tables of Saturn often erred *twenty minutes*. By these brilliant researches theory and observation were reconciled,—the last difficulty which beset the Newtonian theory was removed,—every inequality in the Solar System was explained,—and the law of gravitation established as a law of the universe.

In concluding this brief notice of the progress of physical astronomy since the time of Newton in a few of its leading features, we are naturally led to ponder on the great truth of the stability and permanence of the solar system as demonstrated by the discoveries of Lagrange and Laplace. In the present day, when worlds and systems of worlds, when life physical and life intellectual are supposed to be the result of general law, it is interesting to study those conditions of the planetary system which are necessary to its stability, and to consider whether they appear to be the result of necessity or design. It follows, from the discoveries of Laplace, that there are three conditions essential to the stability and permanence of the solar system, namely, the motion of all the planets in the same direction,—their motion in orbits slightly elliptical, or nearly circular,—and the commensurability of their periods of revolution. That these conditions are not necessary is very obvious. Any one of them may be supposed different from what it is, while the rest remained the same. The planets, like the comets, might have been launched in different directions, and moved in planes of various and great inclinations to the ecliptic. They might have been propelled with such varieties of tangential force as to have moved in orbits of great ellipticity; and no reason, even of the most hypothetical nature, can be assigned why their annual periods might not have been incommensurable. The arrangements, therefore, upon which the stability of the system depends, must have been the result of design, the contrivance of that omniscience which foresaw all that was future, and of that infinite skill which knew how to provide for the permanence of His work. How far the comets, whose motions are not regulated by such laws, and which move in so many directions, may in the future interfere with the order of our system, can only be conjectured. They have not interfered with it in the past, owing no doubt to the smallness of their density; and we cannot doubt that the same wisdom which has established so great a harmony in the movements of the planetary system, that the inequalities which necessarily arise from their mutual action arrive at a maximum, and then disappear, will also have made provision for the future stability of the system.

Although it is only a general view that we can take of the important discoveries in physical astronomy which have sprung from those of Newton, yet we should scarcely be justified in omitting those which relate to the figure of our earth and the tides of its ocean. Newton inferred that the figure of the earth was an oblate spheroid, whose equatorial diameter was to its polar axis in the ratio of 231 to 230, but it was reserved for Maclaurin to demonstrate, *a priori*, that the earth, if homogeneous, might assume such a form. The method which he employed, though synthetical, was remarkable for its accuracy and elegance. In 1743, Clairaut published his Treatise on the Figure of the Earth, in which he investigated the form it would assume on the supposition of its density being heterogeneous. He found that the earth would have the form of an elliptic spheroid, if its mass was arranged in homogeneous concentric strata of the same form; and he investigated the beautiful theorem which bears his name, by which we can determine the ellipticity of the earth from measures of the force of gravity, taken in two different latitudes by the aid of the pendulum. D'Alembert, Lagrange, Legendre, Laplace, Ivory, Plana, Gauss, Poisson, and Airy, have directed their attention to the subject of the earth's figure, but without adding much to the results obtained by Clairaut. In his *Mécanique Céleste*, Laplace has applied the deductions of his calculus to the determination of the figure of the earth, from the measurement of degrees on its surface, and the observations made in different latitudes on the length of a pendulum vibrating seconds, and he finds that the result cannot be reconciled with the hypothesis of an elliptic spheroid, unless a greater error than is probable be admitted in some of the measurements.[1] Upon discussing, however, all the more recent measurements of a degree, and all the observations with the pendulum, the ellipticity of the earth in the former case has been found to be $\frac{1}{299}$, and, in the latter, $\frac{1}{289}$, the ellipticity indicated by the lunar perturbation being $\frac{1}{306}$, an agreement which is very remarkable, when we consider the local causes which necessarily affect the observations with the pendulum, as first noticed by General Sabine, and the measurement of an arc of the meridian.

The theory of the tides of our ocean, though treated by a master mind in the Principia, was nevertheless susceptible of extension and improvement. The Academy of Sciences proposed it as the subject of their prize for 1740. Four dissertations competed for the prize, three of them of great merit, by Euler, Daniel Bernouilli, and Maclaurin, and a fourth by Father Cavalleri, a Jesuit, who founded his investigation on the system of Vortices. The prize was divided among all the four competitors,—a proof, doubtless, that the Cartesian doctrines were not entirely exploded. These dissertations, and others on the same subject, are founded on what is called the equilibrium theory, which supposes that the sun and moon draw the waters of the ocean into the form of an aqueous spheroid, in which

[1] *Mécanique Céleste*, tom. ii. liv. iii. chap. v.; and *Système du Monde*, liv. iv. chap. vii.

the molecules of water are maintained at rest by the action of these forces. In consequence, however, of the daily motion of the earth, such a spheroid never can be formed,—there can only be a tendency to it ; and hence the tides are the consequence of the perpetual oscillation of the waters of the ocean,—a result which the state of mechanical and mathematical science will not allow us to determine. Laplace, however, undertook the task, and communicated to the Academy of Sciences in 1755, 1779, and 1790, a series of valuable memoirs on the subject. The theory to which he was led by these researches rests upon two suppositions not strictly true, namely, that the earth is covered with water, and that the depth of the ocean is uniform under the same parallel of latitude. Regarding every particle of water as under the influence of three forces, namely, the attraction of the earth, the attraction of the sun and moon, and that which arises from the earth's rotation, he found that three kinds of oscillation are produced ; the first depending on the sun and moon, and varying periodically, so as not to return till after a long interval ; the second depending on the earth's rotation, and returning in the same order after the interval of about a day ; and the third depending on double the angular rotation of the earth, and returning after an interval of about half a day. As the oscillations of the second class are affected by the depth of the sea as well as the earth's rotation, and as the differences between the two tides in the same day depend chiefly upon them, Laplace has from this been able to determine that the mean depth of the sea is about four leagues. The general correctness of this theory has been placed beyond a doubt by a comparison of its results, with observations on the tides made at Brest during a long succession of years.[1]

As the ocean is often agitated by several irregular causes, such as storms and earthquakes, which raise it to great heights, and sometimes make it overstep its limits, Laplace has endea-

[1] *Mécanique Céleste*, part i. liv. iv. chap. i. tom. ii. p. 171 ; and *Système du Monde*, liv. iv. chap. x. p. 248.

voured to ascertain the "stability of the equilibrium of our seas." Although we find that the sea falls into its hollow bed after the ordinary commotions to which it is subject, yet we may reasonably fear that some extraordinary cause may communicate to it such a disturbance, that, though inconsiderable in its origin, may go on increasing till it raises it above the highest mountains. As such a result would afford an explanation of several phenomena of natural history, it becomes interesting to determine the conditions necessary to the absolute stability of the equilibrium of our seas, and to see if these conditions exist in nature. In submitting this question to analysis, Laplace has found that *the equilibrium of the ocean is stable if its density is less than the mean density of the earth*, and that its equilibrium cannot be subverted unless these two densities are equal, or that of the earth less than that of its waters. The experiments on the attraction of Schehallion and Mount Cenis, and those made by Mr. Cavendish, Reich, and Bailey, with balls of lead, demonstrate that the mean density of the earth is at least *five* times that of water, and hence the stability of the ocean is placed beyond a doubt. As the seas, therefore, have at one time covered continents which are now raised above their level, we must seek for some other cause of it than any want of stability in the equilibrium of the ocean.[1]

We have already seen how Newton deduced the precession of the equinoxes from the action of the sun and moon upon the excess of matter accumulated at the equator of the terrestrial spheroid. This investigation, however, was founded on principles not rigorously correct, and therefore the complete solution of the problem was left to his successors. The discovery, too, of the nutation and of its cause, by Bradley, gave a new character to the investigation, which now required the aid of the calculus of partial differences. It fell to the lot of D'Alembert to give a complete solution of the problem, whatever were the figure

[1] See *Mécanique Céleste*, part i. liv. iv. chap. ii. tom. ii. p. 204; and *Système du Monde*, liv. iv. chap. xi. p. 265.

and the density of the strata of the terrestrial spheroid. The results which he obtained agreed accurately with observations on the precession, and he obtained also the true measure of the nutation, or the dimensions of the small ellipse described by the pole of the equator, which the observations of Bradley had left in some uncertainty.

In viewing the subject under a more general aspect than D'Alembert, Laplace was led to some very interesting results. From his researches on the oscillations of the ocean, he was led to the remarkable theorem, "that whatever be the law of the depth of the sea, and the form of the spheroid which it covers, the phenomena of the precession and the nutation are the same as if the sea formed a solid mass with this spheroid." Laplace has also shown that the rotation of the earth upon its axis, or the length of the day, cannot be affected either by currents on the ocean, rivers, trade-winds, or even earthquakes, or in general any force which can shake the earth either in its interior or upon its surface. It might have been expected that the trade-winds blowing between the tropics would, by their action upon the sea, and upon the continents and mountains which they meet, insensibly diminish the rotatory motion of the earth; but upon the same principle the other motions of the atmosphere, which take place beyond the tropics, would accelerate that motion by the same quantity. In order to produce any sensible change in the length of the day, a very considerable displacement in the parts of the earth would be required. A great mass of matter, for example, transported from the poles to the equator, would increase the length of the day, and it would be diminished if dense bodies approached either pole, or the axis of the earth. But as there appears to be no cause which is capable of displacing masses sufficiently large to produce such effects, we may regard the length of the day as one of the most unchangeable elements in the system of the world. "The same thing is true," as Laplace observes, "with respect to the points where the earth's axis meets its surface. If this planet turned successively round different diameters inclined to one another at considerable angles, the equator and the poles would change their place upon the earth ; and the seas on rushing to the new equator, would cover and uncover alternately the highest mountains ; but all the researches which I have made on the displacement of the poles of rotation at the surface of the earth, have proved to me that it is insensible."[1] After discussing the consequences respecting the constitution of the earth, which are accordant with his theory of the precession and nutation, Laplace states, that though it does not enable us to determine the ellipticity of the earth, it fixes its limit between $\frac{1}{304}$ and $\frac{1}{578}$ part of the radius of the equator. The same theory indicates as the most probable constitution of the earth, that the density of its strata increases from its surface to its centre.[2]

Such is a brief and general view of the important discoveries in physical astronomy which have illustrated the century that followed the publication of the Principia. Brilliant as they are, and evincing as they do the highest genius, yet the century in which we live has been rendered remarkable by a discovery which, whether we view it in its theoretical relations, or in its practical results, is the most remarkable in the history of physical astronomy. In the motions of the planet Uranus, discovered since the time of Newton, astronomers had been for a long time perplexed with certain irregularities, which could not be deduced from the action of the other planets. M. Bouvard, who constructed tables of this planet, seeing the impossibility of reconciling the ancient with the modern observations, threw out the idea that the irregularities from which this discrepancy arose might be owing to the action of an unknown planet. Our countryman, the Rev. Mr. Hussey, conceived "the possibility of some disturbing body beyond Uranus;" and Hansen, with whom Bouvard corresponded on the subject, was of opinion that there must be

[1] *Système du Monde,* liv. iv. chap. xiii. pp. 276, 277. See also *Mécanique Céleste,* part i. liv. v. chap. i. tom. ii. p. 347. [2] *Méc. Céleste,* tom. ii. pp. 354, 355.

two new planets beyond Uranus to account for the irregularities. In 1834, Mr. Hussey was anxious that the Astronomer-Royal should assist him in detecting the invisible planet, and other astronomers expressed the same desire, to have so important a question examined and settled. On his return to Berlin from the meeting of the British Association in 1846, the celebrated astronomer, M. Bessel, commenced the task of determining the actual position of the planet; but in consequence of the death of M. Flemming, the young German astronomer to whom he had intrusted some of the preliminary calculations, and of his own death not long afterwards, the inquiry was stopped.

While the leading astronomers in Europe were thus thinking and talking about the possible existence of a new planet beyond the orbit of Uranus, two young astronomers, Mr. Adams of St. John's College, Cambridge, and M. Leverrier of Paris, were diligently engaged in attempting to deduce from the irregularities which it produced in the motions of Uranus, the elements of the planet's orbit, and its actual position in the heavens. In October 1845, Mr. Adams had solved this intricate problem—the *inverse problem of perturbations*, as it has been called, placing beyond a doubt the theoretical existence of the planet, and assigning to it a place in the heavens, which was afterwards found to be little more than a single degree from its exact place! Anxious for the discovery of the planet in the heavens, Mr. Adams communicated his results to the Astronomer-Royal and Professor Challis; but more than *nine* months were allowed to pass away before a single telescope was directed in search of it to the heavens. On the 29th July, Professor Challis began his observations, and on the 4th and 12th of August, when he directed his telescope to the theoretical place of the planet as given him by Mr. Adams, *he saw the planet, and obtained two positions of it.*

While Mr. Adams was engaged in this important inquiry, M. Leverrier, who had distinguished himself by a series of valuable memoirs on the great inequality of Pallas,—on the perturbations of Mercury,—and on the rectification of the orbits of comets, was busily occupied with the same problem. In the summer of 1845, M. Arago represented to Leverrier the importance of studying the perturbations of Uranus. Abandoning his researches on comets, he devoted himself to the task suggested by his friend, and on the 10th November 1845, he communicated to the Academy of Sciences his *First Memoir on the Theory of Uranus.* In the following June he submitted to the Academy his Second Memoir, entitled, *Researches on the Motions of Uranus,* in which, after examining the different hypotheses that had been adduced to explain the irregularities of that planet, he is driven to the conclusion that *they are due to the action of a planet situated in the ecliptic at a mean distance double that of Uranus.* He then proceeds to determine where this planet is actually situated, what is its mass, and what are the elements of the orbit which it describes : After giving a rigorous solution of this problem, and showing that there are not two quarters of the heavens in which we can place the planet at a given epoch, he computes its heliocentric place on the 1st January 1847, which he finds to be in the 325th degree of longitude, and he boldly asserts that in assigning to it this place, he does not commit an error of more than 10°. The position thus given to it is within a degree of that found by Mr. Adams. Anxious, like Mr. Adams, for the actual discovery of the planet, M. Leverrier naturally expected that practical astronomers would exert themselves in searching for it. The place which he assigned to it was published on the 1st of June, and yet no attempt seems to have been made to find it for nearly five months. The *exact position* of the planet was published on the 31st August, and on the 18th September was communicated to M. Galle, of the Royal Observatory of Berlin, who discovered it as a star of the eighth magnitude the very evening on which he received the request to look for it. Professor Challis had *secured* the discovery of this remarkable body six weeks before, but the honour of having actually found it belongs to the Prussian astronomer.

With the universal concurrence of the astronomical world, the new planet received the name of NEPTUNE. It revolves round the sun in about 172 years, at a mean distance of thirty, that of Uranus being nineteen, and that of the Earth one; and by its discovery the Solar System has been extended *one thousand millions of miles* beyond its former limits.

The honour of having made this discovery belongs equally to Adams and Leverrier. It is the greatest intellectual achievement in the annals of astronomy, and the noblest triumph of the Newtonian Philosophy. To detect a planet by the eye, or to track it to its place by the mind, are acts as incommensurable as those of muscular and intellectual power. Recumbent on his easy chair, the practical astronomer has but to look through the cleft in his revolving cupola, in order to trace the pilgrim star in its course; or by the application of magnifying power, to expand its tiny disc, and thus transfer it from among its sidereal companions to the planetary domains. The physical astronomer, on the contrary, has no such auxiliaries: he calculates at noon, when the stars disappear under a meridian sun: he computes at midnight, when clouds and darkness shroud the heavens; and from within that cerebral dome, which has no opening heavenward, and no instrument but the Eye of Reason, he sees in the disturbing agencies of an unseen planet, upon a planet by him equally unseen, the existence of the disturbing agent, and from the nature and amount of its action, he computes its magnitude and indicates its place. If man has ever been permitted to see otherwise than by the eye, it is when the clairvoyance of reason, piercing through screens of epidermis and walls of bone, grasps, amid the abstractions of number and of quantity, those sublime realities which have eluded the keenest touch, and evaded the sharpest eye.

Although the philosophy of Newton has since his day enjoyed such signal triumphs, it has yet other strongholds to storm, and other conquests to achieve. In his survey of the sidereal and planetary domains, the practical astronomer has in the present century laid open new fields of research ripe for the intellectual sickle, and fitted to yield to the accomplished analyst the richest harvest of discovery.

Within our own system the detection of a satellite to Neptune, by Mr. Lassels,—of an eighth satellite to Saturn, by Mr. Lassels and Mr. Bond, between the orbits of the 4th and 5th of these bodies,—and of a new fluid ring gradually advancing to the body of the planet, will furnish interesting materials to the physical astronomer. This new and remarkable feature in the system of Saturn has been recently studied by Mr. Bond of the United States, and M. Otto Struve, at the observatory of Pulkova, with the great Munich telescope. With that fine instrument they saw distinctly the dark interval which separates this new ring from the two old ones, and the boundaries of this interval were so well marked, that they succeeded in measuring its dimensions. They perceived, also, at the inner margin of the new ring, an edge or border feebly illuminated, which they conceived might be the commencement of another similar appendage, though the line of separation had not yet become visible. The following are the principal results which these two able astronomers have obtained :—" 1. The new ring is not subject to very rapid changes. 2. It is not of very recent formation ; for it is quite certain that it has been seen, if not recognised, according to its true character, ever since the improvements upon astronomical telescopes have enabled astronomers to see the belts upon the surface of the planet, or at least since the beginning of the last century. 3. *That the inner border of the annular system of Saturn has, since the time of Huygens, been gradually approaching to the body of the planet*, and therefore it follows, that there has been a successive enlargement of this system. 4. That it is at least very probable that the approach of the rings towards the planet is caused particularly by the successive extension of the inner or middle ring. Hence it follows, that Saturn's system of rings does not exist, as has been generally supposed, in a state of stable equilibrium, and that *we may expect sooner or later, perhaps in some dozen of years, to see the rings united with the body of the planet.*"[1]

Of all the celestial phenomena which have been discovered since the time of Newton, the most remarkable are the *fifty-six* small planets which have been discovered between the orbits of Mars and Jupiter. Dr. Olbers of Bremen, who discovered two of them, hazarded the idea that a large planet which had once occupied the same place, had been burst in pieces by some internal force. This opinion, which has been long considered as a very probable one, has only recently been called in question. M. Leverrier, the first mathematician who has directed his attention to the theory of this remarkable group of bodies, considers the opinion of Olbers as contradicted by the great inclination of the orbit of Pallas; and in place of explaining the existence of these planets by an alteration of the primitive system of the universe, he believes "that they have been regularly formed like all the other planets, and in virtue of the same laws." In a very interesting communication on this subject, lately made to the Academy of Sciences,[2] M. Leverrier has endeavoured to ascertain the limit of the sum of the magnitudes of the whole group, known and unknown, by the disturbing action which they exercise on the motion of the perihelion of Mars and the Earth. If the perihelions of these small planets were distributed uniformly in all the regions of the zodiac, the action of these masses of matter, situated in one half or semi-circumference of the heavens, would be destroyed by the action of the equal masses situated in the opposite half

[1] Laplace has shown that the stability of the equilibrium of the rings requires that they be irregular solids, unequally wide in different parts of their circumference, so that their centres of gravity do not coincide with their centres of figure.—See *Mécanique Céleste*, part i. liv. iii. chap. vi. tom. ii. p. 155; *Système du Monde*, liv. chap. viii. p. 242.

[2] *Considérations sur l'ensemble du Système des petites Planètes situées entre Mars et Jupiter, par* M. U. J. Leverrier. Lu 28 Nov. 1853. *Comptes Rendus*, &c., tom. xxxvii. pp. 793-798.

or semi-circumference. But M. Leverrier finds that *twenty* out of *twenty-six* of the planets have the longitudes of their perihelion between 4° and 184°, a semi-circumference of the heavens, and hence their action as one mass on Mars and the Earth is not destroyed by the action of the other six planets. It is possible that the small planets, which may yet be discovered, may have more of their perihelions in the latter of their semi-circumferences than in the former; but the possibility is that there will be more of them conjoined with the larger than the smaller group, or, at least, that they will be equally diffused over the zodiac in reference to their perihelion points.

Having shown that the perihelion of Mars is placed much more advantageously than that of the Earth, in relation to the mean direction of the perihelions of the small planets, and that the greater eccentricity of the orbit of Mars is more favourable for determining the amount of their action, he finds that if the total mass of the small planets were equal to the mass of the Earth, it would produce in the heliocentric longitude of the perihelion of Mars, an inequality which in a century would amount to *eleven seconds*, a quantity which could not have escaped the notice of astronomers. Considering, therefore, that this inequality would become particularly sensible at the oppositions of Mars, M. Leverrier is led to believe, that though the orbit of Mars has not received its final improvements, yet it will not admit of an error in longitude greater than *one-fourth* of the above quantity, and hence he concludes, *that the sum total of the matter constituting the small planets situated between the mean distances* 2·20 *and* 3·16, *cannot exceed about the fourth part of the mass of the Earth.*

In examining the place of the nodes of twenty-six of the small planets, M. Leverrier finds that *twenty-two* of the ascending nodes of their orbits have their longitudes between 36° and 216°, that is, within a semi-circumference of the heavens,[1] a result almost

[1] M. Leverrier takes occasion to remark, "that we might perhaps find some systematic difference between the mean direction of the ascending nodes of the planets near the

the same as that which takes place in their perihelions. From this fact he observes, that in considering the motion of the plane of the ecliptic, we may arrive at conclusions of the same kind respecting the magnitude of the mass of the small planets, though the limit would be less strict than in that which is derived from the grouping of their perihelions.

In his theory of the motion of comets, Sir Isaac Newton did not anticipate that bodies of this kind would be discovered moving in elliptical orbits, contained within the limits of our own system, and thus affording a new application of the law of gravity, and remarkable examples of the action of the planets upon this new class of wandering stars. It had long been the universal belief among astronomers, that every comet strayed far beyond the limits of our system, the shortest period being about seventy years. In 1818, however, M. Pons announced the discovery of a very faint comet, without a tail, the motions of which could not be reconciled with a parabolic orbit. After its fourth appearance, Professor M. Encke of Berlin, whose name is now attached to the comet, found that it moved in an elliptic orbit with a period of about 1211 days, or three years and a third, and that its orbit was included within our system, extending inward as far as Mercury, and outward only a little beyond the orbit of Pallas. He computed the perturbations produced by the action of Venus, the Earth, Mars, Jupiter, and Saturn, and he found that its periods had been diminishing between 1786 and 1838, at the rate of about $2\frac{1}{2}$ hours in each revolution—an effect which he ascribed to the resistance of an ethereal medium.

A still more remarkable comet, supposed to be the same as that of 1772, 1805, 1839, &c., was discovered in 1826 by M. Biela. Its period was found to be about 2410 days, or $6\frac{3}{4}$ years, and its orbit did not reach so far as that of Saturn.

Sun, and that of the ascending nodes of the more distant planets, and that we may thus conjecture that these planets belong in reality to three distinct groups."—*Comptes Rendus*, &c., tom. xxxvii. p. 795.

M. Damoiseau found that its arrival at its perihelion would be retarded nine days and sixteen hours by the action of Saturn, Jupiter, and the Earth ; and that on the 29th October 1832, about a month before its perihelion passage, it would cross the plane of the ecliptic, within 18,000 miles of a point in the Earth's orbit. The announcement of this fact excited such an alarm in Paris, that M. Arago was summoned to allay the fears of the community. According to prediction, the comet returned in 1839 and 1846 ; but, strange to say, it was on this last occasion *separated into two distinct comets*, the one a little fainter than the other. Their tails were parallel, and their distance, which was the same till the comet became single by the gradual disappearance of the smaller one, was found by M. Plantamour to be equal to about two-thirds of the radius of the moon's orbit, that is, about 160,000 miles ![1]

Another comet belonging to our system was discovered by M. Faye in November 1843. Dr. Goldsmicht found that its period was about 2718 days, or $7\frac{1}{2}$ years, and M. Leverrier computed that its arrival would be delayed 7 days and 16 hours by the action of the planets. Its orbit is more circular than that of any other comet, and is included between the orbits of Mars and Saturn. It had been suggested by M. Valz, that this comet might be Lexell's comet of 1770,[2] which had been rendered visible by the action of Jupiter in 1767, and which was afterwards thrown into a larger orbit and rendered invisible in 1779 by the action of the same planet. M. Leverrier, however, has shown that the two bodies cannot be identical.

Before another year had expired, a fourth comet belonging to our system was discovered by M. De Vico of Rome. He first

[1] Sir John Herschel has ventured to say, "that the orbit of Biela's comet so nearly intersects that of the Earth, that an actual collision is not impossible, and indeed (supposing neither orbit variable) must, *in all likelihood*, happen in the lapse of some millions of years."—*Outlines of Astronomy*, § 585.

[2] This comet ought to have appeared *thirteen* times since 1770, and, as it has not been since seen, it must be lost. Burckhardt supposed that it might have become a satellite to Jupiter, from its aphelion being near that planet !

saw it on the 29th August 1844, and M. Faye found that it revolved in an elliptic orbit with a period of about 2000 days, or $5\frac{1}{2}$ years. It was supposed by some astronomers that this comet was the same as that of 1585, observed by Tycho; but M. Leverrier has shown that they are not identical, and that the comet of De Vico is not the same as that of Lexell. He discovered, however, such a striking similarity between it and the comet observed by De la Hire in 1678, that he considers them clearly identical. It is strange, however, that this comet should only have been seen once previous to 1844, although it has frequently come very near the Earth.

Another comet of the Solar system was discovered by M. Brorsen on the 26th February 1846. Its period is 2042 days, or about $5\frac{4}{7}$ years. It is very faint, and is almost identical in its elements with the comet of 1532.

A seventh comet, discovered by M. Peters on the 26th June 1846, has been placed by the calculations of M. Arrest among those having elliptic elements and a short period, and therefore belonging to our system. Its period is 5804 days, or about 16 years.

Such are some of the important celestial phenomena within the limits of our own Solar system, to which the Newtonian theory is applicable, and to which it has been to some extent successfully applied. The sidereal phenomena which have been discovered beyond our system, in which movements of long periods, round visible and invisible centres, have been traced and measured, possess a higher interest, and to some of them also the Newtonian law of gravitation has been actually extended.

The most important of this class of phenomena are those of binary and multiple systems of stars. Among the many stars of this kind which have been discovered by Sir William Herschel and succeeding observers, there must be a large number in which the two, three, or four stars constituting a group have no other connexion than that of being placed nearly in the same line. There are others, however, in which, as Sir W.

Herschel long ago announced, one of two stars revolves round the other in regular orbits, and with periods which have been determined—that of Castor, being 334 years, γ Virginis 708 years, and γ Leonis 1200 years. Although the list of double stars has been greatly extended, yet those whose orbits and periods have been determined with any accuracy, amount only to twenty-one. Nine of these have been computed by Mr. Madler of Dorpat, five by Sir John Herschel, four by Mr. Hind, and one by M. Savary.[1] The first calculation of the orbit of a double star was made in 1830 by M. Savary (in the case of ξ of the Great Bear), who showed that the changes of place in one of the stars could be explained by an elliptic orbit, and a period of $58\frac{1}{4}$ years. The periods of the other twenty double stars vary from $31\frac{1}{2}$ to 737 years, eleven having their periods below 100 years, three below two hundred, two below 300, and three between 600 and 700 years. These orbits are calculated on the supposition that the force exerted by the stars varies inversely as the square of the distance, and the accuracy with which the observations are represented allows us to conclude that the Newtonian law of gravity extends to the distant region of the double stars.[2]

Another sidereal phenomenon, in which we have the appearance of motion round a centre, is displayed in the spiral nebulæ discovered by Lord Rosse; that the stars which compose these spirals have been placed there in virtue of some movement related to the central mass, cannot be doubted, although it is vain for man to attempt the solution of such a problem. To suppose these spirals to be nothing more than vaporous matter, like the tail of a comet, whirled round into spiral branches, because we cannot find any explanation compatible with the almost universally admitted fact, that every nebula is composed

[1] A table of the elements of their orbits is given by Sir John Herschel in his *Outlines of Astronomy*, § 843.

[2] M. Madler has adduced an instance (p Ophiuchi), where he regards the deviations from an elliptic orbit too considerable to be accounted for by an error of observation; but we cannot view a single fact of this kind as affecting the generality of the law of gravity.

of stars, is to renounce all faith in the great truths of astronomy, and seek for some resting-place to the mind, when reason stands aghast amid the infinite and the incomprehensible.

Beside the motions of one of the bodies which compose a binary system, a proper motion of a very peculiar kind has been observed in the stars. In one region of the heavens the distance between the stars is increasing, and in the opposite region diminishing, while in intermediate localities little or no change of place is observed. It is obvious that such changes indicate a motion of our earth, and consequently of the whole Solar system, to a point in the heavens where the increasing distance of the stars is a maximum. Before the proper motion of the fixed stars had been measured, various speculators, among whom Hooke was the earliest, hazarded the supposition that the whole Solar system was in continual motion. Tobias Mayer, in 1771, attempted in vain to deduce such a movement of the system from the proper motions of eighty stars; but a few years afterwards, in 1783, when better observations were accessible, Sir W. Herschel and M. Prévost came to the conclusion that the Solar system was advancing to a point in the heavens whose right ascension was 257°,[1] and north declination 25°. Although both Biot and Bessel came to the same conclusion as Tobias Mayer, that no such motion existed, yet the existence of a proper motion has been more recently placed beyond a doubt by the observations made at the observatories of Dorpat, Abo, and Pulkova: And it has been shown by the united studies of Argelander, Otto Struve, and Peters, that the point to which the Solar system is advancing at the epoch of 1840, is situated in,—

Right ascension, 259° 35′, with a probable error of 2° 57′
North declination, 34° 33′, with a probable error of 3° 24′

Not content with determining the direction of the solar motion, Otto Struve has computed the angular value of this motion, as seen at a right angle to the Sun's path, and at the

[1] M. Prévost, who used Mayer's proper motions, made the right ascension only 230°.

mean distance of the stars of the first magnitude. His results are as follows :—

From the right ascension of the stars,	$0''\cdot 32122$, with a probable error of	$0''\cdot 03684$
From their declination, . .	$0\ \cdot 35719$, with a probable error of	$0\ \cdot 03562$
Or, taking the mean of these results,	$0''\cdot 33920$	$0''\cdot 03623$

But as the parallax of stars of the first magnitude is $0''\cdot 209$, we can change the angular motion of the Sun into a linear motion in space ; and hence taking the radius of the Earth's orbit as unity, M. Struve finds that the annual motion of the Sun in space is $\frac{0^{\circ}\cdot 3392}{0\cdot 209} = 1\cdot 623$ radii of the Earth's orbit, with a probable error of 0·229.

In his interesting work on Stellar Astronomy,[1] he has expressed these results in the following manner :—" *The motion of the solar system in space is directed to a point of the celestial vault situated on the right line which joins the two stars* π *and* μ *Herculis at a quarter of the apparent distance of these stars, reckoning from* π *Herculis. The velocity of this motion is such, that the Sun, with all the bodies which depend upon it, advances annually in the above direction* 1·623 *times the radius of the Earth's orbit, or* 33,550,000 *geographical miles. The possible error of this last number amounts to* 1,733,000 *geographical miles, or to a seventh part of the whole. We may then wager* 400,000 *to* 1 *that the Sun has a proper progressive motion, and* 1 *to* 1 *that it is comprised between the limits of thirty-eight and twenty-nine millions of geographical miles.*

If we take 95 millions of English miles as the mean radius of the Earth's orbit, we have $95 \times 1\cdot 623 = 154\cdot 185$ millions of miles, and, consequently,

The velocity of the Solar system is		154,185,000 miles in the year.
,,	,,	422,424 miles in a day.
,,	,,	17,601 miles in an hour.
,,	,,	293 miles in a minute.
,,	,,	4·9 miles in a second.

As none of the celestial motions are rectilineal, the advance

[1] *Etudes d' Astronomie Stellaire*, of which we have given a copious abstract in the *North British Review*, vol. viii. pp. 523-534.

of the system in space must be round some distant centre, which M. Madler, without much reason, supposes to be *Alcyone*, the brightest star in the Pleiades. In the course of time, however, the point to which the system is advancing must change its place, and from the nature and magnitude of that change, its curvilineal motion, and perhaps the form of its orbit, may be established. But even if so grand a result were obtained, we may never be able to ascertain whether our Sun and planets revolve like a multiple star round a single centre, or, as in our planetary system, they form only one of a number of systems revolving round the same centre. On such a subject speculation is vain. We must rest satisfied with the simple truth, that since the earliest observation of the stars, our system has described so small a portion of its curvilineal orbit, that it cannot be distinguished from a straight line. If the buried relics of primeval life have taught us how brief has been our tenure of this terrestrial paradise, compared with its occupancy by the brutes that perish, the great sidereal truth which we have been expounding, impresses upon us the no less humbling lesson, that from the birth of man to the extinction of his race, the system to which he belongs will have described but an infinitesimal arc of that immeasurable circle in which it is destined to revolve.

Such are the great sidereal movements to some of which the law of gravitation has been already applied, and nobody has ventured to doubt that all of them will, in due time, come under its rule. Every new satellite, every new asteroid, every new comet, every new planet, every new star circulating round its fellow, proclaims the universality of Newton's philosophy, and adds fresh lustre to his name. It is otherwise, however, in the general history of science. The reputation achieved by a great invention is often transferred to another which supersedes it, and a discovery which is the glory of one age is eclipsed by the extension of it in another. The fame of having invented the steam-engine has disappeared beside the reputation of the philosophers who have improved it; and the laurels which the discoverer of Ceres has worn for half a century, have been almost withered by the discovery of fifty-six similar bodies. It is the peculiar glory of Newton, however, that every discovery in the heavens attests the universality of his laws, and adds a greener leaf to the laurel chaplet which he wears.

CHAPTER XIV.

History of the Infinitesimal Calculus—Archimedes—Pappus—Napier—Edward Wright—Kepler's Treatise on Stereometry—Cavalieri's Geometria Indivisibilium—Roberval—Toricelli—Fermat—Wallis's Arithmetica Infinitorum—Hudde—Gregory—Slusius—Newton's discovery of Fluxions in 1665—General Account of the Method, and of its Applications—His Analysis per Equationes, etc.—His Discoveries communicated to English and Foreign Mathematicians—The Method of Fluxions and Quadratures—Account of his other Mathematical Writings—He solves the Problems proposed by Bernouilli and Leibnitz—Leibnitz visits London, and corresponds with the English Mathematicians, and with Newton through Oldenburg—He discovers the Differential Calculus, and communicates it to Newton—Notice of Oldenburg—Celebrated Scholium respecting Fluxions in the Principia—Account of the Changes upon it—Leibnitz's Manuscripts in Hanover.

In the history of Newton's optical and astronomical discoveries, which we have given in the preceding chapters, we have seen him involved in disputes with his own countrymen as well as with foreigners, in reference to the value and the priority of his labours. Such extreme sensitiveness as that with which he felt the criticisms and discussed the claims of his opponents, has been seldom exhibited in the annals of science; and so great was his dread of controversy, and so feeble his love of wealth and of fame, that, but for the importunities of his friends, his most important researches would have been withheld from the world. If he had been warned of the dangers of a scientific career by the troubled lives of Galileo and of Kepler, he must have learned from their history that great truths have never been received with implicit submission, and that in every age and every state of society the newest and the highest must undergo more than one ordeal—the ordeal of the ignorant, whose capacity they transcend—the ordeal of philosophy, which they are to be tested and confirmed—and the ordeal of personal jealousy and rival schools, by which they are to be misrepresented and condemned. The discoveries of Newton were tried by all these tests: they emerged purer and greater from the opposition of the Dutch savans: they were placed on a firmer basis by the skilful analysis of Hooke and of Huygens; and they were more warmly received and more widely extended after they had triumphed over the rival speculations of the followers of Aristotle and Descartes.

In the history of Newton's mathematical discoveries, which the same dread of controversy had induced him to withhold from the world, we shall find him involved in more exciting discussions,—in what may even be called quarrels, in which both the temper and the character of the disputants were severely tried. In the controversy respecting the discovery of fluxions, or of the differential calculus, Newton took up arms in his own cause, and though he never placed himself in the front rank of danger, he yet combated with all the ardour of his comrades. Hitherto it had been his lot to contend with individuals unknown to science, or with the philosophers of his own country who were occupied with the same studies; but interests of a larger kind, and feelings of a higher class, sprung up around him. National sympathies mingled themselves with the abstractions of number and of quantity. The greatest mathematicians of the age took the field, and statesmen and princes contributed an auxiliary force to the settlement of questions upon which, after the lapse of nearly 200 years, a verdict has not yet been pronounced.

Painful as the sight must always be when superior minds are brought into collision, society gains from the contest more than the parties lose. We are too apt to regard great men, of the order of Newton and Leibnitz, as exempt from the common infirmities of our nature, and to worship them as demigods more than to admire them as sages. In the history upon which we are about to enter we shall see distinguished philosophers upon the stage, superior, doubtless, to their fellows, but partaking in all the frailties of temper, and exposed to all the suspicions of injustice, which embitter the controversies of ordinary life.

Although the honour of having invented the calculus of fluxions, or the differential calculus, has been conferred upon Newton and Leibnitz, yet, as in every other great invention, they were but the individuals who combined the scattered lights of their predecessors, and gave a method, a notation, and a name, to the doctrine of quantities infinitely small.

By an ingenious attempt to determine the area of curves the ancients made the first step in this interesting inquiry. Their principles were sound, but their want of an organized method of operation prevented them from even forming a calculus. The method of exhaustions which they employed for this purpose consisted in making the curve a limiting area, to which the inscribed and circumscribed polygonal figures continually approached by increasing the number of their sides. The area thus obtained was obviously the area of the curve. In the case of the parabola, Archimedes showed that its area is two-thirds of its circumscribing rectangle, or of the product of the ordinate and the abscissa ; and he proved that the superficies of the sphere was equal to the convex superficies of the circumscribing cylinder, or to four times one of its great circles, and that the solidity of the sphere was two-thirds of that of the cylinder. His writings abound in trains of thought, which are strictly conducted on the principles of the modern calculus, but in place of this calculus we have only an imperfect arithmetic.

Pappus of Alexandria, who flourished about the close of the fourth century, followed Archimedes in the same inquiries, and his celebrated theorems on the centre of gravity[1] is the only

[1] Guldinus gave this theorem in 1635, and seeing that he was acquainted with Pappus, Montucla and others were disposed to regard him as a plagiarist. Had they studied Pappus in Condamine's Latin, in place of that of Halley, they never would have known the theorem but from Guldinus.

fruit which sprung from the seed sown by the Greek geometer till we reach the commencement of the seventeenth century. We search in vain the writings of Cardan, Tartaglia, Vieta, and Stevinus, for any proof of their power to employ the infinitesimal principle.

Our countryman, John Napier of Merchiston, and his contemporary, Edward Wright, were not only the first to revive the use of the infinitesimal principle, but the first who applied it in an arithmetical form. They distinctly apprehended the idea of a sufficient approach to the calculation of gradual change by the substitution of small and discontinuous changes. In this way Napier arrived at the representation of the results of arithmetical and geometrical progression taking place continuously in two different magnitudes, and associated the logarithm of any quantity with its primitive. In this manner, too, Wright exhibited what we now call an integration by quadrature, in his celebrated construction of the meridional parts. Both of these geometers fully conceived the idea, as it was embodied in their several problems; and though we cannot ascribe to either a distinct conception of it, we cannot withhold from them the honour of being the first of modern writers who assisted their successors in its conception.

In his treatise on Stereometry, published in 1615, Kepler made some advances in the doctrine of infinitesimals. In consequence of a dispute with a wine-merchant he studied the mensuration of round solids, or those which are formed by the revolution of the conic sections round any line whatever within or without the section. He considered the circle as consisting of an infinite number of triangles, having their vertices in the centre, and their infinitely small bases in the circumference. In like manner, he considered the cone as composed of an infinite number of pyramids, and the cylinder of an infinite number of prisms, and by thus rendering familiar the idea of quantities infinitely great and infinitely small, he gave an impulse to this branch of mathematics.

The failure of Kepler in solving some of the more difficult problems which he himself proposed, drew the attention of geometers to the subject of infinitely small quantities, and seems particularly to have attracted the attention of Cavalieri. This celebrated mathematician, who was the friend as well as the disciple of Galileo, was born at Milan in 1598, and was professor of geometry at Bologna. Although he had invented his method of indivisibles so early as 1629, his work entitled *Geometria Indivisibilium* did not appear till 1635, nor his *Exercitationes*, containing his most remarkable results, till 1647. He considers a line as composed of an infinite number of points, a surface of an infinite number of lines, and a solid of an infinite number of surfaces, and he assumes as an axiom, that the infinite sums of such lines and surfaces have the same ratio, existing in equal numbers in different surfaces or solids, as the surfaces or solids to be determined. As it is not true that an infinite number of infinitely small points can make a line, nor an infinite number of infinitely small lines a surface, Pascal proposed to return to the idea of Kepler by considering a line as composed of an infinite number of infinitely short lines,—a surface as composed of an infinite number of infinitely narrow parallelograms, and a solid of an infinite number of infinitely thin solids. If Cavalieri had been more advanced in algebra he might, perhaps, have gone farther; but he was undoubtedly the first who applied the algebraical process to the quadrature of parabolas of an integer order; and his chief instrument, as it was afterwards that of Wallis, was the theorem, that $1^n + 2^n + \ldots x^n$, divided by $x^n + x^n \ldots x^n$, is $1 : (n + 1)$ when x is infinite.

Previous to the publication of Cavalieri's work, Roberval had adopted the same principle, and proved that the area of the cycloid was equal to three times that of its generating circle. He determined also the centre of gravity of its area, and the solids formed by its revolution about its axis or its base. We owe to the same mathematician a general method of drawing tangents to certain curves, mechanical and geometrical, which was in some respects similar to that of fluxions. Regarding every curve as described by a point, Roberval[1] considered the point as influenced by two motions, by the composition of which it moves in the direction of a tangent ; and had he possessed the method of fluxions he could have determined in every case the relative velocities of these motions, which depend on the nature of the curve, and, consequently, the direction of the tangent, which he assumed to be the diagonal of a parallelogram whose sides were as the velocities.

Without knowing what had been done by Roberval, Toricelli, a pupil of Galileo, published, in 1644, a solution of the cycloidal problems ; but though the demonstrations were so different as to prove that he had not seen those of Roberval, and though his character and talents might have protected him from so ungenerous a charge, the French mathematician did not scruple to accuse him of plagiarism. Toricelli made much use of the infinitesimal methods, and was one of those who most clearly foresaw the approach of a new calculus.

The methods of Peter Fermat, counsellor of the Parliament of Toulouse, for obtaining maxima and minima, and for drawing tangents to curves, had such a striking resemblance to those of the differential calculus, that Laplace, and, in a more qualified manner, Lagrange, have pronounced Fermat[2] to be the inventor. We need not say that this is an exaggeration : Fermat and others came so close to the calculus as actually to invent cases of it ; but none before Newton and Leibnitz ever imagined, far less organized, a general method which should combine the scattered cases of their predecessors into a uniform and extensible system.

[1] Roberval's concealment of his discovery, and his forgery of a work of Aristarchus, greatly lower his credit, when he bears testimony in his own favour.

[2] These methods were published in the sixth or supplemental volume of the second edition of Herigon's *Cursus*, Paris, 1644, 8vo ; and an example was given by Schooten in the second edition of his Commentary on the second Book of Descartes's Geometry, in 1659.

As the time for the real invention approached, the anticipatory cases were multiplied. The *Arithmetica Infinitorum* of Wallis (1655), not to speak of any other of his writings, applied and extended the ideas of Cavalieri, and produced an ample field of results. It appears, in modern language, like a treatise on $\int x^n dx$ for all values of n except -1, and on $\int (a^2-x^2)^n dx$ for all integer values of n. It gives the first description of the method of rectifying a curve. In the work before cited, Schooten publishes a letter from Henry Van Heuraet, written in 1659, giving the algebraic rectification of every parabola of the form $y^n = ax^{n+1}$, except in the case of $n=1$, which case is shown to depend on the quadrature of the hyperbola. This had been completed a year or two before, about the same time at which William Neile communicated to Wallis his rectification of the semicubical parabola. Fermat also did the same as Neile, under the forms of the old geometry. Descartes, in 1648, showed that he had made progress in a method of finding areas, centres of gravity, and tangents ; and he afterwards determined the character of a curve by what we should now call a transformation of a differential equation.

In his Commentary on Descartes, Schooten published two letters of John Hudde, the second of which is dated January 27, 1658. It shows how to make a rational function integral or fractional, a maximum or minimum, and even treats the case in which the function and its variable are connected by an unsolved rational equation. The rules are, for the first time, extricated from algebraical process, and presented in calcular form. These very remarkable results were well known to both Newton and Leibnitz, and are freely cited by both.

James Gregory, in 1668, gave two of what we should now call integrations of trigonometrical functions. He demonstrated the connexion which had been observed between Wright's meridional parts and the logarithms of cotangents.

The methods of drawing tangents, invented by Barrow and by Slusius, were published in 1670 and in 1673. Such methods were then common; and Barrow, in announcing his, says he scarcely perceives the use of publishing it, because several similar methods were well known. But both these methods obtained an undue importance in the great controversy, and this probably arose from their being both published in England.

Such are the methods which Newton and Leibnitz received from their predecessors, and, were we obliged to describe them in modern terms, we should call them isolated instances of differentiation and integration, of calcular rules of differentiation, of quadrature, rectification, and determination of centres of gravity, of determination of maxima and minima, both of explicit and implicit functions, &c. But we can hardly permit ourselves to invite the reader to look back under general terms, because he can hardly use the general terms without having the idea of a general system. Some will almost be inclined to ask what was left for Newton and Leibnitz to do? The best answer is, that it was left for them to put the querist in a position to ask the question. Had it not been for Newton and Leibnitz, that is, supposing their place had never been supplied, the close approach of the investigators to each other, and to a common method, would never have been visible.

We have already seen[1] that the attention of Newton had been directed to these subjects so early as 1663 and 1664. Upon reading Dr. Wallis's work in the winter of 1664-5, he obtained an expression in series for the area of circular sectors; and from the consideration that the arch has the same proportion to its sector that an arch of 90° has to the whole quadrant, he found an expression for the length of the arch. At the same time he determined the area of the rectangular hyperbola intercepted between the curve, its asymptote, and two ordinates parallel to the other asymptote; and it was by this series that he computed the area of the hyperbola to fifty-two figures, when the plague had, in the summer of 1669, driven him from

[1] See pp. 20-22.

Cambridge to Boothby. At the same time he was led, by the happy thought of substituting indefinite indices of powers for definite ones, to give a more general form to the 59th proposition of Dr. Wallis's Arithmetic of Infinites. In the beginning of 1665, he likewise discovered a method of tangents similar to those of Hudde, Gregory, and Slusius, and a method of finding the curvature of curve lines at any given point; and, continuing to pursue the method of interpolation, he found the quadrature of all curves whose ordinates are the powers of binomials affected with indices whole, fractional, or surd, affirmative or negative; together with a rule for reducing any power of a binomial into an approximating or converging series. In the spring of the same year he discovered a method of doing the same thing by the continual division and extraction of roots; and he soon after extended the method to the extraction of the roots of adfected equations in species.

Having met with an example of the method of Fermat, in Schooten's Commentary on the Second Book of Descartes, Newton succeeded in applying it to adfected equations, and determining the proportion of the increments of indeterminate quantities. These increments he called *moments*, and to the velocities with which the quantities increase he gave the names of *motions*, *velocities* of *increase*, and *fluxions*. He considered quantities not as composed of indivisibles, but as generated by motion; and as the ancients considered rectangles as generated by drawing one side into the other, that is, by moving one side upon the other to describe the area of the rectangle, so Newton regarded the areas of curves as generated by drawing the ordinate into the abscissa, and all indeterminate quantities as generated by continual increase. Hence, from the flowing of time and the moments thereof, he gave the name of *flowing quantities* to all quantities which increase in time, that of *fluxions* to the velocities of their increase, and that of *moments* to their parts generated in moments of time. He considered time as flowing uniformly, and represented it by any other quantity, which is regarded as flowing uniformly, and its fluxion by a unit. These moments of time, or of its exponent, he considers as equal to one another, and represents by the letter o, or by any other mark multiplied by unity. The other flowing quantities are represented by any letters or marks, but most commonly by the letters at the end of the alphabet; while their fluxions are represented by any other letters or marks, or by the same letters in a different form or size, and their moments by their fluxions multiplied by a moment of time.

In a manuscript, dated 13th November 1665, the direct method of fluxions is described with examples, and the following problem is resolved :—"An equation being given expressing the relation of two or more lines, x, y, z, &c., described in the same time by two or more moving bodies, A, B, C, &c., to find the relation of their velocities, p, q, r, &c., with which these lines are described." In the same manuscript we find an application of this method to the drawing of tangents, by determining the motion of any point which describes the curve, and also to the determination of the radius of curvature of any curve line, by making the perpendicular to the curve move upon it at right angles, and finding that point of the perpendicular which is in least motion, for that point will be the centre of curvature of the curve at that point upon which the perpendicular stands. On another leaf of the same book, dated May 20, 1665, the same method is given, but in different words, and fluxions are represented with dots superfixed. In another leaf, dated May 16, 1666, there is given a general method, consisting of seven propositions, of solving problems by motion, the seventh proposition being the same, though differently expressed, from that in the paper of November 13, 1665.

In a small tract, written in October 1666, we find the same method in the same number of propositions; but the seventh is improved by showing how to proceed in equations involving fractions and surds, and such quantities as were afterwards called transcendental. To this tract there is added an eighth proposition, containing the inverse method of fluxions, in so far as he had then attained it, namely, by the method of quadratures, and by most of the theorems in the Scholium to the tenth proposition of his Book of Quadratures, which with many more are contained in this tract. Newton then proceeds to show the application of the propositions to the solution of the twelve following problems, many of which were at that time entirely new :—

"1. To draw tangents to curve lines.

"2. To find the quantity of the crookedness of lines.

"3. To find the points distinguishing between the concave and convex portions of curved lines.

"4. To find the point at which lines are most or least curved.

"5. To find the nature of the curve line whose area is expressed by any given equation.

"6. The nature of any curve line being given, to find other lines whose areas may be compared to the area of that given line.

"7. The nature of any curve line being given, to find its area when it may be done ; or two curved lines being given, to find the relation of their areas when it may be.

"8. To find such curved lines whose lengths may be found, and also to find their lengths.

"9. Any curve line being given, to find other lines whose lengths may be compared to its length, or to its area, and to compare them.

"10. To find curve lines whose areas shall be equal, or have any given relations to the length of any given curve line drawn into a given right line.

"11. To find the length of any curve line when it may be.

"12. To find the nature of a curve line whose length is expressed by any given equation when it may be done."

Such were the improvements in the higher geometry which

Newton had made before the end of 1666. His analysis, consisting of the method of series and fluxions combined, was so universal as to apply to almost all kinds of problems. He had not only invented the method of fluxions in 1665, in which the motions or velocities of flowing quantities increase or decrease, but he had considered the increase or decrease of these motions or velocities themselves, to which he afterwards gave the name of *second fluxions*,—using sometimes letters with one or two dots, to represent first and second fluxions.

It does not appear that Newton imparted any of these methods to his mathematical friends; but in order to communicate some of his results, he composed his treatise entitled *Analysis per Equationes Numero Terminorum Infinitas*, in which the method of fluxions and its applications are supposed by some to be explained; while others are of opinion, that it treats only of moments or infinitely small increments, and exhibits the algebraical processes involved in their use. In June 1669, he communicated his work to Dr. Barrow, who, in letters to Collins of the 20th June, the 31st July, and the 20th August, mentions it, as we have already seen,[1] as the production of Newton, a young man of great genius. Having taken a copy of this treatise, Collins returned the original to Dr. Barrow, from whom it again passed into the hands of Newton. At the death of Collins, Mr. William Jones found the copy among his papers; and having compared it with the original given him by Newton, it was published, along with some other analytical tracts of the same author, in 1711, nearly fifty years after it was composed.

Although the discoveries contained in this treatise were not at first given to the world, yet they were generally known to mathematicians by the correspondence of Collins, who communicated them to James Gregory in Scotland; to M. Bertet, and an English gentleman, Francis Vernon, secretary to the English ambassador in Paris; to Slusius in Holland; to Bo-

[1] See p. 32, and note 3, p. 24.

relli in Italy; and to Thomas Strode, Oldenburg, Dary, and others in England, in letters dated between 1669 and 1672.

In the years 1669 and 1670, Newton had prepared for the press a new and enlarged edition of Kinckhuysen's Introduction to Algebra.[1] He at first proposed to add to it, as an introduction, a treatise entitled, a *Method of Fluxions and Quadratures;* but the fear of being involved in disputes as annoying as those into which his optical discoveries had led him, and which were not yet concluded, prevented him from giving this treatise to the world. At a later period of our author's life, Dr. Pemberton had prevailed upon him to publish it, and for this purpose had examined all the calculations and prepared the diagrams. The latter part of the treatise, however, in which he intended to show the manner of resolving problems which cannot be reduced to quadratures, was never finished; and when Newton was about to supply this defect, his death put a stop to the plan.[2] It was therefore not till the year 1736 that a translation of the work appeared, with a commentary by Mr. John Colson, Professor of Mathematics in Cambridge.[3]

Between the years 1671 and 1676, Newton did not pursue his mathematical studies. His optical researches, and the disputes in which they involved him, occupied all his time; and there is reason to believe, that as soon as these disputes were over, he directed the whole energy of his mind to those researches which constitute the *Principia.*

Hitherto the method of fluxions was known only to the

[1] This task seems to have been pressed upon him by some friends in London. In sending to Collins the notes upon the book, in July 1670, he wishes his name to be suppressed, and suggests that in the title-page, after the words *Nunc e Belgico Latine versa*, the words *et ab alio authore locupletata* should be added. In a letter to Collins, dated September 5, 1676, he thus speaks of the work:—"I have nothing in the press, only Kinckhuysen's Algebra, I would have got printed here, to satisfy the expectation of some friends in London, but our press cannot do it. This, I suppose, is the book Dr. Lloyd means. It is now in the hands of a bookseller here to get it printed; but if it do come out, I shall add nothing to it.—Macclesfield *Correspondence*, vol. ii. p. 398.

[2] Pemberton's *Account of Sir Isaac Newtons Discoveries*, Pref. p. 6.

[3] It is entitled *Method of Fluxions and Infinite Series.* Lond. 1736, 1737. 4to.

friends of Newton and their correspondents ; but in the first edition of the *Principia*, which appeared in 1687, he published for the first time one of the most important rules of the fluxionary calculus, which forms the Second Lemma of the Second Book, and points out the method of finding the moment of the products of any power whatsoever.

In writing the *Principia*, Newton made great use of both the direct and the inverse method of fluxions ; but though all the difficult propositions in that work were invented by the aid of the calculus, yet the calculations were not put down, and the propositions were demonstrated by the method of the ancients, shortened by the substitution of the doctrine of limits for that of exhaustions. No information, however, is given in the *Principia* respecting the algorithm or notation of the calculus ; and it was not till 1693 that it was communicated to the mathematical world, in the Second Volume of Dr. Wallis's Works, which was published in that year. The friends of Newton in Holland had informed Dr. Wallis that Newton's "Method of Fluxions" had passed there with great applause by the name of Leibnitz's *Calculus Differentialis*. The Doctor, who was at that time printing the Preface to his First Volume, inserted in it a brief notice of Newton's claim to the discovery of fluxions, and published in his second volume some extracts from the *Quadratura Curvarum*, with which Newton had furnished him.[1]

To the first edition of Newton's Optics, which appeared in 1704, there were added two mathematical treatises, entitled, *Tractatus duo de speciebus et magnitudine figurarum curvilinearum*, the one bearing the title of *Tractatus de Quadratura Curvarum*,[2] and the other *Enumeratio linearum tertii ordinis*.[3] The first contains an explanation of the doctrine of fluxions, and

[1] Wallisii *Opera*, tom. i. Præf. pp. 2, 3 ; and tom. iii. cap. xciv. xcv. See also Letter of Wallis to Newton, April 10, 1695, in Edleston's *Correspondence*, &c., p. 309, and part of it in Raphson's *Hist. of Fluxions*, pp. 120, 121.

[2] Newtoni *Opera*, tom. i. pp. 333-386.

[3] Ibid. tom. i. pp. 531-560.

of its application to the quadrature of curves; and the second a classification of seventy-two curves of the third order, with an account of their properties. The reason for publishing these two tracts in his Optics (in the subsequent editions of which they are omitted) is thus stated in the advertisement:—"In a letter written to M. Leibnitz in the year 1679, and published by Dr. Wallis, I mentioned a method by which I had found some general theorems about squaring curvilinear figures on comparing them with the conic sections, or other the simplest figures with which they might be compared. And some years ago I lent out a manuscript containing such theorems; and having since met with some things copied out of it, I have on this occasion made it public, prefixing to it an introduction, and joining a scholium concerning that method. And I have joined with it another small tract concerning the curvilinear figures of the second kind, which was also written many years ago, and made known to some friends, who have solicited the making it public."

In the year 1707, Mr. Whiston published the algebraical lectures which Newton had delivered at Cambridge, under the title of *Arithmetica Universalis, sive de Compositione et Resolutione Arithmetica Liber.*[1] This work, which is still in the University Library, was soon afterwards translated into English by Mr. Raphson; and a second edition of it, with improvements by the author, was published at London in 1712, by Dr. Machin, secretary to the Royal Society. With the view of stimulating mathematicians to write annotations on this admirable work, the celebrated S'Gravesande published a tract, entitled, *Specimen Commentarii in Arithmeticam Universalem;* and Maclaurin's Algebra seems to have been drawn up in consequence of this appeal.

Among the mathematical works of Newton we must not omit to enumerate a small tract entitled, *Methodus Differentialis,* which was published with his consent in 1711. It

[1] Newtoni *Opera,* tom. i. pp. 1-251.

consists of six propositions, which contain a method of drawing a parabolic curve through any given number of points, and which are useful for constructing tables by the interpolation of series, and for solving problems depending on the quadrature of curves.

Another mathematical treatise of Newton was published for the first time in 1799, in Dr. Horsley's edition of his works. It is entitled, *Artis Analyticæ Specimina, vel Geometria Analytica.*[1] In editing this work, which occupies about 130 quarto pages, Dr. Horsley used three manuscripts, one of which was in the handwriting of the author; another, written in an unknown hand, was given by Mr. William Jones to the Honourable Charles Cavendish; and a third, copied from this by Mr. James Wilson, the editor of Robins's works, was given to Dr. Horsley by Mr. John Nourse, bookseller to the king. Dr. Horsley has divided it into twelve chapters, which treat of infinite series, of the reduction of affected equations, of the specious resolution of equations, of the doctrine of fluxions, of maxima and minima, of drawing tangents to curves, of the radius of curvature, of the quadrature of curves, of the area of curves which are comparable with the conic sections; of the construction of mechanical problems, and on finding the length of curves.

In enumerating the mathematical works of our author, we must not overlook his solutions of the celebrated problems proposed by John Bernoulli and Leibnitz. In June 1696, John Bernoulli addressed a letter to the most distinguished mathematicians in Europe,[2] challenging them to solve the two following problems:—

1. To determine the curve line connecting two given points which are at different distances from the horizon, and not in the same vertical line, along which a body passing by its own gravity, and beginning to move at the upper point, shall descend to the lower point in the shortest time possible.

[1] Newtoni *Opera*, tom. i. pp. 388-519.

[2] "Acutissimis qui toto orbe florent Mathematicis."

2. To find a curve line of this property that the two segments of a right line drawn from a given point through the curve, being raised to any given power, and taken together, may make everywhere the same sum.[1]

This challenge was first made in the Leipsic Acts, for June 1696.[2] Six months were allowed by Bernoulli for the solution of the problem, and in the event of none being sent to him he promised to publish his own. The six months, however, elapsed without any solution being produced; but he received a letter from Leibnitz, stating that he had "cut the knot of the most beautiful of these problems," and requesting that the period for their solution should be extended to Christmas next, that the French and Italian mathematicians might have no reason to complain of the shortness of the period. Bernoulli adopted the suggestion, and publicly announced the prorogation for the information of those who might not see the Leipsic Acts.

On the 29th January 1696-7, Newton received from France two copies of the printed paper containing the problems, and on the following day he transmitted a solution of them to Charles Montague, Chancellor of the Exchequer, and then President of the Royal Society.[3] He announced that the curve required in the first problem must be a cycloid, and he gave a method of determining it. He solved also the second problem, and he showed that by the same method other curves might be found which shall cut off three or more segments having the like properties. Solutions were also obtained from Leibnitz and the Marquis de l'Hôpital; and although that of Newton was anonymous, yet Bernoulli recognised in it his powerful mind; "*tan-*

[1] John Bernoulli had already published, in the Leipsic Acts for June, p. 266, a solution of the most simple case in which the exponent of the power was unity.

[2] *Acta Lipsiensia*, in June, p. 269.

[3] The original manuscript of this letter with the solution of the problem is preserved at the Royal Society; and one of the two papers, a folio printed half-sheet, still exists in their archives. At the bottom, in Newton's hand, are the words, "Chartam hanc ex Gallia missam accepi, Jan 29, 1696-7."—Edleston's *Correspondence*, &c. &c., p. lxviii. For a copy of the document, see Newtoni *Opera*, tom. iv. pp. 411-418.

quam," says he, "*ex ungue leonem*," as the lion is known by his claw.

One of the *last* mathematical efforts of our author was made, with his usual success, in solving a problem which Leibnitz proposed in 1716, in a letter to the Abbé Conti, "for the purpose, as he expressed it, of feeling the pulse of the English analysts." The object of this problem was to determine the curve which should cut at right angles an infinity of curves of a given nature, but expressible by the same equation. Newton received this problem about five o'clock in the afternoon, as he was returning from the Mint; and though the problem was difficult, and he himself fatigued with business, he reduced it to a fluxional equation before he went to bed.

In his reply to Leibnitz,[1] Conti does not even mention the solution of Newton; but as if such a problem had been beneath the notice of the English geometers, he says:—"Your problem was very easily resolved, and in a short time. Several geometers, both in London and Oxford, have given the solution. It is general, and extends to all sorts of curves, whether geometrical or mechanical. The problem is proposed somewhat equivocally; but I believe that M. De Moivre is not wrong when he says that we must fix the idea of a series of curves, and suppose, for example, that they have the same subtangent for the same abscissa, which would correspond not only with the conic sections, but with an infinity of other curves, both geometrical and mechanical."

Such is a brief account of the mathematical writings of Sir Isaac Newton, not one of which was voluntarily communicated to the world by himself. The publication of his Universal Arithmetic is said to have been made by Whiston against his will; and, however this may be, it was an unfinished work, never designed for the public. The publication of his *Quadrature of Curves*, and of his *Enumeration of Curve Lines*, was in Newton's opinion rendered necessary, in consequence of plagiar-

[1] Dated London, March 1716.

isms from the manuscripts of them which he had lent to his friends, and the rest of his analytical writings did not appear till after his death. It is not easy to penetrate into the motives by which this great man was actuated. If his object was to keep possession of his discoveries till he had brought them to a higher degree of perfection, we may approve of the propriety, though we cannot admire the prudence, of such a step. If he wished to retain to himself his own methods, in order that he alone might have the advantage of them, in prosecuting his physical inquiries, we cannot reconcile so selfish a measure with that openness and generosity of character which marked the whole of his life, nor with the communications which he so freely made to Barrow, Collins, and others. If he withheld his labours from the world in order to avoid the disputes and contentions to which they might give rise, he adopted the very worst method of securing his tranquillity. That this was the leading motive under which he acted, there is little reason to doubt. The early delay in the publication of his *Method of Fluxions*, after the breaking out of the plague at Cambridge, was probably owing to his not having completed the whole of his design ; but no apology can be made for the imprudence of withholding it any longer from the public,—an imprudence which is the more inexplicable, as he was repeatedly urged by Wallis, Halley, and his other friends, to present it to the world.[1] Had he published this noble discovery previous to 1673, when his great rival had made but little progress in those studies which led him to the same method, he would have secured to himself the undivided honour of the invention, and Leibnitz could have aspired to no other fame but that of an improver of the doctrine of fluxions. But he unfortunately acted otherwise. He announced to his friends that he possessed a method of great generality and power : He communicated to them a general account of its principles and applications ; and the information which was thus conveyed, might have directed the attention of

[1] Wallis to Newton, April 10, 1695. See Edleston's *Correspondence*, pp. 301, 302.

mathematicians to subjects to which they would not have otherwise applied their powers. The discoveries which he had previously made were made subsequently by others; and Leibnitz, instead of appearing on the theatre of science as the disciple and the follower of Newton, stood forth with all the dignity of a second inventor; and, by the early publication of his discoveries, had nearly placed himself on the throne which Newton was destined to ascend.

It would be inconsistent with the nature of this work to enter into a detailed history of the dispute between Newton and Leibnitz respecting the invention of fluxions. A brief and general account of it, however, is indispensable.

In the beginning of 1673, when Leibnitz came to London in the suite of the Duke of Hanover, he became acquainted with the great men who then adorned the capital of England. Among these was Henry Oldenburg, a countryman of his own, who was at that time Secretary to the Royal Society, Leibnitz had not then, as he himself assures us,[1] entered upon the study of the higher geometry, but he eagerly embraced the opportunity which was now offered to him of learning the discoveries of the English mathematician. With this view he kept up a correspondence with Oldenburg, communicating to him freely certain arithmetical and analytical methods of his own, and receiving in return an account of the discoveries in series made by James Gregory and Newton. In the two letters[2] written in London to Oldenburg, and in the first four which he addressed to him from Paris,[3] he refers only to certain properties of numbers which he had discovered; but in those of a subsequent date, he mentions a theorem of his own for expressing the area of a circle, or of any given sector of it, by an infinite series of

[1] Two years before this, in 1671, Leibnitz presented to the Academy of Sciences a paper containing the germ of the differential method, so that he must have been able to appreciate the information he received in England.—See page 71.

[2] Dated February 3d and 20th, 1673.

[3] March 30, April 26, May 26, and June 8, 1673.

rational numbers;[1] and of deducing, by the same method, the arc of a circle from its sine.[2] In reply to these letters, Oldenburg acquainted him with the previous discoveries of Newton, and transmitted to him a communication from Collins, describing several series which had been sent to him by Gregory on the 15th February 1671. Leibnitz stated in reply,[3] that he was so much distracted with business, that he had not time to compare these series with his own; and he promises to communicate his opinion to Oldenburg as soon as he has made the comparison. In continuing his correspondence with Oldenburg, Leibnitz requested farther information respecting the analytical discoveries recently made in England; and it was in compliance with this request that Newton, at the pressing solicitation of Oldenburg and Collins, wrote a long letter, dated 13th June 1676, to be communicated to Leibnitz.

This letter, which was sent to Leibnitz in Paris, along with extracts from Gregory's letters, on the 26th June, contained Newton's method of series, and, after describing it, he added, "that analysis, by the assistance of infinite equations of this kind, extends to almost all problems except some numerical ones like those of Diophantus, but does not become altogether universal without some farther methods of reducing problems to infinite equations, and infinite equations to finite ones, when it might be done."

Leibnitz answered this letter on the 27th August, and, in return for Newton's method of series, he sent to Oldenburg a theorem for transmuting figures into one another; and thus demonstrated the series of Gregory for finding the arch from its tangent. In consequence of Leibnitz having requested still farther information, Newton addressed to Oldenburg his celebrated letter of the 24th October 1676. In this letter he gave an account of his discovery of the method of series before the plague in the summer of 1665. He stated, that on the publication of Mercator's *Logarithmotechnia*, he had communicated

[1] July 15, 1673. [2] October 26, 1673. [3] May 20, 1675.

a compendium of this method through Dr. Barrow to Mr. Collins, and, that five years after, he had, at the suggestion of the latter, written a large tract on the same subject, joining with it a method from which the determination of maxima and minima, and the method of tangents of Slusius and some others flowed. "This method," he continued, "was not limited to surds, but was founded upon the following proposition, which he communicated enigmatically in a series of transposed letters, *Data equatione quotcunque fluentes quantitates involvente, fluxiones invenire, et vice versa.* This proposition," he added, "facilitated the quadrature of curves, and afforded him infinite series, which broke off and became finite when the curve was capable of being squared by a finite equation." In the conclusion of this letter, Newton stated that his method extended to inverse problems of tangents, and others more difficult, and that in solving these he used two methods, one more general than the other, which he expressed enigmatically in transposed letters, which formed the following sentence :—"Una methodus consistit in extractione fluentis quantitatis ex equatione simul involvente fluxionem ejus : altera tantum in assumptione seriei pro quantitate quâlibet incognita, ex quâ cetera commodè derivari possunt, et in collatione terminorum homologorum æquationis resultantis, ad eruendos terminos assumptæ seriei."

This letter, though dated 24th October, had not been forwarded to Leibnitz on the 5th March 1677. At the time Newton was writing it, Leibnitz spent a week in London, on his return from Paris to Germany; but it must have reached him in the spring of that year, as he sent an answer to it dated June 21, 1677.

In this remarkable letter he frankly describes his differential calculus and its algorithm. He says that he agrees with Newton in the opinion that Slusius's methods of tangents is not absolute, and that he himself had long ago (*a multo tempore*) treated the subject of tangents much more generally by the differences of ordinates. He gives an example of drawing tangents, and shows how to proceed, as Newton expresses it, "without sticking at surds." He then expresses the opinion, that the method of drawing tangents, which Newton wished to conceal, does not differ from his; and he regards this opinion as confirmed by the statement of Newton, that his method facilitated the quadrature of curves.

No answer seems to have been returned to this communication either by Oldenburg or Newton, and, with the exception of a short letter from Leibnitz to the former, dated 12th July 1677, no farther correspondence between them seems to have taken place. This no doubt arose from the death of Oldenburg in the month of September 1677;[1] and the two rival geometers, having through him become acquainted with each other's labours, were left to pursue them with all the ardour which the importance of the subject could not fail to inspire.

In the hands of Leibnitz, the differential calculus made rapid progress. In the *Acta Eruditorum*, which appeared at Leipsic in October 1684, he describes its algorithm in the same manner as he had done in his letter to Oldenburg. He points out its

[1] Henry Oldenburg, whose name is so intimately associated with the history of Newton's discoveries, was born at Bremen, and was consul from that town to London during the usurpation of Cromwell. Having lost his office, and been compelled to seek the means of subsistence, he became tutor to an English nobleman, whom he accompanied to Oxford in 1656. During his residence in that city he was introduced to the philosophers who established the Royal Society, and, upon the death of William Crown, the first secretary, he was appointed, in 1663, joint secretary along with Mr. Wilkins. He kept up an extensive correspondence with more than *seventy* philosophers and literary men in all parts of the world,—a privilege especially given to the Society in their charter. The suspicions of the Government, however, were, somehow or other, excited against him, and he was committed to the Tower on the 20th June 1667, "for dangerous designs and practices." Although no evidence was produced to justify so harsh a proceeding, he was kept a close prisoner till the 26th August 1667, when he was discharged. "This remarkable event," as Mr. Weld remarks, "had so much influence on the Society as to cause a suspension of the meetings from the 30th May to the 3d October." It is remarkable that there is no notice of this fact in the council or journal-books of the Society.

Oldenburg was the author of several papers in the Philosophical Transactions, and of some works which have not acquired much celebrity. He died at Charlton, near Greenwich, in August 1678. See Weld's *History of the Royal Society*, vol. i. pp. 200-204.

application to the drawing of tangents, and the determination of maxima and minima;[1] and he adds, that these are only the beginnings of a much more sublime geometry, applicable to the most difficult and beautiful problems even of mixed mathematics, which, without his differential calculus, or one SIMILAR to it, could not be treated with equal facility. The suppression of Newton's name in this reference to a *similar* calculus, which was obviously that of Newton, indicated in the letters of 1676, was the first false step in the fluxionary controversy, and may be regarded as its commencement.

While Leibnitz was thus making known the principles of his Calculus, Newton was occupied in preparing his Principia for the press. In the autumn of 1684, he had sent the principal propositions of his work to the Royal Society; but it would appear from his letter to Halley of the 20th June 1686, that the second book of the Principia had not then been sent to him. He must therefore have been acquainted with the paper of Leibnitz in the *Acta Eruditorum*, before he sent the manuscript of the second book to press; and it was doubtless from this cause that he was led to compose the second lemma of that book, in which he, for the first time, explains the fundamental principle of the fluxionary calculus. This lemma, which occupies only three pages, was terminated with the following scholium, which has been the subject of such angry discussion.

" The correspondence which took place about ten years ago, between that very skilful geometer G. G. Leibnitz and myself, when I had announced to him that I possessed a method of determining maxima and minima, of drawing tangents, and of performing similar operations, which was equally applicable to surds and to rational quantities, and concealed the same in transposed letters, involving this sentence (*Data Æquatione quotcunque Fluentes quantitates involvente, Fluxiones invenire,*

[1] This article was entitled, " Nova methodus pro maximis et minimis, itemque tangentibus quæ nec fractas nec irrationales moratur, et singulare pro illis calculi genus, per G. G. L."—*Acta Erudit.* 1684, pp. 472, 473.

et vice versa), this illustrious man replied that he also had fallen on a method of the same kind, and he communicated his method, which scarcely differed from my own,[1] except in the forms of words and notation (and in the idea of the generation of quantities[2]). The fundamental principle of both is contained in this lemma."

This celebrated scholium has been viewed in different lights by Leibnitz and his followers. Leibnitz asserts,[3] that Newton "has accorded to him in this scholium the invention of the differential calculus independently of his own;" and M. Biot considers the scholium as "eternalizing the rights of Leibnitz by recognising them in the Principia." But the scholium has no such meaning, and it was not the intention of the author that it should be thus understood. It is a statement of the simple fact, that Leibnitz communicated to him a method which was nearly the same as his own,—a sentiment which he might have expressed whether he believed that Leibnitz was an independent inventor of his calculus, or had derived it from his communication and correspondence with his friend.[4]

The manuscripts of Newton furnish us with some curious information on this subject, and place it beyond a doubt that he regarded the silence of Leibnitz, in his communication of 1684, as an aggressive movement, which he was bound to repel. "After seven years," says Newton,[5] "viz., in October

[1] "*A mea vix abludentem*"—the same expression which Leibnitz used in his letter to Oldenburg of June 21, 1677, "*ab his non abludere.*" The similarity of the Method of Fluxions and the Differential Calculus, may be considered as admitted both by Newton and Leibnitz.

[2] These words were inserted in the 2d edition of the Principia.

[3] Letter to the Abbé Conti, April 9, 1716, and to Madame de Kilmansegg, April 18, 1716.

[4] We have, fortunately, Newton's own opinions on the subject. "And as for the *scholium* upon the second *lemma* of the second book of the *Principia Philosophiæ Mathematicæ*, which is *so much wrested against me*, it was written not to give away that *lemma* to *Mr. Leibnitz*, but, on the contrary, to assert it to myself. Whether *Mr. Leibnitz* invented it after me, or had it from me, is a question of no consequence; for second inventors have no right."—Raphson's *History of Fluxions*, 1715, p. 122, see also p. 115; and Newtoni *Opera*, tom. iv. p. 616.

[5] In a manuscript of seven closely written pages, entitled, "A Supplement to the

1684, he published the elements of this method (the method mentioned to Leibnitz in his letter of October 24, 1676), as his own, without referring to the correspondence which he formerly had with the English about these matters. He mentioned, indeed, a *methodus similis, but whose that method was,* and *what he knew of it,* he did not say, as he should have done. And thus *his silence put me upon a necessity* of writing the scholium upon the second lemma of the second Book of Principles, *lest it should be thought that I borrowed* that lemma from Mr. Leibnitz. In my letter of 24th October 1676, when I had been speaking of the Method of Fluxions, I added, Fundamentum harum operationum, satis obvium quidem, quoniam non possum explicationem ejus prosequi, sic potius celavi *6æccdæ* 13*eff* 7*i* 3*l* 9*n* 4*o* 4*qrr* 4*s* 9*t* 12*vx*. And in the said scholium I opened this enigma, saying, that it contained the sentence, *Data æquatione quotcunque fluentes quantitates involvente, fluxiones invenire, et vice versa;* and was written in the year 1676, for I looked upon this *as a sufficient security, without entering into a wrangle;* but Mr. Leibnitz was of another opinion."

In 1724, when the third edition of the Principia was preparing for the press, Newton had resolved to substantiate his claims to the first, if not the sole invention, of the new calculus, and we have found several rough draughts of the changes which he intended to have made upon the scholium. In one of these[1] he gives an account of the fundamental principle of the fluxionary calculus, and distinctly states that it "might have been *easily collected* even from the letter which he wrote to Collins on the 10th December 1672,[2] a copy of which was sent to Leibnitz in 1676."[3]

Remarks;" that is, to some observations upon Leibnitz's letter to Conti, dated 9th April 1716, published in Raphson's *Fluxions*, p. 111.

[1] The title of this addition, which occupies more than a folio page, is, "In the end of the Scholium in Princip. Philos., p. 227, after the words, *Utriusque fundamentum continetur in hoc Lemmate,* add, *Sunto quantitates datæ, a, b, c; fluentes x, y, z,*" &c.

[2] A copy of this letter was sent to Tschirnhausen in May 1675, thirteen months before it was sent to Leibnitz.

[3] "Doubts have been expressed," Mr. Edleston remarks, "whether these papers were

In another folio sheet, we have the scholium in three different forms, including the substance of the one previously published.[1] In all of them it is distinctly stated that Newton's letter to Collins, of the 10th December 1672, containing the

actually sent to Leibnitz." That papers were sent and received by Leibnitz, his own testimony and that of others prove ; but there is some reason to believe, as first indicated by Mr. Edleston, and made much more probable by Professor De Morgan, that Newton's letter of the 10th December was sent, without the example of drawing a tangent to a curve, which it actually contained, and which was relied upon as giving Leibnitz a knowledge of the new calculus. In support of this opinion, we find that what are called the originals, said to have been received by Leibnitz, and Collins' draught of the papers preserved in the Royal Society, contain merely an allusion to that method. These originals have been printed in Leibnitz's *Mathematical Works*, published at Berlin in 1849, but fac-similes have not been given to enable us to judge of their genuineness. It is difficult to reconcile with these statements that of Newton himself, who declares that the *originals* of the letters in question were sent to Leibnitz in Paris to be *returned*, and that these originals were in the archives of the Royal Society. Leibnitz may have retained imperfect copies of these *originals*, which must have contained the method of tangents. If it be true that the original letters of Newton were sent to Leibnitz, we have nothing to do with the copies either at Hanover or the Royal Society.

With regard to the *seven* " study exercises by Leibnitz, on the use of both the differential and integral calculus," as Professor De Morgan calls them, dated November 11, 21, 22, 1675, June 26, July, November 1676, which were published by Gerhardt in 1848, we cannot, without seeing the originals or proper fac-similes of the hand-writing, receive them as evidence. Gerhardt admits that some person had been *turning the 5 of* 1675 *into a* 3 (from an obvious motive) ; and when we recollect how Leibnitz altered grave documents to give him a priority to Bernoulli, as we shall presently see, we are entitled to pause before we decide on any writings that have passed through his hands. But even if we admit these documents to be genuine, the allegation of Newton's friends that copies of his papers were in circulation before 1675, requires to be considered in the controversy. We recommend to the reader the careful study of Mr. Edleston's statement in the *Correspondence of Sir Isaac Newton*, p. xlvii., and of the very interesting paper by Professor De Morgan, on the *Companion to the Almanac for* 1852, p. 8.

To these observations we may add, that Keill published in the *Journal Littéraire* for May and June 1713, vol. i. p. 215, the extract from the letter of December 10, 1672, as the chief document upon which the report of the committee of the Royal Society was founded, and at the same time distinctly stated *that this letter was sent to Leibnitz.* Now Leibnitz, as we know, read this letter, and never contradicted the allegation of Keill. If the paper actually sent to him had been merely an abridgment of that letter, from which the example was omitted, he would undoubtedly have come forward, and proved by the production of what he did receive, and what we know he possessed, that the principal argument used against him had no foundation.

Three years afterwards, in 1716, when Newton had challenged him to the discussion, he had another opportunity which he did not use, of disowning the reception of the letter.

[1] See APPENDIX, No. XIII.

method of drawing tangents, with an example, had been sent to Leibnitz in June 1676, and that on his return from France through England to Germany, he had consulted Newton's letters in the hands of Collins, and had not long after this fallen upon a similar method. We have not succeeded in finding a copy of the scholium, as it was published in the first edition of the Principia,[1] or any traces of the grounds upon which he omitted the historical details in the original draughts of it.

It would be interesting to know why these contemplated additions to the scholium were not adopted, and a single paragraph from the letter of December 10, 1672, substituted for the original scholium. In the letters of Pemberton to Newton, in 1724 and 1725, I have found no reference to this change upon the scholium.

It appears, therefore, that Newton had resolved to overlook the aggressive movement of Leibnitz in 1684; and on another occasion, when he believed his rights to be invaded, he exercised the same forbearance.[2] Circumstances, however, now occurred which induced his friends to come forward in his

[1] On a separate folio sheet I have found the following form of the scholium. The words in italics are not in the printed scholium, in which there is the word *eandem* here omitted. "In literis quæ mihi cum geometra peritissimo G. G. Leibnitio annis abhinc decem intercedebant, cum significarem me compotem esse methodi determinandi maximas et minimas, ducendi tangentes, *quadrandi figuras curvilineas*, et similia peragendi quæ in terminis surdis æque ac in rationalibus procederet, *methodumque exemplis illustrarem, sed fundamentum ejus* literis transpositis hanc sententiam involventibus [Data æquatione quotcunque fluentes quantitates involvente, fluxiones invenire, et vice versa] celarem: rescripsit vir clarissimus, *anno proximo*, se quoque in ejusmodi methodum incidisse, et methodum suam communicavit a mea vix abludentem, præterquam in verborum et notarum formulis. Utriusque fundamentum continetur in hoc Lemmate." This copy does not contain the few words added in the second edition of the Principia.

[2] In the *Acta Eruditorum* for January and February 1689, Leibnitz published two papers, one "On the Motion of Projectiles in a resisting Medium," and the other, "On the Causes of the Celestial Motions." Newton regarded the propositions in these papers, and in a third, *De Lineis Opticis*, as plagiarisms from the Principia, Leibnitz, as he said, "pretending that he had found them all before that book came abroad," and "to make the principal proposition his own, adapting to it an erroneous demonstration, and thereby discovering that he did not yet understand how to work in second differences."—See Raphson's *Fluxions*, p. 117; and *Recensio Commercii Epistolici;* Newtoni *Opera*, tom. iv p. 481, No. lxxii.

cause. Having learned, as we have seen, that Newton's "notions of Fluxions passed there by the name of Leibnitz's Differential Calculus," Dr. Wallis stopped the printing of the Preface to the first volume of his Works, in order to claim for Newton the invention of Fluxions, as contained in the letters of June and October 1676, which had been sent to Leibnitz. In intimating to Newton what he had done, he said, "You are not so kind to your reputation (and that of the nation) as you might be, when you let things of worth lie by you so long, till others carry away the reputation which is due to you."[1]

Early in the year 1691, the celebrated James Bernoulli "spoke contemptuously" of the Differential Calculus, maintaining that it differed from that of Barrow only in notation, and in an abridgment of the operation;[2] but it nevertheless "grew into reputation," and made great progress after the Marquis de l'Hospital had published, in 1696, his excellent work on the Analysis of Infinitesimals. The claims of the two rival geometers increased in value with the stake for which they contended, and an event soon occurred which placed them in open combat. Hitherto neither Newton nor Leibnitz had claimed to

[1] See Appendix, No. XIV. "At the request of Dr. Wallis," says Newton, "I sent to him in two letters, dated 27th August and 17th September 1692, the first proposition of the Book of Quadratures, copied almost verbatim from the book, and also the method of extracting fluents out of equations involving fluxions, mentioned in my letter of 24th October 1676, and copied from an older paper, and an explication of the method of fluxions direct and inverse, comprehended in the sentence, *Data equatione*, &c. &c., and the Doctor printed them all the same year (viz. anno 1692), in the second volume of his works, pp. 391-396. This volume being then in the press, and coming abroad the next year, two years before the first volume was printed off, and this is the first time that the use of letters with pricks, and a rule for finding second, third, and fourth fluxions were published, though they were long before in manuscript. When I considered only first fluxions, I seldom used letters with a prick; but when I considered also second, third, and fourth fluxions, &c., I distinguished them by letters with one, two, or more pricks; and for fluents I put the fluxions either included within a square (as in the aforesaid analysis), or with a square prefixed as in some other papers, or with an oblique line upon it. And these notations by pricks and oblique lines, are the most compendious yet used, but were not known to the Marquis de l'Hospital when he recommended the differential notation, nor are necessary to the method."—*A Supplement to the Remarks*, p. 4.

[2] *Acta Eruditorum*, Jan. 1691, p. 14.

himself the merit of being the sole inventor of the new calculus. Newton was acknowledged even by his rival as the first inventor, and in his scholium he was supposed to have allowed Leibnitz in return the merit of a second inventor. Newton, however, had always believed, without publicly avowing it, that Leibnitz had derived his calculus from the communications made to him by Oldenburg ; and Leibnitz, though he had repeatedly declared that he and Newton had borrowed nothing from each other, was yet inclined to consider his rival as a plagiarist.

This celebrated controversy, rendered interesting by the transcendent talents of its promoters, and instructive by the moral frailties with which it was stained, will form the subject of the following chapter.

APPENDIX.

No. I.

(Referred to in page 30.)

LETTER FROM MR. NEWTON TO FRANCIS ASTON, ESQ., A YOUNG FRIEND WHO WAS ON THE EVE OF SETTING OUT UPON HIS TRAVELS.

Mr. Aston was elected a Fellow of the Royal Society in 1678. He was an active member, and was frequently in the Council. He was chosen one of the Secretaries on the 30th November 1681, and held that office till the 9th December 1685. He had been re-elected on the 30th of November, but, at a meeting of the Council on the 9th December, "he threw up," says Mr. Weld,[1] "the Secretaryship in so sudden and violent a manner, that the Council resolved not to run the risk of being similarly treated on any future occasion, and determined on having an officer more immediately under their command." Halley's letter (dated March 27, 1686, and giving an account of this affair to Mr. William Molyneux) will better explain the circumstances of the case :—

"The history of our affairs," says Halley, "is briefly this. On St. Andrew's day last, being our anniversary day of election, Mr. Pepys was continued President, Mr. Aston, Secretary, and Tancred Robinson chosen in the room of Mr. Musgrave. Every body seemed satisfied, and no discontent appeared anywhere, when, on a sudden, Mr. Aston, as I suppose, willing to gain better terms of reward from the Society than formerly, on December 9th, in Council, declared that he would not serve them as Secretary; and therefore desired them to provide some other to supply that office; and that after such a passionate manner, that I fear he has lost several of his friends by it. The Council, resolved not to be so served for the future, thought it expedient to have only honorary secretaries, and a clerk or amanuensis, upon whom the whole burthen of the business should lie, and to give him a fixed salary, so as to make it worth his while, and he to be accountable to the secretaries for the performance of his office; and, on January 27th last, they chose me for their under officer, with a promise of a salary of fifty pounds per annum at least."[2]

Mr. Aston does not seem to have taken offence at these proceedings of the

[1] *History of the Royal Society*, vol. i. pp. 302, 303.

[2] Notwithstanding Mr. Aston's conduct, the Council ordered that he be presented with a gratuity of £60.

Council. He communicated to the Society some observations on certain unknown ancient characters, which were published in the Philosophical Transactions for 1692; and, previous to his death, which seems to have taken place in 1715, he bequeathed to the Royal Society a small estate, still in their possession, at Mablesthorpe, in Lincolnshire, consisting of 55 acres, 2 roods, and 2 perches. He likewise left to the Society a considerable number of books and some personal property, which, after paying off certain debts, amounted to £445.[1]

On the 27th February 1684-5, Newton addressed to Mr. Aston a letter, in which he states, that the attempt made by himself and Mr. Charles Montague to establish a Philosophical Society at Cambridge, had failed.

The following letter was written when Newton was only twenty-six years of age. We have not been able to find any account of the information which Mr. Aston communicated to his friend, either during his travels or after his return:—

"TRINITY COLLEGE, CAMBRIDGE, *May* 18, 1669.

SIR,—Since in your letter you give mee so much liberty of spending my judgment about what may be to your advantage in travelling, I shall do it more freely than perhaps otherwise would have been decent. First, then, I will lay down some general rules, most of which, I believe, you have considered already; but if any of them be new to you, they may excuse the rest; if none at all, yet is my punishment more in writing than your's in reading.

"When you come into any fresh company, 1. Observe their humours. 2. Suit your own carriage thereto, by which insinuation you will make their converse more free and open. 3. Let your discourse be more in querys and doubtings than peremptory assertions or disputings, it being the designe of travellers to learne, not to teach. Besides, it will persuade your acquaintance that you have the greater esteem of them, and soe make them more ready to communicate what they know to you; whereas nothing sooner occasions disrespect and quarrels than peremptorinesse. You will find little or no advantage in seeming wiser, or much more ignorant than your company. 4. Seldom discommend any thing though never so bad, or doe it but moderately, lest you bee unexpectedly forced to an unhansom retraction. It is safer to commend any thing more than it deserves, than to discommend a thing soe much as it deserves; for commendations meet not soe often with oppositions, or, at least, are not usually soe ill resented by men that think otherwise, as discommendations; and you will insinuate into men's favour by nothing sooner than seeming to approve and commend what they like; but beware of doing it by a comparison. 5. If you bee affronted, it is better, in a forraine country, to pass it by in silence, and with a jest, though with some dishonour, than to endeavour revenge; for, in the first case, your credit's ne'er the worse when you return into England, or come into other company that have not heard of the quarrell. But, in the second case, you may beare the marks of the quarrell while you live, if you outlive it at all. But, if you find yourself unavoidably engaged, 'tis best, I think, if you can command your passion and language, to keep them pretty eavenly at some certain moderate pitch, not much hightning them to exasperate your adversary, or provoke his friends,

[1] Weld's *History of the Royal Society*, vol. i. p. 428.

nor letting them grow over much dejected to make him insult. In a word, if you can keep reason above passion, that and watchfullnesse will be your best defendants. To which purpose you may consider, that, though such excuses as this,—He provok't mee so much I could not forbear,—may pass among friends, yet amongst strangers they are insignificant, and only argue a traveller's weaknesse.

"To these I may add some general heads for inquirys or observations, such as at present I can think on. As, 1. To observe the policys, wealth, and state affairs of nations, so far as a solitary traveller may conveniently doe. 2. Their impositions upon all sorts of people, trades, or commoditys, that are remarkable. 3. Their laws and customs, how far they differ from ours. 4. Their trades and arts, wherein they excell or come short of us in England. 5. Such fortifications as you shall meet with, their fashion, strength, and advantages for defence, and other such military affairs as are considerable. 6. The power and respect belonging to their degrees of nobility or magistracy. 7. It will not be time mispent to make a catalogue of the names and excellencys of those men that are most wise, learned, or esteemed in any nation. 8. Observe the mechanisme and manner of guiding ships. 9. Observe the products of nature in several places, especially in mines, with the circumstances of mining and of extracting metals or minerals out of their oare, and of refining them; and if you meet with any transmutations out of their own species into another (as out of iron into copper, out of any metall into quicksilver, out of one salt into another, or into an insipid body, &c.), those, above all, will be worth your noting, being the most luciferous, and many times lucriferous experiments too in philosophy. 10. The prices of diet and other things. 11. And the staple commoditys of places.

"These generals (such as at present I could think of), if they will serve for nothing else, yet they may assist you in drawing up a modell to regulate your travels by.

"As for particulars, these that follow are all that I can now think of, viz., Whether at Schemnitium, in Hungary (where there are mines of gold, copper, iron, vitriol, antimony, &c.), they change iron into copper by dissolving it in a vitriolate water, which they find in cavitys of rocks in the mines, and then melting the slimy solution in a strong fire, which in the cooling proves copper. The like is said to be done in other places, which I cannot now remember; perhaps, too, it may done in Italy. For about twenty or thirty years agone there was a certain vitrioll came from thence (called Roman vitrioll), but of a nobler virtue than that which is now called by that name; which vitrioll is not now to be gotten, because, perhaps, they make a greater gain by some such trick as turning iron into copper with it, than by selling it. 2. Whether, in Hungary, Sclavonia, Bohemia, near the town Eila, or at the mountains of Bohemia near Silesia, there be rivers whose waters are impregnated with gold; perhaps, the gold being dissolved by some corrosive waters like *aqua regis*, and the solution carried along with the streame, that runs through the mines. And whether the practise of laying mercury in the rivers, till it be tinged with gold, and then straining the mercury through leather, that the gold may stay behind, be a secret yet, or openly practised. 3. There is newly contrived, in Holland, a mill to grind glasses plane withall, and I think polishing them too; perhaps it will be worth the while to see it. 4. There is in Holland one —— Borry, who some years since was imprisoned by the Pope, to have extorted from him secrets (as I am told) of great worth, both as to medicine and profit, but he escaped into Holland, where they have granted him a guard. I think he usually goes cloathed in green. Pray inquire what you can of him, and whether his ingenuity be any profit to the Dutch. You may inform yourself whether the Dutch have any tricks to keep their ships from being all worm-eaten in their voyages to the Indies. Whether pendulum clocks do any service in finding out the longitude, &c.

"I am very weary, and shall not stay to part with a long compliment, only I wish you a good journey, and God be with you.

"Is. Newton.

"Pray let us hear from you in your travels. I have given your two books to Dr. Arrowsmith."

No. II.

(*Referred to in page* 118.)

As Newton's Hypothesis "touching his Theory of Light and Colours," which he communicated to the Royal Society on the 9th December 1675, and which he afterwards illustrated and extended in his celebrated letter to Robert Boyle in 1679, is very little known, and must ever be referred to in the History of Optical Discovery, we have reprinted these two interesting documents :—

AN HYPOTHESIS[1] EXPLAINING THE PROPERTIES OF LIGHT DISCOURSED OF IN MY SEVERAL PAPERS.

"Sir,—In my answer to Mr. Hook, you may remember I had occasion to say something of hypotheses, where I gave a reason why all allowable hypotheses in their genuine constitution should be conformable to my theories, and said of Mr. Hook's hypothesis, that I took the most free and natural application of it to phænomena to be this :—'That the agitated parts of bodies, according to their several sizes, figure, and motions, do excite vibrations in the æther of various depths or bignesses, which being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour ; but if by any means those of unequal bignesses be separated from one another, the largest beget a sensation of a red colour, the least or shortest of a deep violet, and the intermediate ones of intermediate colours, much after the manner that bodies, according to their several sizes, shapes, and motions excite vibrations in the air of various bignesses, which, according to those bignesses, make several tones in sound, &c. I was glad to understand, as I apprehended from Mr. Hook's discourse at my last being at one of your assemblies, that he had changed his former notion of all colours being compounded of only two

[1] In a letter to Oldenburg, dated January 25, 1675-76.

original ones, made by the two sides of an oblique pulse, and accommodated his hypothesis to this my suggestion of colours, like sounds, being various, according to the various bigness of the pulses. For this I take to be a more plausible hypothesis than any other described by former authors; because I see not how the colours of thin transparent plates, or skins, can be handsomely explained without having recourse to ætherial pulses. But yet I like another hypothesis better, which I had occasion to hint something of in the same letter in these words:—'The hypothesis of light's being a body, had I propounded it, has a much greater affinity with the objector's own hypothesis than he seems to be aware of, the vibrations of the æther being as useful and necessary in this as in his. For assuming the rays of light to be small bodies emitted every way from shining substances, those, when they impinge on any refracting or reflecting superficies, must as necessarily excite vibrations in the æther as stones do in water when thrown into it. And supposing these vibrations to be of several depths or thicknesses, accordingly as they are excited by the said corpuscular rays of various sizes and velocities, of what use they will be for explicating the manner of reflexion and refraction, the production of heat by the sunbeams, the emission of light from burning, putrifying, or other substances whose parts are vehemently agitated, the phænomena of thin transparent plates and bubbles, and of all natural bodies, the manner of vision, and the difference of colours, as also their harmony and discord, I shall leave to their consideration who may think it worth their endeavour to apply this hypothesis to the solution of phænomena.' Were I to assume an hypothesis, it should be this, if propounded more generally so as not to determine what light is, further than that it is something or other capable of exciting vibrations in the æther; for thus it will become so general and comprehensive of other hypotheses as to leave little room for new ones to be invented; and therefore because I have observed the heads of some great virtuosos to run much upon hypotheses, as if my discourses wanted an hypothesis to explain them by, and found that some, when I could not make them take my meaning when I spake of the nature of light and colours abstractedly, have readily apprehended it when I illustrated my discourse by an hypothesis; for this reason I have here thought fit to send you a description of the circumstances of this hypothesis, as much tending to the illustration of the papers I herewith send you; and though I shall not assume either this or any other hypothesis, not thinking it necessary to concern myself whether the properties of light discovered by me be explained by this, or Mr. Hook's, or any other hypothesis capable of explaining them; yet while I am describing this, I shall sometimes, to avoid circumlocution and to represent it more conveniently, speak of it as if I assumed it and propounded it to be believed. This I thought fit to express, that no man may confound this with my other discourses, or measure the certainty of one by the other, or think me obliged to answer objections against this script; for I desire to decline being involved in such troublesome, insignificant disputes.

"But to proceed to the hypothesis:—1. It is to be supposed therein, that there is an ætherial medium, much of the same constitution with air, but far rarer, subtiler, and more strongly elastic. Of the existence of this medium, the motion of a pendulum in a glass exhausted of air almost as quickly as in the open air is no inconsiderable argument. But it is not to be supposed that this medium is one uniform matter, but composed partly of the main phlegmatic body of æther, partly of other various ætherial spirits, much after the manner that air is compounded of the phlegmatic body of air intermixed with various vapours and exhalations. For the electric and magnetic effluvia, and the gravitating principle, seem to argue such variety. Perhaps the whole frame of nature may be nothing but various contextures of some certain ætherial spirits or vapours, condensed as it were by precipitation, much after the manner that vapours are condensed into water, or exhalations into grosser substances, though not so easily condensable; and after condensation wrought into various forms, at first by the immediate hand of the Creator, and ever since by the power of nature, which, by virtue of the command, increase and multiply, became a complete imitator of the copy set her by the Protoplast. Thus perhaps may all things be originated from æther.

"At least the electric effluvia seem to instruct us that there is something of an ætherial nature condensed in bodies. I have sometimes laid upon a table a round piece of glass about two inches broad, set in a brass ring, so that the glass might be about one-eighth or one-sixth of an inch from the table, and the air between them inclosed on all sides by the ring, after the manner as if I had whelmed a little sieve upon the table. And then rubbing a pretty while the glass briskly with some rough and raking stuff, till some very little fragments of very thin paper laid on the table under the glass began to be attracted and move nimbly too and fro; after I had done rubbing the glass, the papers would continue a pretty while in various motions, sometimes leaping up to the glass and resting there a while, then leaping down and resting there, then leaping up, and perhaps down and up again, and this sometimes in lines seeming perpendicular to the table, sometimes in oblique ones; sometimes also they would leap up in one arch and down in another divers times together, without sensible resting between; sometimes skip in a bow from one part of the glass to another without touching the table, and sometimes hang by a corner and turn often about very nimbly, as if they had been carried about in the midst of a whirlwind, and be otherwise variously moved,—every paper with a divers motion. And upon sliding my finger on the upper side of the glass, though neither the glass nor the enclosed air below were moved thereby, yet would the papers as they hang under the glass receive some new motion, inclining this way or that way, accordingly as I moved my finger. Now whence all these irregular motions should spring I cannot imagine, unless from some kind of subtile matter lying condensed in the glass, and rarefied by rubbing, as water is rarefied into vapour by heat, and in that rarefaction diffused through the space round the glass to a great distance, and made to move and circulate variously, and accordingly to actuate the papers, till it returns into the glass again, and be recondensed there. And as this condensed matter by rarefaction into an ætherial wind (for by its easy penetrating and circulating through glass I esteem it ætherial) may cause these odd motions, and by condensing again may cause electrical attraction with its returning to the glass to succeed in the place of what is there continually recondensed; so may the gravitating attraction of the earth be caused by the continual condensation of some other such like ætherial spirit, not of the main body of phlegmatic æther, but of something very thinly and subtilely diffused through it, perhaps of an unctuous, or gummy tenacious and springy nature; and bearing much the same relation to æther which the vital aërial spirit requisite for the conservation of flame and vital motions does to air. For if such an ætherial spirit may be condensed in fermenting or burning bodies, or otherwise coagulated in the pores of the earth and water into some kind of humid active matter for the continual uses of nature (adhering to the sides of those pores after the manner that vapours condense on the sides of a vessel), the vast body of the earth, which may be everywhere to the very centre in perpetual working, may continually condense so much of this spirit as to cause it from above to descend with greater celerity for a supply: in which descent it may bear down with it the bodies it pervades with force proportional to the superficies of all their parts it acts upon, nature making a circulation by the slow ascent of as much matter out of the bowels of the earth in an aërial form, which for a time constitutes the atmosphere, but being continually buoyed up by the new air, exhalations, and vapours rising underneath, at length (some part of the vapours which return in rain excepted) vanishes again into the ætherial spaces, and there perhaps in time relents and is attenuated into its first principle. For nature is a perpetual circulatory worker, generating fluids out of solids, and solids out of fluids, fixed things out of volatile, and volatile out of fixed, subtile out of gross, and gross out of subtile, some things to ascend and make the upper terrestrial juices, rivers, and the atmosphere, and by consequence others to descend for a requital to the former. And as the earth, so perhaps may the sun imbibe this spirit copiously, to conserve his shining, and keep the planets from receding further from him; and they that will may also suppose that this spirit affords or carries with it thither the solary fuel and material principle of light, and that the vast ætherial spaces between us and the stars are for a sufficient repository for this food of the sun and planets. But this of the constitution of ætherial natures by the bye.

"In the second place, it is to be supposed that the æther is a vibrating medium like air, only the vibrations far more swift and minute; those of air made by a man's ordinary voice, succeeding one another at more than half a foot or a foot distance, but those of æther at a less distance than the hundred-thousandth part of an inch. And as in air the vibrations are some larger than others, but yet all equally swift (for in a ring of bells the sound of every tone is heard at two or three miles' distance in the same order that the bells are struck), so I suppose the ætherial vibrations differ in bigness, but not in swiftness. Now these vibrations, besides their use in reflection and refraction, may be supposed the chief means by which the parts of fermenting or putrifying substances, fluid liquors, or melted, burning, or other hot bodies, continue in motion, are shaken asunder like a ship by waves, and dissipated into vapours, exhalations, or smoke, and light loosed or excited in those bodies, and consequently by which a body becomes a burning coal, and smoke flame; and I suppose flame is nothing but the particles of smoke turned by the access of light and heat to burning coals, little and innumerable.

"Thirdly, the air can pervade the bores of small glass pipes, but yet not so easily as if they were wider, and therefore stands at a greater degree of rarity than in the free aërial spaces, and at so much greater a degree of rarity as the pipe is smaller, as is known by the rising of water in such pipes to a much greater height than the surface of the stagnating water into which they are dipped. So I suppose æther, though it pervades the pores of crystal, glass, water, and other natural bodies, yet it stands at a greater degree of rarity in those pores than in the free ætherial spaces, and at so much a greater degree of rarity as the pores of the body are smaller. Whence it may be that spirit of wine, for instance, though a lighter body, yet having subtler parts, and consequently smaller pores than water, is the more strongly refracting liquor. This also may be the principal cause of the cohesion of the parts of solids and fluids, of the springiness of glass and other bodies whose parts slide not one upon another in bending, and of the standing of the mercury in the Torricellian experiment, sometimes to the top of the glass, though a much greater height than twenty-nine inches. For the denser æther which surrounds these bodies must crowd and press their parts together, much after the manner that air surrounding two marbles presses them together if there be little or no air between them. Yea, and that puzzling problem, *by what means the muscles are contracted and dilated to cause animal motion, may receive greater light from hence than from any other means men have hitherto been thinking on.* For if there be any power in man to condense and dilate at will the æther that pervades the muscle, that condensation or dilatation must vary the compression of the muscle made by the ambient æther, and cause it to swell or shrink, accordingly; for though common water will scarce shrink by compression and swell by relaxation, yet (so far as my observation reaches) spirit of wine and oil will; and Mr. Boyle's experiment of a tadpole shrinking very much by hard compressing the water in which it swam, is an argument that animal juices do the same: and as for their various pression by the ambient æther, it is plain that that must be more or less, accordingly as there is more or less æther within to sustain and counterpoise the pressure of that without. If both æthers were equally dense, the muscle would be at liberty as if pressed by neither: if there were no æther within, the ambient would compress it with the whole force of its spring. If the æther within were twice as much dilated as that without, so as to have but half as much springiness, the ambient would have half the force of its springiness counterpoised thereby, and exercise but the other half upon the muscle; and so in all other cases the ambient compresses the muscle by the excess of the force of its springiness above that of the springiness of the included. To vary the compression of the muscle therefore, and so to swell and shrink it, there needs nothing but to change the consistence of the included æther; and a very little change may suffice, if the spring of æther be supposed very strong, as I take it to be many degrees stronger than that of air.

"Now for the changing the consistence of the æther, some may be ready to grant that the soul may have an immediate power over the whole æther in any part of the body, to swell or shrink it at will; but then how depends the muscular motion on the nerves? Others therefore may be more apt to think it done by some certain ætherial spirits included within the *dura mater*, which the soul may have power to contract or dilate at will in any muscle, and so cause it to flow thither through the nerves; but still there is a difficulty why this force of the soul upon it does not take off the power of springiness, whereby it should sustain more or less the force of the outward æther. A third supposition may be, that the soul has a power to inspire any muscle with this spirit, by impelling it thither through the nerves; but this too has its difficulties; for it requires a forcible intruding the spring of the æther in the muscles by pressure exerted from the parts of the brain; and it is hard to conceive how so great force can be exercised amidst so tender matter as the brain is; and besides, why does not this ætherial spirit, being subtile enough, and urged with so great force, go away through the *dura mater* and skins of the muscle, or at least so much of the other æther go out to make way for this which is crowded in? To take away these difficulties is a digression, but seeing the subject is a deserving one, I shall not stick to tell you how I think it may be done.

"First, then, I suppose there *is* such a spirit; that is, that the animal spirits are neither like the liquor, vapour, or gas, of spirits of wine; but of an ætherial nature, subtile enough to pervade the animal juices as freely as the electric, or perhaps magnetic, effluvia do glass. And to know how the coats of the brain, nerves, and muscles, may become a convenient vessel to hold so subtile a spirit, you may consider how liquors and spirits are disposed to pervade, or not pervade, things on other accounts than their subtilty; water and oil pervade wood and stone, which quicksilver does not; and quicksilver, metals, which water and oil do not; water and acid spirits pervade salts, which oil and spirit of wine do not; and oil and spirit of wine pervade sulphur, which water and acid spirits do not; so some fluids (as oil and water), though their parts are in freedom enough to mix with one another, yet by some secret principle of *unsociableness* they keep asunder; and some that are *sociable* may become *unsociable* by adding a third thing to one of them, as water to spirit of wine by dissolving salt of tartar in it. The like *unsociableness* may be in ætherial natures, as perhaps between the æthers in the vortices of the sun and planets; and the reason why air stands rarer in the bores of small glass pipes, and æther in the pores of bodies, may be, not want of subtilty, but *sociableness;* and on this ground, if the ætherial vital spirit in a man be very *sociable* to the marrow and juices, and *unsociable* to the coats of the brain, nerves, and muscles, or to anything lodged in the pores of those coats, it may be contained thereby, notwithstanding its subtilty; especially if we suppose no great violence done to it to squeeze it out, and that it may not be altogether so subtile as the main body of æther, though subtile enough to pervade readily the animal juices, and that as any of it is spent, it is continually supplied by new spirit from the heart.

"In the next place, for knowing how this spirit may be used for animal motion, you may consider how some things unsociable are made sociable by the mediation of a third. Water, which will not dissolve copper, will do it if the copper be melted with sulphur. Aquafortis, which will not pervade gold, will do it by addition of a little sal-ammoniac or spirit of salt. Lead will not mix in melting with copper; but if a little tin, or antimony, be added, they mix readily, and part again of their own accord, if the antimony be wasted by throwing saltpetre, or otherwise. And so lead melted with silver quickly pervades and liquifies the silver in a much less heat than is required to melt the silver alone; but if they be kept in the test till that little substance that reconciled them be wasted or altered, they part again of their own accord. And in like manner the ætherial animal spirit in a man may be a mediator between the common æther, and the muscular juices, to make them mix more freely; and so by sending a little of this spirit into any muscle, though so little as to cause no sensible tension of the muscle by its own force, yet by rendering the juices more sociable to the common external æther, it may cause that æther to pervade the muscle of its own accord in a moment more freely and more copiously than it would otherwise do, and to recede again as freely, so soon as this mediator of sociableness is retracted; whence, according to what I said above, will proceed the swelling or shrinking of the muscle, and consequently the animal motion depending thereon.

"Thus may therefore the soul, by determining this ætherial animal spirit or wind into this or that nerve, perhaps with as much ease as air is moved in open spaces, cause all the motions we see in animals; for the making which motions strong, it is not necessary that we should suppose the æther within the muscle very much condensed, or rarefied, by this means, but only that its spring is so very great that a little alteration of its density shall cause a great alteration in the pressure. And what is said of muscular motion may be applied to the motion of the heart, only with this difference; that the spirit is not sent thither as into other muscles, but continually generated there by the fermentation of the juices with which its flesh is replenished, and as it is generated, let out by starts into the brain, through some convenient *ductus*, to perform those motions in other muscles by inspiration, which it did in the heart by its generation. For I see not why the ferment in the heart may not raise as subtile a spirit out of its juices, to cause those motions, as rubbing does out of a glass to cause electric attraction, or burning out of fuel to penetrate glass, as Mr. Boyle has shown, and calcine by corrosion metals melted therein.[1]

"Hitherto I have been contemplating the nature of æther and ætherial substances by their effects and uses, and now I come to join therewith the consideration of light.

"In the fourth place, therefore, I suppose light is neither æther, nor its vibrating motion, but something of a different kind propagated from lucid bodies. They that will may suppose it an aggregate of various peripatetic qualities. Others may suppose it multitudes of unimaginable small and swift corpuscles of various sizes springing from shining bodies at great distances one after another, but yet without any sensible interval of time, and continually urged forward by a principle of motion, which in the beginning accelerates them, till the resistance of the ætherial medium equal the force of that principle, much after the manner that bodies let fall in water are accelerated till the resistance of the water equals the force of gravity. God, who gave animals motion beyond our understanding, is, without doubt, able to implant other principles of motions in bodies which we may understand as little. Some would readily grant this may be a spiritual one; yet a mechanical one might be shown, did not I think it better to pass it by. But they that like not this, may suppose light any other corporeal emanation, or an impulse or motion of any other medium or ætherial spirit diffused through the main body of æther, or what else they imagine proper for this purpose. To avoid dispute, and make this hypothesis general, let every man here take his fancy; only whatever light be, I would suppose it consists of successive rays differing from one another in contingent circumstances, as bigness, force, or vigour, like as the sands on the shore, the

[1] Boyle's Essays of the strange subtilty, &c., of effluviums, &c., together with a discovery of the perviousness of glass to ponderable parts of flame.

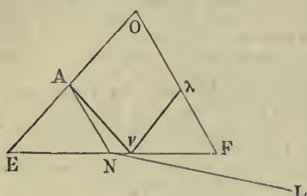
waves of the sea, the faces of men, and all other natural things of the same kind differ, it being almost impossible for any sort of things to be found without some contingent variety. And further, I would suppose it diverse from the vibrations of the æther, because (besides that were it those vibrations, it ought always to verge copiously in crooked lines into the dark or quiescent medium, destroying all shadows, and to comply readily with any crooked pores or passages as sounds do) I see not how any superficies (as the side of a glass prism on which the rays within are incident at an angle of about forty degrees) can be totally opake. For the vibrations beating against the refracting confine of the rarer and denser æther must needs make that pliant superficies undulate, and those undulations will stir up and propagate vibrations on the other side. And further, how light, incident on very thin skins or plates of any transparent body, should for many successive thicknesses of the plate in arithmetical progression, be alternately reflected and transmitted, as I find it is, puzzles me as much. For though the arithmetical progression of those thicknesses, which reflect and transmit the rays alternately, argues that it depends upon the number of vibrations between the two superficies of the plate, whether the ray shall be reflected or transmitted, yet I cannot see how the number should vary the case, be it greater or less, whole or broken, unless light be supposed something else than these vibrations. Something indeed I could fancy towards helping the two last difficulties, but nothing which I see not insufficient.

"Fifthly, it is to be supposed that light and æther mutually act upon one another, æther in refracting light, and light in warming æther, and that the densest æther acts most strongly. When a ray therefore moves through æther of uneven density, I suppose it most pressed, urged, or acted upon by the medium on that side towards the denser æther, and receives a continual impulse or ply from that side to recede towards the rarer, and so is accelerated if it move that way, or retarded if the contrary. On this ground, if a ray move obliquely through such an unevenly dense medium (that is, obliquely to those imaginary superficies which run through the equally dense parts of the medium, and may be called the refracting superficies), it must be incurved, as it is found to be by observation in water,[1] whose lower parts were made gradually more salt, and so more dense than the upper. And this may be the ground of all refraction and reflexion. For as the rarer air within a small glass pipe, and the denser without, are not distinguished by a mere mathematical superficies, but have air between them at the orifice of the pipe running through all intermediate degrees of density, so I suppose the refracting superficies of æther between unequally dense mediums to be not a mathematical one, but of some breadth, the æther therein at the orifices of the pores of the solid body being of all intermediate degrees of density between the rarer and the denser ætherial mediums; and the refraction I conceive to proceed from the continual incurvation of the ray all the while it is passing the physical superficies. Now if the motion of the ray be supposed in this passage to be increased or diminished in a certain proportion, according to the difference of the densities of the ætherial mediums, and the addition or detraction of the motion be reckoned in the perpendicular from the refracting superficies, as

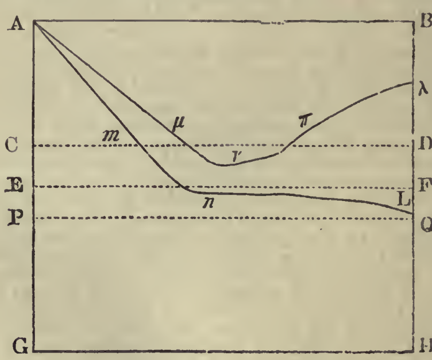
[1] Mr. Hook's Micrographia where he speaks of the inflexion of rays.

it ought to be, the sines of incidence and refraction will be proportional according to what Descartes has demonstrated.

"The ray, therefore, in passing out of the rarer medium into the denser, inclines continually more and more towards parallelism with the refracting superficies; and if the different densities of the mediums be not so great, nor the incidence of the ray so oblique as to make it parallel to that superficies before it gets through, then it goes through and is refracted; but if through the aforesaid causes the ray becomes parallel to that superficies before it can get through, then it must turn back and be reflected. Thus, for instance, it may be observed in a triangular glass prism O E F, that the rays A N that tend out of the glass into air, do, by inclining them more to the refracting superficies, emerge more and more obliquely till they be infinitely oblique, that is, in a manner parallel to the superficies, which happens when the angle of incidence is about 40°; and then if they be a little more inclined, are all reflected, as at A ν λ, becoming, I suppose, parallel to the superficies before they can get through it.



"Let A B C D represent the rarer medium, E F H G the denser, C D F E the space between them or refracting physical superficies, in which the æther



is of all intermediate degrees of density, from the rarest æther at C D to the densest at E F; A m n L a ray, A m its incident part, m n its incurvation by the refracting superficies, and n L its emergent part. Now, if the ray A m be so much incurved as to become at its emergence n, as nearly as may be, parallel to C D, it is plain that if that ray had been incident a little more obliquely, it must have become parallel to C D before it had arrived at E F, the further side of the refracting superficies, and so could have got no nearer to E F, but must have turned back by further incurvation, and been reflected as it is represented at A $\mu \nu \lambda$: and the like would have happened if the density of the æther had further increased from E F to P Q, so that P Q H G might be a denser medium than E F G H was supposed; for then the ray in passing from m to n, being so much incurved as at n to become parallel to C D or P Q, it's impossible it should ever get nearer to P Q, but must at n begin by further incurvation to turn back, and so be reflected. And because if a refracted ray (as n L) be made incident, the incident (A m) shall become the refracted; and therefore if the ray A $\mu \nu$, after it is arrived at ν, where I suppose it parallel to the refracting superficies, should be reflected perpendicularly back, it would return back in the line of incidence $\nu \mu$ A; therefore going forward, it must go forward in such another line $\nu \pi \lambda$, both cases being alike, and so be reflected at an angle equal to that of incidence.

"This may be the cause and manner of reflexion, when light tends from the rarer towards the denser æther; but to know how it should be reflected when it tends from the denser towards the rarer, you are farther to consider, how fluids near their superficies are less pliant and yielding than in their more inward parts, and if formed into thin plates or shells, they become much more stiff and tenacious than otherwise. Thus things which readily fall in water, if let fall upon a bubble of water, they do not easily break through it, but are apt to slide down by the sides of it, if they be not too big and heavy. So if two well-polished convex glasses, ground on very large spheres, be laid one upon the other, the air between them easily recedes till they almost touch, but then begins to resist so much that the weight of the upper glass is too little to bring them together, so as to make the black (mentioned in the papers I sent you) appear in the midst of the rings of colours. And if the glasses be plain, though no broader than a twopence, a man with his whole strength is not able to press all the air out from between them, so as to make them fully touch. You may observe also that insects will walk upon water without wetting their feet, and the water bearing them up; also motes falling upon water will often lie long upon it without being wetted. And so I suppose æther in the confine of two mediums is less pliant and yielding than in other places, and so much the less pliant by how much the mediums differ more in density; so that in passing out of denser æther into rarer, when there remains but a very little of the denser æther to be passed through, a ray finds more than ordinary difficulty to get through, and so great difficulty where the mediums are of a very differing density as to be reflected by incurvation after the manner described above, the parts of æther on the side where they are less pliant and yielding, acting upon the ray much after the manner that they would do were they denser there than on the other side; for the resistance of the medium ought to have the same effect on the ray from whatsoever cause it arises. And this I suppose may be the cause of the reflexion of quicksilver and other metalline bodies. It must also concur to increase the reflective virtue of the superficies when rays tend out of the rarer medium into the denser; and in that case therefore the reflexion having a double cause ought to be stronger than in the æther, as it is apparently. But in refraction this rigid tenacity or unpliableness of the superficies need not be considered, because so much as the ray is thereby bent in passing to the most tenacious and rigid part of the superficies, so much is it thereby unbent again in passing on from thence through the next parts gradually less tenacious.

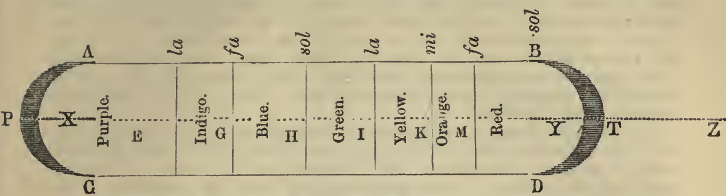
"Thus may rays be refracted by some superficies, and reflected by others, be the medium they tend into denser or rarer. But it remains further to be explained, how rays alike incident on the same superficies (suppose of crystal, glass, or water) may be, at the same time, some refracted, others reflected; and for explaining this, I suppose that the rays when they impinge on the rigid resisting ætherial superficies, as they are acted upon by it, so they react upon it, and cause vibrations in it, as stones thrown into water do in its surface; and that these vibrations are propagated every way into both the rarer and denser mediums, as the vibrations of air which cause sound are from a stroke, but yet continue strongest where they began, and alternately contract and dilate the æther in that physical superficies. For it's plain by the heat which light produces in bodies that it is able to put their parts in motion, and much more to heat and put in motion the more tender æther; and it's more probable that it communicates motion to the gross parts of bodies by the mediation of æther than immediately; as, for instance, in the inward parts of quicksilver, tin, silver, and other very opake bodies, by generating vibrations that run through them, than by striking the outward parts only without entering the body. The shock of every single ray may generate many thousand vibrations, and by sending them all over the body, move all the parts, and that perhaps with more motion than it could move one single part by an immediate stroke; for the vibrations, by shaking each particle backward and forward, may every time increase its motion, as a ringer does a bell by often pulling it, and so at length move the particles to a very great degree of agitation, which neither the simple shock of a ray, nor any other motion in the æther, besides a vibrating one, could do. Thus in air shut up in a vessel, the motion of its parts caused by heat, how violent soever, is unable to move the bodies hung in it with either a trembling or progressive motion; but if air be put into a vibrating motion by beating a drum or two, it shakes glass windows, the whole body of a man, and other massy things, especially those of a congruous tone; yea, I have observed it manifestly shake under my feet a cellared free-stone floor of a large hall; so as I believe the immediate stroke of five hundred drum-sticks could not have done, unless perhaps quickly succeeding one another at equal intervals of time. Ætherial vibrations are therefore the best means by which such a subtile agent as light can shake the gross particles of solid bodies to heat them. And so supposing that light impinging on a refracting or reflecting ætherial superficies puts it into a vibrating motion, that physical superficies being by the perpetual appulse of rays always kept in a vibrating motion, and the æther therein continually expanded and compressed by turns; if a ray of light impinge upon it while it is much compressed, I suppose it is then too dense and stiff to let the ray pass through, and so reflects it; but the rays that impinge on it at other times, when it is either expanded by the interval of two vibrations, or not too much compressed and condensed, go through, and are refracted.

"These may be the causes of refractions and reflexions in all cases, but for understanding how they come to be so regular, it's further to be considered, that, as in a heap of sand, although the surface be rugged, yet if water be poured on it to fill its pores, the water, so soon as its pores are filled, will evenly overspread the surface, and so much the more evenly as the sand is finer; so, although the surface of all bodies, even the most polished, be rugged, as I conceive, yet when that ruggedness is not too gross and coarse, the refracting ætherial superficies may evenly overspread it. In polishing glass or metal, it is not to be imagined that sand, putty, or other fretting powders should wear the surface so regularly as to make the front of every particle exactly plane, and all those planes look the same way, as they ought to do in well-polished bodies, were reflexion performed by their parts; but, that those fretting powders should wear the bodies first to a coarse ruggedness, such as is sensible, and then to a finer and finer ruggedness, till it be so fine that the ætherial superficies evenly overspreads it, and so makes the body put on the appearance of a polish, is a very natural and intelligible supposition. So in fluids it is not well to be conceived that the surfaces of their parts should be all plain, and the planes of the superficial parts always kept looking all the same way, notwithstanding that they are in perpetual motion, and yet without these two suppositions, the superficies of fluids could not be so regularly reflexive as they are, were the reflexion done by the parts themselves, and not by an ætherial superficies evenly overspreading the fluid.

"Further, considering the regular motion of light, it might be suspected whether the various vibrations of the fluid through which it passes may not much disturb it; but that suspicion I suppose will vanish by considering, that if at any time the foremost part of an oblique wave begin to turn it awry, the hindermost part by a contrary action must soon set it straight again.

"Lastly, because without doubt there are in every transparent body pores of various sizes, and I said that æther stands at the greatest rarity in the smallest pores, hence the æther in every pore should be of a differing rarity, and so light be refracted in its passage out of every pore into the next, which would cause a great confusion, and spoil the body's transparency; but, considering that the æther in all dense bodies is agitated by continual vibrations, and these vibrations cannot be performed without forcing the parts of æther forward and backward from one pore to another by a kind of tremor, so that the æther which one moment is in a great pore is the next moment forced into a less; and, on the contrary, this must evenly spread the æther into all the pores not exceeding some certain bigness, suppose the breadth of a vibration, and so make it of an even density throughout the transparent body, agreeable to the middle sort of pores. But where the pores exceed a certain bigness, I suppose the æther suits its density to the bigness of the pore or to the medium within it, and so, being of a divers density from the æther that surrounds it, refracts, or reflects light in its superficies, and so makes the body where many such interstices are, appear opake.

"Thus much of refraction, reflexion, transparency, and opacity;—and now to explain colours. I suppose that as bodies of various sizes, densities, or tensions, do by percussion or other action, excite sounds of various tones, and consequently vibrations in the air of various bignesses; so, when the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the æther, those rays, whatever they be, as they happen to differ in magnitude, strength, or vigour, excite vibrations of various bignesses; the biggest, strongest, or most potent rays, the largest vibrations, and others shorter, according to their bigness, strength, or power; and therefore the ends of the capillamenta of the optic nerve, which front or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations; which vibrations (like those of sound in a trumpet) will run along the aqueous pores or crystalline pith of the capillamenta, through the optic nerves into the sensorium (which light itself cannot do), and there, I suppose, affect the sense with various colours, according to their bigness and mixture: the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green, and a confusion of all, with white; much after the manner that in the sense of hearing nature makes use of aërial vibrations of several bignesses, to generate sounds of divers tones; for the analogy of nature is to be observed. And further, as the harmony and discord of sounds proceed from the proportions of the aërial vibrations, so may the harmony of some colours, as of a golden and blue, and the discord of other, as of red and blue, proceed from the proportions of the ætherial. And possibly colour may be distinguished into its principal degrees: red, orange, yellow, green, blue, indigo, and deep violet,—on the same ground that sound within an eighth is graduated into tones. For, some years past, the prismatic colours, being in a well-darkened room, cast perpendicularly upon a paper about two-and-twenty foot distant from the prism, I desired a friend to draw with a pencil lines across the image or pillar of colours, where every one of the seven aforenamed colours was most full and brisk, and also where he judged the truest confines of them to be, whilst I held the paper so that the said image might fall within a certain compass marked on it. And this I did, partly because my own eyes are not very critical in distinguishing colours, partly because another to whom I had not communicated my thoughts about this matter could have nothing but his eyes to determine his fancy in making those marks. This observation we repeated divers times, both in the same and divers days, to see how the marks on several papers would agree; and comparing the observations, though the just confines of the colours are hard to be assigned, because they passed into one another by insensible gradation, yet the differences of the observations were but little, especially towards the red end; and taking means between those differences that were, the length of the image (reckoned not by the distance of the verges of the semicircular ends, but by the distance of the centres of those semicircles, or length of the straight sides, as it ought to be) was divided in about the same proportion that a string is between the end and the middle to sound the tones in an eighth. You will understand me best by viewing the annexed figure, in which AB and CD represent the straight sides about ten inches long, APC and BTD the semicircular ends, X and Y the centres of those semicircles, XZ the length of a musical string double to XY, and divided between X and Y so as to sound the tones expressed at the side (that is, XH the half, XG and GI the third part, YK the fifth part, YM the eighth part, and GE the ninth part of XY); and the intervals between these divisions express the spaces which the colours written there took up, every colour being most briskly specific in the middle of those spaces. Now for the cause of



these and such like colours made by refraction, the biggest or strongest rays must penetrate the refracting superficies more freely and easier than the weaker, and so be less turned awry by it, that is less refracted; which is as much as to say, the rays which make red are least refrangible, those which which make blue, or violet, most refrangible, and others otherwise refrangible according to their colour. Whence if the rays which come promiscuously from the sun be refracted by a prism, as in the aforesaid experiment, those of several sorts being variously refracted, must go to several places on an opposite paper or wall, and so parted, exhibit every one their own colours, which they could not do while blended together. And because refraction only severs them, and changes not the bigness or strength of the ray, thence it is, that after they are once well-severed, refraction cannot make any further changes in their colour. On this ground may all the phænomena of refractions be understood."

Letter from Newton to Robert Boyle.

"Honoured Sir,—I have so long deferred to send you my thoughts about the physical qualities we speak of, that did I not esteem myself obliged by promise, I think I should be ashamed to send them at all. The truth is, my notions about things of this kind are so indigested, that I am not well satisfied myself in them; and what I am not satisfied in, I can scarce esteem fit to be communicated to others; especially in natural philosophy, where there is no end of fancying. But because I am indebted to you, and yesterday met with a friend, Mr. Maulyverer, who told me he was going to London, and intended to give you the trouble of a visit, I could not forbear to take the opportunity of conveying this to you by him.

"It being only an explication of qualities which you desire of me, I shall set down my apprehensions in the form of suppositions as follows. And first, I suppose that there is diffused through all places an ætherial sub stance, capable of contraction and dilatation, strongly elastic, and, in a word, much like air in all respects, but far more subtile.

"2. I suppose this æther pervades all gross bodies, but yet so as to stand rarer in their pores than in free spaces, and so much the rarer, as their pores are less; and this I suppose (with others) to be the cause why light incident on those bodies is refracted towards the perpendicular; why two well-polished metals cohere in a receiver exhausted of air; why ☿ stands sometimes up to the top of a glass pipe, though much higher than thirty inches; and one of the main causes why the parts of all bodies cohere; also the cause of filtration, and of the rising of water in small glass pipes above the surface of the stagnating water they are dipped into; for I suspect the æther may stand rarer, not only in the insensible pores of bodies, but even in the very sensible cavities of those pipes; and the same principle may cause menstruums to pervade with violence the pores of the bodies they dissolve, the surrounding æther, as well as the atmosphere, pressing them together.

"3. I suppose the rarer æther within bodies, and the denser without them, not to be terminated in a mathematical superficies, but to grow gradually into one another; the external æther beginning to grow rarer, and the internal to grow denser, at some little distance from the superficies of the body, and running through all intermediate degrees of density in the intermediate spaces; and this may be the cause why light, in Grimaldo's experiment, passing by the edge of a knife, or other opake body, is turned aside, and as it were refracted, and by that refraction makes several colours. Let A B C D be a dense body, whether opake or transparent, E F G H the outside of the uniform æther, which is within it, I K L M the inside of the uniform æther, which is without it; and conceive the æther, which is between E F G H and I K L M, to run through all intermediate degrees of density

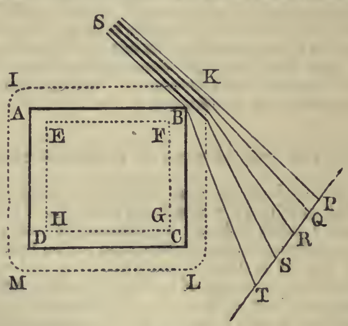


FIG. 1.

between that of the two uniform æthers on either side. This being supposed, the rays of the sun S B, S K, which pass by the edge of this body between B and K, ought in their passage through the unequally dense æther there, to receive a ply from the denser æther, which is on that side towards K, and that the more by how much they pass nearer to the body, and thereby to be scattered through the space P Q R S T, as by experience they are found to be. Now the space between the limits E F G H and I K L M, I shall call the space of the æther's graduated rarity.

"4. When two bodies moving towards one another come near together, I suppose the æther between them to grow rarer than before, and the spaces of its graduated rarity to extend further from the superficies of the bodies towards one another; and this, by reason that the æther cannot move and play up and down so freely in the straight passage between the bodies, as it could before they came so near together: thus, if the space of the æther's graduated rarity reach from the body A B C D F E only to the distance G H L M R S, when no other body is near it, yet may it reach further, as to I K, when another body N O P Q approaches; and as the other body approaches more and more, I suppose the æther between them will grow rarer and rarer. These suppositions I have so described, as if I thought the spaces of graduated æther had precise limits, as is expressed at I K L M

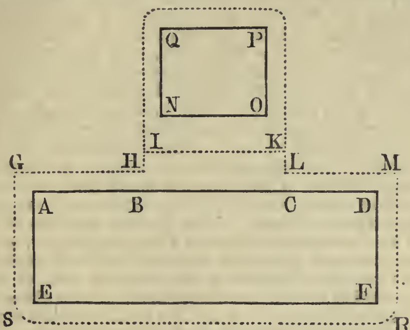


FIG. 2.

in the first figure, and G M R S in the second; for thus I thought I could better express myself. But really I do not think they have such precise limits, but rather decay insensibly, and, in so decaying, extend to a much greater distance than can easily be believed or need be supposed.

"5. Now, from the fourth supposition it follows, that when two bodies approaching one another come so near together as to make the æther between them begin to rarefy, they will begin to have a reluctance from being brought nearer together, and an endeavour to recede from one another; which reluctance and endeavour will increase as they come nearer together, because thereby they cause the interjacent æther to rarefy more and more. But at length, when they come so near together that the excess of pressure of the external æther which surrounds the bodies, above that of the rarefied æther, which is between them, is so great as to overcome the reluctance which the bodies have from being brought together; then will that excess of pressure drive them with violence together, and make them adhere strongly to one another, as was said in the second supposition. For instance, in the second figure, when the bodies E D and N P are so near together that the spaces of the æther's graduated rarity begin to reach to one another, and meet in the line I K, the æther between them will have suffered much rarefaction, which rarefaction requires much force, that is, much pressing of the bodies together; and the endeavour which the æther between them has to return to its former natural state of condensation, will cause the bodies to have an endeavour of receding from one another. But, on the other hand, to counterpoise this endeavour, there will not yet be any excess of density of the æther which surrounds the bodies, above that of the æther which is between them at the line I K. But if the bodies come nearer together, so as to make the æther in the mid-way line I K grow rarer than the surrounding æther, there will arise from the excess of density of the surrounding æther a compressure of the bodies towards one another, which, when by the nearer approach of the bodies it become so great as to overcome the aforesaid endeavour the bodies have to recede from one another, they will then go towards one another and adhere together. And, on the contrary, if any power force them asunder to that distance, where the endeavour to recede begins to overcome the endeavour to accede, they will again leap from one another. Now hence I conceive it is chiefly that a fly walks on water without wetting her feet, and consequently without touching the water; that two polished pieces of glass are not without pressure brought to contact, no, not though the one be plain, the other a little convex; that the particles of dust cannot by pressing be made to cohere, as they would do, if they did but fully touch; that the particles of tinging substances and salts dissolved in water do not of their own account concrete and fall to the bottom, but diffuse themselves all over the liquor, and expand still more if you add more liquor to them. Also, that the particles of vapours, exhalations, and air do stand at a distance from one another, and endeavour to recede as far from one another as the pressure of the incumbent atmosphere will let them; for I conceive the confused mass of vapours, air, and exhalations which we call the atmosphere, to be nothing else but the particles of all sorts of bodies, of which the earth consists, separated from one another, and kept at a distance, by the said principle.

"From these principles the action of menstruums upon bodies may be thus explained: suppose any tinging body, as cochineal or logwood be put into water; so soon as the water sinks into its pores and wets on all sides any particle which adheres to the body only by the principle in the second supposition, it takes off, or at least much diminishes, the efficacy of that principle to hold the particle to the body, because it makes the æther on all sides the particle to be of a more uniform density than before. And then the particle being shaken off by any little motion, floats in the water, and with many such others makes a tincture; which tincture will be of some lively colour, if the particles be all of the same size and density; otherwise of a dirty one. For the colours of all natural bodies whatever seem to depend on nothing but the various sizes and densities of their particles, as I think you have seen described by me more at large in another paper. If the particles be very small (as are those of salts, vitriols, and gums), they are transparent; and as they are supposed bigger and bigger, they put on these colours in order, black, white, yellow, red; violet, blue, pale green, yellow, orange, red; purple, blue, green, yellow, orange, red, &c., as it is discerned by the colours, which appear at the several thicknesses of very thin plates of transparent bodies. Whence, to know the causes of the changes of colours, which are often made by the mixtures of several liquors, it is to be considered how the particles of any tincture may have their size or density altered by the infusion of another liquor. When any metal is put into common water the water cannot enter into its pores, to act on it and dissolve it. Not that water consists of too gross parts for this purpose, but because it is unsociable to metal. For there is a certain secret principle in nature, by which liquors are sociable to some things and unsociable to others; thus water will not mix with oil, but readily with spirit of wine, or with salts; it sinks also into wood, which quicksilver will not; but quicksilver sinks into metals, which, as I said, water will not. So aquafortis dissolves ☽, not ☉; aqua regis ☉, not ☽, &c. But a liquor, which is of itself unsociable to a body, may, by the mixture of a convenient mediator, be made sociable; so molten lead, which alone will not mix with copper, or with regulus of Mars, by the addition of tin is made to mix with either. And water, by the mediation of saline spirits, will mix with metal. Now when any metal is put in water impregnated with such spirits, as into aquafortis, aqua regis, spirit of vitriol, or the like, the particles of the spirits, as they, in floating in the water, strike on the metal, will by their sociableness enter into its pores and gather round its outside particles, and by advantage of the continual tremor the particles of the metals are in, hitch themselves in by degrees between those particles and the body, and loosen them from it; and the water entering into the pores together with the saline spirits, the particles of the metal will be thereby still more loosed, so as by that motion the solution puts them into, to be easily shaken off, and made to float in the water: the saline particles still encompassing the metallic ones as a coat or shell does a kernel, after the manner expressed in the annexed figure, in which figure I have made the particles round, though they may be cubical, or of any other shape.


Fig. 3.

"If into a solution of metal thus made be poured a liquor abounding with particles, to which the former saline particles are more sociable than to the particles of the metal (suppose with particles of salt of tartar), then so soon as they strike on one another in the liquor, the saline particles will adhere to those more firmly than to the metalline ones, and by degrees be wrought off from those to enclose these. Suppose A a metalline particle, inclosed with saline ones of spirit of nitre, E a particle of salt of tartar, contiguous to two of the particles of spirit of nitre, *b* and *c*; and suppose the particle E is impelled by any motion towards *d*, so as to roll about the particle *c* till it touch the particle *d*, the particle *b* adhering more firmly to E than to A, will be forced off from A; and by the same means the particle E, as it rolls about A, will tear off the rest of the saline particles from A one after another, till it has got them all, or almost all, about itself. And when the metallic particles are thus divested of the nitrous ones, which, as a mediator between them and the water, held them floating in it, the alcalizate ones, crowding for the room the metallic ones took up before, will press these towards one another, and make them come more easily together: so that by the motion they continually have in the water, they shall be made to strike on one another; and then, by means of the principle in the second supposition, they will cohere and grow into clus-

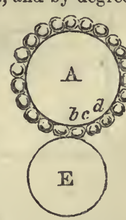


Fig. 4.

ters, and fall down by their weight to the bottom, which is called precipitation. In the solution of metals, when a particle is loosing from the body, so soon as it gets to that distance from it, where the principle of receding described in the fourth and fifth supposition begins to overcome the principle of acceding, described in the second supposition, the receding of the particle will be thereby accelerated; so that the particle shall, as it were, with violence leap from the body, and putting the liquor into a brisk agitation, beget and promote that heat we often find to be caused in solutions of metals. And if any particle happen to leap off thus from the body, before it is surrounded with water, or to leap off with that smartness as to get loose from the water, the water, by the principle in the fourth and fifth suppositions, will be kept off from the particle, and stand round about it, like a spherically hollow arch, not being able to come to a full contact with it any more; and several of these particles afterwards gathering into a cluster, so as by the same principle to stand at a distance from one another, without any water between them, will compose a bubble. Whence I suppose it is, that in brisk solutions there usually happens an ebullition. This is one way of transmuting gross compact substance into aërial ones. Another way is by heat; for as fast as the motion of heat can shake off the particles of water from the surface of it, those particles, by the said principle, will float up and down in the air, at a distance both from one another, and from the particles of air, and make that substance we call vapour. Thus I suppose it is, when the particles of a body are very small (as I suppose those of water are), so that the action of heat alone may be sufficient to shake them asunder. But if the particles be much larger, they then require the greater force of dissolving menstruums to separate them, unless by any means the particles can be first broken into smaller ones. For the most fixed bodies, even gold itself, some have said, will become volatile, only by breaking their parts smaller. Thus may the volatility and fixedness of bodies depend on the different sizes of their parts. And on the same difference of size may depend the more or less permanency of aërial substances, in their state of rarefaction. To understand this, let us suppose A B C D to be a large piece of any metal, E F G H the limit of the interior uniform æther, and K a part of the metal at the superficies A B. If this part or particle K be so little that it reaches not to the limit E F, it is plain that the æther at its centre must be less rare than if the particle were greater; for were it greater, its centre would be further from the superficies A B, that is, in a place where the æther (by supposition) is rarer; the less the particle K therefore, the denser the æther at its centre; because its centre comes nearer to the edge A B, where the æther is denser than within the limit E F G H. And if the particle were divided from the body, and removed to a distance from it, where the æther is still denser, the æther within it must proportionally grow denser. If you consider this, you may apprehend how, by diminishing the particle, the rarity of the æther within it will be diminished, till between the density of the æther without, and the density of the æther within it,

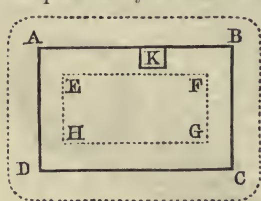


FIG. 5.

there be little difference; that is, till the cause be almost taken away, which should keep this and other such particles at a distance from one another. For that cause explained in the fourth and fifth suppositions, was the excess of density of the external æther above that of the internal. This may be the reason then why the small particles of vapours easily come together, and are reduced back into water, unless the heat, which keeps them in agitation, be so great as to dissipate them as fast as they come together; but the grosser particles of exhalations raised by fermentation keep their aërial form more obstinately, because the æther within them is rarer.

"Nor does the size only, but the density of the particles also, conduce to the permanency of aërial substances; for the excess of density of the æther without such particles above that of the æther within them is still greater; which has made me sometimes think that the true permanent air may be of a metallic original; the particles of no substances being more dense than those of metals. This, I think, is also favoured by experience, for I remember I once read in the Philosophical Transactions, how M. Huygens at Paris, found that the air made by dissolving salt of tartar would in two or three days' time condense and fall down again, but the air made by dissolving a metal continued without condensing or relenting in the least. If you consider then, how by the continual fermentations made in the bowels of the earth there are aërial substances raised out of all kinds of bodies, all which together make the atmosphere, and that of all these the metallic are the most permanent, you will not perhaps think it absurd, that the most permanent part of the atmosphere, which is the true air, should be constituted of these, especially since they are the heaviest of all other, and so must subside to the lower parts of the atmosphere and float upon the surface of the earth, and buoy up the lighter exhalations and vapours to float in greatest plenty above them. Thus, I say, it ought to be with the metallic exhalations raised in the bowels of the earth by the action of acid menstruums, and thus it is with the true permanent air; for this, as in reason it ought to be esteemed the most ponderous part of the atmosphere, because the lowest, so it betrays its ponderosity by making vapours ascend readily in it, by sustaining mists and clouds of snow, and by buoying up gross and ponderous smoke. The air also is the most gross unactive part of the atmosphere, affording living things no nourishment, if deprived of the more tender exhalations and spirits that float in it; and what more unactive and remote from nourishment than metallic bodies?

"I shall set down one conjecture more, which came into my mind now as I was writing this letter; it is about the cause of gravity. For this end I will suppose æther to consist of parts differing from one another in *subtilty* by indefinite degrees; that in the pores of bodies there is less of the grosser æther, in proportion to the finer, than in open spaces; and consequently, that in the great body of the earth there is much less of the grosser æther, in proportion to the finer, than in the regions of the air; and that yet the grosser æther in the air affects the upper regions of the earth; and the finer æther in the earth the lower regions of the air, in such a manner, that from the top of the air to the surface of the earth, and again from the surface of the earth to the centre thereof, the æther is insensibly finer and finer. Imagine now any body suspended in the air, or lying on the earth, and the æther being by the hypothesis grosser in the pores, which are in the upper parts of the body, than in those which are in its lower parts, and that grosser æther being less apt to be lodged in those pores than the finer æther below, it will endeavour to get out and give way to the finer æther below, which cannot be, without the bodies descending to make room above for it to go out into.

"From this supposed gradual subtilty of the parts of æther some things above might be further illustrated and made more intelligible; but by what has been said, you will easily discern whether in these conjectures there be any degree of probability, which is all I am at. For my own part, I have so little fancy to things of this nature, that had not your encouragement moved me to it, I should never, I think, have thus far set pen to paper about them. What is amiss, therefore, I hope you will the more easily pardon in

"Your most humble servant and honourer,

"ISAAC NEWTON.

"CAMBRIDGE, *Feb.* 28, 1678-9."

No. III.

(*Referred to in page* 190.)

THE following is an accurate copy of the large drawing of a sheep's eye, as mentioned in the text, and of the manuscript which accompanied it.

"1. Ellipsis A X T Y talis est ut parallel. (ad medium inter vitrum et aquam medium refractos projiciat in Z).

2. S Z est fere $\frac{1}{4}$ A S.

3. Retinæ superficies plano duplo magis quam superficies utravis crystallini.

4. Crystallini superficies anterior posteriore plenius est.

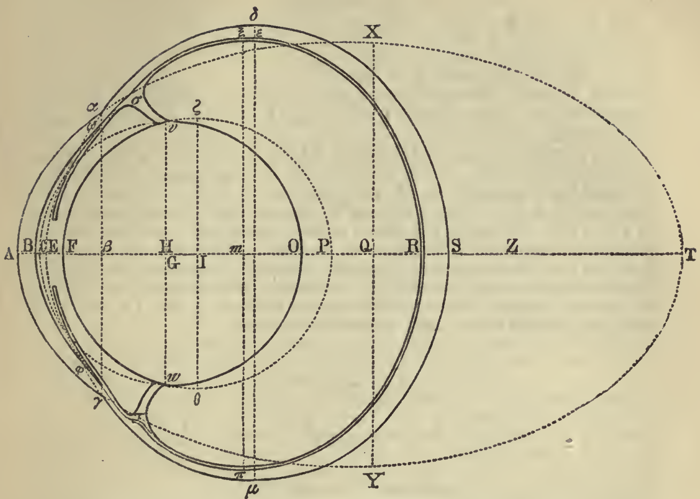
5. Convexitatis corneæ et araneæ fere commune centrum est . . . .

6. Centrum anterioris araneæ istis aliquanto inferior habetur. 2 R Z est $\frac{1}{4}$ A R.

A T : X Y : : 25 : 18 (: : 7 : 5 proxime.)
E R : $\pi\xi$: : 83 : 101 (: : 23 : 28 : : 9 : 11 pr.)
F R : $\zeta\theta$: : 9 : 8.
A R : $\xi\pi$: : 13 : 12.[1]
A T : E B.
A E : E B.
A B : A R.
R S : A R.
I commune centrum curvaturæ tunicarum ad A, B, et R.'
I A : I R : : 13 : 21.
I B : I R : : 19 : 36 (: : 1 : 2 fere.)
F P : $\zeta\theta$.

[1] This is obviously a mistake.

F P : O C.
F O : V W.
F O : O H.
F H : H O.



The dimensions of this figure, taken from a sheep's eye, are as followeth :—

By Experiment.

A S = 975.
$\mu\,\delta$ = 1025.
A B = 52.
R S = 60.
$\epsilon\,\delta$ = 16.
$\alpha\,\gamma$ = 686.
A β = 196.
F O = 429.
$v\,w$ = 530.
H O = 248.
The angle F v O about 160 degrees.
v O w is a circle whose radius is G O = 265; and I A = 350, I B = 298, I R = 565, and I F = 307, are the radii of spheres so much concave or convex as the surfaces of the horny tunic a A γ, ω B ϕ, of the retina ξ R π,

and of the exterior aranea v F w, at their vertices A, B, R, F. Lastly, A $\alpha\ \sigma$ = 968 = arcui A $\gamma\ \tau$.

By Deduction.

A T = 1350 = 2 A Q, and X Y = 972.
E R = 816 = 2 m R, and $\mu\ \delta$ = 993.
E P = 481 = 2 F K, and $\zeta\ \theta$ = 542.
And A Z = 1143. The said lines A T, X Y, E R, &c., being the right and transverse axes of the ellipses A α X τ Y γ, E ξ R π, and F ζ P ω, and Z the exterior focus of A $\alpha\ \tau\ \gamma$.

I was prevented by an accident from taking the distance of the crystalline humour from the horny tunic, which I would gladly have done to have had the conformity of all the parts one to another in one and the same eye; but by all circumstances 'tis near the truth to make A H = 340, or A F = 159. I have made the same centre I to both the horny and network tunic, they happening to be very near together. But I am apt to suspect that it is somewhat too remote from the cornea by reason of the difficulty of measuring the least convexity of the cornea, or the greatest convexity of the retina. Perhaps it may not be amiss to make I A = 344, I R = 571, and the point H (since it is so near it) coincident with I."

No. IV.

(*Referred to in page* 192.)

LETTER FROM NEWTON TO DR. WILLIAM BRIGGS.[1]

"For his Hon^{d} ffriend D^{r} W^{m} BRIGGS.

"S^{r}

"Though I am of all men grown y^{e} most shy of setting pen to paper about any thing that may lead into disputes yet yo^{r} friendship overcomes me so far as y^{t} I shall set down my suspicions about yo^{r} Theory, yet on this condition, that if I can write but plain enough to make you understand me, I may leave all to yo^{r} use wthout pressing it further on. For I designe not to confute or convince you but only to present & submit my thoughts to yo^{r} consideration & judgment.

"First then it seems not necessary that the bending of y^{e} nerves in y^{e} Thalamus opticus should cause a differing tension of y^{e} ffibres. ffor those w^{ch} have y^{e} further way about, will be apt by nature to grow the longer. If y^{e} arm of a tree be grown bent it follows not that the fibres on y^{e} elbow are more stretcht then those on the concave side, but that they are longer. And if a straight arm of a tree be bent by force for some time, the fibres on y^{e} elbow w^{ch} were at first on y^{e} stretch will by degrees grow longer &

[1] Edleston's *Correspondence*, &c., p. 265.

longer till at length the arm stand of it's self in y^e bended figure it was at first by force put into, that is till y^e fibres on y^e elbow be grown as much longer then y^e rest as they go further about, & so have but the same degree of tension w^th them. The observation is ordinary in twisted Codling hedges, fruit trees nailed up against a wall &c. And y^e younger & more tender a tree is the sooner will it stand bent. How much more therefore ought it to be so in that most tender substance of y^e Optick nerves w^ch grew bent from y^e very beginning? And whether if those nerves were carefully cut out of y^e brain & outward coat & put into brine made as neare as could be of the same specific gravity w^th y^e nerves, they would unbend or exactly keep the same bent they had in y^e brain may be worth considering. ffor though y^e strength of a single fibre upon the stretch be inconsiderably little, yet all together ought to have as much strength to unbend y^e nerve, as would suffice by outward application of y^e hand to bend a straight nerve of y^e same thickness, the dura Mater being taken off.

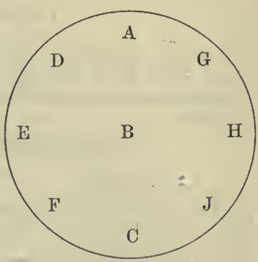
"M^r Sheldrake further suggests wittily that an object whether the axis opticus be directed above it, under it, or directly towards it, appears in all cases alike as to figure & colour excepting that in y^e 3^d case tis distincter, w^ch proceeds not from y^e frame of y^e nerves but from y^e distinctness of y^e picture made in y^e Retina in that case. But in y^e first case where y^e vision is made by y^e fibres above & second where tis made by those below, the object appearing alike: he thinks it argues that the fibres above & below are of y^e same constitution & tension, or at least if they be of a differing tension, that that tension has no effect on y^e mode of vision, but I understand you are already made acquainted w^th his thoughts.

"It may be further considered that the cause of an objects appearing one to both eyes is not its appearing of y^e same colour form & bigness to both, but in y^e same situation or place. Distort one eye & you will see y^e coincident images of y^e object divide from one another & one of them remove from y^e other upwards downwards or sideways to a greater or less distance according as y^e distortion is; & when the eyes are let return to their natural posture the two images advance towards one another till they become coincident & by that coincidence appear but one. If we would then know why they appear but one, we must e[n]quire why they appear in one & y^e same place & if we would know y^e cause of that we must enquire why in other cases they appear in divers places variously situate & distant one from another. ffor that w^ch can make their distance greater or less can make it none at all. Consider whats the cause of their being in y^e same altitude when one is directly to y^e right hand y^e other to y^e left & what of their being in y^e same coast or point of y^e compas, when one is directly over y^e other: these two causes joyned will make them in y^e same altitude & coast at once that is in y^e same place. The cause of situations is therefore to be enquired into. Now for finding out this y^e analogy will stand between y^e situations of sounds & the situations of visible things, if we will compare these two senses. But the situations of sounds depend not on their tones. I can judge from whence an echo or other sound comes tho I see not y^e sounding body, & this judgment depends not at all on y^e tone. I judge it not from east because acute, from west because grave: but be y^e tone what it will I judge it from hence or thence by some other principle. And by that principle I am apt to think a blind man may distinguish unisons one from another when y^e one is on his right hand y^e other on his left. And were our ears as good & accurate at distinguishing y^e^ coasts of audibles as our eyes are at distinguishing y^e^ coasts of visibles I conceive we should judge no two sounds the same for being unisons unless they came so exactly from y^e^ same coast as not to vary from one another a sensible point in situation to any side. Suppose then you had to do with one of so accurate an ear in distinguishing y^e^ situation of sounds. how would you deale with him? Would you tell him that you heard all unisons as but one sound? He would tell you he had a better ear than so. He accounted no sounds y^e^ same w^ch^ differed in situation: & if your eyes were no better at y^e^ situation of things then your ears, you would perhaps think all objects y^e^ same, w^ch^ were of y^e^ same colour. But for his part he found y^t^ y^e^ like tension of strings & other sounding bodies did not make sounds one, but only of y^e^ same tone: & therefore not allowing the supposition that it does make them one, the inference from thence that y^e^ like tension of y^e^ optick fibres made y^e^ object to y^e^ two eyes appeare one, he did not think himself obliged to admit. As he found y^t^ tones depended on those tensions so perhaps might colours, but the situation of audibles depended not on those tensions, & therefore if the two senses hold analogy with one another, that of visibles does not, & consequently the union of visibles as well as audibles which depends on the agreement of situation as well as of colour or tone must have some other cause.

"But to leave this imaginary disputant, let us now consider what may be y^e^ cause of y^e^ various situations of things to y^e^ eyes. If when we look w^th^ one eye it may be asked why objects appear thus & thus situated one to another the answer would be because they are really so situated among themselves & make their coloured pictures in y^e^ Retina so situated one to another as they are & those pictures transmit motional pictures into y^e^ sensorium in y^e^ same situation & by the situation of those motional pictures one to another the soul judges of y^e^ situation of things without. In like manner when we look with two eyes distorted so as to see y^e^ same object double if it be asked why those objects appeare in this or that situation & distance one from another, the answer should be because through y^e^ two eyes are transmitted into y^e^ sensorium two motional pictures by whose situation & distance then from one another the soule judges she sees two things so situate & distant. And if this be true then the reason why when the distortion ceases & y^e^ eyes return to their natural posture the doubled object grows a single one is that the two motional pictures in y^e^ sensorium come together & become coincident.

"But you will say, how is this coincidence made? I answer, what if I know not? Perhaps in y^e^ sensorium, after some such way as y^e^ Cartesians would have beleived or by some other way. Perhaps by y^e^ mixing of y^e^ marrow of y^e^ nerves in their juncture before they enter the brain, the fibres on y^e^ right side of each eye going to y^e^ right side of y^e^ head those on y^e^ left side to y^e^ left. If you mention y^e^ experim^t^ of y^e^ nerve shrunck all y^e^ way on one side y^e^ head, that might be either by some unkind juyce abounding more on one side y^e^ head y^n^ on y^e^ other, or by y^e^ shrinking of y^e^ coate of y^e^ nerve whose fibres & vessels for nourishment perhaps do not cross in y^e^ juncture as y^e^ fibres of y^e^ marrow may do. And its more probable y^t^ y^e^ stubborn coate being vitiated or wanting due nourishment shrank and made y^e^ tender marrow yeild to its capacity, than that y^e^ tender marrow by shrinking should make yᵉ coate yeild. I know not whether you would have yᵉ succus nutricius run along yᵉ marrow. If you would, 'tis an opinion not yet proved & so not fit to ground an argument on. If you say yᵗ in yᵉ Camælion & ffishes yᵉ nerves only touch one another without mixture & sometimes do not so much as touch; 'Tis true, but makes altogether against you. ffishes looke one way with one eye yᵉ other way with yᵉ other: the Chamælion looks up wᵗʰ one eye, down wᵗʰ t'other, to yᵉ right hand wᵗʰ this, to yᵉ left wᵗʰ yᵗ, twisting his eyes severally this way or that way as he pleases. And in these Animals which do not look yᵉ same way wᵗʰ both eyes what wonder if yᵉ nerves do not joyn? To make them joyn would have been to no purpose & nature does nothing in vain. But then whilst in these animals where tis not necessary they are not joyned, in all others wᶜʰ look yᵉ same way wᵗʰ both eyes, so far as I can yet learn, they are joyned. Consider therefore for what reason they are joyned in yᵉ one & not in the other. ffor God in yᵉ frame of animals has done nothing wᵗʰout reason.

"There is one thing more comes into my mind to object. Let yᵉ circle *D J* represent the Retina, or if you will the end of yᵉ optick nerve cut cross. *A* the end of a fibre above of most tension, *C* yᵉ end of one below of least tension. *D* & *G* yᵉ ends of fibres above on either hand almost of as much tension as *A*, *F* & *J* the ends of others below almost of as little tension as *C*. *E* yᵉ end of a fibre of less tension then *A* or *G* & of more then *C* or. *J*. And between *A* & *C*, *G* & *J* there will [be] fibres of equal tension wᵗʰ *E* because between them there are in a continual series fibres of all degrees of tension between yᵉ most tended at *A* & *G* & least tended at *C* & *J*. And by the same argument that 3 fibres *E*, *B* & *H* of like tension are noted let the whole line of fibres of the same Degree of tension running from *E* to *H* be noted. Do you now say yᵗ yᵉ reason why an object seen wᵗʰ two eyes appears but one is that yᵉ fibres in yᵉ two eyes by wᶜʰ 'tis seen are unisons? then all objects seen by unison fibres must for yᵉ same reason appear in one & yᵉ same place that is all yᵉ objects seen by the line of fibres *E B H* running from one side of the eye to yᵉ other. ffor instance two stars one to yᵉ right hand seen by yᵉ fibres about *H*, the other to yᵉ left seen by yᵉ fibres about *E* ought to appear but one starr, & so of other objects. ffor if consonance unite objects seen wᵗʰ the fibres of two eyes much more will it unite those seen wᵗʰ those of yᵉ same eye. And yet we find it much otherwise. What soever it is that causes the two images of an object seen with both eyes to appear in yᵉ same place so as to seem but one can make them upon distorting yᵉ eyes separate one from yᵉ other & go as readily & as far asunder to yᵉ right hand & to yᵉ left as upwards & downwards.



"You have now yᵉ summ of what I can think of worth objecting set down in a tumultuary way as I could get time from my Sturbridge ffair friends. If I have any where exprest myself in a more peremptory way then becomes y^e weaknes of y^e argument pray look on that as done not in earnestness but for y^e mode of discoursing. Whether any thing be so material as y^t it may prove any way useful to you I cannot tell. But pray accept of it as written for that end. ffor having laid Philosophical speculations aside nothing but y^e gratification of a friend would easily invite me to so large a scribble about things of this nature.

"S^r^ I am

"Yo^r^ humble Servant

"Trin. Coll. Cambr. Sept. 12^th^. 1682. Is. NEWTON."[1]

No. V.

(*Referred to in page* 195.)

[This letter is prefixed to the Latin version of Briggs' *Theory of Vision*, Lond. 1685, which was made at Newton's request, and must have been intended as a recommendation of that work as well as of his *Ophthalmographia*.]

"Isaacus Newtonus Doctori Gulielmo Briggio.

"Vir Clarissime,

"Hisce tuis Tractatibus[2] duas magni nominis scientias uno opere promoves, *Anatomiam* dico & *Opticam*. Organi enim (in quo utraque versatur) artificio summo constructi diligenter perscrutaris mysteria. In hujus dissectione peritiam & dexteritatem tuam non exiguo olim mihi oblectamento fuisse recordor. Musculis motoriis secundùm situm suum naturalem elegantèr à te expansis, cæterisque partibus coràm expositis, sic ut singularum usus & ministeria non tàm intelligere liceret quàm cernere, effeceret dudùm ut ex cultro tuo nihil non accuratum sperarem. Nec spem fallebat eximius ille Tractatus Anatomicus, quem postmodùm edidisti. Jam praxeos hujus ἀκρίβειαν pergis ingeniosissimâ Theoriâ instruere & exornare. Et quis Theoriis condendis aptior extiterit, quàm qui phænomenis accuratè observandis navârit operam? Nervos opticos *ex capillamentis variè tensis* constare supponis, eaque magis esse tensa quæ per iter longius porriguntur; ex diversâ autem tensione fieri ut objectorum partes singulæ non coincidant & confundantur inter se, sed pro situ suo naturali diversis in locis appareant; & *capillamentis* amborum oculorum æquali tensione factis *concordibus*, geminas objectorum species uniri. Sic ex tensione chordarum, quâ soni vel variantur vel concordant in Musicâ, colligere videris quid fieri debet in Opticâ. Simplex etenim est *Natura*, & eodem operandi tenore in immensâ

[1] From the original in the British Museum, Add. MSS. 4237, fol. 34.
[2] i.e., *Ophthalmographia*, Cantab. 1676 (2d edit. Lond. 1687), and his *Theory of Vision.*

effectuum varietate sibi ipsa constare solet. Quantò verò magìs in sensuum cognatorum causis? Et quamvis aliam etiam horum sensuum analogiam suspicari possim, ingeniosam tamen esse quam tute excogitasti, certè nemo non lubentèr fatebitur. Nec inutilem censeo Dissertationem ultimam quâ diluis objectiones. Inde Lector attentus & pleniùs intelliget Hypothesin totam, & in quæstiones incidet vel tuis Meditationibus illustratas, vel novis experimentis & disquisitionibus posthàc dirimendas. Id quod in usum cedet juventuti Academicæ, & provectiores ad ulteriores in Philosophiâ progressus manuducet. Pergas itaque, vir ornatissime, scientias hasce præclaris inventis, uti facis, excolere; doceasque difficultates causarum naturaliam tàm facilè solertiâ vinci posse, quàm solent conatibus vulgaribus difficultèr cedere.

"Vale."

"*Dabam Cantabrigiæ* 7 *Kal. Maii.*
1685."

No. VI.

(*Referred to in page* 197.)

NEWTON'S *Fifteenth* QUERY.[1]

ARE not the species of objects seen with both eyes united where the optic nerves meet before they come into the brain, the fibres on the right side of both nerves uniting there, and, after union, going thence into the brain in the nerve which is on the right side of the heart; and the fibres on the left side of both nerves uniting in the same place, and, after union, going into the brain in the nerve which is on the left side of the head, and these two nerves meeting in the brain, in such a manner that their fibres make but one entire species or picture, half of which on the right side of the sensorium, comes from the right side of both eyes through the right side of both optic nerves, to the place where the nerves meet, and from thence on the right side of the head into the brain; and the other half on the left side of the sensorium comes in like manner from the left side of both eyes. For the optic nerves of such animals as look the same way with both eyes (as of men, dogs, sheep, oxen, &c.), meet before they come into the brain, but the optic nerves of such animals as do not look the same way with both eyes (as of fishes and of the chameleon) do not meet, if I am rightly informed.

No. VII.

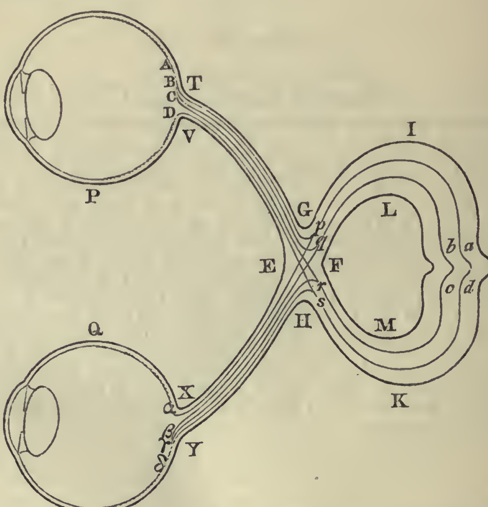
(*Referred to in page* 197.)

ALTHOUGH we have extracted a part of this document in the text, for the sake of illustration, we shall give the whole of it as published by Mr. Harris.

[1] *Optics*, 3d edit. 1721, pp. 320, 321.

Description of the Optic Nerves and their Juncture in the Brain, by Sir Isaac Newton.[1]

"The *tunic retina* grows not from the sides of the optic nerve (as the other two which rise one from the *dura*, the other from the *pia mater*), but it grows from the middle of the nerve, sticking to it all over the extremity of its marrow. Which marrow, if the nerve be any where cut cross-wise betwixt the eye and the union of the nerves, appears full of small spots or pimples, which are a little prominent, especially if the nerve be pressed, or warmed at a candle; and these shoot into the very eye, and may be seen within side, where the retina grows to the nerve; and they also continue to the very juncture E F G H. But at the juncture they end on a sudden into a more tender white pap, like the anterior part of the brain; and so the nerve continues after the juncture into the brain filled with a white tender pap, in which can be seen no distinction of parts as betwixt the said juncture and the eye.



"Now I conceive that every point in the retina of one eye, hath its correspondent point in the other; from which two very slender pipes filled with a most limpid liquor do, without either interruption or any other unevenness or irregularity in their process, go along the optic nerves to the

[1] The original of this drawing and description I found at Hurtzbourne Park in a manuscript book without one of its boards, p. 17.

juncture E F G H, where they meet either betwixt G F or F H, and there unite into one pipe as big as both of them; and so continue in one, passing either betwixt I L or M K, into the brain, where they are terminated perhaps at the next meeting of the nerves betwixt the *cerebrum* and *cerebellum*, in the same order that their extremities were situated in the retina's. And so there are a vast multitude of these slender pipes which flow from the brain, the one half through the right side nerve I L, till they come at the juncture G F, where they are each divided into two branches, the one passing by G and T to the right side of the right eye A B, the other half shooting through the space E F, and so passing by X to the right side of the left eye $\alpha\ \beta$. And in like manner the other half shooting through the left side nerve M K, divide themselves at F H, and their branches passing by E V to the right eye, and by H Y to the left, compose that half of the retina in both eyes, which is towards the left side C D and $\gamma\ \delta$.

"Hence it appears, 1. Why the two images of both eyes make but one image *a b c d* in the brain.

"2. Why, when one eye is distorted, objects appear double; for if the image of any object be made upon A in the one eye, and β in the other, that object shall have two images in the brain at *a* and *b*. Therefore the pictures of any object ought to be made upon the corresponding points of the two retinas; if upon A in the right eye, then upon α in the left; if upon B, then also upon β. And so shall the motions concur after they have passed the juncture G H, and make one image at *a* or *b* more vivid than one eye alone could do.

"3. Why, though one thing may appear in two places by distorting the eyes, yet two things cannot appear in one place. If the picture of one thing fall upon A, and of another upon α, they may both proceed to *p*, but no farther; they cannot both be carried on the same pipes *p u* into the brain; that which is strongest or most helped by phantasy will there prevail, and blot out the other.

"4. Why, if one of the branches of the nerve beyond the juncture, as at G F or F H, should be cut, that half of both eyes towards the wounded nerve would be blind, the other half remaining perfect.

"5. Why the juncture is almost as broad again betwixt G and H, as between E and F; because all the tubuli of both eyes pass between G and H, and but half of them betwixt E and F.

"6. Why the nerve G I L F buts not directly upon the nerve X E H Y, but deviates a little towards T V; because its tubuli are to pass only into that side of the nerve E H X Y towards E X. The like of F M K H.

"7. Why the marrow of the nerve T V E G grows soft on a sudden, when it comes at the juncture E F, and more suddenly on that side towards G than towards E. And the like of the nerve E X Y H: For it being necessary that the nerve T V E G should be stretched and bended several ways by the motion of the eye; therefore the tubuli are involved or wrought up within the substances of several tough skins, which, being folded up together, compose the marrow of the nerve pretty solid and flexible, lest the tubuli should be prejudiced by the several motions of the nerve. And those small pimples or prominencies which appear in the nerve cut crosswise, I conceive to be made by the foldings of those crasser skins. But the nerve at the juncture E G F H, being well guarded from all violence and motion by the bones into which it is closely adapted; 'tis not necessary the said membraneous substance should be continued any farther than E G; therefore the tubuli there on a sudden unsheath themselves, that those on the inner side of the nerves towards V E and X E may severally cross 'twixt E F, and be united with their correspondents on the other sides Y H and T G. Now, because the inner tubuli must first cross, before they can concur with the outmost tubuli of the opposite nerve; hence it is, that the nerves grow soft sooner on the inner side at E, than on the outer side at G and H.

"8. Why the two nerves meet a second time in the brain: because the two half images carried along I L and M K may be united in one compleat image, in the sensory. *Note,* that the nerves at their meeting, are round about disjoined from the rest of the brain; nor are they so thick there, as a little before their meeting. But by their external figure, they seem as if the *capillamenta* concentered like the radii of a hemisphere to a point in the lower part of the juncture. And 'tis probable that the visive faculty is there: for else why do the nerves swell there to so great a bulk, as it were preparing for their last office? Why do they run directly cross from either side the brain to meet there, if the design was to have the motions conveyed by the shortest cut from the eye to the sensorium, before they grew too weak. If they were to proceed farther, they might have gone a shorter cut, and in a less channel. There is indeed a marrow shoots from under them towards the *cerebellum,* to which they are united; but the greatest part of their substance, if not all of it, lies above this marrow, and also shoots cross beyond it to the centre of the brain, where they meet. Lastly, the substance here is most pure, the situation in the midst of the brain, constituting the upper part of that small passage 'twixt all the ventricles, where all superfluous humours have the greatest advantage to slide away, that they may not incumber that precious organ.

"Light seldom strikes upon the parts of gross bodies (as may be seen in its passing through them); its reflection and refraction is made by the diversity of æthers; and therefore its effect upon the retina can only be to make this vibrate: which motion then must be either carried in the optic nerves to the sensorium, or produce other motions that are carried thither. Not the latter, for water is too gross for such subtile impressions; and as for animal spirits, tho' I tied a piece of the optic nerve at one end, and warmed it in the middle, to see if any airy substance by that means would disclose itself in bubbles at the other end, I could not spy the least bubble; a little moisture only, and the marrow itself squeezed out. And indeed they that know how difficultly air enters small pores of bodies, have reason to suspect that an airy body, tho' much finer than air, can pervade and without violence (as it ought to do) the small pores of the brain and nerves, I should say of water; because those pores are filled with water: and if it could, it would be too subtile to be imprisoned by the *dura mater* and skull, and might pass for *æther.* However, what need of such spirits? Much motion is ever lost by communication, especially betwixt bodies of different constitutions. And therefore it can no way be conveyed to the sensorium so entirely, as by the æther itself. Nay, granting me, but that there are pipes filled with a pure transparent liquor passing from the eye to the sensorium, and the vibrating motion of the æther will of necessity run along thither. For nothing interrupts that motion but reflecting surfaces; and therefore also that motion cannot stray through the reflecting surfaces of the pipe, but must run along (like a sound in a trunk) entire to the sensorium. And that vision thus made, is very conformable to the sense of hearing, which is made by like vibrations."

No. VIII.

(*Referred to in page* 269, *as* No. IX.)

The interesting correspondence between Halley and Newton, consisting of fifteen letters, which, with the exception of one given in Chap. XVI., Vol. II., we give in this Appendix, forms an essential part of the History of the Principia, and throws much light on the personal character of Newton. The *eight* letters, Nos. 1, 3, 4, 5, 6, 8, 10, and 11, having been preserved in the archives of the Royal Society, were published in their entire state by my late amiable and learned friend, Professor Rigaud of Oxford, in the *Appendix* to his interesting volume, entitled *Historical Essay on the First Publication of the Principia.*[1] The greater number of them had been printed in a garbled and imperfect state by the authors of the articles Halley, Hooke, and Newton, in the *General Dictionary* and in the *Biographia Britannica,* and therefore Mr. Rigaud had them carefully copied from the guard-book of the Royal Society. At the end of each letter he has mentioned the parts that have been omitted, and the changes that have been made upon it in the different works where it has been used. We have adopted these important notes of Mr. Rigaud.

The other *six* letters, Nos. 2, 7, 9, 12, 13, and 14, which, with the one already referred to, complete the correspondence, were found among the Newtonian papers in the possession of the Earl of Portsmouth, and are now printed for the first time.

1.—Halley to Newton.

"*May* 22, 1686.

"Sir,

"Your incomparable treatise, entitled Philosophiæ Naturalis Principia Mathematica, was by Dr. Vincent presented to the Royal Society on the 28th past; and they were so very sensible of the great honour you do them by your dedication, that they immediately ordered you their most hearty thanks, and that a council should be summoned to consider about the printing thereof; but by reason of the president's attendance upon the King, and the absence of our vice-presidents, whom the good weather has drawn out of town, there has not since been any authentic council to resolve what to do in the matter: so that on Wednesday last the Society, in their meeting, judging that so excellent a work ought not to have its publication any longer delayed, resolved to print it at their own charge in a large quarto of a fair letter; and that this their resolution should be signified to you, and your opinion therein be desired, that so it might be gone about with all speed. I am intrusted to look after the printing it, and will take care that it shall be performed as well as possible; only I would first have your directions in what you shall think necessary for the embellishing thereof, and particularly whether you think it not better that the schemes

[1] Oxford, 1838.

should be enlarged, which is the opinion of some here: but what you signify as your desire shall be punctually observed.

"There is one thing more that I ought to inform you of, viz., that Mr. Hooke has some pretensions upon the invention of the rule of the decrease of gravity being reciprocally as the squares of the distances from the centre. He says you had the notion from him, though he owns the demonstration of the curves generated thereby to be wholly your own. How much of this is so, you know best, as likewise what you have to do in this matter, only Mr. Hooke seems to expect you should make some mention of him in the preface, which 'tis possible you may see reason to prefix. I must beg your pardon, that 'tis I that send you this ungrateful account; but I thought it my duty to let you know it, that so you might act accordingly, being in myself fully satisfied, that nothing but the greatest candour imaginable is to be expected from a person, who has of all men the least need to borrow reputation."

The following paragraph and conclusion of the letter, taken from the original, did not exist in any of the previously printed copies of it.

"When I shall have received your directions, the printing shall be pushed on with all expedition, which, therefore, I entreat you to send me as soon as may be. You may please to direct to me, to be left with Mr. Hunt at Gresham College, and your line will come to the hands of,

"Sir,

"Your most affectionate humble servant,

"EDM. HALLEY."

"This letter was printed from the copy in the Letter Book of the Royal Society (Supplement, vol. iv. p. 340), by Birch, in his History of the Royal Society (vol. iv. p. 484). It is also printed in the Biographia Britannica (vol. v. p. 3225)."

2.—HALLEY TO NEWTON.

"LONDON, *June* 7, 1686.

"SIR,

"I here send you a proof of the first sheet of your book, which we think to print on this paper, and in this character; if you have any objection, it shall be attended to: and if you approve it, we will proceed; and care shall be taken that it shall not be published before the end of Michaelmas term, since you desire it. I hope you will please to bestow the second part, or what remains of this, upon us as soon as you shall have finished it, for the application of this mathematical part to the system of the world, is what will render it acceptable to all naturalists, as well as mathematicians; and must advance the sale of the book. Pray, please to revise this proof, and send it me up with your answer. I have already corrected it, but cannot say I have spied all the faults. When it has passed your eye, I doubt not but it will be clear from errata. The printer begs your excuse of the diphthongs, which are of a character a little bigger, but he has some a casting of the just size. This sheet being a proof, is not so clear as it ought to be; but the letter is new, and I have seen a book of a very fair character, which was the last thing printed from this set of letter; so that I hope the Edition may in that particular be to your satisfaction.—I am, Sir,

"Your most affectionate humble servant,

"E. HALLEY.

"Please to send by the coach, directed to me, to be left with Mr. Hunt, at Gresham College.

"*To his honoured Friend,*
MR. ISAAC NEWTON,
at his Chamber in TRINITY COLLEGE,
CAMBRIDGE."

3.—NEWTON TO HALLEY.

"SIR,

"In order to let you know the case between Mr. Hooke and me, I gave you an account of what passed between us in our letters, so far as I could remember; for, 'tis long since they were writ, and I do not know that I have seen them since. I am almost confident by circumstances, that Sir Chr. Wren knew the duplicate proportion when I gave him a visit; and then Mr. Hooke (by his book Cometa written afterwards) will prove the last of us three that knew it. I intended in this letter to let you understand the case fully; but it being a frivolous business, I shall content myself to give you the heads of it in short, viz., that I never extended the duplicate proportion lower than to the superficies of the earth, and before a certain demonstration I found the last year, have suspected it did not reach accurately enough down so low; and therefore in the doctrine of projectiles never used it nor considered the motions of the heavens; and consequently Mr. Hooke could not from my letters, which were about projectiles and the regions descending hence to the centre, conclude me ignorant of the theory of the heavens. That what he told me of the duplicate proportion was erroneous, namely, that it reached down from hence to the centre of the earth. That it is not candid to require me now to confess myself, in print, then ignorant of the duplicate proportion in the heavens; for no other reason, but because he had told it me in the case of projectiles, and so upon mistaken grounds accused me of that ignorance. That in my answer to his first letter I refused his correspondence, told him I had laid philosophy aside, sent him only the experiment of projectiles (rather shortly hinted than carefully described), in compliment to sweeten my answer, expected to hear no further from him; could scarce persuade myself to answer his second letter; did not answer his third, was upon other things; thought no further of philosophical matters than his letters put me upon it, and therefore may be allowed not to have had my thoughts of that kind about me so well at that time. That by the same reason he concludes me then ignorant of the rest of the duplicate proportion, he may as well conclude me ignorant of the rest of that theory I had read before in his book. That in one of my papers writ (I cannot say in what year, but I am sure some time before I had any correspondence with Mr. Oldenburg, and that's) above fifteen years ago, the proportion of the forces of the planets from the sun, reciprocally duplicate of their distances from him, is expressed, and the proportion of our gravity to the moon's conatus recedendi a centro terræ is calculated, though not accurately enough. That when Hugenius put out his Horol. Oscil., a copy being presented to me, in my letter of thanks to him, I gave those rules in the end thereof a particular commendation for their usefulness in Philosophy, and added out of my aforesaid paper an instance of their usefulness, in comparing the forces of the moon from the earth, and earth from the sun; in determining a problem about the moon's phase, and putting a limit to the sun's parallax, which shows that I had then my eye upon comparing the forces of the planets arising from their circular motion, and understood it; so that a while after, when Mr. Hooke propounded the problem solemnly, in the end of his Attempt to prove the Motion of the Earth, if I had not known the duplicate proportion before, I could not but have found it now. Between ten and eleven years ago, there was an hypothesis of mine registered in your books, wherein I hinted a cause of gravity towards the earth, sun, and planets, with the dependence of the celestial motions thereon; in which the proportion of the decrease of gravity from the superficies of the planet (though for brevity's sake not there expressed) can be no other than reciprocally duplicate of the distance from the centre. And I hope I shall not be urged to declare, in print, that I understood not the obvious mathematical conditions of my own hypothesis. But grant I received it afterwards from Mr. Hooke, yet have I as great a right to it as to the ellipsis. For as Kepler knew the orb to be not circular but oval, and guessed it to be elliptical, so Mr. Hooke, without knowing what I have found out since his letters to me, can know no more, but that the proportion was duplicate quam proximè at great distances from the centre, and only guessed it to be so accurately, and guessed amiss in extending that proportion down to the very centre, whereas Kepler guessed right at the ellipsis. And so Mr. Hooke found less of the proportion than Kepler of the ellipsis. There is so strong an objection against the accurateness of this proportion, that without my demonstrations, to which Mr. Hooke is yet a stranger, it cannot be believed by a judicious philosopher to be any where accurate. And so, in stating this business, I do pretend to have done as much for the proportion as for the ellipsis, and to have as much right to the one from Mr. Hooke and all men, as to the other from Kepler; and therefore on this account also he must at least moderate his pretences.

"The proof you sent me I like very well. I designed the whole to consist of three books; the second was finished last summer being short, and only wants transcribing, and drawing the cuts fairly. Some new propositions I have since thought on, which I can as well let alone. The third wants the theory of comets. In autumn last I spent two months in calculations to no purpose for want of a good method, which made me afterwards return to the first book, and enlarge it with divers propositions, some relating to comets, others to other things, found out last winter. The third I now design to suppress. Philosophy is such an impertinently litigious Lady, that a man had as good be engaged in lawsuits, as have to do with her. I found it so formerly, and now I am no sooner come near her again, but she gives me warning. The two first books, without the third, will not so well bear the title of Philosophiæ Naturalis Principia Mathematica; and therefore I had altered it to this, De Motu Corporum libri duo. But, upon second thoughts, I retain the former title. 'Twill help the sale of the book, which I ought not to diminish now 'tis yours. The articles are, with the largest, to be called by that name; if you please you may change the word to *sections*, though it be not material; which is all at present from

"your affectionate friend,
"and humble servant,
Is. Newton.

"Cambridge, *June* 20, 1686.

"Since my writing this letter, I am told by one, who had it from another lately present at one of your meetings, how that Mr. Hooke should there make a great stir, pretending that I had all from him, and desiring they would see that he had justice done him. This carriage towards me is very strange and undeserved; so that I cannot forbear, in stating the point of justice, to tell you further, that he has published Borell's hypothesis in his own name; and the asserting of this to himself, and completing it as his own, seems to me the ground of all the stir he makes. Borell did something in it, and wrote modestly. He has done nothing, and yet written in such a way, as if he knew and had sufficiently hinted all but what remained to be determined by the drudgery of calculations and observations, excusing himself from that labour by reason of his other business, whereas he should rather have excused himself by reason of his inability. For 'tis plain, by his words, he knew not how to go about it. Now is not this very fine? Mathematicians, that find out, settle, and do all the business, must content themselves with being nothing but dry calculators and drudges; and another, that does nothing but pretend and grasp at all things, must carry away all the invention, as well of those that were to follow him, as of those that went before. Much after the same manner were his letters writ to me, telling me that gravity, in descent from hence to the centre of the earth, was reciprocally in a duplicate ratio of the altitude, that the figure described by projectiles in this region would be an ellipsis, and that all the motions of the heavens were thus to be accounted for; and this he did in such a way, as if he had found out all, and knew it most certainly. And, upon this information, I must now acknowledge, in print, I had all from him, and so did nothing myself but drudge in calculating, demonstrating, and writing, upon the inventions of this great man. And yet, after all, the first of those three things he told of me is false, and very unphilosophical; the second is as false; and the third was more than he knew, or could affirm me ignorant of by any thing that past between us in our letters. Nor do I understand by what right he claims it as his own; for as Borell wrote, long before him, that by a tendency of the planets towards the sun, like that of gravity or magnetism, the planets would move in ellipses, so Bullialdus wrote that all force, respecting the sun as its centre, and depending on matter, must be reciprocally in a duplicate ratio of the distance from the centre, and used that very argument for it, by which you, sir, in the last Transactions, have proved this ratio in gravity. Now if Mr. Hooke, from this general proposition in Bullialdus, might learn the proportion in gravity, why must this proportion here go for his invention? My letter to Hugenius, which I mentioned above, was directed to Mr. Oldenburg, who used to keep the originals. His papers came into

Mr. Hooke's possession. Mr. Hooke, knowing my hand, might have the curiosity to look into that letter, and thence take the notion of comparing the forces of the planets arising from their circular motion; and so what he wrote to me afterwards, about the rate of gravity, might be nothing but the fruit of my own garden. And it's more than I can affirm, that the duplicate proportion was not expressed in that letter. However, he knew it not (as I gather from his books) till five years after any mathematician could have told it him. For when Hugenius had told how to find the force in all cases of circular motion, he had told 'em how to do it in this as well as in all others. And so the honour of doing it in this is due to Hugenius. For another, five years after, to claim it as his own invention is as if some mechanic, who had learned the art of surveying from a master, should afterwards claim the surveying of this or that piece of ground for his own invention, and keep a heavy quarter to be in print for't. But what, if this surveyor be a bungler, and give an erroneous survey? Mr. Hooke has erred in the invention he pretends to, and his error is the cause of all the stir he makes. For his extending the duplicate proportion down to the centre (which I do not) made him correct me, and tell me the rest of his theory as a new thing to me, and now stand upon it, that I had all from that his letter, notwithstanding that he had told it to all the world before, and I had seen it in his printed books, all but the proportion. And why should I record a man for an invention, who founds his claim upon an error therein, and on that score gives me trouble? He imagines he obliged me by telling me his theory, but I thought myself disobliged by being, upon his own mistake, corrected magisterially, and taught a theory, which every body knew, and I had a truer notion of than himself. Should a man who thinks himself knowing, and loves to show it in correcting and instructing others, come to you, when you are busy, and notwithstanding your excuse press discourses upon you, and through his own mistakes correct you, and multiply discourses; and then make this use of it, to boast that he taught you all he spake, and oblige you to acknowledge it, and cry out injury and injustice if you do not; I believe you would think him a man of strange unsociable temper. Mr. Hooke's letters in several respects abounded too much with that humour, which Hevelius and others complain of; and therefore he may do well in time to consider, whether, after this new provocation, I be much more bound (in doing him that justice he claims) to make an honourable mention of him in print, especially since this is the third time that he has given me trouble in this kind. For your further satisfaction in this business, I beg the favour you would consult your books for a paper of mine entitled, An Hypothesis explaining properties of Light. It was dated Dec. 7, 1675, and registered in your book about January or February following. Not far from the beginning there is a paragraph ending with these words: 'And as the earth, so perhaps may the sun imbibe this spirit copiously to conserve his shining, and keep the planets from receding further from him; and they that will may also suppose that this spirit affords or carries thither the solary fuel and material principle of light. And that the vast ethereal spaces between us and the stars are for a sufficient repository for this food of the sun and planets. But this of the constitution of ethereal natures by the by.'

"In these and the foregoing words, you have the common cause of gravity towards the earth, sun, and all the planets, and that by this cause the planets are kept in their orbs about the sun. And this is all the philosophy Mr. Hooke pretends I had from his letters some years after, the duplicate proportion only excepted. The preceding words contain the cause of the phænomena of gravity, as we find it on the surface of the earth, without any regard to the various distances from the centre. For at first I designed to write of nothing more. Afterwards, as my manuscript shews, I interlined the words above cited relating to the heavens; and in so short and transitory an interlined hint of things, the expression of the proportion may well be excused. But if you consider the nature of the hypothesis, you'll find that gravity decreases upwards, and can be no other from the superficies of the planet than reciprocally duplicate of the distance from the centre, but downwards that proportion does not hold. This was but an hypothesis, and so to be looked upon only as one of my guesses, which I did not rely on; but it sufficiently explains to you, why in considering the descent of a body down to the centre, I used not the duplicate proportion. In the small ascent and descent of projectiles above the earth, the variation of gravity is so inconsiderable, that Mathematicians neglect it. Hence the vulgar hypothesis with them is uniform gravity. And why might not I, as a Mathematician, use it frequently, without thinking on the philosophy of the heavens, or believing it to be philosophically true?

"This letter, with the postscript belonging to it, was printed in the General Dictionary by Bernard, Birch, and Lockman (vol. vii. p. 797). The writers in the Biographia Britannica likewise adopted it; but have separated the different parts, and printed one portion twice. They have given the beginning (p. 401 to p. 402, line 37) in the life of Hooke (vol. iv. p. 2659); the next part (p. 402) in the life of Newton (vol. v. p. 3225), and the end (p. 403) is repeated in the life of Halley (vol. iv. p. 2504). The postscript (p. 403 to p. 405) was added to the part annexed to the life of Hooke (vol. iv. p. 2660)."

4.—Halley to Newton.

"Sir, "London, 29 *June*, 1686.

"I am heartily sorry, that in this matter, wherein all mankind ought to acknowledge their obligations to you, you should meet with any thing that should give you disquiet; or that any disgust should make you think of desisting in your pretensions to a Lady, whose favours you have so much reason to boast of. 'Tis not she, but your rivals, envying your happiness, that endeavour to disturb your quiet enjoyment; which when you consider, I hope you will see cause to alter your resolution of supressing your third book, there being nothing which you can have compiled therein, which the learned world will not be concerned to have concealed. Those gentlemen of the Society, to whom I have communicated it, are very much troubled at it, and that this unlucky business should have happened to give trouble, having a just sentiment of the author thereof. According to your desire in your former, I waited upon Sir Christopher Wren, to inquire of him, if he had the first notion of the reciprocal duplicate proportion from Mr. Hooke. His answer was, that he himself very many years since had had his thoughts upon the making out the planets' motions by a composition of a descent towards the sun, and an impressed motion; but that at length he gave it over, not finding the means of doing it. Since which time Mr. Hooke had frequently told him, that he had done it, and attempted to make it out to him; but that he never was satisfied that his demonstrations were cogent. And this I know to be true, that in January 168¾, I having, from the considerations of the sesquialter proportion of Kepler, concluded that the centripetal force decreased in the proportion of the squares of the distances reciprocally, came on Wednesday to town, where I met with Sir Christopher Wren and Mr. Hooke, and falling in discourse about it, Mr. Hooke affirmed, that upon that principle all the laws of the celestial motions were to be demonstrated, and that he himself had done it. I declared the ill success of my own attempts; and Sir Christopher, to encourage the inquiry, said, that he would give Mr. Hooke, or me, two months' time, to bring him a convincing demonstration thereof; and besides the honour, he of us, that did it, should have from him a present of a book of 40 shillings. Mr. Hooke then said, that he had it, but he would conceal it for some time, that others trying and failing might know how to value it, when he should make it public. However I remember, that Sir Christopher was little satisfied that he could do it; and though Mr. Hooke then promised to shew it him, I do not find, that in that particular he has been so good as his word. The August following, when I did myself the honour to visit you, I then learned the good news, that you had brought this demonstration to perfection: and you were pleased to promise me a copy thereof, which the November following I received with a great deal of satisfaction from Mr. Paget; and thereupon took another journey to Cambridge, on purpose to confer with you about it, since which time it has been entered upon the Register Books of the Society. As all this passed, Mr. Hooke was acquainted with it, and according to the philosophically ambitious temper he is of, he would, had he been master of a like demonstration, no longer have concealed it, the reason, he told Sir Christopher and me, now ceasing. But now he says, this is but one small part of an excellent system of nature, which he has conceived, but has not yet completely made out, so that he thinks not fit to publish one part without the other. But I have plainly told him, that unless he produce another differing demonstration, and let the world judge of it, neither I nor any one else can believe it. As to the manner of Mr. Hooke's claiming the discovery, I fear it has been represented in worse colours than it ought; for he neither made public application to the Society for justice, nor pretended you had all from him. The truth is this: Sir John Hoskyns, his particular friend, being in the chair, when Dr. Vincent presented your book, the Doctor gave it its just encomium both as to the novelty and dignity of the subject. It was replied by another gentleman, that you had carried the thing so far, that there was no more to be added. To which the Vice-president replied, that it was so much the more to be prized, for that it was both invented and perfected at the same time. This gave Mr. Hooke offence, that Sir John did not, at that time, make mention of what he had, as he said, discovered to him; upon which they two, who till then were the most inseparable cronies, have since scarce seen one another, and are utterly fallen out. After the breaking up of that meeting, being adjourned to the coffee-house, Mr. Hooke did there endeavour to gain belief, that he had some such thing by him, and that he gave you the first hint of this invention. But I found, that they were all of opinion, that nothing thereof appearing in print, nor on the books of the Society, you ought to be considered as the inventor. And if in truth he knew it before you, he ought not to blame any but himself, for having taken no more care to secure a discovery, which he puts so much value on. What application he has made in private, I know not; but I am sure that the Society have a very great satisfaction, in the honour you do them, by the dedication of so worthy a treatise. Sir, I must now again beg you, not to let your resentments run so high, as to deprive us of your third book, wherein the application of your mathematical doctrine to the theory of comets and several curious experiments, which, as I guess by what you write, ought to compose it, will undoubtedly render it acceptable to those, who will call themselves Philosophers without Mathematics, which are much the greater number. Now you approve of the character and paper, I will push on the edition vigorously. I have sometimes had thoughts of having the cuts neatly done in wood, so as to stand in the page with the demonstrations. It will be more convenient, and not much more charge. If it please you to have it so, I will try how well it can be done; otherwise I will have them in somewhat a larger size than those you have sent up. I am, Sir,

"Your most affectionate humble servant,

"E. HALLEY.

"This letter was printed in the Gen. Dic. (vol. vii. p. 799.) In the Biographia Britannica the parts are separated, and some, as was done for No. 3, are repeated. The beginning (p. 405 to p. 406) is annexed to the life of Newton (vol. v. p. 3226), and the first part of it appears also (to p. 406) in the life of Halley (vol. iv. p. 2504). The middle (p. 406) will be found in the life of Hooke (vol. iv. p. 2661); the end (p. 407) printed in the life of Newton (vol. v. p. 3226), and the latter part also in the life of Halley (vol. iv. p. 2504). The words (p. 406), 'As all this passed, Mr. Hooke was acquainted with it, and'—are wholly omitted."

5.—NEWTON TO HALLEY.

"*July* 14, 1686.

"SIR,

"I have considered your proposal about wooden cuts, and believe it will be much convenienter for the reader, and may be sufficiently handsome, but I leave it to your determination. If you go this way, then I desire you would divide the first figure into these two:[1] I crowded them into one to save the trouble of altering the numbers in the schemes you have. I am very sensible of the great kindness of the gentlemen of your Society to me, far beyond what I could ever expect or deserve, and know how to distinguish between their favour and another's humour. Now I understand he was in some respects misrepresented to me, I wish I had spared the postscript to my last. This is true, that his letters occasioned my finding the method of determining figures, which when I had tried in the ellipsis, I threw the calculations by, being upon other studies; and so it rested for

[1] The figures here are unnecessary.

about five years, till upon your request I sought for that paper; and not finding it, did it again, and reduced it into the propositions shewed you by Mr. Paget: but for the duplicate proportion I can affirm that I gathered it from Kepler's theorem about twenty years ago. And so Sir Christopher Wren's examining the ellipsis over against the focus shews, that he knew it many years ago, before he left off his enquiry after the figure by an impressed motion and a descent compounded together. There was another thing in Mr. Hooke's letters, which he will think I had from him. He told me, that my proposed experiment about the descent of falling bodies was not the only way to prove the motion of the earth; and so added the experiment of your pendulum clock at St. Helena as an argument of gravity's being lessened at the equator by the diurnal motion. The experiment was new to me, but not the notion; for in that very paper, which I told you was writ some time above fifteen years ago, and to the best of my memory was writ eighteen or nineteen years ago, I calculated the force of ascent at the equator, arising from the earth's diurnal motion, in order to know what would be the diminution of gravity thereby. But yet to do this business right, is a thing of far greater difficulty than I was aware of. A third thing there was in his letters, which was new to me, and I shall acknowledge it, if I make use of it. 'Twas the deflexion of falling bodies to the south-east in our latitude. And now having sincerely told you the case between Mr. Hooke and me, I hope I shall be free for the future from the prejudice of his letters. I have considered how best to compose the present dispute, and I think it may be done by the inclosed scholium to the fourth proposition. In turning over some old papers I met with another demonstration of that proposition, which I have added at the end of this scholium. Which is all at present from

"your affectionate friend,
"and humble servant,
"Is. Newton.

"This letter was printed in the Gen. Dic. (vol. vii. p. 800); but the first fourteen lines and the diagrams belonging to them are omitted. It was reprinted in the Biographia Britannica (life of Hooke, vol. iv. p. 2661), where the last sentence ('In turning over,' &c. p. 41) is omitted, as well as the beginning, which was left out in the General Dictionary."

6.—Newton to Halley.

"Sir,

"Yesterday I unexpectedly struck upon a copy of the letter, I told you of, to Hugenius. 'Tis in the hand of one Mr. John Wickins, who was then my chamber-fellow, and is now parson of Stoke Edith near Monmouth [Hereford], and so is authentic. It begins thus, being directed to Mr. Oldenburg.

"'Sir,

"'I receiv'd your letters, with M. Hugens's kind present, which I have viewed with great satisfaction, finding it full of very subtile and useful speculations very worthy of the author. I am glad, that we are to expect another discourse of the Vis Centrifuga, which speculation may prove of good use in Natural Philosophy and Astronomy, as well as Mechanics. Thus, for instance, if the reason, why the same side of the moon is ever towards the earth, be the greater conatus of the other side to recede from it, it will follow (upon supposition of the earth's motion about the sun), that the greatest distance of the sun from the earth is to the greatest distance of the moon from the earth, not greater than 10000 to 56; and therefore the parallax of the sun not less than $\frac{56}{10000}$ of the parallax of the moon; because were the sun's distance less in proportion to that of the moon, she would have a greater conatus from the sun than from the earth. I thought also some time that the moon's libration might depend upon her conatus from the sun and earth compared together, till I apprehended a better cause.'

"Thus far this letter concerning the Vis Centrifuga. The rest of it, for the most part concerning colours, is printed in the Phil. Trans. of July 21, 1673, No. 96. Now from these words it's evident, that I was at that time versed in the theory of the force arising from circular motion, and had an eye upon the forces of the planets, knowing how to compare them by the proportions of their periodical revolutions and distances from the centre they move about: an instance of which you have here in the comparison of the forces of the moon arising from her menstrual motion about the earth, and annual about the sun. So then in this theory I am plainly before Mr. Hooke. For he about a year after, in his Attempt to prove the Motion of the Earth, declared expressly, that the degrees, by which gravity decreased, he had not then experimentally verified; that is, he knew not how to gather it from phenomena; and therefore he there recommends it to the prosecution of others.

"Now, though I do not find the duplicate proportion expressed in this letter (as I hoped it might), yet if you compare this passage of it here transcribed, with that hypothesis of mine, registered by Mr. Oldenburg in your book, you will see that I then understood it. For I there suppose that the descending spirit acts upon bodies here on the superficies of the earth with force proportional to the superficies of their parts; which cannot be, unless the diminution of its velocity in acting upon the first parts of any body it meets with, be recompensed by the increase of its density arising from that retardation. Whether this be true is not material. It suffices, that 'twas the hypothesis. Now if this spirit descend from above with uniform velocity, its density, and consequently its force, will be reciprocally proportional to the square of its distance from the centre. But if it descend with accelerated motion, its density will everywhere diminish as much as its velocity increases; and so its force (according to the hypothesis) will be the same as before, that is, still reciprocally as the square of its distance from the centre.

"In short, as these things compared together shew, that I was before Mr. Hooke in what he pretends to have been my master, so I learned nothing by his letters but this, that bodies fall not only to the east, but also in our latitude to the south. In the rest his correcting and informing me was to be complain'd of. And tho' his correcting my spiral occasioned my finding the theorem, by which I afterwards examined the ellipsis; yet am I not beholden to him for any light into the business, but only for the diversion he gave me from my other studies to think on these things, and for his dogmaticalness in writing, as if he had found the motion in the ellipsis, which inclined me to try it, after I saw by what method it was to be done. Sir, I am,

"your affectionate friend,
"and humble servant,
"IS. NEWTON.

"*July* 27, 1686.

"This letter was printed in the Gen. Dict. (vol. vii. p. 800), and reprinted in the Biographia Britannica (life of Hooke, vol. iv. p. 2661). The original letter to Oldenburg, from which an extract is given here by Newton, is in the guard-book (No. 1), and the date of it is there preserved, 'June 23, 73.' It likewise contains a passage, in addition to what Newton has quoted, and which is omitted in the copy that is printed in the Phil. Transactions (vol. viii. p. 6087). It does not indeed bear upon the present subject, but still the completion of the letter may be some apology for inserting it in this place. It is as follows:

"In the demonstration of the 8th proposition de descensu gravium, there seems to be an illegitimate supposition, namely, that the flexures at B and C do not hinder the motion of the descending body. For in reality they will hinder it, so that a body which descends from A shall not acquire so great velocity, when arrived at D, as one which descends from E. If this supposition be made because a body descending by a curve line meets with no such opposition, and this proposition is laid down in order to the contemplation of motion in curve lines, then it should have been shewn that though rectilinear flexures do hinder, yet the infinitely little flexures which are in curves, though infinite in number, do not at all hinder the motion.

"The rectifying curve lines by that way which Mr. Hugens calls evolution, I have been sometimes considering also, and here met with a way of resolving it, which seems more ready and free from the trouble of calculation than that of M. Hugens. If he please, I will send it him. The problem also is capable of being improved by being propounded thus more generally.

"'Curvas invenire quotascunque, quarum longitudines cum propositæ alicujus curvæ longitudine, vel cum area ejus ad datam lineam applicata, comparari possunt.'"

7.—HALLEY TO NEWTON.

"LONDON, *October* 14, 1686.

"SIR,

"By reason you are desirous that your book should not be public before Hilary Term, the impression has not been expedited as it might have been; but I hope that it is the more correct for proceeding so slow. I have sent you by the coach which goes from hence to-morrow morning, all the sheets that are done, desiring you would be pleased to mark all the errata you shall find, that so if there be any material one, the reader may be advertised thereof, but this at your leisure. At present I more immediately want to be informed concerning your geometrical effection of the problem XXIII, as much as relates to the 63d figure, for upon trial (there being no demonstration annexed) there seems to be some mistake committed: wherefore I intreat you would please to send me, revised by yourself, those few lines that relate thereto, and, if it be not too much trouble, be prevailed upon to subjoin something of the Demonstration. In your transmutation of figures according to the 22d lemma, which you use in the two following problems, to me it seems that the manner of transmuting a trapezium into a parallelogram needs some further explanation; I have printed it as you sent it, but I pray you please a little farther to describe by an example the manner of doing it, for I am not perfectly master of it; a short hint will suffice. Pray, defer the answer hereto as little as may stand with your convenience, for we are now within a sheet of the 23d problem, and shall want your amendments, if there be occasion for them. If there be any service I can do you here in town, pray command, Sir,

"Your most affectionate humble servant,

"EDM. HALLEY.

"*To his honoured Friend,*
MR. ISAAC NEWTON,
at TRINITY COLLEDG, CAMBRIDG,
These."

8.—NEWTON TO HALLEY.

"SIR,

"In the scholium you write of, the words 'vel hyperbolæ' in the 3d line are to be struck out, and in the 5th and 6th lines the words 'quæ sit ad GK' should be 'quæ sit ad ½ GK.' I send you inclosed the beginning of this scholium with the 63d figure as I would have them printed. I thank you heartily for giving me notice that it was amiss. The ground of the transmutation of a trapezium into a parallelogram I lay down, pag. 87, in these words: 'Nam rectæ quævis convergentes transmutantur in parallelas, adhibendo pro radio ordinato primo AO lineam quamvis rectam, quæ per concursum convergentium transit: id adeo quia concursus ille hoc pacto abit in infinitum, lineæ autem parallelæ sunt quæ ad punctum infinite distans tendunt.' In the figure, pag. 86, conceive the curve HGI to be produced both ways till it meet and intersect itself any where in the radius ordinatus primus AO: and when the point G moving up and down in the curve HI arrives at that intersection point, I say the point g moving in like manner up and down in the curve h i will become infinitely distant. For the point G falling upon the line OA, the point D will fall upon the point A, and the line OD upon the line OA; and so becoming parallel to aB their intersection point d will become infinitely distant, and consequently the line d g will become infinitely distant, and so will its point g. Q. E. D. So then if any two lines of the primary figure HGID intersect in the radius ordinatus primus AO, their intersection in the new figure h g i d shall become infinitely distant; and, therefore, if the two intersecting lines be right ones, they shall become parallel. For right lines, which lead to a point infinitely distant, do not intersect one another and diverge, but are parallel. Therefore, if in the primary figure there be any trapezium, whose opposite sides converge to points in the radius ordinatus primus O A, those sides in the new figure shall become parallel, and so the trapezium be converted into a parallelogram.

"The printed sheets I intend to look over. Mr. Paget, in his stay here, has noted these errata, of which the 3d is a fault in the copy.

"P. 6, l. 27, velocitate; p. 8, l. 19, tur Sunt.; p. 14, l. 30, reciproce ut DO; p. 18, l. 1, recta. I wish the printer be careful to mend all you note. Sir, I am very sensible of the great trouble you are at in this business, and the great care you take about it. Pray take your own time. And if you meet with any thing else, which you think need either correcting or further explaining, be pleased to signify it to

"your humble and obliged servant,

"IS. NEWTON.

"TRIN. COLL.
Octob. 18, 1686.

"My thanks for your note of De la Hire."

9.—HALLEY to NEWTON.

"LONDON, *Feb.* 24 [1686-7].

"HONOURED SIR,

"I return you most hearty thanks for the copy you sent me of the sheet which was lost by the printer's negligence; I will now do nothing else till the whole be finished, which I hope may be soon after Easter; and to redeem the time I have lost, I will employ another press to go on with the second part, which I am glad to understand you have perfected, and if you please to send it up to me, as soon as I have it I will set the printer to work on it, and will not be wanting to do my part to let it appear to the world to your satisfaction. I am sorry the Society should be represented to you so unsteady as to fall so frequently into variance,[1] but there is no such thing; and I am bold to say, that I serve them to their satisfaction, though six out of thirty-eight last general election did their endeavour to have put me by.[2] Dr. Wallis his papers I will send you; the result is much the same with yours, and he had the hint from an account I gave him of what you had demonstrated, I will send it you with some more sheets this next week; it is as yours founded on the hypothesis of the opposition being proportionate to the celerity which you say you find reason to dispute. Your demonstration of the parallax of the sun from the inequalities of the moon's motions, is what the Society has commanded me to request of you, it being the best means of determining the dimensions of the planetary system, which all other ways are deficient in; and they entreat you not to desist when you are come so near the solution of so noble a problem. This done, there remains nothing more to be enquired in this matter, and

[1] Mr. Weld, in his History of the Royal Society, does not mention any "variances" as taking place at this time.

[2] Halley was at this time Clerk and Assistant Secretary, and continued so till 1698.

you will do yourself the honour of perfecting scientifically what all past ages have but blindly groped after. I have your two propositions, you sent me some time since, and shall insert them in their proper place.

"I am, Sir, to the utmost of my power,

"Your most affectionate humble servant,

"EDM. HALLEY.

"*To his Honoured Friend,*
Mr. ISAAC NEWTON,
in TRINTY COLLEDG, CAMBRIDG,
These."

10.—NEWTON TO HALLEY.

"SIR,

"I have sent you the sheet you want. The second book I made ready for you in autumn, having wrote to you in summer that it should come out with the first, and be ready against the time you might need it, and guessing by the rate of the press in summer you might need it about November or December. But not hearing from you, and being told (though not truly) that, upon some differences in the Royal Society, you had left your secretary's place, I desired my intimate friend Mr. C. Montague[1] to enquire of Mr. Paget how things were, and send me word. He writes, that Dr. Wallis has sent up some things about projectiles pretty like those of mine in the papers Mr. Paget first shewed you, and that 'twas ordered I should be consulted whether I intend to print mine. I have inserted them into the beginning of the second book with divers others of that kind: which therefore, if you desire to see, you may command the book when you please, though otherwise I should choose to let it lie by me till you are ready for it. I think I have the solution of your problem about the sun's parallax, but through other occasions shall scarce have time to think further on these things: and besides, I want something of observation, for if my notion be right, the sun draws the moon in the quadratures, so that there needs an equation of about 4 or 4½ minutes to be subducted from her motion in the first quarter and added in the last. I hope you received a letter with two corollaries I sent you in autumn. I have eleven sheets already, that is, to M. When you have seven more printed off I desire you would send them. I thank you for putting forward the press again, being very sensible of the great trouble I give you amidst so much business of your own and the Royal Society's. In this, as well as in divers other things, you will much oblige

"your affectionate friend,

"and humble servant,

"IS. NEWTON.

"TRIN. COLL. CAMBRIDGE,
Feb. 28, 1686. [1686-7.]"

[1] Afterwards the Earl of Halifax.

11.—Newton to Halley.

"Sir,

"You'll receive the 2nd book on Thursday night or Friday by the coach. I have directed it to be left with Mr. Hunt at Gresham Coll. Pray let me beg the favour of a line or two to know of the receipt. I am obliged to you for pushing on the edition, because of people's expectation, tho' otherwise I could be as well satisfied to let it rest a year or two longer. 'Tis a double favour, that you are pleased to double your pains about it. Dr. Wallis's papers may be long, and I would not give you the trouble of transcribing them all. The heads may suffice. The resistance, in swift motions, is in a duplicate proportion to the celerity. The deduction of the sun's parallax from the moon's variation, I cannot promise now to consider. When astronomers have examined whether there be such an inequality of her motion in the quadratures, as I mentioned in my last, and determined the quantity thereof, I may take some occasion perhaps to tell them the reason. No more at present from

"your most affectionate humble servant,

"Is. Newton.

"Cambridge.
March 1, 86-7."

12.—Halley to Newton.

"London, *March* 7, 1686-7.

"Honoured Sir,

"I received yours, and according to it your Second Book, which this week I will put to the press, having agreed with one that promises me to get it done in seven weeks, it making much about twenty sheets. The First Book will be about thirty, which will be finished much about the same time. This week you shall have the eighteenth sheet according to your directions. You mention in this Second your Third Book *De Systemate Mundi*, which from such firm principles, as in the preceding you have laid down, cannot choose but give universal satisfaction, if this be likewise ready, and not too long to get printed at the same time, and you think fit to send it; I will endeavour by a third hand, to get it all done together, being resolved to engage in no other business till such time as all is done, desiring hereby to clear myself from all imputations of negligence in a business wherein I am much rejoiced to be any ways concerned in handing to the world that that all future ages will admire, and as being,

"Sir, your most obedient servant,

"Edm. Halley.

"*To* Mr. Isaac Newton,
at Trinity Colledg Cambridg."

No answer to this letter, or any of the subsequent letters, has been preserved.

13.—HALLEY TO NEWTON.

"*March* 14, 1686-7.

"SIR,

"I have now sent you the eighteenth sheet of your book, but could not be as good as my word, by reason of the extraordinary trouble of the last sheet, which was the reason it could not be finished time enough to send it you the last week. I have not been wanting to endeavour the clearing it of errata, but am sensible that, notwithstanding all my care, some have crept in; but I hope none of consequence. Pray, please to examine it yourself, and note what mistakes are committed, that so they may be noted at the end; and if they be very material, the sheet shall be done over again, as I was forced to do the sheet D; and half the sheet P must be done, for the figure is turned upside down by the negligence of the printer, in p. 112. I hope, in a fortnight more, to send you as many more sheets, and very suddenly to have the first part finished—being,

"Sir, your most humble servant,

EDM. HALLEY.

"*To* MR. ISAAC NEWTON,
at TRINITY COLL.,
CAMBRIDGE.

"*Post-pd.*,
These present—
With a small parcel."

14.—HALLEY TO NEWTON.

"LONDON, *April* 5, 1687.

"HONOURED SIR,

"I received not the last part of your divine treatise till yesterday, though it came to town that day se'ennight, having had occasion to be out of town the last week. The first part will be finished within the three weeks, and, considering the shortness of the third over the second, the same press that did the first will get it done so soon as the second can be finished by another press; but I find some difficulty to match the letter justly. Your method of determining the orb of a comet deserves to be practised upon more of them, as far as may ascertain whether any of those that have passed in former times may have returned again; for their nodes and perihelia being fixed, will prove it sufficiently, and, by their periods, the transverse diameters will be given, which possibly may render the problem more easy. If you can remove the fault in the comet's latitudes, 'twill be better; but as it is, the numbers you have laid down do make out the verity of the hypothesis past dispute. I do not find that you have touched that notable appearance of comets' tails, and their opposition to the sun, which seems rather to argue an efflux from the sun than a gravitation towards him. I doubt not but this may follow from your principles with the like ease as all the other phenomena; but a proposition or two concerning these will add much to the beauty and perfection of your theory of comets. I find I shall not get the whole completed before Trinity Term, when I hope to have it published, when the world will not be more instructed by the demonstrative doctrine thereof, than it will pride itself to have a subject capable of penetrating so far into the abstrusest secrets of nature, and exalting human reason to so sublime a pitch by this utmost effort of the mind. But least my affection should make me transgress, I remain,

"Your most obedient servant,
EDM. HALLEY.

"*To* MR. ISAAC NEWTON,
to be left with Mr. Parish Rector of Coulsterworth, in Lincolnshire. These—"

No. IX.

(*Referred to in page* 272, *as* No. XI.)

THE following is a copy of the verses written by Halley, and prefixed to the First Edition of the Principia. In imitation of Professor Rigaud, the original verses are printed in the larger type. The alterations made by Bentley, in the second edition of 1713, are in a smaller type, and the parts between brackets are the alterations adopted in the third edition, published by Pemberton in 1726.

HALLEY'S VERSES PREFIXED TO THE PRINCIPIA.

In
viri præstantissimi
D. ISAACI NEWTONI
opus hocce
mathematico-physicum
sæculi gentisque nostræ decus egregium.

En tibi norma Poli, et divæ libramina Molis,
[en] [et]
Computus atque Jovis ; quas, dum primordia rerum
Conderet, omnipotens sibi ipse
Pangeret, omniparens Leges violare Creator
Dixerit, [atque operum quæ fundamenta locarit.]
Noluit, æternique operis fundamina fixit.
Intima panduntur victi penetralia cœli,
circumrotet,
Nec latet extremos quæ Vis circumrotat Orbes.
Sol solio residens ad se jubet omnia prono
Tendere descensu, nec recto tramite currus
Sidereos patitur vastum per inane moveri ;
Sed rapit immotis, se centro, singula Gyris.

Hinc qua
Jam patet horrificis quæ sit via flexa Cometis ;
Jam non miramur barbati Phænomena Astri.[1]
Discimus hinc tandem qua causa argentea Phœbe
eat, et
Passibus haud æquis graditur ; cur subdita nulli
Hactenus Astronomo numerorum fræna recuset :
remeent progrediantur
Cur remeant Nodi, curque Auges progrediuntur.
Discimus et quantis refluum vaga Cynthia Pontum
impellat ; [fessis dum]
Viribus impellit, dum fractis fluctibus Ulvam
Deserit, ac Nautis suspectas nudat arenas ;
Alternisve ruens spumantia pulsat.
Alternis vicibus suprema ad littora pulsans.
Quæ toties animos veterum torsere Sophorum,
hodie
Quæque Scholas frustra rauco certamine vexant
Obvia conspicimus nubem pellente Mathesi.
Jam dubios nulla caligine prægravat error,[2]
Quæ superas
Queis Superum penetrare domos atque ardua Cœli
Newtoni auspiciis, jam dat contingere Templa.
Scandere sublimis Genii concessit acumen.
Surgite Mortales, terrenas mittite curas ;
cognoscite
Atque hinc cœligenæ vires dignoscite Mentis,
A pecudum vita longe lateque remotæ.
primus
Qui scriptis jussit Tabulis compescere Cædes,
Furta et Adulteria, et perjuræ crimina Fraudis ;
Quive vagis populis circumdare mœnibus Urbes
Autor erat ; Cererisve beavit munere gentes ;
Vel qui curarum lenimen pressit ab Uva ;
Vel qui Niliaca monstravit arundine pictos
Consociare sonos, oculisque exponere Voces ;
Humanam sortem minus extulit ; utpote pauca
In commune ferens miseræ solatia
[*tantum solamina*]
Respiciens miseræ solummodo commoda vitæ.
Jam vero Superis convivæ admittimur, alti
diæ
Jura poli tractare licet, jamque abdita cœcæ
Naturæ, et
Claustra patent Terræ, rerumque[3] immobilis ordo,
præteritis latuere incognita sæclis.
Et quæ præteriti latuerunt sæcula mundi.
justis
Talia monstrantem mecum celebrate Camœnis,

[1] This line was entirely omitted in 1713, and restored in 1726.
[2] This line also was omitted in 1713, and restored in 1726.
[3] que—omitted in 1713, restored in 1726. The parts in italics are alterations, made in the third, though not in the second edition.

[*o cælicolum gaudentes*]
Vos qui cœlesti gaudetis nectare vesci,
Newtonum clausi reserantem scrinia Veri
carum
Newtonum Musis charum, cui pectore puro
Phœbus adest, totoque incessit Numine mentem :
Nec fas est propius Mortali attingere Divos.
EDM. HALLEY.

It does not appear on what authority those changes were introduced into the third edition, which did not exist in the two first. It is quite certain that they were made without the authority either of Halley or Newton. It is probable, from the following anecdote, which we found in Conduitt's manuscripts, that Pemberton was the author of them.

"Bentley," says Conduitt, "altered Halley's verses when he printed the Principia. Halley told me that Sir Isaac Newton made him hope that in Pemberton's edition his verses would be printed from his own copy, but complained they were not, for he made it—

Æternique operis fundamenta fixit.

And it is printed,

Operumque fundamenta locavit.

And when I said that perhaps Sir Isaac Newton did not care for having anything appear before his book, that seemed to favour the idea that the world was eternal ;—'Yes,' said he, 'that is what Pemberton would fix upon me, but *æternum* is only *æviternum*, and I meant no more.'"—Conduitt's MSS.

No. X.

(*Referred to in page* 278, *as* No. XII.)

It is either a great privilege or a great misfortune to be the associate of distinguished individuals. The light of the halo which surrounds them falls brightest on their companions, but though it generally illustrates and adorns, it sometimes displays failings and imperfections of character, and transmits them to posterity. We have already seen how unfortunate for the memory of Mr. Paget was his connexion with Newton and Flamsteed. We shall now see the reverse in the case of Cotes, who, though justly distinguished by his own talents and acquirements, has yet derived a considerable portion of his reputation from being the friend of Newton, and an editor of the Principia. It is probable, indeed, from the fact that Bentley was the proprietor of the second edition of the Principia, and a worshipper of Mammon, that Fame was the only reward which fell to the lot of Cotes.

Roger Cotes was born at Burbage, in Leicestershire, on the 10th July 1682. His father, who was rector of the parish, placed him at Leicester school, where, at the age of twelve, he displayed a great taste for mathematics. At the house of his uncle, the Rev. John Smith, and with his assistance, he made great progress in mathematics, and at St. Paul's School in London, he made equal progress in classical learning.

From St. Paul's School he went to Trinity College, Cambridge, where he was entered pensioner on the 6th April 1699. He was elected scholar in May 1701, took his degree of B.A. in 1703, and was sworn minor Fellow of the College, on the 3d of October 1705. On the 6th October 1707, he was appointed the first Plumian Professor of Astronomy and Experimental Philosophy. In 1713 he took orders, and in the same year undertook to superintend the second edition of the Principia.

In 1714 he published in the Philosophical Transactions a paper, entitled *Logometria,* the first part of the treatise on the same subject, which forms the principal part of his posthumous work, entitled *Harmonia Mensurarum,* edited in 1722 by his cousin, Dr. Robert Smith. In 1716, he communicated to the Society an account of the great fiery meteor seen on the 6th March of that year, but it is obvious from his description of it that it was only an Aurora Borealis.

In a few weeks after he wrote this communication to the Society, he was seized with fever, and, after a relapse, accompanied with violent diarrhœa and constant delirium, he died on the 5th June 1716, amid the deep regrets of the University and the scientific world. When Newton received the sad intelligence of the loss of his friend, he made the memorable observation, "If Mr. Cotes had lived we might have known something."

A short time before his death, when he was only in his thirty-second year, he demonstrated the beautiful optical theorem, that "the magnitude of the image of an object seen through any number of lenses is to that of the object itself, as the distance of the image from the eye is to the apparent distance of the object."[1]

In 1722, there appeared the *Epistola ad Amicum de Cotesii Inventis,* addressed to Mr. James Wilson, by Henry Pemberton, and an Appendix, bearing the date of May 1722. Beside some tracts in Latin, which have not been published, he left behind him a course of lectures on Hydrostatics and Pneumatics, which was published by Dr. Smith in 1738. In Mr. Edleston's *Correspondence,* he has published twenty-four letters from Cotes to his friends, from one of which it appears that he had anticipated S'Gravesende in the invention of the Heliostate.[2]

Cotes was interred in the chapel of Trinity College, and the following and much admired inscription on his monument, was written by Dr. Bentley.

H. S. E.

ROGERUS ROBERTI FILIUS COTES,

Hujus Collegii S. Trinitatis Socius,

Et Astronomiæ et Experimentalis

Philosophiæ Professor Plumianus;

[1] See Smith's *Optics,* vol. i. p. 191, cor. 19; and vol. ii., *Remarks,* p. 76.

[2] Mr. Edleston refers to the Register of the Royal Society for evidence, that Hooke and Halley had previously invented the Heliostate. The first publication, however, of the invention, is due to the Dutch philosopher.—See S'Gravesende's *Physices Elem. Math.* vol. ii. p. 715, § 2660, Tab. 84, 85. Edit. 1742.

Qui immatura morte præreptus,
Pauca quidem ingenii sui
Pignora reliquit,
Sed egregia, sed admiranda,
Ex intimis Matheseos penetralibus
Felici solertia tum primum eruta;
Post magnum illum Newtonum,
Societatis hujus spes altera,
Et decus gemellum;
Cui ad summam doctrinæ laudem
Omnes morum virtutumque dotes
In cumulum accesserunt;
Eo magis spectabilis amabilisque,
Quod in formoso corpore
Gratiores venirent.
Natus Burbagii
In agro Leicestriensi
Jul. x. MDCLXXXII.
Obiit Jun. v. MDCCXVI.

No. XI.

(*Referred to in page* 297, *as* No. XIII.)

THE great interest excited by the Principia even among persons who were not qualified by their mathematical knowledge to comprehend it, led some individuals of active and powerful minds to acquire as much geometrical and analytical knowledge as would enable them to understand and appreciate the leading truths which Newton had discovered. Dr. Bentley, as we have already seen, was anxious to expound the discoveries of Newton[1] in a popular form, and to adduce them as proofs of the wisdom and benevolence of the Deity; and having resolved to study the work which contained them, he applied, through his friend, Mr. William Wotton,[2] to John Craige, an able Scotch mathematician, for a list of works

[1] Dr. Monk is of opinion that Bentley had previously attended Newton's lectures. "The true system of the universe," he says, "and the proper methods of philosophical investigation, had not become public by the writings of Newton, but the light of the Newtonian discoveries was partially revealed to Cambridge before the rest of the world by the lectures of the philosopher himself, delivered in the character of the Lucasian Professor. These Bentley had an opportunity of attending; and that he did not neglect it, I am induced to believe, by his selection of the Newtonian discoveries as a prominent subject of his Boyle's Lectures, and his familiarity with the train of reasoning by which they are established."—Monk's *Life of Bentley*, pp. 6, 7.

[2] William Wotton, the friend of Bentley and of Craige, was a very remarkable person; and Dr. Monk informs us that he was the only one of Bentley's contemporaries with

which should be read in order to understand the Principia. Alarmed with the long list of authors sent him by Craige on the 24th June 1691, Bentley seems to have applied to Newton himself, from whom he received the following directions. Mr. Edleston thinks that the date of it is probably about July 1691:—

Directions given by Newton to Bentley respecting the books necessary to be read before studying the Principia.[1]

"Next after Euclid's Elements the Elements of y^e^ Conic sections are to be understood. And for this end you may read either the first part of y^e^ Elementa Curvarum of John De Witt, or De la Hire's late treatise of y^e^ conick sections, or D^r^ Barrow's Epitome of Apollonius.

"For Algebra read first Barth[ol]in's introduction, & then peruse such Problems as you will find scattered up & down in y^e^ Commentaries on Cartes's Geometry & other Algebraical [*sic*] writings of Francis Schooten. I do not mean y^t^ you should read over all those Commentaries, but only y^e^ solutions of such Problems as you will here & there meet with. You may meet with De Witt's Elementa Curvarum & Bartholin's Introduction bound up together w^th^ Carte's Geometry & Schooten's Commentaries.

"For Astronomy read first y^e^ short account of y^e^ Copernican System in the end of Gassendus's Astronomy & then so much of Mercator's Astronomy as concerns y^e^ same system & the new discoveries made in the heavens by Telescopes in the Appendix.

"These are sufficient for understanding my book: but if you can procure Hugenius's Horologium Oscillatorium, the perusal of that will make you much more ready.

"At y^e^ first perusal of my Book it's enough if you understand y^e^ Propositions w^th^ some of y^e^ Demonstrations w^ch^ are easier than the rest. For when you understand y^e^ easier they will afterwards give you light into y^e^ harder. When you have read y^e^ first 60 pages, pass on to y^e^ 3^d^ Book & when you see the design of that you may turn back to such Propositions

whom he maintained a friendship in after life. "He was," adds Dr. Monk, "the able antagonist of Sir W. Temple on the controversy 'On Ancient and Modern Learning.' As their combined efforts on that occasion have associated together the names of Wotton and Bentley, it is right to take some notice of the former, who, when he entered the University, was a child, and presents the best authenticated instance of a juvenile prodigy that I have ever found upon record. It is certified by the testimony, not of one, but many persons of sense and learning, that at *six* years of age he was able to read and translate Latin, Greek, and Hebrew; to which at *seven* he added some knowledge of the Arabic and Syriac. On his admission at Catherine Hall, in his *tenth* year, the master, Dr. Eachard, the antagonist of Hobbes, recorded '*Gulielmus Wotton, infra decem annos, nec Hammondo nec Grotio secundus.*' This surprising proficiency during his academical career is testified by some of the best scholars of that day. . . . When he proceeded Bachelor of Arts, he was acquainted with twelve languages, and, as there was no precedent of granting that degree to a boy of thirteen, Dr. H. Gower, one of the Caput, thought fit to put upon record a notice of his proficiency in every species of literature, as a justification of the University."—Monk's *Life of Bentley*, pp. 7, 8; see also Nichol's *Literary Anecdotes*, vol. iv. pp. 253-259.

[1] We have given this paper exactly as it is printed in Mr. Edleston's *Correspondence*, &c., pp. 273, 274. It is copied from the original, presented, along with the original MSS. of Newton's four celebrated letters to Bentley, by his grandson, Richard Cumberland, to Trinity College.—Cumberland's *Memoirs*, vol. i. p. 94.

as you shall have a desire to know, or peruse the whole in order if you think fit."

The following memorandum is written upon the MS. by Bentley:—

"Directions from Mr Newton by his own Hand."

Directions given by John Craige for understanding the Principia.

The course of reading proposed by John Craige for understanding the Principia is much more extensive than that of Newton. It is published in Bentley's Correspondence,[1] in a very interesting letter addressed to William Wotton, which, we have no doubt, will be gratifying to some of our readers:—

"WINDSOR, 24 *June*, 1691.

"SIR,

"I would have sent you this line before this, if I had thought you had returned from Cambridge. You may tell your Friend that nothing less than a thorough knowledge of all that is yet known in the most curious parts of Mathematicks can make him capable to read Mr. Newton's book with that advantage which I believe he proposes to himself. Upon this account, then, it may justly seem a very undecent piece of vanity to undertake to give a method for reading a book that involves so much in it, and so far above my strength; however, in compliance with your desire, I shall give you that which appears to me to be the shortest and most proper method for such an end.

"Next to Euclid's Elements, let him apply himself to the Conick Sections, for which he need only read *De Witt's First Book De Elementis Linearum Curvarum;* but let him not meddle with the second, which treats of the *Loca Geometrica.* After he has made himself Master of the Conick Sections, he must read some good System of Algebra: I know none better than *Jo. Prestet's Elémens des Mathématiques,* especially if he can get the new edition: here it is absolutely necessary to be constantly exercising himself in the resolving of Problems; but let him forbear meddling with any geometrical Problem, until he be entirely Master of all the precepts of common Algebra; afterwards he may look over *Wallis, De Beaun, Fermott, Hudden,* and pick out several things which he will scarcely meet with in Prestet, or any one System. When this is done, the great difficulty of the work is over: this is the foundation of all; and, therefore, he must not grudge to bestow more time and application upon it, than, perhaps, he would willingly allow, if he knew how much of both are requisite. I must not forget to desire him to have a care not to begin with Kersey's Algebra, which is apt (by its pompous bulk and title) to deceive new beginners, as sad experience has taught myself. I can assure him there was never a duller book writ; and, as far as I can judge, there was never a man who pretended to write of Algebra that understood the design of it less than Mr. Kersey did: but, to do him justice, he treats the Arithmetical part of Algebra (both as to rational and Surd Quantities) in a very plain, full, and clear method. The prodigious loss of time which this unlucky book made me sustain (when I had no guide to direct me in my studies of this kind), drew this severe character of Mr. Kersey from me; and I doubt

[1] See vol. ii. pp. 736-740; and vol. i. p. xxxii.

not but this advertisement will be of some use to your friend. When he is thus well instructed with the Elements both of Geometry and Algebra, he must study the use of both, which consists in these two things, viz., the inventing of Theorems and resolving of Geometrical Problems; for which end he must begin with *Cartes his Geometry*, reading only the first and third book; but let him forbear the second till such time as he perfectly understands the first and last, which is Cartes his own advice in one of his Letters, and, indeed, the nature of the thing shows it should be so. This will give him a vaster idea of Geometry and of the great use of Algebra than is possible for me to express, or for one that has not read it to imagine. In the next place, let him peruse diligently *De Witt's* second book, which treats of the *Loca Geometrica;* and immediately after that read Cartes his second book, which treats of the same subject: and because the method of Tangents is the chief part of this 2nd book, and, indeed, of all his whole Geometry (as he himself confesses), let him read *Slusius his Method,* which he'll find in the *Philosophical Transactions; Dr. Barrow's Method,* which he'll find (if I remember right) at the end of his *10th Geometrical Lecture;* and *Mr. Leibnitz his Method,* which he'll find in the *Acta Eruditorum,* which is the best of all; for by these four (not to mention several others of less note) various methods he will become master of this famous Problem, which, of all others, is of the greatest use in the solution of the hardest Problems in Geometry. Here it will be again necessary to exercise his pen much in the solving of Geometrical, as before in the solving of Arithmetical Problems; which he may furnish himself with out of any books that are by him, particularly out of *Vieta, Reinaldini, Henderson, Schooten, Kersey,* &c.; but he must keep close to Cartes his General Method, and make no other use (as yet) of those books, but only to provide himself with good store of Problems.

"Another great Invention, which has extremely promoted Geometry in our Age, is the Method of Indivisibles. Wherefore, in the next place, let him read the famous *Cavalerius* on that subject, who is, if not the Inventor, yet, at least, the great Restorer of that Method. After him must be read *Dr. Barrow's Geometrical Lectures,* who has carried that Method further than any, and who will inform him with more excellent and universal Theorems than any book that has been written in this Age. When your friend has gone so far, he needs not be much solicitous in what order he read any book of pure Geometry or Algebra, but may take them promiscuously as they come to his hand; for scarce any thing will occur which he will not be able to overcome: but the books that I think will be most worthy of his application are, *Archimedes and Apollonius,* his works of Dr. Barrow's edition; *Slusius his Mesolabium; Vieta; Gregorius a Sancto Vincentio; Mr. James Gregory's Works; Hugenius his Horologium Oscillatorium; La Hire his Conick Sections;* and *Tschirnhaus his Medicina Mentis:* but in this last, as also in Archimed and Hugenius, he must pass over all that is not pure Geometry or Algebra.

"Then he must advance to those parts that are of a more compounded nature, and which have a more immediate Relation to Mr. Newton's book. First, then, he must read with a great deal of care *Galileus his Works De Motu;* in reading of which he will find vast help from *Dr. Barrow's five first Lectures.* Then he must read *Torricelli's book De Motu,* who carries on Galileus his design. He will find also much to the same purpose in *Gas-*

sendus, Hugenius, and Mersennus; after them he must read *Mariot, who treats of the Laws of Motion;* then let him read what *Sir Christopher Wren* and *Dr. Wallis* hath printed in the *Philosophical Transactions* concerning the said Laws; after this it will not be amiss to read *Dr. Wallis his Mechanicks,* but he may pass over all that part *De Calculo Centri gravitatis.* There are several things in *Mr. Hobbes De Motu* which will be of some use to him: and indeed, without a good understanding of what these Authors have already written concerning Motion, it is simply impossible to understand this unparalleled book of Mr. Newton's, which treats of nothing else but Motion, but in such a manner as tends to the perfecting of Philosophy, and particularly that part of it which relates to the motion of the Stars and Planets. Therefore, in the next place, your friend must make himself perfect in Astronomy, in studying of which let him begin with *Tacquet;* for though he follows a false Hypothesis, yet none has treated this matter in so clear and full a method. But here I suppose your friend to be skilled in Trigonometry (both plane and spherical, for which *Norwood* first, and *Ward* afterwards, are to be read), and the use of the Sphere. When he has done with Tacquet, let him get *Kepler, Bulliald, Seth Ward, Mercator and Gassendus, and Copernicus,* who ought to have been first mentioned: by the help of these he will have a perfect understanding of the state of Astronomy as it was before Mr. Newton published his book; which he might safely now begin with, were it not for some collateral things which he brings in from the Opticks, Hydrostaticks, &c. For the Opticks he must read *Cartes, Ja. Gregory, Dr. Barrow, Honoratus Fabri, and Tacquet;* and till he hath read these, he must pass over what Cartes speaks of his Ovals in the 2d book of his Geometry. For Hydrostaticks, he must read *Archimed and Borelli,* and something which he'll find in *Dr. Wallis his Mechanicks.* And because much of Master Newton's book refers to the Quadratures of Figures, he must read what has been written on this subject by *Dr. Wallis and Mr. David Gregory.*

"Here, you see, is a vast deal to be done, even enough to discourage a man whose inclinations have not a great bias this way; but he that seriously considers the real pleasure and advantage that arises from this, and (if I be not mistaken) only from this kind of study, will not be disheartened either by the tediousness or difficulty that attends it; but my business was not to persuade, but, as far as I am able, to instruct your friend in what Order he ought to proceed in this matter; which I have done with all the care and exactness that was possible. And if this shall chance to be of no use to him, yet I shall not fail entirely in the end for which I writ it, which was to show my readiness, at least, to serve you, for whose sake there is nothing that I will refuse to do that lies within the compass of my power, though it were even to the discovery of my own weakness and ignorance, which, perhaps, I have sufficiently done already; and, therefore, shall add no more, but that I am and ever shall be,

"Your most real friend and humble servant,

"JO. CRAIGE.

"*For Mr.* WILLIAM WOTTON,
Chaplain to The EARLE OF NOTTINGHAME,
at CLEVELAND-HOUSE,
LONDON."

The following note in Bentley's handwriting is written on the fourth page or cover of Mr. Craige's letter :—

"Ex Newtono
Cartesii geometria in De la Hire Lectiones Conicæ
Barthii Introductio in Algebra
Mercatoris Astronomia
Hugenii Horologium Oscillatorium."

THE most complete and successful attempt to make the *Principia* accessible to those who are "little skilled in mathematical science," has been made by Lord Brougham, in his admirable analysis of that work, which forms the greater part of the second volume of his edition of Paley's *Natural Theology.*[1]

"The reader of the *Principia,*" says Lord Brougham, "if he be a tolerably good mathematician, can follow the whole chain of demonstration by which the universality of gravitation is deduced from the fact, that it is a power acting inversely, as the square of the distance to the centre of attraction. Satisfying himself of the laws which regulate the motion of bodies in trajectories around given centres, he can convince himself of the sublime truths unfolded in that immortal work, and must yield his assent to this position, that the moon is deflected from the tangent of her orbit round the earth, by the same force by which the satellites of Jupiter are deflected from the tangent of theirs, the very same force which makes a stone unsupported fall to the ground. The reader of the *Mécanique Céleste,* if he be a still more learned mathematician, and versed in the modern improvements of the calculus which Newton discovered, can follow the chain of demonstration by which the wonderful provision made for the stability of the universe, is deduced from the fact, that the direction of all the planetary motions is the same—the eccentricity of their orbits small, and the angle formed by the plane of their orbits with the ecliptic acute. Satisfying himself of the laws which regulate the mutual actions of these bodies, he can convince himself of a truth yet more sublime than Newton's discovery, though flowing from it, and must yield his assent to the marvellous position, that all the irregularities occasioned in the system of the universe, by the mutual attraction of its members, are periodical, and subject to an eternal law which prevents them from ever exceeding a stated amount, and secures through all time the balanced structure of a universe composed of bodies whose mighty bulk and prodigious swiftness of motion, mock the utmost efforts of the human imagination. All these truths are to the skilful mathematician as thoroughly known, and their evidence is as clear, as the simplest proposition of arithmetic is to common understandings. But how few are those who thus know and comprehend them! Of all the millions that thoroughly believe these truths, certainly not a thousand individuals are capable of following even any considerable portion of the demonstrations upon which they rest; and probably not a hundred now living have ever gone through the whole steps of these demonstrations."[2]

[1] *Dissertations on Subjects of Science connected with Natural Theology.* By Henry Lord Brougham, F.R.S., and Member of the National Institute of France. Vol. ii. pp. 243-480. Lond. 1839.

[2] Ibid. vol. ii. pp. 172, 173.

This *analytical view* of the *Principia* has since been published in a separate form (1855) by Lord Brougham and Mr. Routh of St. Peter's College, Cambridge. Mr. Routh has extended it to the second and third book. The only addition made by Lord Brougham to the publication of 1839, is an elaborate Appendix, chiefly upon central forces directed to more than one centre, which it is greatly to be lamented that Sir Isaac Newton did not treat of.

I have mentioned in page 271, on the authority of Conduitt's MSS., the time when different parts of the *Principia* were written. I have found, in Sir Isaac's own handwriting, the following memorandum, which contains some additional information of considerable interest :—

"In the tenth proposition of the second book, there was a mistake in the first edition, by drawing the tangent of the arch GH from the wrong end of the arch, which caused an error in the conclusion ; but in the second edition I rectified the mistake. And there may have been some other mistakes occasioned by the shortness of the time in which the book was written, and by its being copied by an amanuensis who understood not what he copied ; besides the press faults, for I wrote it in seventeen or eighteen months, beginning in the end of December 1684, and sending it to the Royal Society in May 1686, excepting that about ten or twelve of the propositions were composed before, viz., the 1st and 11th in December 1679, the 6th, 7th, 8th, 9th, 10th, 12th, 13th, and 17th, Lib. I., and the 1st, 2d, 3d, and 4th, Lib. II., in June and July 1684."

No. XII.

(*Referred to in page* 360, *as* No. XIII.)

After the publication of the second edition of the *Principia*, when an erroneous interpretation had been given of the Scholium, Newton was very anxious that the motive under which he wrote it, and the precise meaning which he attached to it, should be understood. I have, therefore, given in page 359 an explanation of his views, which is more full than that quoted in the note from Raphson ; but I have found another MS. in which an additional motive is stated. "And because," he says, "Mr. Leibnitz had published those elements (meaning those in the Lemma) a year and some months before, without making any mention of the correspondence which I had with him by means of Mr. Oldenburg ten years before that time, I added a Scholium, not to give away the Lemma, but to put him in mind of that correspondence, *in order to his making a public acknowledgment thereof before he proceeded to claim that Lemma from me*."[1]

[1] The words in Italics are an interlineation.

DRAUGHT COPIES OF THE SCHOLIUM TO THE LEMMA.

SCHOLIUM.

In literis quæ mihi cum Geometra peritissimo G. G. Leibnitio, anno 1676, *intercedebant, cum significarem me compotem esse methodi* analyticæ *determinandi Maximas et Minimas, ducendi Tangentes,* quadrandi figuras curvilineas, conferendi easdem inter se, *et similia peragendi quæ in terminis surdis æque ac in rationalibus procederent, et* Tractatus quos de hujusmodi rebus scripsisse, alterum quem Barrovius, anno 1669, ad Collinium misit, et alterum anno 1671 in quo hanc methodum prius exposueram; cumque fundamentum hujus methodi *literis transpositis hanc sententiam involventibus* (*Data Equatione quotcunque fluentes quantitates involvente Fluxiones invenire et vice versa*), *celarem,* specimen vero ejusdem in curvis quadrandis subjungerem et exemplis illustrarem; et cum Collinius Epistolam, 10 Decem. 1672 datam, a me accepisset in qua methodum hanc descripseram et exemplo Tangentium more Slusiano ducendarum illustraveram, et hujus Epistolæ exemplar mense Junio anno 1676 in Galliam ad D. Leibnitium misisset, *et vir clarissimus* sub finem mensis Octobris, in reditu suo e Gallia per Angliam in Germaniam, epistolas meas in manu Collinii insuper consuluisset: incidit is tandem *in methodum* similem sub diversis verborum et notarum formulis, et mense Junio sequente specimen ejusdem in Tangentibus more Slusiano ducendis ad me misit, et subjunxit se credere *methodum meam a sua non abludere* presertim cum quadraturæ curvarum per utramque methodum faciliores redderentur. Methodi vero *utriusque fundamentum continetur in hoc Lemmate.*

Almost the whole of the Scholium printed in the *first* and *second* editions of the *Principia* is put in Italics, in order to show the change upon it which Sir Isaac had proposed for the *third* edition.

In the other two forms of the Scholium, written on the same sheet with the preceding, the first and second half of it are partly transposed; and at the end of one of them after *in hoc Lemmate,* are the words *et hæc methodus plenius exponitur in Tractatu.*

It is singular that both Newton and Cotes should have permitted the words *annis abhinc decem* to remain in the second edition, seeing that in 1713, *thirty-seven years* had elapsed. In the draughts, however, of the Scholium under consideration, the more correct words *anno* 1676 are substituted.

I have found another draught of the Scholium, distinctly written, without any important correction, which differs only from the printed one in the first edition of the *Principia* in the following points:—

1. After *ducendi tangentes* the words *quadrandi figuras curvilineas* are added.

2. After *procederet,* the words *methodumque exemplis illustrarem* are added; and

3. Before *celarem,* the word *eandem* is inserted.

No. XIII.

(*Referred to in page* 362, *as* No. XIV.)

John Wallis, D.D., the author of the following letter, was one of the most distinguished mathematicians of the seventeenth century. He was born at Ashford in Kent on the 23d November 1616. He studied at Emanuel College, Cambridge, and was a Fellow of Queens'. In 1644 he was chosen one of the Secretaries to the Westminster Assembly of Divines, and in 1649 Savilian Professor of Geometry at Oxford. Between the years 1654 and 1662 he carried on a keen controversy with Hobbes. His principal work is his *Arithmetica Infinitorum*, published in 1655.[1] His works, both theological and mathematical, were published by the curators of the University of Oxford in 1699, in 3 vols. folio. He died at Oxford on the 28th October 1703, and was in the 82d year of his age when he wrote the two following letters :—

1.—LETTER FROM WALLIS TO NEWTON.

"OXFORD, *April* 30, 1695.

"SIR,

"I thank you for your letter of April 21st by Mr. Conon. But I can by no means admit your excuse for not publishing your Treatise of Light and Colours. You say you dare not *yet* publish it. And why *not yet?* Or if not now, when then? You add, lest it create you some trouble. What trouble *now* more than at another time. Pray consider how many years this hath layn upon your hands already, and while it lyes upon your hands it will still be some trouble; (for I know your thoughts must still be running upon it.) But when published that trouble will be over. You think, perhaps, it may occasion some letters (of exceptions) to you, which you shall be obliged to answer. What if so? 'Twill be at your choice whether to answer them or not. The treatise will answer for itself. But are you troubled with no letters for not publishing it? I suppose your other friends call upon you for it as well as I, and are as little satisfied with the delay. Meanwhile you lose the reputation of it, and we the benefit, so that you are neither just to yourself nor kind to the public. And perhaps some other may get scraps of the notion and publish it as his own; and then 'twill be his, not yours, though he may perhaps never attain to the tenth part of what you be already master of. Consider that 'tis now about thirty years since you were master of these notions about *Fluxions* and *Infinite Series*; but you have never published aught of it to this day (which is worse than *nonumque prematur in annum*). 'Tis true I have endeavoured to do you right in that point. But if I had published the same or like notions without naming you, and the world possessed of another *calculus differentialis* instead of your *fluxions:* how should this or the next age know of your share therein? And even what I have said is but playing an after game for you to recover (precariously *ex postliminio*) what you had let slip in its due time. And even yet I see you make no great haste to

[1] See this Volume, page 340.

publish these letters[1] which are to be my vouchers for what I say of it. And even these letters at first were rather extorted from you than voluntary. You may say, perhaps, the last piece of this concerning colour is not quite finished. It may be so (and perhaps never will), but pray let us have what is; and while that is printing, you may (if ever) perfect the rest. But if, during the delay, you chance to die, or those papers chance to take fire (as some others have done), 'tis all lost both as to you and as to the public. It hath been an old complaint that an Englishman never knows when a thing is well (but will still be overdoing), and thereby loseth, or spoils many times what was well before. I own that modesty is a virtue, but too much diffidence (especially as the world now goes) is a fault. And if men will never publish aught till it be so perfect that nothing more can be added to it, themselves and the public will both be losers. I hope, Sir, you will forgive me this freedom (while I speak the sense of others as well as my own), or else I know not how we shall forgive these delays. I could say a great deal more, but if you think I have said too much already, pray forgive this kindness of

"Your real friend and humble servant,

"JOHN WALLIS.

"Dr. Gregory gives you his service."

2.—LETTER FROM WALLIS TO NEWTON.

The following letter, written more than two years afterwards, is partly on the same subject, but is interesting from the message which it contains from Leibnitz in the postscript of a letter to Wallis, dated May 28, 1697:—[2]

"OXONIÆ, *Julii* 1, 1697.

"CLARISSIME VIR,

"Accepi nuper a D. Leibnitio literas Hanoveræ datas Mai 28, 1697. In quibus cum nonnulla sint quæ te quadamtenus spectant, liberem tibi suis verbis exponere, viz., '*Si qua esset occasio, D. Newtono, summi ingenii viro (forte per amicum) salutem officiosissimam a me nunciandi, eumque meo nomine precandi ne se ab edendis præclaris meditationibus diverti pateretur, beneficio hoc a te petere auderem. Item methodum Fluxionum profundissimi Newtoni cognitam esse methodo mea differentiali non tamen animadverti, postquam opus ejus ad lucem prodiit, sed etiam professus sum in Actis Eruditorum; et alios quoque monui. Id enim candori meo convenire judicavi non minus quam ipsius merito. Itaque communi nomine designare soleo Analyseos Infinitesimalis (quæ latius quam Methodus Tetragonista patet) interim; quemadmodum et Vietiana et Cartesiana methodus, Analyseos Speciosa nomine venit; discrimina tamen nonnulla supersunt. Ita fortasse Newtoniana et mea differunt in nonnullis.*' Hæc ea verbatim transcripsi ex nobilissimi Leibnitii literis ut videas id ab exteris etiam desiderari, quod ego non tantum petii sed obtestatus sum aliquoties, aliique mecum, nec tamen hactenus obtinuimus ut quæ apud te primis desideratissima

[1] The letters on Fluxions in Wallis's *Works*, vol. ii. pp. 391-396.

[2] The letter of Leibnitz is dated 28*th March*, though in the title prefixed to it by Wallis, and in the following letter, the date is made 28th May.

ederentur. Quippe cum hoc aut negas aut differs; non tantum tuæ famæ sed et bono publico deesse videris. Duas illas Epistolas (longiusculas et refertissimas) anno 1676 scriptas (unde ego Excerpta quædam antehac edidi) curabo ego (nisi me id vetes) subjungi volumini cuidam meo (jam aliquandiu sub prælo) quamprimum per præli moras licebit. Tuam de Lumine et Coloribus Hypothesin novam (cujus aliquot specimina jam ante multis annis dederis) quam per annos (si recte conjicio) triginta apud te supprimere dictum est, spero ut propediem edendam cures; ut quam ego insignem Naturali Philosophiæ accessionem jamdudum existimavi et publice deberi: Quam et Prælo fuisse diu paratam audio.) Idem dixerim de pluribus quæ apud te latent, quorum ego non sum conscius. Hæc interim raptim monenda duxi.

"Tuus ad officia,

"JOHANNES WALLIS.

"I put it into this form, that if you think it proper you may desire Dr. Sloan to insert it in the Transactions."[1]

The letter is addressed on the back,

"*To* MR. ISAAC NEWTON, *Controller*
of the Mint at The Tower,
LONDON."

The first paragraph of this message to Newton, in the preceding letter, is given in Leibnitz's letter to Wallis, as printed in the third volume of his works, and the following reason is assigned for withholding the rest of the message:—[*Sequebantur pauca quæ rem Mathematicam non spectant.*]—Wallisii *Opera*, tom. iii. p. 680.

In Wallis's reply to Leibnitz, dated July 30, 1697, he says,—"Quæ Newtonum spectant, ad eum scripsi tuis verbis, simulque obtestatus sum meo nomine ut imprimi curet quæ sua supprimit scripta. Quod et sæpe ante feceram, sed hactenus in cassum."—*Ibid.* p. 685.

[1] This memorandum is placed at the very foot of the page, apparently for the purpose of its being cut off.

END OF VOLUME FIRST.

29

EDINBURGH: T. CONSTABLE,
PRINTER TO THE QUEEN, AND TO THE UNIVERSITY.

MEMOIRS

OF

THE LIFE, WRITINGS, AND DISCOVERIES

OF

SIR ISAAC NEWTON.

EDINBURGH: PRINTED BY THOMAS CONSTABLE,

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← see plate at front
"J" → A.R. McLeo
26 Oct
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MEMOIRS

OF

THE LIFE, WRITINGS, AND DISCOVERIES

OF

SIR ISAAC NEWTON.

BY SIR DAVID BREWSTER, K.H.

A.M., LL.D., D.C.L., F.R.S., AND M.R.I.A.,

One of the Eight Associates of the Imperial Institute of France—Officer of the Legion of Honour—Chevalier of the Prussian Order of Merit of Frederick the Great—Honorary or Corresponding Member of the Academies of St. Petersburg, Vienna, Berlin, Turin, Copenhagen, Stockholm, Munich, Göttingen, Brussels, Haerlem, Erlangen, Canton de Vaud, Modena, Florence, Venice, Washington, New York, Boston, Quebec, Cape Town, etc. etc.; and Principal and Vice-Chancellor of the University of Edinburgh.

Second Edition.

VOL. II.

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MDCCCLX.

Ergo vivida vis animi pervicit, et extra
Processit longe flammantia mœnia mundi :
Atque omne immensum peragravit mente animoque.
LUCRETIUS, Lib. i. l. 73.

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MEMOIRS

OF THE

LIFE AND WRITINGS OF SIR ISAAC NEWTON.

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NICOLAS FACIO DE DUILLIER, a Genevese by birth, came to England in the spring of 1687, and, with the exception of a visit to Switzerland in 1699, 1700, and 1701, remained there during the rest of his life. He had become acquainted with the celebrated Huygens at the Hague in 1686, and had attained to such a proficiency in mathematics, that he was introduced to Sir Isaac Newton, and visited him at Cambridge in the month of November 1692. Though only in the 28th year of his age, his health was precarious, and he seems to have consulted Newton on the subject of his spiritual as well as of his bodily condition. On his return from Cambridge, he caught a severe cold, which affected his lungs, and gave him great alarm. In communicating to Sir Isaac an account of his symptoms, he says, "I thank God that my soul is extremely quiet, in which you have had the chief hand;" and fearing that his illness would prove fatal, he expresses the "wish that his eldest brother, a man of an extraordinary integrity, should succeed him in his friendship." Sir Isaac answered this letter in course of post, making inquiries about his brother, and telling Facio that he remembered him in his prayers. In his reply, Facio gave him his most humble thanks, both for his prayers and his kindness,—requested him thus to remember him as long as he lived, and assured him that he always remembered him in a similar manner.[1]

[1] Nicolas Facio de Duillier, an eminent mathematician, was born at Basle on the 16th February 1664. In 1684 and 1685 he became acquainted with Count Fenil, a Piedmontese, who, having incurred the displeasure of the Duke of Savoy, took refuge in France, where he became captain of a troop of horse. Having quarrelled one day with the commanding officer of his regiment, when drawn up on parade, the Count shot him dead, and, being well mounted, escaped from his pursuers. He fled to Alsace, where he took refuge in the house of Mr. Facio's maternal grandfather; but, in order to assist him more effectually, he was sent to the house of Facio's father, who lived at Duillier. When walking alone with young Facio, the Count told him that he had offered to M. De Louvois to seize the Prince of Orange, and deliver him into the hands of the King; and he showed him the letter of M. Louvois, offering him the King's pardon, approving of the plan, and enclosing an order for money. The Prince of Orange was in the habit of taking a drive on the sands at Scheveling, a village three miles from the Hague, and the Count proposed, with the aid of ten or twelve men, to land in a light ship with Dutch colours, and carry off the Prince to Dunkirk. The scheme was ripe for execution in 1686; but Facio, aware of the Count's design to take the life of his son, felt it his duty to thwart him in the commission of the two crimes which he had in view. He had become acquainted with Dr. Burnet at Geneva, and knowing that he was going to Holland to visit the Prince of Orange, he acquainted the doctor with the Count's scheme, and agreed to accompany him to Holland with the view of explaining it to the Prince. The scheme was accordingly communicated to the Prince and Princess, and, though

Having been elected a Fellow of the Royal Society in 1687, he took an active part in its proceedings, and communicated papers to its *Transactions*. In the year 1699 he published a tract entitled a "Geometrical Investigation of the Solids of least Resistance," in which he made the following reference to the history of the new calculus.[1]

"The celebrated Leibnitz may perhaps inquire how I became acquainted with the calculus which I use. About the month

seconded by the latter, Monsieur Fagel and others had great difficulty in inducing the Prince to have the protection of a guard when he went abroad. In return for the services of Facio, it was resolved, on the strength of testimonials from Huygens, to create for him a professorship of mathematics for instructing the nobility and gentry of Holland, with a salary of 1200 florins, and a pension from the Prince.

Some delay having taken place in completing this arrangement, Facio got leave to pay a visit to England, where he arrived in 1687; but having been taken ill at Oxford, elected a Fellow of the Royal Society in 1687, and treated with much kindness by the English mathematicians, he remained till the accession of William III. When he visited Switzerland in 1699, 1700, and 1701, he learned that Count Fenil had received from the French Court a situation at Pignerol, a fortified city not far from Turin; and that in consequence of having conspired to surrender the place to the Duke of Savoy, he was condemned to be beheaded. In 1732, Facio endeavoured, but we believe unsuccessfully, to obtain, through the influence of Mr. Conduitt, some reward for having saved the life of the Prince of Orange. He assisted Conduitt in making out the design, and writing the inscription, for Newton's Monument in Westminster Abbey.

In 1704, when Facio taught mathematics in Spitalfields, he unfortunately became secretary to the Camisards, or fanatical prophets from the Cevennes, who pretended to raise the dead, and perform other miracles. Lord Shaftesbury attacked them in his *Letter on Enthusiasm*; and having been unjustly suspected of some political scheme, Facio and other two prophets were seized by the police in 1707, and condemned to the pillory. On the 2d of December 1707, Facio stood on the pillory at Charing Cross with the following inscription on his hat: "Nicolas Facio convicted for abetting Elias Moner in his wicked and counterfeit prophecies, and causing them to be printed and published to terrify the Queen's people." It is stated by Spence (*Observations, Anecdotes*, &c., 1820, p. 159), on the authority of Lockier, Dean of Peterborough, "that Sir Isaac Newton had a strong inclination to go and hear the French prophets, and was restrained from it with difficulty by some of his friends, who feared he might be infected by them as Facio had been." Facio spent the rest of his life at Worcester, where he died in 1753, nearly ninety years of age.—See *Phil. Trans.* 1713, and *Gentleman's Magazine*, 1737, 1738.

[1] Dr. Guhrauer, in his biography of Leibnitz, published in 1842, has most unjustly stated that Newton prompted this attack of Facio. We have carefully inspected all the manuscripts of Newton, and cannot discover the slightest evidence in support of a charge which deserves the severest reprobation.

of April, and the following months in the year 1687, and subsequent years, when nobody, as I thought, used such a calculus but myself, I invented its fundamental principles, and several of its rules. Nor would it have been less known to me if Leibnitz had never been born. He may, therefore, boast of other disciples, but certainly not of me. And this would be sufficiently evident if the letters which passed between me and the illustrious Huygens were given to the public.[1] Compelled by the evidence of facts, I hold Newton to have been *the first inventor* of the calculus, and the earliest by several years: And whether Leibnitz, *its second inventor*, has borrowed anything from him, I would prefer to my own judgment that of those who have seen the letters of Newton and copies of his other maunscripts. Nor will the silence of the more modest Newton, or the active exertions of Leibnitz in everywhere ascribing the invention of this calculus to himself, impose upon any person who shall examine these documents as I have done."[2]

Strong as these expressions are, they cannot be regarded as charging Leibnitz with plagiarism. He is styled *the second inventor*, the title with which he, on many occasions, expressed himself satisfied, and he is blamed only for everywhere ascrib-

[1] These letters do not appear in the Correspondence of Huygens with Leibnitz and the other distinguished men of the seventeenth century, lately published by Professor Uylenbroek. There are no letters dated between 1680 and 1690; but it appears from a letter to Leibnitz from Huygens, dated 18th November 1690, that he was acquainted with the calculus of Facio above referred to, and that it had been the subject of correspondence between these two celebrated mathematicians. Huygens tells Leibnitz that he had some share in the rule of Facio, and that it was Facio who first pointed out the mistake of Tschirnhaus. He adds that his method was a very beautiful one; and Uylenbroek, in a note on the subject, pointing at what Huygens had done in the matter, speaks of it as a fine invention. In a subsequent letter, dated 26th April 1690, Leibnitz pays a high compliment to Facio. "As Facio has much penetration," he says, "I expect from him fine things when he comes to details; and having profited by your instruction and that of Newton, he will not fail to produce works which will gain him distinction. I wish I were as fortunate as he is in being able to consult two such oracles." See *Christiani Huygenii, aliorumque seculi* xvii. *virorum celebrium. Exercit. Math. et Philos.* Fascic. i. p. 41, and Fascic. ii. pp. 56, 175. Hagæ Comitum, 1833.

[2] *Investigatio Geometrica*, &c., p. 18. Lond. 1699.

ing the invention to himself. In replying to Facio,[1] Leibnitz appealed to Newton himself as having stated, in the celebrated scholium, that the new calculus was common to them both, and that neither had received any light from the other;[2] and, without disputing or acknowledging the priority of Newton's claim, he asserted his own right to the discovery of the differential calculus. Facio sent a reply to the editors of the *Acta Eruditorum*, but they refused to print it on the ground of their aversion to controversy.[3] The controversy therefore terminated for the present, and the contending parties laid down their arms, ready to resume them on the slightest provocation.

When Newton published his Treatise on the Quadrature of Curves, along with his Optics, in 1704, he mentioned in his preface that he had gradually found the method of fluxions in the year 1665 and 1666. A review of this work, by Leibnitz,[4] but without his name, was published in the *Acta Eruditorum* for January 1705. After giving an imperfect analysis of its contents, he compared the method of fluxions with the differential calculus, and, in a sentence of some ambiguity, he states that Newton employed fluxions in place of the differences of Leibnitz, and made use of them in his Principia in the same manner as Honoratus Fabri, in his Synopsis of Geometry, had substituted progressive motion in place of the indivisibles of Cavalieri. As Fabri, therefore, was not the inventor of the method which is here referred to, but borrowed it from Cavalieri, and only changed the mode of its expression, there can be no doubt that the artful insinuation contained in the above passage was intended to convey the impression that Newton

[1] *Acta Eruditorum*, 1700, p. 203.

[2] We have already proved that Newton did not attach this meaning to his scholium; and, in replying to this passage in the *Recensio Commercii Epistolici*, he himself distinctly denies having "acknowledged that Leibnitz invented his method by his own genius, unassisted by the letters of Newton."—Newtoni *Opera*, tom. iv. p. 489.

[3] *Acta Eruditorum*, 1701, p. 134.

[4] Guhrauer, the biographer of Leibnitz, proves that he was the author of the review, and affirms that Leibnitz constantly denied any knowledge of the authorship.—See *Essays from the Edinburgh Review*, by Henry Rogers, pp. 226, 227.

had *stolen* his method of fluxions from Leibnitz. That this was the view of it taken by the friends of Newton will presently appear. That it was the view taken by Newton himself we are fortunately able to prove from the following passage in his own handwriting,[1] which is so important that we copy it without any other change than the use of Leibnitz's own words.

"In the *Acta Eruditorum* for 1705,[2] an account of the Introduction to the Book of Quadratures was published in these words:—'Quæ [Isagoge or Preface] ut MELIUS intelligatur, sciendum est, cum magnitudo aliqua continue crescit, veluti linea, (exempli gratia) crescit fluxu puncti, quod eam describit, incrementa illa momentanea [producta] appellavi DIFFERENTIAS, nempe inter magnitudinem quæ antea erat et quæ per mutationem momentaneam est producta; atque hinc natum esse calculum Differentialem, eique reciprocum summatorium,[3] cujus elementa ab INVENTORE Dn. Godefrido Guilelmo Leibnitio in his actis sunt tradita, variique usus tum ab ipso, tum a Dnn. Fratribus Bernoulliis, tum et Dn. Marchione Hospitalio sunt ostensi. Pro Differentiis IGITUR Leibnitianis Dn. Newtonus adhibet, semperque [pro iisdem] adhibuit fluxiones, iisque tum in suis Principiis Naturæ Mathematicis, tum in aliis postera editis [pro Differentiis Leibnitianis] eleganter est usus, QUEMADMODUM ut Honoratus Fabrius in sua synopsi Geometrica motuum progressus Cavallerianæ methodo SUBSTITUIT.' And all this is as much as to say that I did not invent the method of fluxions in the years 1665 and 1666, as I affirmed in this Introduction, but that after Mr. Leibnitz, in his letter of 21st June 1677, had sent me his differential method, instead of that method, I began to use, and have ever since used, the method of fluxions."[4]

[1] *A Supplement to the Remarks*, p 6.

[2] January, p. 34.

[3] This was the name given by Leibnitz to the integral calculus, or the inverse method of fluxions.

[4] The words within brackets are added by Newton, and bring out very distinctly the meaning of Leibnitz. In his letter to the Abbé Conti, dated 9th April 1716, Leibnitz virtually admits the authorship of the review, endeavours to give a different meaning

That Newton was virtually accused of plagiarism by the reviewer, cannot, we think, admit of a doubt. The indirect and ambiguous manner in which the charge is couched, and the artful reference to the case of Fabri and Cavalieri, make it doubly reprehensible; and we are persuaded that no candid reader can peruse the passage without a strong conviction that it justifies, in the fullest manner, the indignant feelings which it excited among the English philosophers. If Leibnitz, in place of being the author of the review, had been merely a party to it, he merited the full measure of rebuke which was dealt out to him by the friends of Newton, and deserved those severe reprisals which doubtless embittered the rest of his days. He who dares to accuse a man like Newton, or indeed any man holding a fair character in society, of the odious crime of plagiarism, places himself without the pale of the ordinary courtesies of life, and deserves to have the same charge thrown back upon himself. The man who conceives his fellow to be capable of such intellectual felony, avows the possibility of himself committing it, and almost substantiates the weakest evidence of the worst accusers.

Dr. Keill, as the representative of Newton's friends, could not brook this concealed attack upon his countryman. In a letter on the Laws of Centripetal Forces, addressed to Halley, and printed in the Philosophical Transactions for 1708,[1] he stated that Newton was "beyond all doubt" the first inventor of fluxions; and he asserted "that the same calculus was afterwards published by Leibnitz, the name and the mode of notation being changed." If the reader is disposed to consider this passage as retorting the charge of plagiarism upon Leibnitz, he will readily admit that the mode of its expression is neither so coarse nor so insidious as that which is used by

to the words *semperque adhibuit*, and maintained that Newton allowed himself to be deceived by a man who poisoned his words, and sought a quarrel by the malignant interpretation of them. Newton was himself the interpreter. See Raphson's *History of Fluxions*, p. 103.

[1] For September and October, p. 185.

the writer in the *Acta Eruditorum*. In a letter to Hans Sloane, dated 4th March 1711, Leibnitz complained to the Royal Society of the treatment he had received. "Nobody, says he, "knew better than Newton that this charge is false, for certainly I never heard of the name of the *Calculus of Fluxions*, nor saw with these eyes the characters which Newton used." He expressed his conviction that Keill had erred more from rashness of judgment than from any improper motive. He did not regard the accusation as a calumny; and he requested that the Society would desire Mr. Keill to disown publicly the injurious sense which his words might bear. When this letter was read to the Society, Keill justified himself to Sir Isaac Newton and the other members, by showing them the obnoxious article on the Quadrature of Curves in the *Acta Eruditorum*, and they all agreed in attaching the same injurious meaning to the passage in the review. The discussion excited so much interest, that, on the 5th April 1711, Newton gave, from the chair of the Society, "a short account of his invention, with the particular time of his first mentioning or discovering it;[1] upon which Mr. Keill was desired to draw up an account of the matter in dispute, and set it in a just light."[2] This account, contained in a letter to Sir Hans Sloane, was read at the Society on the 24th May 1711, and a copy of it was ordered to be sent to Leibnitz. In this letter, which is

[1] This account was probably given to the Society in consequence of the following unpublished letter from Keill to Newton, written two days before the meeting, that is on the 3d April 1711:—"I have now sent you the *Acta Lipsiæ* (1705), where there is an account given of your book (on Quadratures), and I desire you will read from page 34, &c. (namely, the passage which we have given from Newton's MS. in pages 3, 4). I hold not the volume (1710, p. 78) in which Wolfius has answered my letter, but I have sent you his letter transcribed from thence, and also a copy of my letter to him. I wish you would take the pains to read that part of their supplements, wherein they give an account of Dr. Friend's book, and from them you may gather how unfairly they deal with you; but really these things are trifles, not worth your while, since you can spend your time to much better purpose than minding anything such men can say. However, if you would look upon them so far as to let me hold your sentiments on that matter, you will much oblige, your most humble servant, Jo. Keill."

[2] Weld's *History of the Royal Society*, vol. i. p. 410.

one of considerable length, Dr. Keill declares that he never meant to state that Leibnitz knew either the name of Newton's method or the form of notation, and that the real meaning of the passage was, "that Newton was the first inventor of fluxions, or of the differential calculus, and that he had given, in two letters to Oldenburg, and transmitted to Leibnitz, indications of it sufficiently intelligible to an acute mind,[1] from which Leibnitz derived, or was able to derive, the principles of his calculus."

The charge of plagiarism which Leibnitz thought was implied in the former letter of his antagonist, is here greatly modified, if not altogether denied. Keill expresses an *opinion* that the letters *seen* by Leibnitz contained intelligible indications of the fluxionary calculus, from which he either derived, or might derive, the principles of his calculus. Even if this opinion were correct, it is no proof that Leibnitz either saw these indications, or availed himself of them; or if he did perceive them, it might have been in consequence of his having previously been in possession of the differential calculus, or having enjoyed some distant view of it. Leibnitz should, therefore, have allowed the dispute to terminate here; for no ingenuity on his part, and no additional facts, could affect an opinion which any other person as well as Keill was entitled to maintain.[2]

[1] "Indicia perspicacissimi ingenii viro satis obvia, unde Leibnitius principia illius calculi hausit aut haurire potuit."

[2] These sentiments, which we had formerly expressed, and which we again repeat, have been singularly misrepresented by Dr. Guhrauer in his *Life of Leibnitz*. A distinguished writer (Mr. Henry Rogers), in giving an account of this work, has defended us better than we could have done ourselves. "Dr. Guhrauer," he remarks, "is not a little indignant with Sir David Brewster for the supposed injustice which, in his *Life of Newton*, he has done to Leibnitz, and to which he frequently refers with much bitterness. Never was a complaint more unreasonable. Our distinguished countryman does not question Leibnitz's claim to be regarded as a true inventor of the calculus; he merely asserts the undoubted *priority* of Newton's discovery. He expressly affirms that there is no reason to believe Leibnitz a plagiarist; but that if there were any necessity for believing either to be so, it must be Leibnitz, and not Newton, who is open to the charge. Guhrauer angrily replies, not simply by saying (which is true) that there

Leibnitz, however, took a different view of the subject, and wrote a letter to Sir Hans Sloane, dated December 29, 1711, which excited new feelings, and involved him in new embarrassments. Insensible to the mitigation which had been kindly impressed upon the supposed charge against his honour, he alleges that Keill had attacked his candour and sincerity more openly than before; that he acted without any authority from Sir Isaac Newton, who was the party interested; and that it was in vain to justify his proceedings by referring to the provocation in the *Acta Eruditorum*, because, in that journal, *no injustice had been done to any party, but every one had received what was his due.* He asserts that he discovered the calculus *some* years before he published it, that is in 1675, or earlier. He brands Keill with the odious appellation of an upstart, and one little acquainted with the circumstances of the case;[1] and he calls upon the Society to silence his vain and unjust clamours,[2] which, he believed, were disapproved by Newton himself, who was well acquainted with the facts, and who, he was persuaded, would willingly give his opinion on the matter.

is no sufficient evidence of Leibnitz's having stolen Newton's invention, but by denying the essential identity of the two methods, and by affirming that they are so different as to be considered 'unlike things,' than which nothing can, in our judgment, be more uncandid.

"There is only one statement which, as respects Leibnitz, Dr. Gubrauer could fairly find fault with in Sir David Brewster's work; and that is, that Keill had a 'right to express his opinion' that the letters of Newton of 1676 gave indications from which Leibnitz 'derived, or might derive,' the principles of his calculus. For reasons already assigned, we do not think that any man *had a right to say this*, nor that any one could say it without being *of a different opinion from Newton himself*, who undoubtedly must have thought that he had not disclosed what he designed to conceal With no other statement of Sir David Brewster, as regards Leibnitz, are we disposed to find fault."—*Essays from the Edinburgh Review*, by Henry Rogers, vol. i. pp. 227, 228. *Edin. Review*, vol. lxxxiv. pp. 43, 44. Mr. Rogers has certainly misapprehended the meaning of our statement, which amounts to nothing more than that Dr. Keill, or any other man, had a right to express his opinion on any subject whatever, whether they are sound or unsound. We have already proved that the opinion of Keill was the opinion of Newton himself, and, as he knew this, he had a right of a higher kind to express the same opinion.

[1] Homo doctus, sed novus, et parum peritus rerum anteactarum cognitor.

[2] Vanæ et injustæ vociferationes.

This unfortunate letter was doubtless the cause of all the rancour and controversy which so speedily followed, and it placed his antagonist in a new and a more favourable position. It may be correct, though few will admit it, that Keill's second letter was more injurious than the first; but it was not true that Keill acted without the authority of Newton, because Keill's letter was approved of, and transmitted, by the Royal Society, of which Newton was the president, and therefore became the act of that body. The obnoxious part, however, of Leibnitz's letter, consisted in his appropriating to himself the opinions of the reviewer in the Leipsic Acts, by declaring that, in a review which charged Newton with plagiarism, every person had got what was their due. The whole character of the controversy was now changed: Leibnitz places himself in the position of the party who had first disturbed the tranquillity of science by maligning its most distinguished ornament; and the Royal Society was imperiously called upon to throw all the light they could upon a transaction which had exposed their venerable president to so false a charge. The Society, too, had become a party to the question, by their approbation and transmission of Keill's second letter, and were on that account alone bound to vindicate the step which they had taken.

When the letter of Leibnitz, therefore, was read, Keill appealed to the registers of the Society for the proofs of what he had advanced. Sir Isaac also expressed his displeasure at the obnoxious passage in the *Acta Eruditorum*, and at the defence of it by Leibnitz, and he left it to the Society to act as they thought proper.

In this emergency, a committee of the Royal Society was appointed on the 6th March 1712, "to inspect the letters and papers relating to the dispute, consisting of Dr. Arbuthnot, Mr. Hill, Dr. Halley, Mr. Jones, Mr. Machin, and Mr. Burnet." Mr. Robarts, a contributor to the Transactions, was added to the committee on the 20th of March, M. Bonet, the Prussian Minister, on the 27th, and Mr. De Moivre, Mr. Aston, and

Dr. Brook Taylor, on the 17th of April.[1] The committee, thus constituted, was instructed to examine the registers of the Society, and to lay before it such documents as they might discover, with their own opinions on the subject. This committee, probably from being called *consessus arbitrorum*, has been supposed to have been a judicial committee; but, as Professor De Morgan has shown, and as Newton himself has asserted, it had no such character, since none of Leibnitz's friends were placed upon it, and no invitation given him to produce documents in his defence. The committee consisted entirely of Newton's friends; and several of them, though qualified to attest the genuineness of the documents in the report, were not fitted, by their mathematical acquirements, to give an opinion on the subject.[2]

On the 24th of April the committee gave in the following report, which was in the handwriting of Halley:—

"We have consulted the letters and letter-books in the custody of the Royal Society, and those found among the papers of Mr. John Collins, dated between the years 1669 and 1677 inclusive; and showed them to such as knew and avouched the hands of Mr. Barrow, Mr. Collins, Mr. Oldenburg, and Mr. Leibnitz; and compared those of Mr. Gregory with one another,

[1] The additions thus made at different times to the original committee, were first pointed out by Professor De Morgan, and were unknown to all preceding writers. The discovery was a very important one, as it had been asserted by Newton that the committee was a numerous one, consisting of persons of different nations, which was certainly not the character of the original committee. As Professor De Morgan has been led, after an anxious examination of the subject, "to differ from the general opinion in England as to the manner in which Leibnitz was treated," his defence of Newton's veracity was a graceful contribution, and cannot fail to give weight to his other opinions.—See his paper in the *Philosophical Transactions*, vol. xlvi. pp. 107-109.

[2] "There may have been," says Professor De Morgan, "and I often suspect there was, something of truth in the surmise of Leibnitz, who thought that the near prospect of the Hanoverian succession created some dislike against the subject and servant of the obnoxious Elector on the minds of the Jacobite portion of English science." "Amicus Anglus ad me scribit," says Leibnitz, "videri [eos qui parum Domui Hanoveranæ favent] aliquibus non tam et Mathematicos et Societatis Regiæ Socios in Socium, sed ut Toryos in Whigium quosdam egesse."—*Philosophical Transactions*, 1846, p. 108. Newton himself was a *Whig*, and a friend of the House of Hanover.

and with copies of some of them taken in the hand of Mr. Collins; and have extracted from them what relates to the matter referred to us; all which extracts herewith delivered to you, we believe to be genuine and authentic; and by these letters and papers we find,—

"I. That Mr. Leibnitz was in London in the beginning of the year 1673; and went thence, in or about March, to Paris; where he kept a correspondence with Mr. Collins, by means of Mr. Oldenburg, till about September 1676, and then returned by London and Amsterdam to Hanover: and that Mr. Collins was very free in communicating to able mathematicians, what he had received from Mr. Newton and Mr. Gregory.

"II. That when Mr. Leibnitz was the first time in London, he contended for the invention of another differential method, properly so called, and notwithstanding that he was shown by Dr. Pell, that it was Mouton's method, he persisted in maintaining it to be his own invention, by reason that he had found it by himself, without knowing what Mouton had done before, and had much improved it. And we find no mention of his having any other differential method than Mouton's, before his letter of 21st June 1677, which was a year after a copy of Mr. Newton's letter, of the 10th December 1672, had been sent to Paris to be communicated to him; and above four years after, Mr. Collins began to communicate that letter to his correspondents; in which letter the method of fluxions was sufficiently described to any intelligent person.

"III. That by Mr. Newton's letter of the 13th June 1676, it appears that he had the method of Fluxions above five years before the writing of that letter, and by his Analysis, *per Æquationes numero Terminorum Infinitas*, communicated by Dr. Barrow to Mr. Collins in July 1669, we find that he had invented the method before that time.

"IV. That the differential method is one and the same with the method of fluxions, excepting the name and the mode of notation; Mr. Leibnitz calling those quantities differences, which

Mr. Newton calls moments or fluxions; and marking them with the letter *d*, a mark not used by Mr. Newton. And therefore we take the proper question to be, not who invented this or that method, but who was the first inventor of the method; and we believe, that those who have reputed Mr. Leibnitz the first inventor, knew little or nothing of his correspondence with Mr. Collins and Mr. Oldenburg long before; nor of Mr. Newton's having that method above fifteen years before Mr. Leibnitz began to publish it in the *Acta Eruditorum* of Leipsic.

"For which reasons we reckon Mr. Newton the first inventor; and are of opinion that Mr. Keill, in asserting the same, has been noways injurious to Mr. Leibnitz. And we submit to the judgment of the Society, whether the extracts and letters and papers now presented, together with what is extant to the same purpose, in Dr. Wallis's third volume, may not deserve to be made public."

This report being read and agreed to, the Society unanimously adopted it, ordered the collection of letters and manuscripts to be printed, and appointed Dr. Halley, Mr. Jones, and Mr. Machin, to superintend the press. Complete copies of it, under the title of *Commercium Epistolicum D. Johannis Collins et aliorum de analysi promota*, were laid before the Society on the 8th January 1713; and Sir Isaac Newton, as president, ordered a copy to be delivered to each person of the Committee appointed for that purpose, to examine it before its publication.[1]

According to Leibnitz, he received information of the appearance of the *Commercium Epistolicum* when he was at Vienna, and "being satisfied, as he expresses it, that it must contain *malicious falsehoods*, I did not think proper to send for it by post, but wrote to M. Bernoulli to give me his sentiments.[2]

[1] This work was not published for sale, and as the few copies of it which were printed were chiefly distributed as presents, it became so scarce that Raphson tells us "it was not to be met with among the booksellers." Twenty-five copies were disposed of to a bookseller at the Hague. See Edleston's *Correspondence*, Notes, p. lxxiii.

[2] Newton states that a copy of the *Commercium* was sent to Leibnitz by the Resident

M. Bernoulli wrote me a letter, dated at Basle, June 7, 1713, in which he said, *that it appeared probable that Sir Isaac Newton had formed his calculus after having seen mine.*"[1] This letter was published in Latin, by Leibnitz, with reflections, in a loose sheet, entitled *Charta Volans*, dated July 29, 1713, and was widely circulated, without either the name of the author, printer, or place of publication, and giving the names of N—n and L—z, with their initial and final letters.

The origin of this letter is curious and instructive. In writing to Leibnitz on the 28th February 1713, Bernoulli says, that he has informed Newton of some of his mistakes,[2] but in a very gentle manner, that he might not give offence to one who had been very kind to him in getting him elected a Fellow of the Royal Society, and as showing much attention to his son when in London. In Leibnitz's reply of the 16th March, he remarks that Newton wishes to ingratiate himself with him, and he adds, we shall see what can be elicited from the correspondence with Collins, which, owing to his absence from home, he may not see so early as he will. Bernoulli had now received from Paris a copy of the *Commercium Epistolicum*, and in replying to Leibnitz on the 7th of June he gives him a general account of the Report of the Committee, and adds in a couple of pages his own opinion of it, which constitutes the celebrated letter of the 7th June 1713, inserted by Leibnitz in the *Charta Volans.* He concludes the letter by imploring Leibnitz " to make a right use of what he has written, and not compromise him with Newton and his countrymen, as he was unwilling to be mixed up with these controversies."[3] In spite of this request, Leibnitz not only gave up Bernoulli as the author of the letter, but

of the Elector of Hanover, above a year before this, and several copies to Leipsic, one of which was for him. MS.

[1] Letters to the Count de Bothmar in Des Maizeaux's *Recueil de Diverses Pièces*, &c. tom. ii. p. 44.

[2] See *Acta Eruditorum*, 1713, p. 77, and Mart., p. 155.

[3] *Commerc. Phil. et Math. G. G. Leibnitii et J. Bernoullii*, tom. ii. pp. 308, 311.

had insidiously inserted in a parenthesis, and in the same type, as if it had been written by the author, the words, *as was long ago remarked by a certain eminent mathematician*, which placed Bernoulli in the ridiculous position of praising himself.

Previous to the publication of the *Charta Volans*, Dr. Keill sent to the *Journal Littéraire* for 1713,[1] some remarks on the controversy, with the Report of the Committee, and Newton's important letter to Collins, dated 10th December 1672. An anonymous answer,[2] but certainly written by Leibnitz, appeared in the same work for November and December 1713. It contained a French translation of the *Charta Volans*, and of the letter *of a very eminent mathematician*, dated 7th June 1713, on the subject of the controversy, the same letter which Leibnitz mentions to Count Bothmar, as the production of Bernoulli.[3] In this letter Bernoulli asserts that Newton in his researches confesses that he never even thought of Fluxions, and had not invented them before the differential calculus. He maintains that he was ignorant, when he wrote the Principia, of the true way of taking the fluxions of fluxions, and he accused him of having deprived Hooke and Flamsteed of their just honours, the one for his hypothesis of the planets, and the other for the use of his observations.

Newton was indignant at this new attack upon his character, which was sent to him in the autumn of 1713, by Mr. Chamberlayne, who then kept a correspondence with Leibnitz, and he immediately drew up a sharp reply,[4] which was probably

1 For May and June, pp. 208-217.

2 *Remarques sur le Différent entre M. de Leibnitz et M. Newton*, November and December 1713, pp. 445-453.

3 This letter, in the Latin edition of it in the *Charta Volans*, referred, as we have stated, to Bernoulli, in the sentence *quemadmodum ab eminente quodam mathematico dudum notatus est*. The reference was continued in the French edition; but in another edition of the *Charta Volans*, which Leibnitz published two years afterwards in the *Nouvelles Littéraires*, December 28, 1715, p. 414, he omitted the above passage, as if to fix the authorship on Bernoulli; and in a letter to Madame Kilmansegg, dated April 1716, he inserted a copy of the obnoxious letter, without the passage referred to, and without any hesitation ascribed it to Bernoulli.

4 There are several copies of this paper among Newton's manuscripts.

sent to Keill, as the groundwork of his long and elaborate answer, which appeared in the *Journal Littéraire* for July and August 1714.[1] Bernoulli was supposed by both to be the very eminent mathematician[2] who wrote the letter of the 7th June 1713, and but for Leibnitz's indiscretion, his name would never have been known. Never doubting that Bernoulli was the author, Keill endeavoured to prove it, and exposed with great severity the incorrectness and injustice of his charges against Newton. Notwithstanding the repeated declarations of Leibnitz, that Bernoulli was the author of this letter, Bernoulli himself disavowed it to M. Des Maizeaux, to M. Montmort, and to the Abbé Varignon; and in a letter to Newton, dated 10th July 1719, he declared that he was not the author of it, and that too with such solemnity that Newton believed him, and would not listen to Keill and his other friends when they expressed an opposite opinion. "I beseech you," says he,[3] and I adjure you, by all

[1] This paper, occupying forty-two pages, was drawn up with great care with the assistance of Sir Isaac, four of whose letters to Keill on the subject, dated April 2, 20, May 11, 15, 1714, have been published by Mr. Edleston. I have now before me the originals of six letters from Keill to Newton, dated May 2, 17, 19, 21, and June 29, 1714. In Newton's letter of April 2, he says that Keill "need not set his name to it." In Keill's reply of the 2d May, sending a part of his answer, he says, that "he never saw a bad cause defended with so much face and impudence before." He is to take Leibnitz "to task for filching of series," and he is "for putting his name to it;" for he adds, "I have said nothing but what is fully made out, and they have, on the contrary, thrown all the dirt and scandal they could without proving anything they have said, and therefore they thought it best to conceal their names. I believe Wolfius is the author of the Latin letter, for it is exactly agreeable to his caution and honesty, who is inferior to nobody but Mr. Leibnitz in prevarication. Dr. Halley and I do often drink your health. He and I are both of opinion that there should be fifty copies of the *Commercium* sent over to Johnson (the publisher of the *Journal Littéraire*, to whom they were subsequently sent), and that there should be advertisements in the foreign Gazettes, that the original letters of the Commercium are in such a man's hands, to be viewed by gentlemen that are to travel in England, and particularly the letter with Gregory's quadrature of the circle." In his letters of the 25th and 29th June, he sends "the whole of his answer to Bernoulli and the Leipsic rogues, for you and Dr. Halley to change or take away what you please."

[2] Leibnitz had not at this time written the letter to Bothmar or Madame Kilmansegg, declaring that Bernoulli was the author of it.

[3] "Fallunt haud dubie qui me tibi detulerunt tanquam auctorem quarundam ex

that is sacred, that you will firmly believe that anything published without a name, in which a sufficiently honourable mention of you has not been made, has been falsely imputed to me. . . . Far be it from me to believe that Leibnitz, that truly excellent man, wished to deceive you by mentioning me. It is more credible that he was deceived either by his own conjecture or that of others, and yet he was not altogether blameless, in so far as he rashly and imprudently committed to writing anything of which he had no knowledge." The dishonesty of Bernoulli, thus placed beyond a doubt, is equalled only by the dishonourable conduct of Leibnitz in betraying his friend.[1] Anxious to obtain the opinion of a great mathematician in favour of his own claims, and against those of Newton, he asked Bernoulli, as we have seen, to do him this favour. This request of his patron and friend was readily granted, but under the obligation that his name should be concealed. Leibnitz, however, was not satisfied with this anonymous tribute to his genius, and did not scruple to obtain for it all its value by violating his word, and exposing his friend to the enmity of Newton, and the keen shafts of Keill, of which we shall presently see he stood in great alarm. During the interval between the date of Bernoulli's letter, namely, the 7th June, and that of the *Charta Volans*, in which Leibnitz published it, namely, the 29th July 1713, he seems to have felt how little was the value of the anonymous testimony which he had received; he therefore writes to Bernoulli on the 28th June, "that he expects from his justice and candour that he will, as

Schedis istis volantibus, in quibus forsan non satis honorifica tui fit mentio. Sed obsecro te, vir inclyte, atque per omnia humanitatis sacra obtestor ut tibi certo persuadeas, quicquid hoc modo sine nomine in lucem prodierit, id mihi falso imputari. . . . Absit autem ut credam Leibnitium, virum sane optimum me nominando fucum vobis facere voluisse. Credibile namque potius est ipsum vel sua vel aliorum conjectura fuisse deceptum. . . . Non tamen omni culpa vacabit quod tam temere et imprudenter aliquid proscripserit cujus nullam habebat notitiam."—See APPENDIX, No. XX.

[1] The late John Bernoulli, speaking of the conduct of Leibnitz to his grandfather, says, *Il commit l'indiscrétion de le trahir.—Mém. Acad. Berlin*, 1799, 1800. *Hist.* p. 41.

soon as possible, declare *publicly* among his friends, when the opportunity occurs, that the *Calculus of Newton was posterior to his*."[1] In replying to this letter, Bernoulli assures him that he will conceal nothing either among his friends or publicly, when the occasion demands it, and he comforts Leibnitz by saying that his fate was like that of his prince, the Elector of Hanover, whom the villanous English wished to deprive of the succession to the kingdom, in the same manner as they wished to deprive him of the possession of his calculus. Leibnitz, however, was very uneasy on the subject. He was anxious to know what the Parisians thought, for though he had no doubt that Varignon would be his friend, he feared that others would take the opportunity of attacking him.[2] He expresses the hope, however,[3] in a letter containing some severe strictures on Newton, that Varignon would take care, Bernoulli prompting him, that nothing was done in France of which he might complain.

This extreme sensitiveness, on the part of Leibnitz, we can readily excuse, but we can find no apology for his conduct in betraying so ardent a friend as Bernoulli. On a future occasion we shall find him prompting the German mathematician to another act of hostility against Newton and Keill, and a second time divulging the secret under which the favour was granted. And at the very close of his career, when his great powers had been appreciated by the world, and an immortality of reputation was dazzling his failing sight, he did not scruple to conspire with Wolf, another German mathematician of feeble morality, to vitiate a letter of Bernoulli, and leave a shadow

[1] The passage is curious, and it is obvious that the editor has omitted a part of the letter unfit for the public eye. "Satis apparet Newtonum id egisse suis blanditiis, ut benevolentiam tuam captaret; conscium sibi quam non recto stent talo quæ molitus est. Ego tamen etsi nolim, ut in mei gratiam tibi negotium facessas, *expecto tamen ab equitate tua et candore, ut profitearis apud amicos quam primum, et publice data occasione, calculum Newtoni nostro posteriorem tibi videri*." . . .—*Commercium Phil. et Math. G. G. Leibnitii et J. Bernoullii*, tom. ii. pp. 313, 314.

[2] *Ibid. Ibid.* tom. ii. p. 314.

[3] *Ibid. Ibid.* pp. 320, 321.

upon his name which the lustre of his genius will never be able to efface.

Amid the feelings excited by the letter of *the eminent mathematician*, Mr. Chamberlayne, whom we have already mentioned as the correspondent of Leibnitz, conceived the design of reconciling the two distinguished philosophers; and, in a letter, dated April 28th, 1714,[1] he addressed himself to Leibnitz, who was still at Vienna. In replying to this letter, Leibnitz declared that he had given no occasion for the dispute; "that Newton procured a book to be published, which was written purposely to discredit him, and sent it to Germany, France, and Italy, as in the name of the Society;" and he stated "*that there was great room to doubt whether Newton knew his invention before he had it from him.*" Mr. Chamberlayne communicated this letter to Sir Isaac Newton, who replied, that Leibnitz had attacked his reputation in 1705, by intimating that he had borrowed from him the method of fluxions; that if Mr. C. could point out to him anything in which he had injured Mr. Leibnitz, he would endeavour to give him satisfaction; that he would not retract things which he knew to be true; and that he believed that the committee of the Royal Society had done no injustice by the publication of the *Commercium Epistolicum.* In another letter, Leibnitz expressed his entire disapprobation of the report of the committee, and of the *Commercium*, declaring at the same time, more than a year and a half after two copies had been sent to him, that *he had not yet seen the book published against him*, and requesting Mr. Chamberlayne to submit his letter to the Society.

When the letter was laid before a meeting of the Society on the 20th of May 1714, they came to the following resolution:—

"It was not judged proper (since this letter was not directed to them) for the Society to concern themselves therewith, nor were they desired so to do. But if any person had any mate-

[1] See Des Maizeaux, tom. ii. p. 116.

rial objection against the *Commercium*, or the report of the committee, it might be re-considered at any time."

This resolution was sent to Leibnitz, who, in a letter to Chamberlayne, dated 25th August 1714, justly observes that the Society "did not pretend that the report of the committee should pass for a decision of the Society."[1] Along with the resolution, Mr. Chamberlayne sent to Leibnitz Sir Isaac's letter and Dr. Keill's answer to the papers inserted in the *Journal Littéraire*, and, after perusing them, he replied, "that Sir Isaac's letter was written with very little civility; that he considered it *non scripta*, as well as the piece printed in French (by Dr. Keill); that he was not in a humour to put himself in a passion against such people; that there were other letters among those of Oldenburg and Collins which should have been published; and that on his return to Hanover he would be able to publish a Commercium Epistolicum, which would be of service to the history of learning." When this letter was read to the Royal Society, Sir Isaac remarked that the last part of it injuriously accused the Society of having made a partial selection of papers for the *Commercium Epistolicum;* that he

[1] Mr. Weld, in his History of the Royal Society, vol. i. p. 415, and *Phil. Mag.* July 1847, p. 35, states that Professor De Morgan and I have committed a curious and grave mistake in adopting this opinion of Leibnitz; and that it was at the request of some of our most eminent philosophers that he corrected the mistake by publishing the resolution of the Society, as, if our views of the resolution were adopted, "a strong case would be made out against Newton." The Society never adopted the Report, in the sense of adopting, as a body, the opinion of their committee. They simply agreed to *receive* it, and ordered it to be printed. *His autem die Aprilis* 24, 1712, *acceptis. Societas Regia Collectionem, &c. &c., imprimi jussit.* The cause of Newton was not affected by the adoption of the Report as their decision, and the resolution to re-consider it can mean nothing more than to express their willingness, which Newton himself often did, to receive any new information from Leibnitz or his friends, and even to publish it in the Transactions. That Newton himself was of the opinion which we have been maintaining, is proved by a passage in his *Remarks* on Leibnitz's letter to Conti, where he says, in the month of May 1716, "If they (the Royal Society) *have not yet given judgment against him,* it is because the committee did not act as a jury, nor the Royal Society as a formal court of justice." . . . "And *it is sufficient* that the Society ordered their report, with the papers upon which it is grounded, to be published."—Raphson's *Fluxions*, p. 112.

did not interfere in any way in the publication of that work, and had even withheld from the committee two letters, one from Leibnitz in 1693, and another from Wallis in 1695, which were highly favourable to his cause.[1] He stated that he did not think it right for Mr. Leibnitz himself to publish a Commercium Epistolicum, but if he had letters to produce in his favour, that they might be published in the *Philosophical Transactions*, or in Germany.

About this time the Abbé Conti, a noble Venetian, came to England. He was a correspondent of Leibnitz, and in the postscript of a letter which he had received from him soon after his arrival,[2] and written in November or December 1715, he enters upon his dispute with Newton. He charges the English with "wishing to pass for almost the only inventors." He declares "that Bernoulli has judged rightly in saying, that Newton did not possess before him the infinitesimal characteristic and algorithm." He remarks that Newton preceded him only in series; and he confesses that during his second visit to England, "Collins showed him part of his correspondence," or, as he afterwards expresses it, he saw "some of the letters of Newton at Mr. Collins's." He represents the metaphysics of the English as narrow (bornée), and their mathematics as common or superficial. He then attacks Sir Isaac's philosophy, particularly his opinions about gravity and a vacuum, the intervention of God for the preservation of his creatures; and he accuses him of reviving the occult qualities of the schools. But the most remarkable passage in the letter is the following: "I am a great friend of experimental philosophy, but Newton deviates much from it *when he pretends that all matter is heavy*, or that each particle of matter attracts every other particle." The letter concludes with a problem, which he requests Conti to propose, "in order to feel the pulse of the English analysts."

[1] Published in Raphson's *History of Fluxions*, pp. 119, 121, and in the *Additamenta Com. Epist.*, Newtoni *Opera*, tom. iv. pp. 614, 615.

[2] It is published in Raphson's *History of Fluxions*, p. 97.

Under these cirumstances, and influenced by the advice of Keill, which we have already mentioned, Sir Isaac became anxious that foreigners of distinction should see the original papers which had been preserved in the archives of the Society, and compare them with the other letters of Leibnitz. He therefore requested the Abbé Conti to assemble the ambassadors and other foreign ministers for this purpose, and when they had met in the apartment of the Society and collated the papers, the Baron de Kilmansegg, the Hanoverian minister, remarked that this measure was not a sufficient one, and that the right way of terminating the dispute was that Newton himself should write a letter to Leibnitz, stating to him "his reasons," and demanding a direct answer to them. All the ministers who were present approved of this suggestion, and the king, to whom it was mentioned in the evening, gave it his hearty approbation.

Conti reported these proceedings to Sir Isaac, and in five or six days he received a letter from him, dated February 26, 1715-16, to be sent to Leibnitz, who was then in Hanover. As this letter was addressed to Conti, he enclosed it in one of his own, dated — March 1716, which he had previously read to Newton. Mr. Demoivre had corrected it, and added the part which related to the equivocal manner in which Leibnitz had proposed the problem for the English analysts. The letter of Conti, with Newton's enclosed, which was to be taken to Hanover by the Baron de Discau, remained more than a month in London. Madame de Kilmansegg had it translated into French. The king read it, and approved of it so highly as to say, that the reasons were very simple and clear, and that it would be difficult to reply to the facts.[1]

This letter of Newton's was the first occasion on which he appeared in the controversy in his own person. Reluctantly driven into the field, he did not hesitate to give utterance to

[1] These facts are stated in a very interesting letter from Conti to Brook Taylor, dated May 21, 1721. It was published in the *Memoirs* of Brook Taylor, p. 121, and is of such importance that we have given it in Appendix, No. I.

the opinions which had been maintained by Keill. In a tone of dignified severity he gave a brief notice of the controversy, and triumphantly refuted the allegations of his adversary. "Finding it impossible," he says, "to reply to matter of fact, Leibnitz invoked the opinion of a mathematician or pretended mathematicians, dated 7th June 1713, and inserted it in an anonymous defamatory letter of the 29th July, which he circulated in Germany,"—a letter which had been answered by Keill, and to which no reply had been returned. He charges Leibnitz with trying to engage him in philosophical disputes, and challenging him to the solution of problems which have no relation whatever to the question in dispute; and he makes some severe observations on Leibnitz's doctrine of the *Pre-established Harmony*, which he pronounces a true miracle, and contrary to all experience. He cites a passage from Leibnitz's letter to himself, dated March 7, 1693, in which he acknowledges the value of Newton's discoveries; and he makes the following observations on that branch of the dispute which relates to Leibnitz's having seen part of Newton's letters to Mr. Collins. "He (Leibnitz) complains of the committee of the Royal Society, as if they had acted partially in omitting what made against me; but he fails in proving the accusation. He quotes a passage concerning my ignorance, pretending that it was omitted in the *Commercium Epistolicum*, and yet you will find it there in p. 74, lines 10, 11, and I am not ashamed of avowing it. He says that he saw this paragraph in the hands of Mr. Collins when he was in London the second time, that is in October 1676; and as this is in my letter of the 24th of October 1676, he therefore then saw that letter. And in that and some other letters writ before that time, I described my method of fluxions; and in the same letter I described also two general methods of series, one of which is now claimed from me by Mr. Leibnitz." The letter concludes with the following paragraph: "But as he has lately attacked me with an accusation which amounts to plagiary; if he goes on to accuse me, it lies upon him by the laws of all nations to prove his accusations, on pain of being accounted guilty of calumny. He hath hitherto written letters to his correspondents full of affirmations, complaints, and reflections, without proving anything. But he is the aggressor, and it lies upon him to prove the charge."

In transmitting this letter to Leibnitz, the Abbé Conti informed him that he himself had read with great attention, and without the least prejudice, the *Commercium Epistolicum*, and the little piece[1] that contains the extract; that he had also seen at the Royal Society the original papers of the Commercium Epistolicum, and some other original pieces relating to it. "From all this," says he, "I infer, that, if all the digressions are cut off, the only point is, whether Sir Isaac Newton had the method of fluxions or infinitesimals before you, or whether you had it before him. You published it first, it is true, but you have owned also that Sir Isaac Newton had given many hints of it in his letters to Mr. Oldenburg and others. This is proved very largely in the Commercium, and in the extract of it. What answer do you give? This is still wanting to the public, in order to form an exact judgment of the affair." The Abbé adds, that Mr. Leibnitz's own friends waited for his answer with great impatience, and that they thought he could not dispense with answering, if not Dr. Keill, at least Sir Isaac Newton himself, who had given him a defiance in express terms. In the close of his letter he informs Leibnitz that several geometers in London and Oxford have solved his problem, and he tells him that he will take some other opportunity of speaking to him of Newton's philosophy, which has been greatly misapprehended.

[1] This is the *Recensio Commercii Epistolici*, or an abstract or review of it. It occupies forty-one quarto pages, and has a preface *Ad Lectorem*. It was written by Sir Isaac Newton, a fact which Professor De Morgan had deduced from a variety of evidence. It was first published in the *Phil. Trans.* 1715, in English, was reprinted in Newtoni *Opera*, tom. iv. p. 445, and in the *Journal Littéraire*, tom. vii. pp. 113, 345. See *Phil. Mag.* June 1852.

Leibnitz was not long in replying to the request of the Abbé Conti, and the defiance of Newton. He addressed a letter to the former on the 9th of April 1716, but he sent it through M. Remond de Montmort, to be communicated to the mathematicians[1] in Paris, as neutral and intelligent witnesses, and then transmitted to Conti. In the letter to the Abbé, which was dated 14th April, he tells him that he may judge from all this, if "the wicked chicanery of his new friends has greatly embarrassed him," and he closes his letter with a reference to the problem, "for feeling the pulse of the English analysts," which he tells him was proposed by Bernoulli.[2]

The letter of Leibnitz of the 9th April is bold and ingenious. He defends the statements in the anonymous attacks upon Newton as if they were his own. He gives an account of his two visits to London, and mentions what he there saw and learned. He charges Newton with retracting his admission in the scho-

[1] A few days after this letter was written, April 13, Leibnitz wrote to Bernoulli that the "English dispute was renewed, and that Newton, *when he saw that Keill was reckoned unworthy of an answer*, had descended into the arena." He tells him "that Newton knows that the letter (of June 7, 1713) was his, and that he had described it 'as written by a mathematician, or a *pretended mathematician*,' as if he were ignorant of your merits, calling *the whole Chart* (*the Charta Volans*) *defamatory*, as if it were more calumnious than the additions to the *Commercium Epistolicum*." In replying to this letter, on the 20th May 1716, Bernoulli considers it fortunate that Newton has descended into the arena to fight in his own name, and without a mask. He expresses much confidence in his candour, and hopes that the historical truth will now be elicited. The most curious part of the letter, however, is the following passage: "*I wonder how Newton could know that I was the author of that letter which you inserted in the Charta published against Newton, since no mortal knew that I wrote it except yourself to whom it was written, and I, by whom it was written.*" He then refers to Leibnitz's interpretation of the phrase *pretended mathematician*, as if it accused him of ignorance, and he shows very satisfactorily that it bore another meaning (the real meaning of Newton as avowed in his remarks on Leibnitz's letter), in no way derogatory from his mathematical knowledge. In Leibnitz's next letter of the 7th June, he makes no reference to Bernoulli's expression of wonder, and has not the honesty to tell him that he had himself communicated the secret to Count Bothmar, and published it. See the *Commercium Epistolicum Phil. et. Math. Leibnitii et Bernoullii*, tom. ii. pp. 375, 377, 378.

[2] Some time after this M. Remond de Montmort seems to have remonstrated with John Bernoulli, on the subject of defying the English analysts to the solution of problems. We do not know where this letter is to be seen, but we have found among New-

lium, and thus considers himself entitled to retract his admission in favour of Newton. He introduces again his metaphysical opinions as having been misrepresented by Newton, and he concludes by denying that he was the aggressor, and had accused Newton of plagiarism.

On the very day when Leibnitz was writing this letter, Bernoulli was engaged in composing his famous *Epistola pro Eminente Mathematico*, which has formed so curious and instructive an episode in the fluxionary controversy. He had been stung by the poignancy of Keill's reply to the *Charta Volans*, and the severity of its animadversions on the letter of his own which it contained; and, as will be seen from his own acknowledgment, he was *afraid* to encounter without a mask so bold and uncompromising an antagonist. He therefore resolved to attack Keill in an anonymous letter, addressed to Christian Wolf, one of the editors of the *Acta Eruditorum*. This letter, dated April 8, 1716,[1] which Bernoulli, the grandson, admits was particularly

ton's papers Bernoulli's reply to it written after the death of Leibnitz, and dated 8th April 1717. In this reply, which he requested Remond to send to Newton, he protests that he had neither the inclination nor the leisure to enter into disputes with the English, or to defy them. It was Leibnitz, he says, who asked him for some problem which could be proposed to the English, and particularly to Keill, and of such a nature that it required a knowledge of their methods to solve. Leibnitz asked him to keep this a secret, in order that it might some day be of use to them against those who wished to defy them. "I imagined," he says, "a problem which seemed to have the qualities he desired, and I sent him two solutions that he might propose it to the English under his own name. I had reason, therefore, to be astonished when I saw that he had given me up as the author, and proposed the problem in spite of me, and even as if it had been done at my instigation. Have the goodness then to disabuse Mr. Newton of his opinion on this matter, and assure him from me that I never had the intention of trying the English by these sort of defiances, and I desire nothing so much as to live in friendship with him, and to find an opportunity of showing him how much I esteem his rare merit. I never speak of him, indeed, without much praise. It is, however, greatly to be desired that he would take the trouble of inspiring his friend Mr. Keill with sentiments of kindness and equity towards foreigners, and leave such in possession of what really belongs to them. For to desire to exclude us from every pretension would be a crying injustice. —See APPENDIX No. II.

[1] It was entitled *Epistola pro Eminente Mathematico, Do. Joanne Bernoullio contra quendam ex Anglia antagonistam scripta*, and was published in the *Acta Eruditorum* for July 1716, pp. 296-315.

directed against Newton, was sent to Wolf on condition of the most inviolable secrecy. It was to be first communicated to Leibnitz with power to change or omit what was necessary, and to print it as a letter from an anonymous person, or as if it were written by some other person with a real or a feigned name ; but in whatever way this was done, Wolf was directed to manage the whole matter with his usual skill, lest Keill should suspect Bernoulli to be the author ; "for," he adds, "it would be very disagreeable to me to be irritated and contumeliously treated by his bile, as his antagonists usually are, after he has hitherto treated me with sufficient politeness." Wolf expressed his great satisfaction with the attack upon "that trifler Keill," and promised to communicate it to Leibnitz, and decide according to his opinion on the form and manner in which it was to be published. The two critics, Wolf and Leibnitz, made such changes in the epistle as were agreeable to the latter, and every means were taken to keep the secret. Herman suspected that Bernoulli was the author of it, and, when he mentioned his suspicion, Wolf denied it, as he declares he always did ; but though every precaution was taken to keep the secret, it was discovered by means of the phrase *meam formulam*, which had been either heedlessly overlooked, or, as we believe, willingly left, in order to fix it upon Bernoulli, whose public declaration against Newton, and in favour of himself, Leibnitz had expressed his anxiety to obtain. The changes made on the letter were very considerable. M. Bernoulli, the grandson, who had a copy of the original, has published the two in opposite columns,[1] and, after a careful comparison of them, he observes, "that not a single disobliging expression against Newton and Keill had been omitted or softened. "It is true," he continues, "with respect to Keill, who was the more ill-treated, that his name was everywhere suppressed ; and when Wolf (and Leibnitz too) calls him an *audacious* antagonist in one place, where the letter only called him Keill, he did

[1] *Mém. de Berlin*, 1802, *Hist.* pp. 60-65.

worse than mention him by his name." Without noticing the fulsome praise of Leibnitz and Bernoulli, inserted in the letter, Bernoulli, the grandson, calls our attention to another point,—"to a species of fraud which Leibnitz and Wolf committed against their friend by interpolations, which they ought not to have made without his consent, seeing that they were to his disadvantage, and *had principally for their object* to claim for Leibnitz discoveries which Bernoulli attributed to himself. There can be no doubt that these interpolations were made by Leibnitz alone, for Wolf would not of his own accord have permitted them; but he was too much devoted to Leibnitz not to adopt what was done by his great patron."[1]

Although this letter was published in July 1716, yet Bernoulli was not aware, even in the month of March 1717, of the trick that had been played him, or of the injurious interpolations which had been made in it, or, of the worst fact of all, that the words *meam formulam* proved him to be the author. It was not till he heard of Herman's conjecture that he was the author, that he was induced to read the letter, and discover these unfortunate words. He immediately saw the use which would be made of them. His friends had already been laughing at the mistake, and his enemies accusing him of being the author of the letter; and he expressed his dread that Keill would seize every opportunity of cutting him up, and employing the matter against him for his own purposes. He, therefore, implored Wolf to think of some method of correcting the blunder in the *errata*, and he suggested that *meam* should read *eam;* but seeing that this would not answer

[1] *Mém. Acad. Berlin*, 1799, 1800, p. 47. The interpolation here referred to as an act of Leibnitz, is one of singular dishonesty. Bernoulli, in his letter to Wolf, states that he first taught the *exponential calculus;* but in place of this statement, they make Bernoulli say that he was only the first who *taught it publicly*, and then they add what he never said, "Far be it for me to deny that it was *first made known by Leibnitz*,"—thus making Bernoulli himself surrender his discovery to his rival.—*Mém. Acad. Berlin*, 1802, pp. 57, 58.

the purpose, he begged Wolf to think of some better method, by which the mistake should be laid upon the printer. Wolf did not obey the mandate of his friend, and, on the advice of Montmort, Bernoulli was induced to avow the letter through his son Nicolas, and to make the best apology he could by throwing much of the blame upon the friends who had deceived him. The avowal, which forms the appendix to a mathematical paper, is written in a good spirit, and concludes by expressing the ardent wish of his father, that the disputants would become good friends, and unite their powers, as citizens of the republic of mathematics, in labouring to extend its domains.[1] In concluding this strange history, in which Leibnitz performs the least creditable part, we are scarcely surprised at the fact stated by Bernoulli's grandson, that Wolf had the effrontery, in a history of his life and writings, to claim for himself the authorship of the letter *Pro Eminente Mathematico!*[2]

This celebrated letter, as might have been expected, excited the indignation of Newton and his friends. They had no difficulty in discovering its author;[3] and a long and elaborate reply to it, in the form of a letter to Bernoulli, was immediately prepared by Dr. Keill, and submitted to Newton, who proposed numerous alterations, and made many important additions to it. It is written with Keill's usual boldness, and ends with the following observation:—"If any person shall think that you have been treated too severely, I request them to take the trouble of reading your letter, a worthy effort of your

[1] *De Trajectoriis*, &c. &c., in the *Acta Eruditorum*, 1718, pp. 261, 262.

[2] *Mém. Acad. Berlin*, 1799, 1800, pp. 41, 42.

[3] In a letter to Newton, dated May 17, 1717, Keill thus speaks of it:—"A friend of mine brought me the *Acta* the other day, and I was amazed at the impudence of Bernoulli. I believe there was never such a piece for falsehood, malice, envy, and ill-nature, published by a mathematician before. It is certainly wrote by himself, for though he speaks of Bernoulli always in the third person, yet towards the latter end of his paper he forgot himself, and says that nobody but the antagonist can persuade himself that *my formula* was taken from Newton's." In a letter from Newton to Keill, May 2, 1718, he says that the *meam solutionem* "lays the letter upon Bernoulli."—See Edleston's *Correspondence*, Lett. xciii. p. 186.

genius, and then let them consider if you have not well deserved it.

"Si pergis dicere quæ vis, audies quæ non vis."[1]

After the death of Leibnitz, which took place on the 14th November 1716, the controversy to a considerable extent ceased.[2] His champion Bernoulli withdrew from the field, when no longer influenced by his patron and friend; and though Newton has been charged with having made an improper attack upon Leibnitz after his death, he did nothing more than publish an answer, which had been previously in circulation among his friends, in the form of Remarks on the letter of Leibnitz to the Abbé Conti.[3] This paper, which is erroneously characterized by Biot as a *bitter refutation*, is, on the contrary, an argumentative defence of his claims—an interesting notice of his own mathematical discoveries—a defence of the Royal Society and of Dr. Keill—and a frank expression of his feelings in reference to the conduct both of Leibnitz and Bernoulli.

Although Bernoulli felt the severity of Newton's censure, he was now more anxious to explain his own conduct than to

[1] We have found among Newton's papers a fair copy of this answer in French in the form of a letter to Bernoulli; and also Newton's annotations in separate folio sheets. It is doubtless another copy of the same piece which Mr. Edleston found among the Lucasian papers, and which he justly supposes to be the *libellum editum aut non editum* to which Bernoulli refers in the *Acta Eruditorum* for May 1719, p. 218, containing some vulgar and impertinent abuse of Keill as his *antagonista Scotus—homo quidem natione Scotus, qui apud suos inclaruit moribus, ita apud exteros jam passim notus odio plusquam vatiniano quo flagrat*, &c.—See Edleston's *Correspondence*, &c. p. 178; see also Newton's letter to Keill in p. 185, and *note*, p. 186, of the same *Correspondence*.

[2] The death of Leibnitz was notified to Newton by the Abbé Conti, who was then at Hanover, in a letter dated 10th December 1716. "M. Leibnitz," he says, "est mort, et la dispute est finie." After mentioning the manuscripts of Leibnitz, which he hopes the King will show him, he adds, "Je remarquerai s'il y a quelque chose touchant votre dispute, mais peut-être qu'on cachera ce qui ne fait point d'honneur à la mémoire de M. Leibnitz."—See APPENDIX, No. III. Lett. 2.

[3] These remarks, without a date, but written on the receipt of Leibnitz's letter of the 9th April, were first printed in Raphson's *Fluxions*, p. 111. They were afterwards translated into French, and published in Des Maizeaux's *Recueil*. I have found in the Portsmouth Papers the French proof, containing, in Newton's own hand, numerous corrections and several small additions to the Remarks, one of which mentions the month of May 1716 as the date when they were written.

retaliate upon his adversaries, and a few months had scarcely elapsed after the death of Leibnitz, before he sent messages of kindness to Newton. We have already seen that in April 1717, he not only threw the blame of the pulse-feeling problems upon Leibnitz, but blamed him highly for betraying the secret under which they were sent to him. In a subsequent letter from Montmort to Newton, dated 7th March 1718, he conveys similar messages from Bernoulli and his son Nicolas, expressing their fears that their disputes with Keill had deprived them of his friendship. In replying to Bernoulli, Montmort pointed out to him the inconsistency of these expressions with the *Epistola pro Eminente Mathematico*, and seems to have suggested to him the propriety of disowning it. Bernoulli, however, took a middle course. He acknowledges that he had, at Leibnitz's request, sent him the facts necessary to defend himself against Keill, and was not answerable either for the praise given to himself, or the harsh language applied to his antagonists.

When Sir Isaac received Montmort's letter, he enclosed it to Keill,[1] requesting " his sense upon this matter." In his reply,[2] Keill observes that Bernoulli " is sensible that he had burnt his fingers ; that he should beg Newton's pardon for saying that he did not understand second differences,—that no notice should be taken of these letters," and that " it lay on Bernoulli to clear himself and produce the author of the scurrilous paper.

The celebrated Abbé Varignon had been long desirous of reconciling Newton and Bernoulli, and at last succeeded in the attempt. Sir Isaac had in 1718 sent Varignon three copies of the English edition of his Optics, and in 1719, as many of the Latin edition, to be presented to his friends. Varignon sent a copy of each to Bernoulli in the name of Newton,[3] and

[1] This letter, dated May 2, 1718, has been published by Mr. Edleston, in his *Correspondence*, &c. in pp. 185, 186.

[2] In an unpublished letter, dated May 23, 1718.

[3] Newton had, in 1717, sent to Nicolas Bernoulli a copy of the second edition of the Principia. Bernoulli's letter of thanks, dated Pavia, 31st May 1717, has been preserved.

it was in Bernoulli's reply, dated 10th July 1719, thanking him for these presents, that he gave the solemn denial we have already quoted, that he was not the author of the celebrated letter to Leibnitz of 7th June 1713. In answering this letter,[1] Newton thus expressed himself, "When I first received your letters, through the mediation of the Abbé Varignon, and understood from them that you were not the author of a certain letter to Mr. Leibnitz, dated 7th June 1713, I at once resolved not only to forget the mathematical disputes which had lately taken place, but to cultivate your friendship, and to estimate highly your great mathematical merits. I have never grasped at fame among foreign nations, but I am very desirous to preserve my character for honesty, which the author of that epistle, as if by the authority of a certain great judge, had endeavoured to wrest from me. Now that I am old, I have little pleasure in mathematical studies, and I have never tried to propagate my opinions over the world, but have rather taken care not to involve myself in disputes on account of them."

The dignified tone of this letter could not fail to disturb the tranquillity of Bernoulli. Conscious of having written the letter which Newton condemns as an attack upon his honesty, he could hardly avoid referring to it in his reply, and we cannot but regret that the terms in which he again denies it are essentially different from those which he had used only five months before.[2] "I am not aware," says he, "of the nature of the letter to Leibnitz, dated 7th June 1713, of which you speak. I do not remember that I wrote to him on that day, nor do I altogether deny it, as I do not keep copies of my letters. But if, perhaps, among the innumerable letters which I have written to him, one should be found to which the said day and year is prefixed, I dare solemnly assure you, that nothing is contained in it which could in any way injure your character, and that I never gave

[1] We quote from the Latin scroll, which has no date, and of which there are two copies among the Portsmouth Papers.

[2] See p. 17.

him leave to publish any of my letters, and especially one which would not be agreeable to you. I implore you, therefore, to be persuaded, that I never intended to speak of you otherwise than as a great man, and much less to attack your character and probity."

The letter of Newton, to which this was the reply, was enclosed in one to Varignon,[1] in which he thanks him for having reconciled him to Bernoulli, and mentions as the ground of his embracing him as a friend, his denial of having written the obnoxious letter to Leibnitz. Varignon was much gratified at having brought about this reconciliation, but it was a reconciliation merely nominal, and led only to a few interchanges of civility.—Bernoulli sought an explanation through Varignon, of the charge of knight errantry which Newton had made against Leibnitz and his "army of disciples," for challenging the English to the solution of mathematical problems. Newton explained that the phrase was used in a jocular sense, and applied to Leibnitz;[2] and we believe that no farther communication took place between them till 1723, when Newton sent Bernoulli a copy of the French edition of his Optics.[3] In returning thanks for this present, Bernoulli takes occasion to introduce for the *third* time the subject of the celebrated letter of the 7th June 1713. Hartsoecker, a Dutch philosopher, had attacked Newton's Theory of Colours, and had referred to him as his authority for charging Bernoulli with having called himself "an excellent mathematician" in the *Charta Volans*. After directing the attention of Newton to the attack upon his Theory of

[1] This letter, of which an imperfect scroll has been published in the Macclesfield *Correspondence*, vol. ii. p. 436, as a letter from Newton to ——, is supposed by Mr. Edleston to have been addressed to Montmart. The copy which I have found is a fuller and more perfect scroll than the one published by Mr. Rigaud.—See Edleston's *Correspondence*, &c. p. 187, note.

[2] Letter of Newton to Varignon, September 26, 1721.

[3] This work was translated by M. Coste and corrected by the Abbé Varignon, whose correspondence with Newton relates principally to certain difficulties which arose with the publisher, and to Newton's reconciliation to Bernoulli.

Colours, Bernoulli denies the truth of the charge against himself without distinctly denying, as he formerly did, the authorship of the letter, and seems to expect that Newton should take some step in the matter. "Although the fellow," says he, "is unworthy of any answer from me, yet one thing irritates me greatly, namely, that he exposes me to the laughter of every person, and impudently maintains that I took to myself the title of an excellent mathematician; and in order that the crime of calumny should not attach to himself, he makes you the author of it by citing the passage in which you speak of that letter of 7th June 1713, which Leibnitz maintained was written by me, and in which that eulogium, within parentheses, was ascribed to me. Hence the calumniator maliciously concludes that you wished to insinuate that I had been so arrogant as to assume this title to myself. . . . . In the meantime, whatever be the calumny of Hartsoecker, it applies more to you than to me, for he malignantly endeavours to draw it from your express words. What you think should be done, therefore, that my innocence may be protected among those who do not see the Collection of Des Maizeaux,[1] I would willingly learn from yourself, if you are disposed to honour me with an answer."[2]

In the year 1725, a new edition of the *Commercium Epistolicum* was published, with notes, a general review of it,[3] and a preface of some length. A question has arisen respecting the authorship of the review and the preface, some ascribing them to Keill, and others to Newton. From similarity of style, but chiefly on the authority of Dr. James Wilson, the friend of Pemberton, Professor De Morgan had made it highly probable that both the review and the preface were written by Newton.[4] Of the correctness of this opinion I have found ample evidence

[1] "Des Maizeaux, *Recueil de Diverses Pièces*, &c. tom. ii. p. 125, line 32."

[2] Dated Basle, Feb. 6, 1723.

[3] This review is the *Recensio*, mentioned in page 25, note.

[4] *Phil. Mag.* June 1852, vol. iii. p. 440.

in the manuscripts at Hurtsbourne Park;[1] and it is due to historical truth to state, that Newton supplied all the materials for the *Commercium Epistolicum*, and that, though Keill was its editor, and the committee of the Royal Society the authors of the Report, Newton was virtually responsible for its contents.

The share which Newton took in the fluxionary controversy either directly or through Dr. Keill, who did nothing without his approbation, and the mass of papers which he has left behind him on the subject, show the great anxiety which he felt not only to be considered the first inventor of the calculus, but the only inventor who had a right to the reputation which it gave. He firmly believed, not only that Leibnitz might have derived the differential calculus from the papers actually communicated to him, but that he did derive it from that source, or from his ideas either oral or written, which were in circulation at the time of his visit to London.[2] That these were the views of Newton, and, we may add, of all his friends in England, is evident from the new form given to the celebrated scholium in the third edition of the Principia, which appeared in 1725, under the editorship of Pemberton. The reference to Leibnitz and his method was wholly omitted, and replaced by a quotation from a letter to Collins in December 1672, containing, or supposed to contain, the Germ of Fluxions. This step was perhaps unwise. The statement in the two first editions granted nothing to Leibnitz, and even if it had, the truth which it embodied was not cancelled by its omission from the third; but viewing the matter as Newton did, we think he was justified in omitting the scholium. He had stated it, as he himself has

[1] I find among these MSS. scrolls of almost the whole of the *Recensio*, and *five* or *six* copies in his own hand of the *Ad Lectorem*.

[2] In reference to this subject, I find two remarkable letters addressed to Newton in 1720, by Dr. James Wilson, mentioning to him that he possessed several of his manuscripts, and had seen others which had been in general circulation. "Among the papers," he says, "I likewise observed there were some which deduced even the first principles of geometry from the fluxion of points." These letters seem to me of such importance, that I have given them in the APPENDIX, No. III.

said, as a mere historical fact, that Leibnitz had sent him a method which was similar to his own; and when he found that the German mathematician had regarded this simple statement as a recognition of his independent discovery of the calculus, he was not only entitled but constrained to cancel a passage which had been so erroneously interpreted, and so improperly used.

Some time after the death of Leibnitz, Newton drew up a *History of the Method of Fluxions*, the Preface to which has been found among his papers. I am disposed to think that this Preface was intended as an Introduction to the new edition of the *Commercium Epistolicum* and *Recensio*, which was published in 1725, and that it had not been thought advisable to enter into any fresh discussions on the subject. In the first paragraph of the Preface, Sir Isaac remarks, that as only a few copies of the *Commercium* had been published and sent to those only who could judge in such matters, that work and the *Recensio* should be again printed, in order that a true history of the calculus, drawn from ancient documents, might descend to posterity without any disputes, and "put an end to a controversy which was no longer necessary after the charge of plagiarism had been repelled." He then proceeds to enumerate by their dates *seventeen* letters from Leibnitz to Oldenburg, written between the 3d February 1673 and the 12th July 1677; and, after establishing his claim to the invention of the new calculus, he concludes with these words: "These things being premised, the *Recensio* of the *Commercium Epistolicum* should be read, and the *Commercium* itself consulted, if any doubt be entertained respecting the facts."

The following is an exact copy of the title-page of the manuscript:[1]—

[1] In the first copy of this manuscript the word *Prefatio* is not inserted after the title *Historia*, &c. In the second it is inserted, and the title erased; and in the third the title is omitted, and the word *Prefatio* alone inserted. Newton seems to have had much difficulty in fixing upon a title. Upon a separate folio which I have found, occupying a page and a half, there are no fewer than *twelve* forms of it. The first is *Introductio ad Recen-*

HISTORIA METHODI ANALYSEOS

QUAM NEWTONUS METHODUM FLUXIONUM

LEIBNITIUS METHODUM DIFFERENTIALEM

VOCAVIT

IN COMMERCIO EPISTOLICO COLLINII ET ALIORUM

ET RECENSIONE COMMERCII

CONTENTA

QUORUM PRIUS EX ANTIQUIS LITERIS JUSSU REGIÆ SOCIETATIS

COLLECTUM FUIT ET EDITUM

ANNO 1712

ALTERA IN ACTIS PHILOSOPHICIS EJUSDEM SOCIETATIS

ANNO 1715

(ANNO ET ALIQUOT MENSIBUS ANTE OBITUM LEIBNITII)

LUCEM VIDIT.

In studying this controversy, after the lapse of nearly a century and a half, when personal feelings have been extinguished, and national jealousies allayed, it is not difficult, we think, to form a correct estimate of the claims of the two rival analysts, and of the spirit and temper with which they were maintained. The following are the results at which we have arrived :—

1. That Newton was the first inventor of the *Method of Fluxions ;* that the method was incomplete in its notation ; and that the fundamental principle of it was not published to the world till 1687, twenty years after he had invented it.

2. That Leibnitz communicated to Newton, in 1677, his *Differential Calculus*, with a complete system of notation, and that he published it in 1684, three years before the publication of Newton's Method.

sionem Libri, &c., but all the rest are *Historia Methodi,* &c., with eleven variations. In the second, third, and fourth, it is *Historia Methodi Analyseos,* &c. In the fifth and sixth the names of both the mathematicians are omitted. In the seventh it is *Historia Methodi Differentialis,* with both names omitted. In the eighth the change is remarkable. The title is *Historia Methodi Analyseos per Fluxiones et Momenta a D. Newtoni inventa, a D. Leibnitio Differentialis nominatæ, ex literis antiquis deducta.* In the ninth, tenth, and eleventh, it is *Hist. Meth. Fluxionum,* &c. ; and in the twelfth *Differentialis* is placed above *Fluxionum.*

The admission of these two facts ought to satisfy the most ardent friends of the rival inventors; but in apportioning to each the laurels which they merit, new considerations have been introduced into the controversy. Conscious of his priority, Newton persisted in maintaining that the only question was, who was the *first* inventor, and that "*second* inventors have little or no honour, and no rights."[1] Upon this principle, which we cannot admit, the whole merit of the new calculus must be given to Newton, and he undoubtedly claimed it. But at variance with this, there is another principle maintained in modern times, and by distinguished men, which transfers all the merit of an invention or discovery to the person who first gives it to the world. Upon this principle the merit of the new calculus must be adjudged to Leibnitz. These two extreme principles have not in the present case been adopted by the mathematical world. No writer has urged the second against the claims of Newton, or the first against those of Leibnitz. Priority of invention may be established otherwise than by publication; and the merit of a second inventor, when really such, is intellectually as great as the first.[2] There is a merit, however, of a peculiar kind which must ever attach to the first inventor who freely gives his invention to the public. While society concedes to him a high niche in the temple of fame, it cherishes also a feeling of gratitude for the gift it has received. To a second inventor society owes but its respect and admiration.

Hitherto we have taken it for granted that Newton and Leibnitz had borrowed nothing from each other; and, in stating the result of our inquiry, we have supposed this to be true. A very different opinion, however, has been maintained during the

[1] "Secundis Inventoribus, *etiam revera talibus*, vel exiguus ve nullus honor, tituli vel juris nihil est."—*Recensio*, Newtoni *Opera*, tom. iv. p. 487.

[2] We cannot here discuss this important subject. Such of our readers as take an interest in it, are referred to the *North British Review*, vol. vii. p. 233, &c., where it is treated in reference to the rival claims of Adams and Leverrier.

controversy. The unquestioned priority of Newton's discoveries has preserved him from the charge of having borrowed anything from Leibnitz, excepting his ideas of notation; but a variety of circumstances, which it is unnecessary to mention, have given a certain degree of plausibility to the opinion that Leibnitz may have derived assistance, even of the highest kind, from the previous labours of his rival. At an early period Newton had communicated to his friends, orally and in writing, the elements, or the germ of his method of fluxions, but certainly his discoveries in series. His manuscripts were copied, and, to a considerable extent, circulated in England. The letters and extracts, actually communicated to Leibnitz, may, or may not, have contained the information which Newton and his friends considered as sufficient to convey to him a knowledge of the method of fluxions; but the fact that he was twice in England in 1673 and 1676, and was in communication with the mathematicians who then adorned the metropolis, justified the idea that either orally, or from the circulated manuscripts[1] of Newton casually seen, or actually communicated to him, he might have derived that information.[2]

Had Leibnitz been an ordinary man, these views might have had much weight; but his powerful intellect, his knowledge of the subject, and the great improvements which he made in the new calculus, place it beyond a doubt that he was capable of inventing the differential method without any extraneous aid. His *Theoria Motus Abstracti*, dedicated to the Academy of Sciences in Paris in 1671, before he visited England, contains, according to Dr. William Hales,[3] "no obscure seeds of his differential method;" and shows, in the opinion of Professor De Morgan, "that in 1671 it was working in Leibnitz's mind,

[1] See APPENDIX, No. III.

[2] We have made no reference to the singular opinion of Raphson and of Dr. James Wilson, that Leibnitz may have deciphered the anagram in which Newton concealed his method. See APPENDIX, No. III.—P.S. to letter of January 21, 1720-1. See also Professor De Morgan's paper in the *Companion to the Almanac for* 1852, p. 15.

[3] *Analysis Fluxionum*, p. 2, § 5.

that in the doctrine of infinitely small quantities lay the true foundation of that approach to the differential calculus which Cavalieri presented."[1] Another argument in favour of Leibnitz is deduced by Professor De Morgan from seven MSS. of his, bearing the dates of November 11, 21, 22, 1675; and June 26, July, and November 1676, one bearing no date, and recently published by M. Gerhardt from the originals in the Royal Library at Hanover.[2] These MSS., of which Professor De Morgan has given a specimen, are, as he says, "study exercises in the use of both the differential and integral calculus,"[3] and, if genuine, and correct in their dates, possess a historical interest.

In adjudicating on a great question like the present, surrounded as it has been with national sympathies, we are compelled to look into the character of the parties at our bar. We cannot commend the conduct of Newton in concealing from Leibnitz, in transposed letters, the discoveries which he had made, nor can we justify his personal retreat from the battlefield, and his return under the vizor of an accomplished champion.[4] His representatives, however, were men of station and character, who gave their names, and staked their reputation in the contest; while Leibnitz and his disciples wielded the anonymous shafts of the slanderer, denied what they had written, and were publicly exposed through the very rents which they had left in their masks.

Instead of striving to prove that he was the inventor of the new calculus, Leibnitz evaded the discussion by attacking the

[1] Professor De Morgan, *ut supra.*

[2] Die Entdeckung der Differentialrechnung durch Leibnitz. Von der C. G. Gerhardt, 4to. Halle, 1848. See Professor De Morgan, *Companion to the Almanac for* 1852 pp. 17, 18.

[3] *Ibid.* p. 17. See vol. i. p. 358, note 4.

[4] Dr. Keill, Newton's principal champion, and who so nobly fought his battles, has been ungenerously treated by some of the historians of science. With his private letters to Newton before us, we have formed a high opinion both of his talents and character. Everything he did was open and manly, and he did nothing without the instruction and approbation of Newton and his friends.

philosophy of Newton, which he did not understand, and challenging the English to the solution of mathematical problems. Nor were these problems his own. He obtained them, as we have seen, under the pledge of secrecy, from a friend whose name he did not scruple to betray ; and, when the controversy was at its crisis, he tried to substitute authority for argument, by imploring the most distinguished mathematician on the Continent to declare that he was the first and the sole inventor of the new calculus. Bernoulli rashly yielded to the urgency of his patron, but, in the anonymous testimonial which he gave, Leibnitz inserted a parenthetic eulogy on the writer, which had the effect of removing his mask, and exposing him to the ridicule and laughter of the scientific world. Nor is it difficult to discover, or uncharitable to expose, the motive for the interpolation. It was intended to prove that the "eminent mathematician" was John Bernoulli, and, lest the proof should not be thought sufficient, Leibnitz publicly declared, while Bernoulli as publicly denied, that he was the author. Thus, to a certain extent, baffled in his schemes, Leibnitz, as we formerly stated, implored Bernoulli, when an opportunity should present itself, to make an early and public declaration that the method of fluxions was posterior to his calculus—that is, that Newton was a plagiarist. An opportunity soon occurred for the perpetration of this fresh act of injustice. Bernoulli unscrupulously prepared the document,[1] and, when it came into the hands of Leibnitz, he imparted to it new elements of bitterness,—interpolating passages in praise of himself and Bernoulli,—altering other passages, so as to give to himself a discovery which belonged to his friend,—and, finally, leaving the words *meam formulam* to prove, as it did prove, to the world, that the testimony in the letter was the testimony of John Bernoulli. We have found nothing in the records of science so dishonest as this. As a portion of

[1] His celebrated letter of the 9th April 1716, already described. See p. 26, and APPENDIX, No. II. An instructive account of an instance of bad faith towards Leibnitz, on the part of Bernoulli, is given by his own grandson in the *Mém. Acad. Berlin*, 1802, pp. 51-56.

scientific history, closely connected with the fluxionary controversy, we have submitted it to the reader; but we have not allowed it to influence the decision which we have ventured to pronounce.

In charging Newton with plagiarism, and in persuading others to repeat and enforce the charge, we may find some apology in the excited feelings of Leibnitz, and in the insinuations which were occasionally thrown out against the originality of his discovery; but for other parts of his conduct, we seek in vain for an excuse. When he assailed the philosophy of Newton in his letters to the Abbé Conti, he exhibited only the petty feelings of a rival; but when he dared to calumniate that great and good man in his correspondence with the Princess of Wales, by whom Newton was respected and loved,—when he ventured to denounce his philosophy as physically false and dangerous to religion,—and when he founded these accusations on passages in the *Principia* and *Optics*, glowing with all the fervour of genuine piety, he cast a blot upon his name which all his talents as a philosopher will never be able to efface.[1]

[1] This anecdote is given in still stronger language by M. Biot in his *Life of Newton*, *Biog. Univers.* tom. xxxi. p. 178.

CHAPTER XVI.

Newton declines taking Orders—His Rooms in Trinity College—John Wickins his chamber-fellow—Letter from Mr. Nicolas Wickins his Son—Dr. Humphrey Newton his Amanuensis from 1684-1689—His two Letters to Conduitt—Newton's Speculations on the Theory of the Earth—James II. attacks the Rights of the Charter-House, and sends an illegal Mandamus to the University of Cambridge—Newton one of the Delegates to resist this encroachment on its privileges—The Vice-Chancellor deposed—The object of the deputation gained—Neglect of the Scottish Universities—Newton elected Member for Cambridge to the Convention Parliament—His habits of business—His Letters to Dr. Covel—His acquaintance with Locke—His Theological inquiries—Locke exerts himself to procure for him some permanent appointment in King's College, the Charter-House, and the Mint—Failure of that attempt—Newton's disappointment—Ingratitude of his Country—Death of his Mother at Stamford—Writes an account of Fluxions and Fluents for Wallis—His Letter to Locke on multiplying Gold—Boyle's recipes and belief in Alchemy.

In the early chapters of this work we have brought down the personal history of Newton to the year 1675, when he was permitted by the Crown to retain the Lucasian Chair without going into orders. At a future period of his life, he was urged by some of the highest dignitaries in the Church to enter its pale, but feeling that his opinions were not in accordance with its Articles, he invariably declined, assigning as a reason that he could do more good to religion as a layman.

During the first twenty years of his residence at Cambridge, from 1667 to 1687, when the *Principia* was published, he was wholly occupied with those profound researches, of which we have given a full account, and tradition has preserved but a few anecdotes of a life so quiet and unvaried. Having outlived almost all his companions at school and at college, it became difficult, even at the time of his death, to obtain authentic materials for an account of his early and middle life, and his successors in Trinity College have therefore not been able to discover the locality of his early apartments. The chamber which was allotted to him as a Fellow in October 1667, was called the "Spiritual Chamber," which Mr. Edleston conjectures may have been "the ground room next the chapel in the north-east corner of the great court," but, as he adds, "it does not follow that he actually dwelt there," as it might have been occupied by a tenant. The rooms in which Newton lived from the year 1682 till he left Cambridge, are in the north-east corner of the great court. They are on the first floor of the staircase, on the right hand, or to the north of the gateway or principal entrance to the College, the outward door fronting the staircase, and the rooms being to the right.[1] His laboratory, as Dr. Humphrey Newton tells us, was "on the left end of the garden, near the east end of the chapel," and his telescope, which, according to the same authority, was five feet long,[2] was placed at the head of the stairs going down into the garden looking towards the east.[3] The east side of Newton's rooms has been altered within the last fifty years. The wooden room, which projects into the garden, as seen in Loggan's engraving, is supported on pillars forming an arcade, and Professor Sedgwick, who came up to college in 1804, recollects it in that state. The arcade is now replaced by a wooden wall and brick chimney. The drawing on the next page is a view of Newton's rooms copied from Loggan's Plate.

Mr. John Wickins, a Fellow of Trinity College, two years junior to Newton, was one of his earliest and most esteemed friends, a similar dislike to their disorderly companions having

[1] *Memorandum* sent to me by the late Rev. Mr. Turnor, and Edleston's *Correspondence*, &c., p. xlii.

[2] This must have been a refracting telescope.

[3] In the Memorandum by the late Rev. Mr. Turnor above-mentioned, he says, "I have some recollection that Mr. Jones the tutor mentioned, in one of his lectures on optics, that the reflecting telescope belonging to Sir Isaac Newton was then lodged in the observatory over the gateway; and I am inclined to think that I once saw it, and that a finder was affixed to it."

SIR ISAAC NEWTON'S ROOMS IN TRINITY COLLEGE.

induced them to live together so early as 1665. Wickins continued to be Newton's chamber-fellow till he left college, and on the 4th April 1684, he was presented to the living of Stoke Edith, near Monmouth, by Paul Foley, Esq., afterwards Speaker of the House of Commons.[1] While Wickins retained his Fellowship, Newton drew for him his dividends and chamber rent, and when Newton himself quitted Trinity College, he left to his friend the whole furniture of his chamber, with a wooden pint flagon, and other articles, which were preserved in his family so recently as 1802.[2] Nicolas Wickins, the son of John, succeeded to the living at Stoke Edith; and having been requested by Professor Smith of Trinity College to furnish him with some particulars of Newton's college life, he addressed to him the following interesting letter:[3]—

"STOKE EDITH, *Jan.* 16*th*, 1727-8.

"DEAR SIR,—It was an unspeakable pleasure to me to see the hand of my old acquaintance; and I wish, in return, I could send something considerable to give you a pleasure relating to the great man you write about, but I am so unhappy as to find very little under Sir Isaac's own hand of what passed between him and my father.

"I guess from a small book I found among my father's papers, that he had a design to collect into one all that he had of Sir Isaac's writing, but he went no farther than transcribing three short letters he received from him, and a Common Place of his, part of which I find under Sir Isaac's own hand; the rest, with the original of these three letters, is lost. Besides these transcribed letters and the Common Place, I can meet with nothing but four or five letters under Sir Isaac's own

[1] Turner's *Newtoniana*, in the possession of the Royal Society.

[2] Wickins (Ds. Wickins), to whom Newton had frequently lent money, as we have stated in vol. i. p. 28, note, died on the 19th April 1719. See *Gentleman's Magazine*, April 1802.

[3] We have given this and the two following letters verbatim, as possessing a higher degree of interest than any abstract of them that could be made.

hand, very short, and relating to dividends and chamber rent, which he was so kind as to receive for my father when at Monmouth, where he was most part of the time he continued Fellow. There being so little in these letters, I do not now send them, but wait for your commands ; for whatever I can meet with of this worthy man, shall be at your service.

"My father's intimacy with him came by mere accident. My father's first chamber-fellow being very disagreeable to him, he retired one day into the walks, where he found Mr. Newton solitary and dejected. Upon entering into discourse, they found their cause of retirement the same, and thereupon agreed to shake off their present disorderly companions and chum together, which they did as soon as conveniently they could, and so continued as long as my father staid at college.

"I have heard my father often say that he has been a witness of what the world has so often heard of Sir Isaac's forgetfulness of his food when intent upon his studies ; and of his rising in a pleasant manner with the satisfaction of having found out some proposition without any concern for a seeming want of his night's sleep, which he was sensible he had lost thereby.

"He was turning grey, I think, at thirty, and when my father observed that to him as the effect of his deep attention of mind, he would jest with the experiments he made so often with quicksilver, as if from hence he took so soon that colour.

"He sometimes suspected himself to be inclining to a consumption, and the medicine he made use of was the Leucatello's Balsam,[1] which, when he had composed himself, he would now

[1] The following method of making the Leucatello's Balsam I have found in Sir Isaac's own hand : "Put Venus turpentine one pound into a pint of the best damask rose-water ; beat these together till it look white, then take four ounces of bees-wax, red sanders half an ounce, oil of olives of the best a pint, one ounce of oil of St. John's wort, and half a pint of sack. Set it (the sack) on the fire on a new pipkin, add to it the oil and wax, let it stand on a soft fire where it must not boil, but melt till it be scalding hot. Then take it off. When it is cold, take out the cake, and scrape off the dirt from the bottom. Take out the sack, wipe the pipkin, put in the cake again, set it on the fire, let them melt together, and then put in also the turpentine and sanders ; let them not

and then melt in quantity about a quarter of a pint, and so drink it.

"It is now eight years since my father's death, in which time many things my father used to relate of him are slipped out of my memory; but being mostly of such a nature as I have now mentioned, I suspect would be of no service could I recollect any more.

"But there is one thing, upon account of which not only my father, but myself also, shall always pay a peculiar regard to his memory, which was a charitable benefaction which has privately passed from him through my father's, and since his death through my own hands. We have been the dispensers of many dozens of Bibles sent by him for poor people, and I have now many by me sent from him for the same purpose, which, as it shows the great regard he had for religion, I cannot but desire that by you it may be made public to the world.

"Dear Sir, my thoughts dwell with wonderful delight upon the memory of this great and good man, and therefore I have troubled you with so long a letter, which I now beg pardon for, and in hope of again hearing soon from you, conclude with my brother's hearty service and respects to you. I beg my humble service to all my old acquaintance, and am,

"Dear Sir, your much obliged humble servant,

"NIC. WICKINS.

"To Mr. Professor SMITH, at
Trinity College, Cambridge."

boil, but be well melted and mixed together; take it off and stir it now and then till it is cold. If you would have it to take inwardly, add to it when it is off from the fire, half an ounce of powder of scuchineal (cochineal) and a little natural balsam.

"For the measell, plague, or small-pox, a half an ounce in a little broth, take it warm, and sweat after it. And against poison and the biting of a mad dog; for the last you must dip lint and lay it upon the wound, besides taking it inwardly. There are other virtues of it; for wind, cholic, anoint the stomach, and so for bruises."

Mrs. Vincent told Dr. Stukely that Sir Isaac was a great *Simpler*. The Doctor says that "his breakfast was orange-peel boiled in water, which he drank as tea, sweetened with sugar, and with bread and butter. He thinks this dissolves phlegm." Lord Pembroke told the Doctor that when Newton "got a cold, he lay in bed till it was gone, though for two or three days' continuance, and thus came off the illness by perspiration."

In the year 1683, Newton requested Mr. Walker, who was then schoolmaster at Grantham, to engage Mr. Humphrey Newton of that town as an assistant and amanuensis. Mr. Newton willingly accepted of the offer, and remained with Sir Isaac nearly five years, from the end of 1683 to 1689, that interesting period during which the *Principia* was written and published. When Mr. Conduitt was in search of materials for the life of his relative, he naturally applied to Dr. Newton for information, and he obtained from him the two following letters, which throw much light on the "life and actions" of the great philosopher :—

"Sir,—Receiving yours, I return as perfect and faithful an account of my deceased friend's transactions, as possibly does at this time occur to my memory. Had I had the least thought of gratifying after this manner Sir Isaac's friends, I should have taken a much stricter view of his life and actions.

"In the last year of King Charles II., Sir Isaac was pleased, through the mediation of Mr. Walker (then schoolmaster at Grantham), to send for me up to Cambridge, of whom I had the opportunity, as well as honour, to wait of for about five years.[1] In such time he wrote his *Principia Mathematica*, which stupendous work, by his order, I copied out before it went to the press. After the printing, Sir Isaac was pleased to send me with several of them in presents to some of the heads of Colleges, and others of his acquaintance, some of which (particularly Dr. Babington of Trinity) said that they might study seven years before they understood anything of it. His carriage then was very meek, sedate, and humble, never seemingly angry, of profound thought, his countenance mild, pleasant, and comely. I cannot say I ever saw him laugh but once, which was at that passage which Dr. Stukely mentioned in his letter to your

[1] Dr. Stukely says, that "Mr. Newton of this town was five years under Sir Isaac's *tuition* at Cambridge."

honour,[1] which put me in mind of the Ephesian philosopher, who laughed only once in his lifetime, to see an ass eating thistles when plenty of grass was by. He always kept close to his studies, very rarely went a visiting, and had as few visitors, excepting two or three persons, Mr. Ellis,[2] Mr. Laughton of Trinity,[3] and Mr. Vigani,[4] a chemist, in whose company he took much delight and pleasure at an evening when he came to wait upon him. I never knew him to take any recreation or pastime either in riding out to take the air, walking, bowling, or any other exercise whatever, thinking all hours lost that was not spent in his studies, to which he kept so close that he seldom left his chamber except at term time, when he read in the schools as being Lucasianus Professor, where so few went to hear him, and fewer that understood him, that ofttimes he did in a manner, for want of hearers, read to the walls. Foreigners he received with a great deal of freedom, candour, and respect. When invited to a treat, which was very seldom, he used to return it very handsomely, and with much satisfaction to himself. So intent, so serious upon his studies, that he ate very sparingly, nay, ofttimes he has forgot to eat at all, so that, going into his chamber, I have found his mess untouched, of

[1] The passage alluded to in Dr. Stukely's letter was the following:—When Sir Isaac once laughed, " 'twas upon occasion of asking a friend, to whom he had lent Euclid to read, what progress he had made in that author, and how he liked him ? He answered by desiring to know what use and benefit in life that study would be to him. Upon which Sir Isaac was very merry."—Stukely's *Letter to Dr. Mead.*

[2] Afterwards Sir John Ellis, Master of Caius.

[3] Sir Charles Montague's letter to Newton in Chapter xix., and Monk's *Life of Bentley,* pp. 224, 226, 346, 360.

[4] John Francis Vigani, a native of Verona, after having taught chemistry at Cambridge for twenty years, was invested by the University with the title of Professor of Chemistry. Dr. Bentley fitted up for him in Trinity College an old lumber house, as an elegant chemical laboratory, in which he lectured for some years.—Monk's *Life of Bentley,* p. 159. His lectures still exist in manuscript in the University library.

Among the anecdotes collected by Conduitt, I find the following relative to this chemist. It is signed C. C. (Catherine Conduitt), Sir Isaac's niece. " Upon Vigani's (with whom he was very intimate, and took great pleasure in discoursing with him on chemistry) telling him a loose story about a nun, he broke off all acquaintance with him."—C. C.

which, when I have reminded him, he would reply,—'Have I!' and then making to the table, would eat a bit or two standing, for I cannot say I ever saw him sit at table by himself.[1] At some seldom entertainments, the Masters of Colleges were chiefly his guests. He very rarely went to bed till *two* or *three* of the clock,[2] sometimes not till *five* or *six*, lying about *four* or *five* hours, especially at spring and fall of the leaf, at which times he used to employ about six weeks in his elaboratory, the fire scarcely going out either night or day, he sitting up one night and I another, till he had finished his chemical experiments, in the performances of which he was the most accurate, strict, exact. What his aim might be I was not able to penetrate into, but his pains, his diligence at these set times made me think he aimed at something beyond the reach of human art and industry. I cannot say I ever saw him drink either wine, ale, or beer, excepting at meals, and then but very sparingly. He very rarely went to dine in the hall, except on some public days, and then if he has not been minded, would go very carelessly, with shoes down at heels, stockings untied, surplice on, and his head scarcely combed.

"As for his *Optics* being burned, I knew nothing of it but as I had heard from others, that accident happening before he writ his *Principia*.[3] He was very curious in his garden, which

[1] Dr. Stukely mentions some other anecdotes of Newton's absence:—"When he had friends to entertain, if he went into his study to fetch a bottle of wine, there was danger of his forgetting them. He would sometimes put on his surplice to go to St. Mary's church." When he was "going home to Colsterworth from Grantham, he once led his horse up Spittlegate Hill, at the town-end. When he designed to remount, his horse had slipped the bridle and gone away without his perceiving it, and he had only the bridle in his hand all the while."—*Letter to Conduitt.*

"Newton formerly would go the length of a street before he came to himself and saw that he was not dressed, and therefore had to hasten back to his house quite ashamed." Krausen's *Umstaendliche* Bücher *Historie*, part i. p. 2. Leipsic, 1715.—

[2] Dr. Stukeley informs us, "that he heard him say, that during the course of his most intense studies, he learned to go to bed at twelve, finding, by experience, that if he exceeded that hour but a little, it did him more harm in his health than a whole day's study."

[3] Dr. Stukely says, that "he wrote a piece of chemistry, explaining the principles of

was never out of order in which he would at some seldome time take a short walk or two, not enduring to see a weed in it. On the left end of the garden was his elaboratory, near the east end of the chapel, where he at these set times employed himself in with a great deal of satisfaction and delight. Nothing extraordinary, as I can remember, happened in making his experiments; which, if there did, he was of so sedate and even temper, that I could not in the least discover it. He very seldom went to the chapel, that being the time he chiefly took his repose; and, as for the afternoon, his earnest and indefatigable studies retained him, so that he scarcely knew the house of prayer. Very frequently, on Sundays, he went to St. Mary's church, especially in the forenoon. I knew nothing of the writings[1] which your honour sent, only that it is his own hand, I am very certain of, believing he might write them at some leisure hours, before he set upon his more serious and weighty matters. Sir Isaac at that time had no pupils nor any chamber-fellow, for that, I would presume to think, would not have been agreeable to his studies. He was only once disordered with pains at the stomach, which confined him for some days to his bed, which he bore with a great deal of patience and magnanimity, seemingly indifferent either to live or die. He seeing me much concerned at his illness, bid me not trouble myself; 'For if,' said he, 'I die, I shall leave you an estate,' which he then mentioned.

"Sir, this is what I can at present recollect, hoping it may in some measure satisfy your queries.

that mysterious art upon experimental and mathematical proof, and he valued it much; but it was unluckily burned in his laboratory, which casually took fire. He would never undertake that work again,—a loss much to be regretted. Mr. Newton, of this town, tells me likewise, that several sheets of his *Optics* were burnt by a candle left in his room, but I suppose he could recover them again." Dr. Newton, as we see above, gives this only as a report.

[1] I have not been able to discover what writings are here alluded to. They may have been his theological writings, such as his *Irenicum*, or, "Doctrines tending to Peace," which will be afterwards noticed.

"My wife at this time is brought to bed of a son, whom I intend to nominate after my dear deceased friend. Would you please to honour me so far as to substitute Dr. Stewkely to stand as witness. I should take it as a very singular favour, and would very much oblige, Sir, your most humble and obedient servant,

"HUMPHREY NEWTON.

"GRANTHAM, *January* 17, —$2\frac{7}{8}$."

After trying, for a month nearly, to recollect some other particulars respecting Newton, which he had been requested to do by Mr. Conduitt, he addressed to him the following letter:—

"SIR,—I return y^r^ honour a great many thanks for y^e^ favour you have done me in deputing Dr. Stewkley to stand in y^r^ stead as witness to my son. It is out of my sphere to make any grateful return, therefore doubt not but y^r^ goodness will in that point excuse my deficiency. I have bethought myself about Sir Isaac's life as much as possibly I can. About 6 weeks at spring, and 6 at y^e^ fall, y^e^ fire in the elaboratory scarcely went out, which was well furnished with chymical materials as bodyes, receivers, heads, crucibles, &c., which was made very little use of, y^e^ crucibles excepted, in which he fused his metals; he would sometimes, tho' but very seldom, look into an old mouldy book w^ch^ lay in his elaboratory, I think it was titled *Agricola de Metallis*, the transmuting of metals being his chief design, for which purpose antimony was a great ingredient. Near his elaboratory was his garden, w^ch^ was kept in order by a gardiner. I scarcely ever saw him do anything as pruning, &c. at it himself. When he has sometimes taken a turn or two has made a sudden stand, turn'd himself about, ran up y^e^ stairs like another Archimedes, with an εὕρηκα, fall to write on his desk standing, without giving himself the leisure to draw a chair to sit down on. At some seldom times when he designed to dine in y^e^ hall, would turn to the left hand and go out into the street, when making a stop when he found his mistake, would hastily turn back, and then sometimes instead of going into y^e^ hall, would return to his chamber again. When he read in y^e^ schools he usually staid about half an hour, when he had no auditors, he commonly returned in a 4th part of that time or less. Mr. Laughton who was then y^e^ library keeper of Trin. Coll. resorted much to his chamber; if he commenced Dr. afterwards I know not. His telescope, w^ch^ was at that time, as near as I could guess, was near 5 foot long, w^ch^ he placed at y^e^ head of y^e^ stairs going down into y^e^ garden, buting towards y^e^ east. What observations he might make I know not, but several of his observations about comets and y^e^ planets may be found scattered here and there in a book intitled *The Elements of Astronomy*, by Dr. David Gregory. He would with great acuteness answer a question, but would very seldom start one. Dr. Boerhaave (I think it is) Prof. Lips, in some of his writings, speaking of Sir Is.: That man, says he, comprehends as much as all mankind besides. In his chamber he walked so very much y^t^ you might have thought him to be educated at Athens among y^e^ Aristotelian sect. His brick furnaces, *pro re nata*, he made and altered himself without troubling a bricklayer. He very seldom sat by the fire in his chamber excepting y^t^ long frosty winter,[1] which made him creep to it against his will. I can't say I ever saw him wear a night gown, but his wearing clothes that he put off at night, at night do I say, yea rather towards y^e^ morning, he put on again at his rising. He never slept in y^e^ daytime y^t^ I ever perceived; I believe he grudged y^e^ short time he spent in eating and sleeping. Ἀνέχου καὶ ἀπέχου may well and truly be said of him, he always thinking with Bishop Saunderson, temperance to be the best physick. In a morning he seemed to be as much refreshed with his few hours' sleep as though he had taken a whole night's rest. He kept neither

[1] This was the famous frost of 1683-4, which began early in December, and continued without intermission till the 5th of February.

dog nor cat in his chamber, wh^{ch} made well for y^e old woman his bedmaker, she faring much y^e better for it, for in a morning she has sometimes found both dinner and supper scarcely tasted of, w^{ch} y^e old woman has very pleasantly and mumpingly gone away with. As for his private prayers I can say nothing of them ; I am apt to believe his intense studies deprived him of y^e better part. His behaviour was mild and meek, without anger, peevishness, or passion, so free from that, that you might take him for a stoick. I have seen a smal past-board box in his study set against y^e open window, no less as one might suppose than a 1000 guin. in it crowded edgeways, whether this was suspicion or carelessness I cannot say ; perhaps to try the fidelity of those about him. In winter time he was a lover of apples, and sometimes at a night would eat a small roasted quince. His thoughts were his books ; tho' he had a large study seldom consulted with them. When he was about 30 years of age his grey hairs was very comely, and his smiling countenance made him so much y^e more graceful. He was very charitable, few went empty handed from him. Mr. Pilkinton, who lived at Market Overton, died in a mean condition (though formerly he had a plentiful estate), whose widow with 5 or 6 children Sir Is. maintained several years together. He commonly gave his poor relations (for no family so rich but there is some poor among them), when they apply'd themselves to him, no less than 5 guineas, as they themselves have told me. He has given the porters many a shilling not for leting him (in?) at y^e gates at unreasonable hours, for that he abhorred, never knowing him out of his chamber at such times. No way litigious, not given to law or vexatious suits, taking patience to be y^e best law, and a good conscience y^e best divinity. Says Seneca, somebody will demonstrate which way comets wander, why they go so far from y^e rest of y^e celestial bodies, how big, and what sort of bodies they are, w^{ch} had he been contempory with Sir Is. he might have seen this prophecy of his fulfilled by y^e wonder of his age. Could y^r Hon^r pick somethings out of this indigested mass worthy to be inserted into y[e] life of so great, so good, and so illustrious a person as Sir Isaac Newton! it would be of infinite satisfaction to him, Sir, who is y[r] Hon[r's] most humb. and most obedient serv[t],

H. Newton.

"*Feb.* 14, 172⅔, Grantham."

After Newton had completed his optical researches, and brought to a close his controversy with the Dutch philosophers, he was called upon to direct his attention to a less congenial subject. His friend, Dr. Thomas Burnet, who had been Senior Proctor when Newton took his degree in 1668, and who, as we shall presently see, set him the noble example of resisting arbitrary power, had printed, in 1680, his *Theoria Telluris Sacra*,[1] an eloquent physico-theological romance, which not only received the warm approbation of the King, but was applauded by the poets, and, to a certain extent, adopted even by Newton. Abandoning, as some of the fathers had done, the hexaemeron, or six days of Moses, as a physical reality, and having no knowledge of geological phenomena, he gives loose reins to his imagination, combining passages of Scripture with those of ancient authors, and presumptuously describing the future catastrophes to which the earth is to be exposed. Previous to its publication, Burnet presented a copy of his book to Newton, and requested his opinion of the theory which it expounded. In a letter dated 24th December 1680, Newton sent him "some exceptions to particular passages," which elicited explanations from their author, and led him to propose new questions of a theological as well as of a scientific nature. To this letter of Burnet's, which was of great length, and dated January

1 "The Sacred Theory of the Earth, containing an account of the original of the Earth, and of all the general changes which it hath already undergone, or is to undergo, till the consummation of all things." The Latin edition was published in 4to in 1681, and at the King's request, it was translated into English, the first part, in folio, appearing in 1684, and the second in 1689.

13, 1680-81, Newton replied in one nearly as long,[1] and possessing a very considerable degree of interest. He treats of the formation of the earth, and the other planets, out of a general chaos,—of the figure assumed by the earth,—of the length of the primitive days,—of the formation of hills and seas,—and of the creation of the two ruling lights as the result of the clearing up of the atmosphere. He considers the account of the creation in Genesis as adapted to the judgment of the vulgar. "Had Moses," he says, "described the processes of creation as distinctly as they were in themselves, he would have made the narrative tedious and confused amongst the vulgar, and become a philosopher more than a prophet." After referring to several "causes of meteors, such as the breaking out of vapours from below, before the earth was well hardened, the settling and shrinking of the whole globe after the upper regions or surface began to be hard," Newton closes his letter with an apology for its being tedious, which, he says, "he has the more reason to do, as he has not set down any thing he has well considered, or will undertake to defend."[2]

The primitive condition of the earth, and its preparation for man, was a subject of general speculation at the close of the seventeenth century. Leibnitz, like his great rival, attempted to explain the formation of the earth, and of the different substances which composed it; and he had the advantage of possessing some knowledge of geological phenomena. The earth he regarded as having been originally a burning mass, whose temperature gradually diminished till the vapours were condensed into a universal ocean which covered the highest mountains, and gradually flowed into vacuities and subterranean cavities, produced by the consolidation of the earth's crust. He regarded fossils as the real remains of plants and animals which

[1] The copy of this letter, which I have found along with the last of Burnet's, among the Portsmouth papers, is in Newton's own hand, but has no date or signature. The two first letters of the correspondence I have not met with.

[2] As this letter is very interesting, I have given it in the APPENDIX, No. IV.

had been buried in the strata; and, in speculating on the formation of mineral substances, he speaks of crystals as the geometry of inanimate nature.[1]

While Newton was thus speculating on geology, we find him in communication with Flamsteed, through their mutual friend, Mr. Crompton of Cambridge, on the subject of the comet of 1680, and, in so far as we are informed by the two letters of Newton, published in the General Dictionary, we have, in a preceding chapter,[2] given an account of the object and results of their correspondence. Since that chapter was printed, however, we have obtained the originals and draughts of these letters, and of other three, which passed between Newton and Flamsteed on the same subject.[3] In an unpublished memorandum, dated the Observatory, December 15, 1680, Flamsteed mentions that he had not seen the comet before sunrise in November, but that it was seen by Cuthbert; and that by what he learned from others, he concluded, that having passed the sun, it would appear in December after sunset. He accordingly discovered it on the Friday before the 11th of December; and, in another memorandum, dated January 3d, he sends to his correspondent (Crompton probably) the observation he has made. He tells him that the tail, which was 35° long, is a little curved and best defined on the left hand, and he asks his opinion on a conjecture, that the comet may be "a consuming substance, much decayed, and the fuel spent which nourished its blaze." In the first of these unpublished letters addressed

[1] These views of Leibnitz are contained in his *Protogæa*, an Essay which he published in the Leipsic Journal for 1683. It was published separately at Göttingen by Scheidius in 1749. See the *Acta Eruditorum*, 1717.

[2] See vol. i. chap. xii. p. 263. One of these letters is addressed to Crompton, and the other to Flamsteed. This last letter is dated 1680 in place of 1681, in the *General Dictionary*, vol. vii. p. 791.

[3] I find among these papers a table showing the R. ascension, declination, and culmination of the comet, from December 16, 1680, to February 1, 1681, as made in Maryland, America, in west longitude 75°, and north latitude 38° 30′, by Mr. Arthur Storer, a nephew of Dr. Babington, at the river Patuxant, near Hunting Creek. See Newtoni *Opera*, tom. iii. p. 145; *Principia*, lib. iii. prop. xli.

to Crompton,[1] Flamsteed transmits, for Newton's information, the observations made by Gallet at Rome, which Cassini had sent to Halley, others made at Rome by Maria Antonio Cellio, sent also by Halley from Paris, and an observation made at Canterbury by one Hill,[2] an artificer, with an instrument four feet radius; and, from a comparison of these, he proves, in opposition to the opinions of Newton and Cassini, that there were not two comets.[3] We have already seen[4] that Newton at one time believed that comets moved in straight lines. This opinion seems to have been adopted from Flamsteed, who says, in the continuation of his letter, "By this indented figure Mr. Newton will see that the comet ran up towards the sun nearly in a straight line, and returned from him in a like one, for the places of these lines, however altered, will not remove far from where I have designed them, except he will suppose the acceleration of its motion in its progress, and retardation in its return, to be much different from what I have made it, which also I am apt to think he will find not likely." He then proceeds to give the following views respecting comets, and the structure of the sun:—

"Hence it should seem that the comet was attracted and repelled by the sun, as I imagined and proposed in my last letter. Mr. Newton brings an experiment of a red-hot magnet not attracting iron, or a cold magnet, or red-hot iron, against which I have this to assume, that the attraction of the sun may be of a different nature, and that I even suppose the sun to be not like a mass of red-hot iron, but *a solid globe of gross matter*, encompassed with a spirituous liquid, which, by its violent motion, striking the particles of the air, causes the heat we

[1] March 7, 1681.

[2] I found half of Hill's letter to Flamsteed, dated Canterbury, Dec. 29, 1681, containing observations on the comet in November 11, 1680, and January 3, and February 3, 1681.

[3] Newton afterwards acknowledged, in the *Principia*, the correctness of Flamsteed's opinion.

[4] See vol. i. p. 264, note 2.

feel from him sometimes so intolerable ; for, if I remember right, I have read in the journals of travellers, that when they have travelled over high mountains in hot countries, they have found the heat far less than in the valleys, and that *the substance of the sun is terrestial matter, his light but liquid menstruum encompassing him*, the phenomena of the spots I think will prove. Admit this, and it will still follow that he may attract the comet and swell it, as the observations evidence he did. Mr. Halley thinks the comet to be a body that has lost its principle of gravitation, and yet I perceive would have it attracted by the sun, which I cannot assent to, for then I see no reason why its mass should not dissipate, and the atoms composing it separate themselves and scatter over the wide æther."

In the conclusion of this long letter, Flamsteed answers very fully the queries of Newton respecting the state through which the tail of the comet passed every day, and the alterations he observed in its head ; and he sends him the observations made by Cellio, and by himself at Greenwich, from Dec. 12, 1680, to Feb. 5, 1681.

In Newton's *published* reply to this letter,[1] he endeavours to reconcile the various observations with his idea of two comets, and transmits some of his own, made with a three-feet perspective, having a great magnifying power ; but he takes no notice whatever of Flamsteed's speculations on the sun, and on the action of heated magnets. I found, however, the original scroll of the letter, which is quite different from the one actually sent, and which contains, on a separate sheet, a long discussion of Flamsteed's hypothesis, which possesses considerable interest.[2] The other letters of Flamsteed were addressed to Newton while he was writing and printing the *Principia*, and contain many instructive details. They complete the in-

[1] April 16, 1681. *General Dictionary*, vol. vii. p. 791.

[2] This portion of the letter seems to have been intended to be sent to Flamsteed through Crompton. See APPENDIX, No. V.

teresting correspondence, a part of which had been long ago published in the General Dictionary.[1]

When Newton was engaged in writing the second and third book of the *Principia*, an event occurred at Cambridge which drew him from his seclusion, and placed him in a noble position on the theatre of public life. Desirous of re-establishing the Catholic religion in its former supremacy, James II. had begun to tamper with the rights and privileges of his Protestant subjects, and to sap the foundations of the Established Church. He had in 1686, and in open violation of the laws, conferred the Deanery of Christchurch, an office of the highest dignity in Oxford, upon John Massey, a person who had no other qualification than that of being a member of the Church of Rome; and boasting of this exercise of power, he told the Pope's nuncio, that "what he had done at Oxford would very soon be done at Cambridge."

Before making this attempt, however, he tried his apprentice hand upon an inferior institution, which was more likely to comply with a royal demand. He sent a *mandamus* to the Masters and Governors of the Charterhouse, requiring them to admit as a pensioner into the hospital under their care, an old Catholic gentleman of the name of Popham. The Board of Governors, before whom this mandate was laid, consisted of the Lord Chancellor Jeffrys, the Archbishop of Canterbury, the Bishop of Winchester, the Earl of Danby, the Earl of Rochester, the Earl of Mulgrave, and Mr. Thomas Burnet, the Master. By the rule of election, in giving their votes, the Master, as the humblest of the electors, speaks first. In the exercise of this privilege, Mr. Burnet, a clergyman of high character, stated to the Board, that the mandate to elect a Catholic was contrary

[1] All the published letters, except one, are from Newton to Flamsteed; and this one from Flamsteed to Newton, dated Sept. 25, 1685, is very different from the one published, which must have been printed from a scroll, and greatly altered by Flamsteed. The unpublished letters, six in number, were written between December 1684 and October 1686.

both to the will of the founder and an Act of Parliament. "What is that to the purpose?" said one of the Governors. "It is very much to the purpose, I think," replied a voice feeble with age,[1] "an Act of Parliament is, in my judgment, no light thing." A vote was then taken, which stood thus ;—

AGAINST THE MANDATE.	FOR THE MANDATE.
Thomas Burnet, Master.	Chancellor Jeffrys.
Archbishop of Canterbury.	Earl of Rochester.
Bishop of Winchester.	Earl of Mulgrave.
Earl of Danby.	

On the rejection of the royal candidate, Jeffrys decamped in a rage, and being followed by others of the minority, a quorum was not left to make a formal reply to the royal mandate.[2]

This defeat of the king might have induced him to pause in his threatened attack upon the University of Cambridge. He had supposed, however, that the heads of colleges, and the other members of the senate, would submit more readily to his power than the Peers and Bishops who governed the Charterhouse, and he accordingly issued a mandate in February 1687, directing that Father Alban Francis, a Benedictine monk, should be admitted a Master of Arts without taking the oaths of allegiance and supremacy. Upon receiving the mandamus, Dr. Pechel, Master of Magdalen College, who was then Vice-chancellor, sent a messenger to the Duke of Albemarle the Chancellor, to request him to get the mandamus recalled ; and the registrary and bedells waited upon Francis to offer him instant admission, if he took the necessary oaths. The king and the monk were equally inexorable. The Chancellor was received coldly and ungraciously ; and Francis, after refusing to subscribe the oaths, took horse and hastened to the palace to make known his disappointment.

[1] Mr. Macaulay says, and no doubt on good authority, that this was the venerable Duke of Ormond. I have followed, in the list of governors present, a manuscript account of the meeting, which was sent to Sir Isaac Newton, and which contains the names of those who voted for and against the mandate.

[2] See Macaulay's *Hist. of England*, vol. ii. pp. 293, 294.

Thus placed in open collision, the Court and the University used every means to support their cause. The royal commands had hitherto been obeyed; and when foreign princes or their ambassadors visited the Universities, the honour of a degree was invariably conferred upon them: Even the ambassador of the Emperor of Morocco, though a Mahometan, had received this distinction. On the part of the University the difference between honorary degrees to foreigners, and ordinary degrees to residents, was strongly urged. As every Master of Arts has a vote in the Senate, a majority in that court might be obtained by the admission of Catholic priests, and the Protestant character of the University overturned. Influenced by views like these, and fortified by charters and statutes, and by the best legal opinions, the Senate unanimously refused to obey the king. A menacing letter from Sunderland was despatched to shake the firmness of the University; but though humble and respectful explanations were sent in reply, no hope of compliance was breathed, and no compromise proffered to the Crown. In consequence of these proceedings, the Vice-chancellor and deputies from the senate were summoned before the new High Commission at Westminster on the 21st of April.

The deputation appointed by the senate consisted of Mr. Newton, Mr. Stanhope, the Chancellor of Ely, and other six deputies; but before they went to London, they held a previous meeting, in order to prepare their explanations and defence for the Court. "Some feeble or false men," as Burnet calls them, "had proposed to grant the degree on the condition that it should not be drawn into a precedent, and this contemptible proposal had recommended itself to the Chancellor of Ely." He accordingly produced a paper, which he hoped the other deputies would sign, and in which this measure was presented in the most plausible form. A disposition to approve of it was manifested by the other deputies; but Newton seeing the character of the compromise, rose from his chair, took two or three turns round the room, and addressing the University bedell, then standing at the fire, said to him, "This is giving up the question." "So it is," replied the bedell, "why didn't you go and speak to it?" Upon which Newton went to the table, expressed his opinion, and proposed that the paper should be shown to counsel. This suggestion was adopted. The paper was submitted "to Mr. Finch, afterwards solicitor to Lord Guernsey," and when he had given the same opinion as Newton, the Chancellor of Ely and the rest of the deputies concurred.[1]

On the 21st of April the Council-chamber was filled with a large assemblage. Jeffrys presided at the board, and the Earl of Mulgrave, the sceptic and the hypocrite sat there, a worthy companion to the judge. The members of the deputation appeared as a matter of form before the Commissioners, and were dismissed. On the 27th of April they gave in their plea. On the 7th of May it was discussed and feebly defended by their incompetent Vice-chancellor. The deputies maintained, that in the late reign several royal mandates had been withdrawn, and that no degree had ever been conferred without the oaths of supremacy and obedience being taken. Jeffrys let loose his insolence against the timid Vice-chancellor, silenced the other deputies when they offered to speak, and without a hearing ordered them out of Court. When recalled the deputies were reprimanded. Pechel was deprived of his office as Vice-chancellor, and of his emoluments as Master of Magdalen College, and the following words closed the address of Jeffrys :—"Therefore I shall say to you what the Scripture says, and rather because most of you are divines, 'Go your way and sin no more, lest a worse thing come unto you.'"[2]

[1] This interesting anecdote I found in a manuscript of Mr. Conduitt, intended for insertion in his proposed Life of Newton.

[2] "The Chancellor Jeffrys," says Mr. Edleston, "alluded twice to his having himself formerly been a member of the University. Until some other College can establish a claim to him, Trinity College is liable to the suspicion of having had him for an *alumnus*. A 'Georgius Jeffreys' was admitted pensioner there March 15th, 1661-2, under Mr. Hill, and he would therefore be a year junior to Newton."—*Correspondence*, &c. p. viii. note 90.

Under this rebuke, and in front of such a judge, the vilest and most ferocious that ever sat upon the judgment-seat, stood the immortal author of the *Principia*, who had risen from the invention of its problems to defend the religion which he professed, and the University which he loved and adorned. The mandate which he resisted—a diploma to a monk—was in one sense an abuse of trivial magnitude, unworthy of the intellectual sacrifice which it occasioned ; but the spark is no measure of the conflagration which it kindles, and the arm of a Titan may be required to crush what the touch of an infant might have destroyed.

Deprived of their vice-chancellor, the University chose for his successor John Balderston, Master of Emanuel College, "a man of much spirit," who promised to his constituents at his election, that while he held office neither religion nor the rights of the body should, through his means, be invaded.[1] Thus unanimously and nobly defended, Protestantism was now firmly established, the rights of the University protected, and the Court taught a lesson by which it had not the wisdom to profit.[2] The University of Oxford, however, drew instruction from the wisdom of its younger sister, and in the noble stand which, in the case of Magdalen College,[3] she made against a similar abuse of power, she triumphed over the tyrant that assailed her, and contributed to his fall.

Under their Protestant constitution, the Universities of England have risen to a distinguished place among the literary and scientific institutions of Europe ; and though attempts have been recently made in Oxford to tamper with the national faith,

[1] See Burnet's *Hist. of his Times*, vol. ii. p. 697, or 8vo edit. vol. iii. p. 149.—Macaulay's *Hist. of England*, vol. ii. p. 180.

[2] Dr. Pechel was restored to his offices on the 24th of October 1688. "After the Revolution he starved himself to death, in consequence of having been rebuked by Archbishop Sancroft for drunkenness and other loose habits; and after four days' abstinence, would have eaten, but could not."—Note of Lord Dartmouth upon Burnet's *Hist.* vol. ii. p. 698, or vol. iii. 8vo, p. 150.

[3] See Macaulay's *Hist.* &c. vol. ii. p. 287, &c.

we trust that the new system of government which Parliament has provided, will protect her youth against religious innovation, and obtain for them a course of instruction in which science as well as literature shall be taught.

In our Scottish Universities, once favoured by the Sovereign, and honoured by distinguished names, we would desire to see some approximation, in character and endowment, to our English Institutions. Although the Scottish Commissioners provided, in the Treaty of Union, for the maintenance of their Colleges, their endowments have been permitted to decay—their rights and privileges, protected by ancient charters, have been invaded by the Crown—incompetent Professors, the creatures of political subserviency, have, by royal and private patronage, been appointed to their most important chairs; and the sons of the nobility and gentry of the land have been driven to complete their education in the schools and universities of England.

From the precincts of the High Court of Commission, Newton returned to Trinity College to complete the *Principia*, and in the course of six weeks, in the month of June 1687, this great work was given to the public.[1]

At the time when Flamsteed was supplying Newton with observations for the *Principia*, Halley was carrying on that interesting correspondence, of which we have published all the letters that had at that time been found.[2] I have been so fortunate, however, as to discover the letters of Halley which were wanting, and which add greatly to the value of the collection. The last of them possesses a peculiar interest, from being the one in which Halley announces to Newton the completion of the *Principia*, and gives him notice of the copies of the work which he despatched to Cambridge:—

[1] When the late Duke of Somerset, as his Grace informed me, visited the Marquis de Laplace at Arcueil, he found him in his study dressed in a sort of uniform, prepared to go to the Senate. Having in his hand the first edition of the *Principia*, he said to the Duke, "This is the best book that was ever written"

[2] See vol. i. p. 269, and APPENDIX, vol. i. No. VIII.

"London, *July* 5, 1687.

"Honoured Sir,—I have at length brought your book to an end, and hope it will please you. The last errata came just in time to be inserted. I will present from you the book you desire to the Royal Society, Mr. Boyle, Mr. Paget, Mr. Flamsteed, and if there be any else in town that you design to gratify that way; and I have sent you to bestow on your friends in the University 20 copies, which I entreat you to accept. In the same parcel you will receive 40 more, which having no acquaintance in Cambridge, I must entreat you to put into the hands of one or more of your ablest booksellers to dispose of them. I intend the price of them, bound in calves' leather, and lettered, to be 9 shillings here. Those I send you I value in quires at 6 shillings, to take my money as they are sold, or at 5sh. for ready, or else at some short time; for I am satisfied there is no dealing in books without interesting the booksellers; and I am contented to let them go halves with me, rather than have your excellent work smothered by their combinations. I hope you will not repent you of the pains you have taken in so laudable a piece, so much to your own and the nation's credit, but rather, after you shall have a little diverted yourself with other studies, that you will resume those contemplations wherein you had so great success, and attempt the perfection of the lunar theory, which will be of prodigious use in navigation, as well as of profound and public speculation. Sir, I shall be glad to hear that you have received the books, and to know what farther presents you would wish in town, which shall be accordingly done. You will receive a box from me on Thursday next by the waggon, that starts from town to-morrow. I am your most obliged humble servant,

"Edm. Halley.

"To Mr. Isaac Newton,
In Trinity Colledg. Cambridg.—These."

The active and influential part which Newton had taken in defending the privileges of the University, more than his high scientific attainments, not yet sufficiently appreciated even at Cambridge, induced his friends to bring him forward as a candidate for a seat in the Convention Parliament. The other candidates were Sir Robert Sawyer and Mr. Finch. Newton was elected by a majority of five over Mr. Finch,[1] and he sat in Parliament from January 1689 till its dissolution in February 1690.

Thus launched into public life from the seclusion of a college, and residing in London away from his books and instruments, Newton abandoned for a time his scientific researches, devoting himself, when free from parliamentary duty, to theological studies, and looking forward to some higher station in the University, or some permanent appointment from the Government. As a member of the Legislature at an eventful epoch in the history of England, he conducted himself with firmness and moderation, maintaining the principles of civil and religious liberty,[2] and exhibiting a capacity for business which could scarcely have been expected from a philosopher who had mixed so little with society. During the thirteen months that he sat in the House of Commons, he seems to have taken no share in the debates or in the business of the House. On the 30th April 1689, he moved for leave to bring in a bill to settle the charters and privileges of the University of Cambridge, and Sir Thomas Clarges did the same for Oxford, yet neither of them seems to have made any speech on the occasion. But though a silent he was an active member, and it appears from his

[1] The votes stood thus :—

Sir Robert Sawyer, ...	125
Mr. Newton, ...	122
Mr. Finch, ...	117

In some of the voting papers he is called præclarus vir, and in others, doctissimus, integerrimus, venerabilis et reverendus.—Edleston's *Correspondence*, &c., p. lix.

[2] In referring to the publication of the *Principia*, Laplace remarks "that the principles of the social system were laid in the following year, and that Newton concurred in their establishment."—*Système du Monde*, p. 372. Edit. 1824.

letters to Dr. Covel,[1] the Vice-chancellor, that he had an onerous duty to perform to his constituents as well as to the Government. The friends of James were still numerous at Cambridge. Disturbances had broken out at the end of the year, and so "many scholars were in arms," that the Vice-chancellor was obliged to address the heads of Colleges on the subject. Considering the effect of these disturbances "as very dangerous to the University, as well as destructive to all good manners, he conceived that the best course to reduce them would be to convene the students in some place of the College next morning, if they returned, and gravely but calmly advise them to all civil behaviour, believing all severity at this juncture might rather tend to exasperate them more, and bring the unruly people's fury upon us all."[2]

Some of the members of the University, who had lately sworn allegiance to the exiled king, had some difficulty in vowing fidelity to his successor, and it required more sagacity to deal with conscientious scruples than with positive discontent. On the 12th of February, the day after King William and Queen Mary were proclaimed at Whitehall, Newton intimated to the Vice-chancellor that he would soon receive an order to proclaim them at Cambridge. He enclosed a form of the proclamation, and "heartily" expresses "the wish that the University would so compose themselves as to perform the solemnity with a reasonable decorum; because I take it to be their interest to set the best face upon things they can, after the example of the London divines." He advises Dr. Covel to grant no degrees till he is authorized to administer the new oaths, and when they are administered, to administer them in English.

In replying to this letter, Dr. Covel seems to have suggested some arguments that might be employed to remove the scruples of "the dissatisfied part of the University," and in order that

[1] *Thirteen Letters from Sir Isaac Newton to Dr. Covel,* printed in 1848 by Dawson Turner, Esq., from the originals in his possession.

[2] *Thirteen Letters,* &c., pp. 9, 10.

he might "have a fuller argument for convincing them," Newton sends him his views upon the subject, as "he cannot do the University better service than by removing the scruples of as many as have sense enough to be convinced with reason." He then lays down three propositions, the illustrations of which will be found in the letter itself, which we cannot withhold from the reader.[1] Faith and allegiance, he says, are due to the king by the law of the land, and were it "more than what the law requires, we should swear ourselves slaves, and the king absolute, whereas by the law we are freemen, notwithstanding these oaths." . . . "Allegiance and protection are always mutual, and therefore when King James ceased to protect us, we ceased to owe him allegiance by the law of the land. And when King William began to protect us, we began to owe him allegiance." . . . "If the dissatisfied party accuse the convention for making the Prince of Orange king, 'tis not my duty to judge those above me, and therefore I shall only say that if they have done ill, *Quod fieri non debuit, factum valet.* And those at Cambridge ought not to judge and censure their superiors, but to obey and honour them according to the law, and the doctrine of passive obedience."[2]

During his residence in London, Newton became acquainted early in 1689 with John Locke, whom he doubtless met at the weekly parties given by his friend Lord Pembroke, "for the purpose of conversation and discussion." Locke had taken a great interest, as we have already seen,[3] in the sublime truths demonstrated in the *Principia*, and lived on the most affectionate terms with its author till the time of his own death. In

[1] See Appendix, No. VI. In the library of Queen's College, Oxford (cclxxxiv. fol. 143), there is a paper entitled "Reasons given for the taking the oaths of allegiance to King William, by I. N." This is doubtless an extract from Newton's letter to Covel.

[2] Newton appears not to have enjoyed good health during his residence in London. He was confined to his room for some days in the middle of March, and in May he was attacked by "a cold and bastard pleurisy." His address was "at Mr. More's house, in the broad century at the west end of Westminster Abbey."

[3] See vol. i. p. 296.

the summer of the same year, Newton had the gratification of becoming personally acquainted with Christian Huygens, one of the most illustrious of his contemporaries. At the meeting of the Royal Society on the 12th of June, each of them addressed the members,—Huygens on the subject of gravity, of which he knew little compared with Newton, and Newton on the subject of the double refraction and polarisation of Iceland crystal, of which he knew little compared with Huygens.[1]

We have already mentioned that Newton and his friends were looking out for some public situation worthy of his acceptance. While living in London he no doubt experienced the unsuitableness of his income to the new position in which he was placed. He had made nothing by his writings; and with a generous disposition, to which frequent appeals were made by some of his less wealthy relatives, he must have felt unselfishly the bitterness of poverty, nor was that feeling diminished by the consideration that his academical contemporaries, whom he had outstripped in talent, were occupying the highest positions in the Church or at the Bar, or basking in the more genial sunshine of official ease.

The death of the Provost of King's College, Cambridge, gave his friends an opportunity, not wisely embraced, of showing their disposition to serve him. The King had issued a mandamus commanding the College to choose Mr. Upman, Fellow of Eton, but an outcry having been raised against him for having preached in favour of King James's Declaration of Indulgence, a new mandamus was issued in favour of Mr. Newton. The College, however, resisted his appointment, as it was required by the statutes that the Provost should be in priest's orders, and chosen from among the Fellows of the Society.[2] His appointment, therefore, would have been contrary to law; and when, on the 29th of August 1689, the case was heard before the King and Council, he was found to be disqualified for the

[1] See vol. i. p. 187.

[2] Cole's MSS., vol. xvi. folio 250.

office.[1] In consequence of this disappointment the friends of Newton were more solicitous to serve him. The Parliament was dissolved on the 6th of February, and at the new election Newton was not returned.

On his way to Cambridge, he had spent some time, along with Locke, at Sir Francis and Lady Masham's at Oates, and as he had then no occupation but that of the Lucasian Chair, a public provision for him must have been there a topic of discussion. Locke had interested in his favour Lord and Lady Monmouth, and in a letter to him, dated October 28, 1690, he requests Locke to thank them for their kind remembrance of him, and speaks of his obligations to them "whether their design succeeded or not." The office which they had in view was probably that of Comptroller of the Mint, for we find him in the following year thanking Locke "heartily for being so mindful of him, and ready to assist him with his interest," and asking him for the "scheme he has laid of managing the Comptroller's place of the Mint."[2]

In the same year an attempt was made to obtain for Newton the Mastership of the Charterhouse, but he disliked the project, and seems to have been inactive in the matter. Locke put him in mind of it, and drew from him the reply, "that he saw nothing in the situation worth making a bustle for. Besides a coach," he adds, "which I consider not, 'tis but £200 per annum, with a confinement to the London air, and to such a way of living as I am not in love with, neither do I think it advisable to enter into such a competition as that would be for a better place."[3] After these repeated failures, he seems to have thought that his friends were inactive, if not insincere; and he does not scruple to tell Locke "that he is fully convinced that Mr. Montague, upon an old grudge which he thought had been worn out, was false to him, and that he had done with him, intending to sit still unless my Lord Monmouth

1 Edleston's *Correspondence*, &c., p. lix. note 96.

2 June 30th, 1691.

3 December 13, 1691.

was still his friend."[1] Though assured by Locke, in reply, that Lord Monmouth was still his friend, he expressed his happiness at the intelligence, and stated in his answer[2] that "his inclinations were to sit still," and that he intended not to give his Lordship and him any farther trouble.[3]

We do not envy the reader who peruses these simple details without a blush of shame for his country. That Locke, and Lord Monmouth, and Charles Montague, could not obtain an appointment for the author of the *Principia*, will hardly be believed in any country but our own. Had he been ambitious of honours, to which the philosophers of other lands have since his time attained, or had he aimed at those official positions to which merit has no claim in England, we might have felt a modified sympathy in his failure; but in aspiring only to the presidency of a college, to the mastership of a school, or to an inferior office in the Mint, and obtaining none of them, we participate in that depth of feeling which the language we have quoted so clearly indicates. The ingratitude of his country disturbed, as we shall see, the tranquillity of a mind sensitively organized, and intellectually overwrought. At the age of fifty, the high priest of science found himself the inmate of a college, and, but for the generous patronage of a friend, he would have died within its walls.

While Newton was discharging his duties in Parliament, he experienced a severe domestic affliction in the loss of his mother. The anxious and tender care with which she had watched over his helpless infancy, and reared to a vigorous manhood her only and sickly child, had produced, on his part, an attachment more than filial, while she had followed, with a mother's pride, the rising reputation of her son. In 1689, Benjamin Smith, the half-brother of Newton, had been seized,

[1] Jan. 26, 1691-2. [2] Feb. 16, 1691-2.

[3] In these letters, which are published in Lord King's *Life of Locke*, Edit. 1830, vol. i. pp. 400-411, there are interesting details about Newton's *Historical account of two notable corruptions of Scripture*, to which we shall return when we treat of his theological writings.

while at Stamford, with a malignant fever. His mother, who had hastened to attend his sick-bed, was taken ill with the same complaint, and Newton left his duties and his studies to watch at her couch. He sat up with her whole nights, administered with his own hands the necessary medicines, and prepared and dressed her blisters with all the dexterity of a practitioner. His skill, however, was unavailing. She sank under the disease, and her remains were carried to Colsterworth, and deposited in the north aisle of the church, where the family had generally been interred.

After the dissolution of the Convention Parliament, Newton had resumed his philosophical and mathematical studies. In July 1691, he drew up the directions to Dr. Bentley, to enable him to understand the *Principia*.[1] In introducing to Flamsteed Mr. David Gregory, whom he had recently recommended to the vacant chair of astronomy at Oxford, he mentions his anxiety to have his observations on Jupiter and Saturn for the next twelve or fifteen years, adding, "If you and I live not long enough, Mr. Gregory and Mr. Halley are young men;" and he expressed an anxiety to know "if in long telescopes the light of Jupiter's satellites, before they disappear, incline either to red or blue, or become more ruddy or more pale than before."[2] One of his occupations at this time was drawing up for Wallis his explanation of fluxions and fluents, in two problems, with illustrations, being the first account of the new calculus published by himself.[3] Wallis had requested him to give an explanation of the two methods, namely, of finding fluxions and fluents, which he concealed in transposed letters in his epistle to Oldenburg; and it was in obedience to this request that he sent him his account of fluxions. While Wallis's volume was in the press, Leibnitz had addressed a letter[4] to

[1] See vol. i. APPENDIX No. XI., p. 420. [2] Baily's *Flamsteed*, p. 129.

[3] Wallisii *Opera*, vol. ii. pp. 391-396. This communication was contained in two letters, dated August 27, and September 17, 1692.

[4] Dated $\frac{7}{17}$ March 1693, published in Raphson's *Fluxions*, pp. 119, 120.

Newton, in which he mentioned his expectation of receiving from him something great on the subjects of tangents and quadratures; but especially, what he particularly wished, his method of reducing quadratures to the rectifications of curves. In consequence, however, of having mislaid this letter, Newton wrote to him the day after he found it,[1] apologizing for the delay, and transmitting the method he requested. He mentions to him also that he had sent to Wallis a brief explanation of his method of fluxions, which he had previously concealed, and expressed the hope that he had written nothing which would be displeasing to him. "But," he added, "if he found in it anything worthy of reprehension, he hoped he would signify it to him in writing, as he valued friends more than mathematical inventions."

While Newton was corresponding with Locke in 1692, the process of Boyle for "multiplying gold," by combining a certain red earth with mercury, became the subject of discussion. Mr. Boyle having "left the inspection of his papers" to Locke, Dr. Dickison, and Dr. Cox, Mr. Locke became acquainted with the particulars of the process we have referred to. Boyle had, before his death, communicated this process both to Locke and Newton, and procured some of the red earth for his friends. Having received some of this earth from Locke, Newton tells him, that, though he has "no inclination to prosecute the process," yet, as he had "a mind to prosecute it," he would "be glad to assist him," though "he feared he had lost the first and third of the process out of his pocket." He goes on to thank Locke for "what he communicated to him out of his own notes about it," and adds, in a postscript, that "when the hot weather is over, he intends to try the beginning (that is the first of the three parts of the recipe), though the success seems improbable."[2]

[1] This letter is dated Cambridge, $\frac{16}{26}$ October 1693, and is published in Edleston's *Correspondence*, &c., Appendix, No. xxiv. p. 276.

[2] This letter, of which there is only a fragment, is dated Cambridge, July 7, 1692, and is published in Edleston's *Correspondence*, &c., Appendix, No. xxiii. p. 275.

In Locke's answer of the 26th July,[1] he sends to Newton a transcript of two of Boyle's papers, as he knew he wished it; and it is obvious from their letters, that both of them were desirous of "multiplying gold." In Newton's very interesting reply[2] to this communication, he "dissuades Locke against incurring any expense by a too hasty trial of the recipe." He says, that several chemists were engaged in trying the process, and that Mr. Boyle, in communicating it to himself, "had reserved a part of it from my knowledge, though I knew more of it than he has told me." This mystery on the part of Boyle is very remarkable. In "offering his secret" to Newton and Locke, he imposed conditions upon them, while, in the case of Newton at least, he did not perform his own part in the arrangement. On another occasion, when he communicated two experiments in return for one, "he cumbered them," says Newton, "with such circumstances as startled me, and made me afraid of any more." It is a curious fact, as appears from this letter, that there was then a Company established in London to multiply gold by this recipe, which Newton "takes to be the thing for the sake of which Mr. Boyle procured the repeal of the Acts of Parliament against multipliers." The pretended truths in alchemy were received by men like Boyle on the same kind of evidence as that by which the phrenology and clairvoyance of modern times have been supported. Although Boyle possessed the golden recipe for twenty years, yet Newton could not find that he had "either tried it himself, or got it tried successfully by anybody else; for," he says, "when I spoke doubtingly about it, he confessed that he had not seen it tried, but added, *that a certain gentleman was now about it, and it succeeded very well so far as he had gone, and that all the signs appeared, so that I needed not doubt of it.*"

[1] I have given this unpublished letter in the APPENDIX, No. VII.

[2] August 2, 1692, published in King's *Life of Locke*, vol ii. pp. 410-414.

CHAPTER XVII.

Newton's health impaired—The Boyle Lectures by Bentley, who requests Newton's assistance—Newton's first Letter to Bentley on the formation of the Sun and Planets —His second Letter—Rotation of the Planets the result of Divine Power—His third Letter—Hypothesis of Matter evenly diffused—Letter of Bentley to Newton—Reply to it by Newton in a fourth Letter—Opinion of Plato examined—Supposed mental illness of Newton ascribed to the burning of his MSS.—Referred to in the Letters of Huygens and Leibnitz—Made public by M. Biot—mentioned in the Diary of Mr. de la Pryme—The Story referred to disproved—Newton's Papers burnt before 1684—Newton's Letter to Mr. Pepys—Letter of Mr. Pepys to Mr. Millington—Mr. Millington's Reply—Mr. Pepys' second Letter to Mr. Millington—Newton solves a Problem in Chances—His letter to Locke—Reply of Locke—Newton's Answer, explaining the cause of his illness—His Critical Letter to Dr. Mill—His mind never in a state of derangement, but fitted for the highest intellectual efforts.

In the autumn of 1692, when Newton had finished his letters on fluxions, he did not enjoy that degree of health with which he had so long been favoured. The loss of appetite and want of sleep, of which he now complained, and which continued for nearly a twelvemonth, could not fail to diminish that mental vigour, and that "consistency of mind," as he himself calls it, which he had hitherto displayed. How far this ailment may have arisen from the disappointment which he experienced in the application of his friends for a permanent situation for him, we have not the means of ascertaining, but it is impossible to read his letters to Locke, and other letters from his friends, without perceiving that a painful impression had been left upon his mind, as well as upon theirs. This state of his health, however, did not unfit him for studies that required perhaps more profound thought than his letters on fluxions and fluents, for it was at the close of 1692, and during the first two months of

1693, that he composed his four celebrated letters to Dr. Bentley.[1]

Upon the death of the celebrated Robert Boyle, who died on the 30th December 1691, it was found that, in a codicil to his will, he had left £50 per annum to establish a lectureship, in which eight discourses were to be preached annually in one of the churches of the metropolis, in illustration of the evidences of Christianity, and in opposition to the principles of infidelity. Dr. Bentley, then chaplain to the Bishop of Worcester, and a very young man, was appointed to preach the first course of sermons, and the manner in which he discharged this important duty gave the highest satisfaction not only to the trustees of the lectureship, but to the public in general. In the first six lectures Bentley exposed the folly of atheism even in reference to the present life, and derived powerful arguments for the existence of a Deity from the faculties of the soul, and the structure and functions of the human frame. In order to complete his plan, he proposed to devote his seventh and eighth lectures to the demonstration of a Divine Providence from the physical constitution of the universe, as established in the *Principia*. To qualify himself for this task, he received from Sir Isaac Newton directions respecting a list of books necessary to be perused previous to the study of that work;[2] and having made himself master of the system which it contained, he applied it with irresistible force of argument to establish the existence of an overruling mind. Previous to the publication of these lectures, Bentley encountered a difficulty which he was not able to solve, and he transmitted to Sir Isaac, during 1692, a series of queries on the subject. This difficulty occurred in an argument urged by Lucretius, to prove the eternity of the world

[1] These letters, which were first printed by Richard Cumberland in 1756, and reviewed by Dr. Samuel Johnson in the *Literary Magazine*, vol. i. p. 89, have been reprinted in Dr. Horsley's Newtoni *Opera*, vol. iv. pp. 429-442; and in Nichol's *Illustrations of the Literary History of the Eighteenth Century*, vol. iv. pp. 50-60; but in both these works, the *third* and *fourth* letters are transposed, as their dates will show.

[2] See vol. i. Appendix, No. XI. p. 420.

from a hypothesis of deriving the frame of it, by mechanical principles, from matter endowed with an innate power of gravity, and evenly scattered throughout the heavens. Sir Isaac willingly entered upon the consideration of the subject, and transmitted his sentiments to Dr. Bentley in the four letters which we have mentioned.

In the *First*[1] of these letters Sir Isaac informs him, that when he wrote his treatise about our system, viz., the Third Book of the *Principia*, "he had an eye upon such principles as might work, with considering men, for the belief of a Deity," and he expresses his happiness that it has been found useful for that purpose. "But if I have done," he adds, "the public any service this way, it is due to nothing but industry and patient thought." In answering the first query of Dr. Bentley, the exact import of which we do not know, he states, that, if matter were evenly diffused through a finite space, and endowed with innate gravity, it would fall down into the middle of the space, and form one great spherical mass; but if it were diffused through an infinite space, some of it would collect into one mass, and some into another, so as to form an infinite number of great masses. In this manner the sun and stars might be formed if the matter were of a lucid nature. But he thinks it inexplicable by natural causes, and to be ascribed to the counsel and contrivance of a voluntary agent, that the matter should divide itself into two sorts, part of it composing a shining body like the sun, and part an opaque body like the planets. Had a natural and blind cause, without contrivance and design, placed the earth in the centre of the moon's orbit, and Jupiter in the centre of his system of satellites, and the sun in the centre of the planetary system, the sun would have been a body like Jupiter and the earth, that is, without light and heat; and consequently, he knows no reason why there is

[1] Dated December 10, 1692. This letter is indorsed in Bentley's hand.—"Mr. Newton's answer to some queries sent by me after I had preached my two last sermons."—Monk's *Life of Bentley*, p. 34, note.

only one body qualified to give light and heat to all the rest, but because the Author of the system thought it convenient, and because one was sufficient to warm and enlighten all the rest.

To the second query of Dr. Bentley, he replies that the motions which the planets now have could not spring from any natural cause alone, but were impressed by an intelligent agent. "To make such a system with all its motions, required a cause which understood, and compared together the quantities of matter in the several bodies of the sun and planets, and the gravitating powers resulting from thence; the several distances of the primary planets from the sun, and of the secondary ones from Saturn, Jupiter, and the earth, and the velocities with which those planets could revolve about those quantities of matter in the central bodies; and to compare and adjust all these things together in so great a variety of bodies, argues that cause to be not blind and fortuitous, but very well skilled in mechanics and geometry." In his answer to the third query, he expresses the opinion that the interior parts of all the planets are "as much heated, concocted, and coagulated by interior fermentation as our earth is," and that the exterior planets, Jupiter and Saturn, have a smaller density than the rest, not because they are at a greater distance from the sun, but because if their density had been greater they would "have caused a considerable disturbance in the whole system."

In answering the fourth query, he says that, in the system of vortices, even if "the sun could, by his rays, carry about the planets, yet he does not see how he could thereby effect their diurnal motion."

In the *Second* letter,[1] he admits that the spherical mass formed by the aggregation of particles would affect the figure of the space in which the matter was diffused, provided the matter descends directly downwards to that body, and the body has no diurnal rotation; but he states, that by earthquakes loosening the parts of this solid, the protuberances might sink

[1] Dated Jan. 17, 1692-3.

a little by their weight, and the mass by degrees approach a spherical figure. He then proceeds to correct an error of Dr. Bentley's in supposing that all infinites are equal, and refers him for information to Dr. Wallis's Arithmetic of Infinites. He admits that gravity might put the planets in motion, but he maintains that, without the Divine power, it could never give them such a circulating motion as they have about the sun, because a proper quantity of a transverse motion is necessary for this purpose ; and he concludes that he is compelled to ascribe the frame of this system to an intelligent agent.

In the *Third* letter,[1] he states, that the hypothesis that matter is at first evenly diffused through the universe, is in his opinion inconsistent with the hypothesis of innate gravity without a supernatural power to reconcile them, and therefore it infers a Deity. "For if there be innate gravity, it is impossible now for the matter of the earth and all the planets and stars to fly up from them, and become evenly spread throughout all the heavens without a supernatural power ; and certainly that which can never be hereafter without a supernatural power, could never be heretofore without the same power."

Having learned from his bookseller that the publication of his sermons might be delayed, Bentley, upon the receipt of the preceding letter, wrote to Newton a long letter,[2] containing "an abstract, and thread of his first unpublished sermon," and requested him, in order to make "his mind at ease," to "acquaint him with what he found in it not conformable to truth and his hypothesis." In citing, in his abstract, Newton's opinions on gravity, he gives the full passage in his sermon, and adds in a parenthesis, "I have written these words at large that you may see if I am tender enough how I engage your name in this matter."

To this letter Newton replied in a few days by a fourth

[1] Dated February 11, 169$\frac{2}{3}$.

[2] Dated February 19, 169$\frac{2}{3}$, and printed in Appendix, No. VIII. This is the only letter of Bentley's on this subject which I have found among the Portsmouth Papers.

letter[1] of great interest, and touching on all the points to which his correspondent had called his attention.

The *Fourth* letter contains opinions confirming or correcting several positions which Dr. Bentley had laid down, and closes with a curious examination of the opinion of Plato, that the motion of the planets is such as if they had been all created by God in some region very remote from our system, and let fall from thence towards the sun, their falling motion being turned aside into a transverse one whenever they arrived at their several orbits. Sir Isaac shows that there is no common place such as that conjectured by Plato, provided the gravitating power of the sun remains constant; but that Plato's affirmation is true if we suppose the gravitating power of the sun to be doubled at that moment of time when they all arrive at their several orbits. "If we suppose," says he, "the gravity of all the planets towards the sun to be of such a quantity as it really is, and that the motions of the planets are turned upwards, every planet will ascend to twice its height from the sun. Saturn will ascend till he be twice as high from the sun as he is at present, and no higher; Jupiter will ascend as high again as at present, that is, a little above the orb of Saturn; Mercury will ascend to twice his present height, that is, to the orb of Venus; and so of the rest; and then, by falling down again from the places to which they ascended, they will arrive again at their several orbs with the same velocities they had at first, and with which they now revolve.

"But if, so soon as their motions by which they revolve are turned upwards, the gravitating power of the sun, by which their ascent is perpetually retarded, be diminished by one-half, they will now ascend perpetually, and all of them, at all equal distances from the sun, will be equally swift. Mercury, when he arrives at the orb of Venus, will be as swift as Venus; and he and Venus, when they arrive at the orb of the earth, will be as swift as the earth; and so of the rest. If they begin

[1] Dated February 25, 169$\frac{2}{3}$.

all of them to ascend at once, and ascend in the same line, they will constantly, in ascending, become nearer and nearer together, and their motions will constantly approach to an equality, and become at length slower than any motion assignable. Suppose, therefore, that they ascended till they were almost contiguous, and their motions inconsiderably little, and that all their motions were at the same moment of time turned back again, or, which comes almost to the same thing, that they were only deprived of their motions, and let fall at that time, they would all at once arrive at their several orbs, each with the velocity it had at first; and if their motions were then turned sideways, and at the same time the gravitating power of the sun doubled, that it might be strong enough to retain them in their orbs, they would revolve in them as before their ascent. But if the gravitating power of the sun was not doubled, they would go away from their orbs into the highest heavens in parabolical lines."[1]

These letters, of which we have endeavoured to give a brief summary, will well repay the most attentive perusal by the philosopher as well as the divine. They are written with much perspicuity of language, and great power of thought, and contain results which incontestably prove that their author was fully master of his noblest faculties, and comprehended the profoundest parts of his own writings.[2] In the present day they possess a peculiar interest. They show that the *Nebular hypo-*

[1] "These things," says he, "follow from my *Principia Math.*, lib. i. prop. 33-36."

[2] The originals of these four letters "were given by Dr. Richard Bentley to Richard Cumberland, his nephew and executor, while a student at Trinity College, and were printed by him in a separate pamphlet in 1756. This publication was reviewed by Dr. Samuel Johnson in the *Literary Magazine*, vol. i. p. 89. See Johnson's Works, vol. ii. p. 328. In one or two cases Newton acknowledges that he had not before considered some of the conclusions from his own discoveries, and that some of the queries proposed by Bentley were new to him. Whence Dr. Johnson beautifully remarks "how even the mind of Newton gains ground gradually upon darkness." Dr. Monk, who notices this remark, justly observes, that as Bentley "availed himself of all the suggestions of his illustrious correspondent, his reasonings and conclusions appear with the highest of all human sanctions, and this department of natural theology has perhaps never yet been so satisfactorily illustrated."—*Life of Bentley*, p. 34.

thesis, the dull and dangerous heresy of the age, is incompatible with the established laws of the material universe, and that an omnipotent arm was required to give the planets their position and motions in space, and a presiding intelligence to assign to them the different functions they had to perform.[1]

The illness of Newton, which increased till the autumn of 1693, was singularly misrepresented by foreign contemporary authors, to whom an erroneous account of it had been communicated. During the century and a half which has elapsed since that event, it has never been mentioned by any of his biographers; and it was not till 1822 that it was brought before the public as a remarkable event in the life of Newton. The celebrated Dutch philosopher, Van Swinden, made the following communication to M. Biot, who published it,[2] with comments, that gave great offence to the friends of Newton :—

"There is among the manuscripts of the celebrated Huygens," says Van Swinden, "a small journal in folio, in which he used to note down different occurrences. It is note ζ, No. 8, in the Catalogue of the Library of Leyden, p. 112. The following extract is written by Huygens himself, with whose handwriting I am well acquainted, having had occasion to peruse several of his manuscript and autograph letters :—

"'*On the 29th May* 1694, *M. Colin*,[3] *a Scotchman, informed me, that eighteen months ago the illustrious geometer, Isaac Newton, had become insane, either in consequence of his too*

[1] The views of Newton and Bentley, so distinctive of the College which they adorned, have been maintained and illustrated, with all the lights of modern science, by Professor Sedgwick in his noble *Discourse on the Studies of the University.*

[2] Life of Newton, *Biog. Universelle*, tom. xxxi. p. 168.

[3] It appears from a letter of Newton to Flamsteed, that he had proposed Sir Collins, of "this University," as one of the candidates for the vacancy in Christ Hospital, occasioned by the resignation of Mr. Paget. He thought that he had mathematics enough, though young and inexperienced. From Flamsteed's unpublished reply to this letter, it would appear that Sir Collins was a son of John Collins, Newton's great and early friend. "Young Collins," he says, "may live to restore it (the Hospital), whom, therefore, you may do well to encourage to mind these studies. I doubt not he will be good in algebra; that was his father's talent. Astronomy will be most useful in the school. Our teachers in town understand little of it. Pray advise him to study the theory of

intense application to his studies, or from excessive grief at having lost, by fire, his chemical laboratory and several manuscripts. When he came to the Archbishop of Cambridge, he made some observations which indicated an alienation of mind. He was immediately taken care of by his friends, who confined him to his house, and applied remedies, by means of which he had now so far recovered his health that he began to understand the Principia.' "[1] Huygens mentioned this circumstance in a letter to Leibnitz, dated 8th June 1694,[2] in the following terms :—" I do not know if you are acquainted with the accident which has happened to the good Mr. Newton, namely, that he has had an attack of phrenitis, which lasted eighteen months, and of which they say that his friends have cured him by means of remedies, and keeping him shut up." To which Leibnitz replied in a letter, dated the 22d June :—" I am very glad that I received information of the cure of Mr. Newton at the same time that I first heard of his illness, which doubtless

the planets, and to make himself expert in calculation. Though I never saw him, yet for his father's sake, my good friend, and his own good report, he shall find me always ready to serve him."—*April* 27, 1695.

[1] M. Uylenbroek, the editor of the correspondence between Huygens and Leibnitz, has given in an appendix the correct text of this passage, with his own observations upon it :—

" 29 *Maj.* 1694.—Narravit mihi D. Colm (not Colin) Scotus virum celeberrimum ac summum geometram Is. Neutonum in phrenesin incidisse abhinc anno et sex mensibus. An ex nimia studii assiduitate, an dolore infortunii, quod incendio laboratorium chymicum et scripta quædam amiserat? Cum ad Archiepiscopum Cantabrigiensem (Cantuariensem, as Mr. Edleston conjectures) venisset, ea locutum, quæ alienationem mentis indicarent. Deinde ab amicis cura ejus susceptam, domoque clauso remedia volenti nolenti adhibita, quibus jam sanitatem recuperavit, ut jam rursus librum suum Principiorum Philosophiæ Mathematicorum intelligere incipiat."

M. Uylenbroek adds his own opinion of the matter, as explained in my former Life of Newton :—" Hæc Colmi narratio, quam ex his ipsis MSS., Hugeniensis petitam, quondam evulgaverat Biotus, nuperrime Brewstero ansam præbuit inquirendi utrum revera Newtonus mentis morbo correptus fuerit necne. Testimonia, quæ attulit vir Cl. ea esse videntur e quibus probabiliter efficias Newtonum, currente anno 1692, solita mentis, corporisque valetudine non fuisse usum, at non ita eum morbo decubuisse ut eo impeditus fuerit quo minus studiis suis vacaret."—Christiani Hugenii *Exercitationes Mathematicæ*. Ed. P. J. Uylenbroek, fascic. ii. p. 171, Hag. An. 1833.

[2] He made the same communication to the Marquis L'Hospital on the 16th June.—Ch. Hug. *Exercit. Math.* fascic. i. p. 318.

must have been very alarming. 'It is to men like you and him, sir, that I wish a long life and much health, more than others, whose loss, comparatively speaking, would not be so great.'"[1]

The first publication of the preceding statement produced a strong sensation among the friends and admirers of Newton. They could not easily believe in the prostration of that intellectual strength which had unbarred the strongholds of the universe. The unbroken equanimity of Newton's mind, the purity of his moral character, his temperate and abstemious life, his ardent and unaffected piety, and the weakness of his imaginative powers, all indicated a mind which was not likely to be overset by any affliction to which it could be exposed. The loss of a few experimental records could never have disturbed the equilibrium of a mind like his. If they were the records of discoveries, the discoveries, themselves indestructible, would have been afterwards given to the world. If they were merely the details of experimental results, a little time could have easily reproduced them. Had these records contained the first-fruits of youthful genius—of obscure talent, on which fame had not yet shed its rays, we might have supposed that the first blight of early ambition would have unsettled the stability of a mind unannealed by the world. But Newton was satiated with fame. His mightiest discoveries were completed, and diffused over all Europe, and he must have felt himself placed on the loftiest pinnacle of earthly ambition. The incredulity which such views could not fail to encourage, was increased by the novelty of the information. No English biographer had ever alluded to such an event. History and tradition were equally silent, and it was not easy to believe that the Lucasian Professor of Mathematics at Cambridge, recently a Member of the English Parliament, and the first philosopher and mathematician in Europe, could have lost his reason without the dreadful fact being known to his countrymen.

But if the friends of Newton were surprised by the nature of

[1] Ch. Hug. *Exercit. Math.* fascic. i. p 182.

the intelligence, they were distressed at the view which was taken of it by foreign philosophers. "The fact," says M. Biot, "of the derangement of his intellect, whatever may have been the cause of it, will explain why, after the publication of the *Principia* in 1687, Newton, though only forty-five years old, never more published a new work on any branch of science, but contented himself with giving to the world those which he had composed long before that epoch, confining himself to the completion of those parts which might require development. We may also remark, that even these developments appear always to be derived from experiments and observations formerly made, such as the additions to the second edition of the *Principia*, published in 1713, the experiments on thick plates, those on diffraction, and the chemical queries placed at the end of the *Optics* in 1704; for in giving an account of these experiments Newton distinctly says, that they were taken from ancient manuscripts which he had formerly composed; and he adds, that though he felt the necessity of extending them, or rendering them more perfect, he was not able to resolve to do this, these matters being no longer in his way. Thus it appears that though he had recovered his health sufficiently to understand all his researches, and even in some cases to make additions to them, and useful alterations, as appears from the second edition of the *Principia*, for which he kept up a very active mathematical correspondence with Mr. Cotes, yet he did not wish to undertake new labours in those departments of science where he had done so much, and where he so distinctly saw what remained to be done." Under the influence of the same opinion, M. Biot finds it "extremely probable that his dissertation on the scale of heat was written before the fire in his laboratory;" and he describes Newton's conduct about the longitude bill as exhibiting an inexplicable timidity of mind, and as "so puerile for so solemn an occasion, that it might lead to the strangest conclusions, particularly if we refer it to the fatal accident which befell him in 1695."

The illness of Newton was viewed in a light still more painful to his friends. It was maintained that he never recovered the vigour of his intellect, and that his theological inquiries did not commence till after that afflicting epoch of his life. In reply to this groundless assertion, it may be sufficient to state, in the words of his friend John Craig,[1] that his theological writings eviden were composed "while his understanding was in its greatest PROOF perfection, lest the infidels might pretend that his applying himself to the study of religion was the effect of dotage."

Such having been the consequences of the disclosure of Newton's illness by the manuscript of Huygens, I felt it to be a sacred duty to the memory of that great man, and to the feelings of his countrymen, to inquire into the nature and history of that indisposition which seems to have been so much misrepresented and misapplied. From the ignorance of so extraordinary an event which has prevailed for such a long period in England, it might have been urged with some plausibility, that Huygens had mistaken the real import of the information that was conveyed to him; or that the person from whom he received it had propagated an idle and a groundless rumour. But we are fortunately not confined to this very reasonable mode of defence. There exists at Cambridge a manuscript journal written by Mr. Abraham de la Pryme, who was a student in the University while Newton was a Fellow of Trinity. This manuscript is entitled "*Ephemeris Vitæ*, or Diary of my own Life, containing an account likewise of the most observable and remarkable things that I have taken notice of from my youth up hitherto." Mr. A. de la Pryme was born in 1671, and begins the Diary in 1685. This manuscript is in the possession of his collateral descendant, George Pryme, Esq., Professor of Political Economy at Cambridge, to whom I have been indebted for the following extract, which is given verbatim, and occurs during the period when Mr. de la Pryme was a student in St. John's College, Cambridge:—

[1] Unpublished letter to Conduitt, April 7, 1727.

"1692, *February 3d.*—What I heard to-day I must relate. There is one Mr. Newton (whom I have very oft seen), Fellow of Trinity College, that is mighty famous for his learning, being a most excellent mathematician, philosopher, divine, &c. He has been Fellow of the Royal Society these many years; and amongst other very learned books and tracts he's written one upon the mathematical principles of philosophy, which has got him a mighty name, he having received, especially from Scotland, abundance of congratulatory letters for the same; but of all the books that he ever wrote, there was one of colours and light, established upon thousands of experiments, which he had been twenty years of making, and which had cost him many hundred of pounds. This book, which he valued so much, and which was so much talked of, had the ill luck to perish, and be utterly lost, just when the learned author was almost at putting a conclusion at the same, after this manner:—In a winter's morning, leaving it amongst his other papers on his study table whilst he went to chapel, the candle, which he had unfortunately left burning there too, catched hold by some means of other papers, and they fired the aforesaid book, and utterly consumed it and several other valuable writings; and, which is most wonderful, did no further mischief. But when Mr. Newton came from chapel, and had seen what was done, every one thought he would have run mad, he was so troubled thereat that he was not himself for a month after. A long account of this his system of light and colours you may find in the Transactions of the Royal Society, which he had sent up to them long before this sad mischance happened unto him."

The story of the burning of Newton's laboratory and papers, as stated by Mr. de la Pryme, has been greatly exaggerated and misrepresented, and there can be no doubt that it was entirely unconnected with Newton's illness. Mr. Edleston[1] has placed it beyond a doubt that the burning of the manuscripts took place between 1677 and 1683, and I have found ample

[1] *Correspondence*, &c. pp. lxii. lxiii.

confirmation of the fact from other sources of information. Dr. H. Newton, as we have seen, tells us that he had heard a report that Newton's *Optics* had been burnt before he wrote his *Principia*, and we know that no such accident took place during the five years that Dr. Newton lived with him at Cambridge. The following memorandum of Mr. Conduitt's, written after conversing on the subject with Newton himself, appears to place the event at an early period :—" When he was in the warmest pursuit of his discoveries, he going out, left a candle upon his table amongst his papers, he went down into the bowling-green, and meeting somebody who diverted him from returning as he intended, the candle set fire to his papers (and he could never recover them[1]). Upon my asking him whether they related to his *Optics* or the *Method of Fluxions*, he said he believed there was some relating to both, and that he was obliged to work them all over again." The version of the burnt papers in which "Diamond" is made the perpetrator, and in which the scene of the story is laid in London, and in Newton's later years, we may consign to a note, with the remark of Dr. Humphrey Newton, that Sir Isaac never had any communion with dogs or cats.[2]

[1] This observation, which is in another edition of the manuscript, is not inconsistent with the statement of Newton's having "worked them over again."

[2] "Newton's temper was so mild and equal, that scarce any accident disturbed him. One instance in particular, which is authenticated by a person now living (1780), brings this assertion to a proof. Sir Isaac being called out of his study to a contiguous room, a little dog called Diamond, the constant but incurious attendant of his master's researches, happened to be left among the papers, and by a fatality not to be retrieved, as it was in the latter part of Sir Isaac's days, threw down a lighted candle, which consumed the almost finished labours of some years. Sir Isaac returning too late but to behold the dreadful wreck, rebuked the author of it with an exclamation (*ad sidera palmas*), 'O Diamond! Diamond! thou little knowest the mischief done!' without adding a single stripe."—Notes to Maude's *Wensleydale*, p. 102, 4th edit. 1816. M. Biot gives this piece of fiction as a true story, which happened in some year after the publication of the *Principia*, and he characterizes the accident as having deprived the sciences for ever of the fruit of so much of Newton's labours. Dr. Wallis received another edition of the story from his correspondent Sturm, a professor at Altorf. "Sturm sends me word of a rumour amongst them concerning Mr. Newton, as if *his house and books, and all his goods were burnt*, and himself so disturbed in mind thereupon as to be reduced to very

By means of this extract from Mr. de la Pryme's Diary, we are enabled to fix the latest date of the accident by which Newton lost his papers. It must have been previous to the 3d January 1692, a month before the date of the extract; but if we fix it by the dates in Huygens' manuscript, we should place it about the 29th November 1692, eighteen months previous to the conversation between Colin and Huygens. The manner in which Mr. Pryme refers to Newton's state of mind is that which is used every day when we speak of the loss of tranquillity which arises from the ordinary afflictions of life; and the meaning of the passage amounts to nothing more than that Newton was very much troubled by the destruction of his papers, and did not recover his serenity, and return to his usual occupations, for a month. The very phrase, that every person thought he would have run mad, is in itself a proof that no such effect was produced; and, whatever degree of indisposition may be implied in the phrase, "he was *not himself* for a month after," we are entitled to infer that one month was the period of its duration, and that previous to the 3d February 1692, the date of Mr. Pryme's memorandum, "Newton was himself again."[1]

These facts and dates cannot be reconciled with those in Huygens' manuscript.[2] It appears from that document, that, so late as May 1694, Newton had only *so far* recovered his health as *to begin to again understand the Principia.* His

ill circumstances; which being all false, I thought fit presently to rectify that groundless mistake."—Letter to Waller, Secretary to the Royal Society, quoted by Mr. Edleston from the Letter-book of the Royal Society. See pp. 93 and 97.

[1] We entirely concur with Mr. Edleston in his opinion that this story refers to an antecedent period. It is obviously a repetition of the story referred to by Dr. Newton respecting the burning of the *Optics* before 1684.

[2] In the *Journal des Savans*, 1832, p. 295, M. Biot has tried to reconcile these facts and dates by arguments which have been so ably exposed and refuted by Mr. Edleston, who entirely concurs with the view I have taken of the subject, that any further controversy is unnecessary. The evidence of Dr. Humphrey Newton leaves no doubt whatever that the fire in Sir Isaac's room took place before 1684.—See *Correspondence*, &c. pp. lx.-lxii.

supposed malady, therefore, was in force from the 3d of January 1692, till the month of May 1694,—a period of more than two years. Now, it is a most important circumstance, which M. Biot ought to have known, that in *the very middle of this period*, Newton wrote his four celebrated letters to Dr. Bentley on the Existence of a Deity,—letters which evince a power of thought and a serenity of mind absolutely incompatible even with the slightest obscuration of his faculties. No man can peruse these letters without the conviction that their author then possessed the full vigour of his reason, and was capable of understanding the most profound parts of his writings. The first of these letters was written on the 10th December 1692, the second on the 17th January 1693, the third on the 11th February, and the fourth on the 25th February 1693. His mind was, therefore, strong and vigorous on these four occasions; and as the letters were written at the express request of Dr. Bentley, to assist him in preparing his lectures for publication, we must consider such a request as showing his opinion of the strength and freshness of his friend's mental powers.

In August and September 1692, as we have already seen, Newton transmitted to Dr. Wallis the first proposition of his book on quadratures, with examples of it in first, second, and third fluxions.[1] These examples were written at the request of his friend; and the author of the review of the *Commercium Epistolicum*, in which this fact is quoted, draws the conclusion, that he had not at that time forgotten his method of second fluxions. It appears, also, from the second book of the *Optics*,[2] that in the month of June 1692, he had been occupied with the subject of haloes, and had made accurate observations both on the colours and the diameters of the rings in a halo which he had then seen around the sun. We find also from his manuscripts, that he was deeply engaged in chemical experiments in

[1] See Newtoni *Opera*, tom. iv. p. 480; and Wallisii *Opera*, 1693, tom. ii. pp. 391-396.

[2] *Optics*, part iv. obs. 13.

the months of December 1692 and January 1693; and on the 26th October 1693, he wrote a letter to Leibnitz, giving him, at his request, an account of his method of reducing quadratures to the rectification of curves, and, three months afterwards, another letter to Dr. Mill at Oxford.[1] In addition to these facts, it may be useful to mention that Facio Duillier visited Newton at Cambridge in the middle of November 1692;[2] and it is evident from Facio's letter to him, dated November 17, and from a letter of Newton's to Facio of the 14th March 1693,[3] that he was in comparatively good health.

But though these facts stand in direct contradiction to the statement recorded by Huygens, the reader will be naturally anxious to know the real nature and extent of the indisposition to which it probably refers. The following letters, written by Newton himself to Mr. Pepys, Secretary to the Admiralty, and Mr. Millington of Magdalene College, Cambridge, for which I have been indebted to the kindness of Lord Braybrooke, will throw much light upon the subject.

Newton, as will be presently seen, had fallen into a bad state of health in the autumn of 1692, in consequence of which both his sleep and his appetite were greatly affected. About the middle of September 1693, he had been kept awake for five nights by this nervous disorder, and in this condition he wrote the following letter to Mr. Pepys:—

"*September* 13, 1693.

"Sir,—Some time after Mr. Millington had delivered your message, he pressed me to see you the next time I went to London. I was averse; but upon his pressing consented, before I considered what I did, for I am extremely troubled at the embroilment I am in, and have neither ate nor slept well this twelvemonth, nor have my former consistency of mind. I never designed to get anything by your interest, nor by King James's

[1] Dated January 29, 1694. [2] See p. 2.

[3] *Gentleman's Magazine*, tom. lxxxiv. p. 3, 1814.

favour, but am now sensible that I must withdraw from your acquaintance, and see neither you nor the rest of my friends any more, if I may but leave them quietly. I beg your pardon for saying I would see you again, and rest your most humble and most obedient servant,

"Is. NEWTON."

From this letter we learn, on his own authority, that his complaint had lasted for a twelvemonth, and that during that period he neither ate nor slept well, nor enjoyed his former *consistency of mind.* It is not easy to understand exactly what is meant by not enjoying his former consistency of mind; but whatever be its import, it is obvious that he must have been in a state of mind which enabled him to compose the four letters to Bentley, and the other productions we have mentioned.

On the receipt of this letter, his friend, Mr. Pepys, seems to have written to Mr. Millington, to inquire after Mr. Newton's health; but the inquiry having been made in a vague manner, an answer equally vague was returned. Mr. Pepys, however, who seems to have been deeply anxious about Newton's health, addressed the following more explicit letter to Mr. Millington:—

"*September* 26, 1693.

"SIR,—After acknowledging your many old favours, give me leave to do it a little more particularly upon occasion of the new one conveyed to me by my nephew Jackson. Though, at the same time, I must acknowledge myself not at the ease I would be glad to be at in reference to the excellent Mr. Newton; concerning whom (methinks) your answer labours under the same kind of restraint which (to tell you the truth) my asking did. For I was loth at first dash to tell you that I had lately received a letter from him so surprising to me for the inconsistency of every part of it, as to be put into great disorder by it, from the concernment I have for him, lest it should arise from that which of all mankind I should least dread from him and most lament for,—I mean a discomposure in head, or mind, or both. Let me, therefore, beg you, Sir, having now told you the true ground of the trouble I lately gave you, to let me know the very truth of the matter, as far at least as comes within your knowledge. For I own too great an esteem for Mr. Newton, as for a public good, to be able to let any doubt in me of this kind concerning him lie a moment uncleared, where I can have any hopes of helping it.—I am, with great truth and respect, dear Sir, your most humble and most affectionate servant,

"S. Pepys."

To this letter Mr. Millington made the following reply :—

"Coll. Magd. Camb., *Sept. the* 30, 1693.

"Honor'd Sir,—Coming home from a journey on the 28th instant at night, I met with your letter which you were pleased to honour me with of the 26th. I am much troubled I was not at home in time for the post, that I might as soon as possible put you out of your generous payne that you are in for the worthy Mr. Newton. I was, I must confess, very much surprised at the inquiry you were pleased to make by your nephew about the message that Mr. Newton made the ground of his letter to you, for I was very sure I never either received from you or delivered to him any such; and therefore I went immediately to wayt upon him, with a design to discourse him about the matter, but he was out of town, and since I have not seen him, till upon the 28th I met him at Huntingdon, where, upon his own accord, and before I had time to ask him any question, he told me that he had writt to you a very odd letter, at which he was much concerned; added, that it was in a distemper that much seized his head, and that kept him awake for above five nights together, which upon occasion he desired I would represent to you, and beg your pardon, he being very much ashamed he should be so rude to a person for whom he hath so great an honour. He is now very well, and, though I fear he is under some small degree of melancholy, yet I think there is no reason to suspect it hath at all touched his understanding, and I hope never will; and so I am sure all ought to wish that love learning or the honour of our nation, *which it is a sign how much it is looked after, when such a person as Mr. Newton lyes so neglected by those in power.* And thus, honoured Sir, I have made you acquainted with all I know of the cause of such inconsistencys in the letter of so excellent a person; and I hope it will remove the doubts and fears you are, with so much compassion and publickness of spirit, pleased to entertain about Mr. Newton; but if I should have been wanting in any thing tending to the more full satisfaction, I shall, upon the least notice, endeavour to amend it with all gratitude and truth. Honored Sir, your most faithfull and most obedient servant,

"JOH. MILLINGTON."

Mr. Pepys was perfectly satisfied with this answer, as appears from the following letter:—

"*October* 3d, 1693.

"SIR,—You have delivered me from a fear that indeed gave me much trouble, and from my very heart I thank you for it, an evil to Mr. Newton being what every good man must feel for his own sake as well as his. God grant it may stopp here. And for the kind reflection hee has since made upon his letter to mee, I dare not take upon mee to judge what answer I should make him to it, or whether any or no; and therefore pray that you will bee pleased either to bestow on mee what directions you see fitt for my own guidance towards him in it, or to say to him in my name, but your own pleasure, whatever you think may be most welcome to him upon it, and most expressive of my regard and affectionate esteem of him, and concernment for him. I have a debt to acknowledge to you (but was prevented in my last, by the thoughts I was then overborne with in this matter), from the great satisfaction you was pleased to give me by your pupil (on whose behalf I have lasting thanks also to pay you) to my enquiries about Mr. Pyets, beseeching you to make the same scruplelesse use of me in whatever relation you can think me capable of rendering you any service, for I would do it with great pleasure, remaining, dear Sir, your most humble and most faithful servant,

"S. PEPYS."

It does not appear from the Memoirs of Mr. Pepys that he returned any answer to the letter of Mr. Newton which occasioned this correspondence; but we find, that in less than two months after the date of the preceding letter, an opportunity occurred of introducing to him a Mr. Smith, who took a journey to Cambridge to obtain his opinion on a problem in the doctrine of chances. This problem related to "the project of Mr. Neale, the groom-porter's lottery," which Pepys says Newton "cannot but have heard of," as it "has almost extinguished, for some time, at all places of public conversation, especially among men of numbers, every other talk but what relates to the doctrine of determining between the true proportion of the hazards incident to this or that given chance or lot." "Mr. Smith," he says, "was concerned (more than in jest) to compass a solution, that may be relied on beyond what his modesty would suffer him to think his own alone, or any less than Mr. Newton's to be."

Mr. Pepys's introductory letter was dated November 22, 1693, and Newton returned an answer on the 26th, in which he explains the ambiguity of the question as proposed to him. He takes the question, however, to be,—

"What is the expectation of A to throw every time *one* six at least with *six* dice?

"What is the expectation of B to throw every time *two* sixes at least with *twelve* dice?

"What is the expectation of C to throw every time *three* sixes at least with *eighteen* dice?

"And whether has not B and C as great an expectation to hit every time what they throw for?

"If the question be thus stated, it appears by an easy computation that the expectation of A is greater than that of B and C,—that is, the task of A is the easiest,—and the reason is because A has all the chances in sixes on his dice for his expectation; but B and C have not all the chances upon theirs, for, when B throws a single six, or C but one or two sixes, they miss of their expectations."

In his reply, which I have not found among the Portsmouth papers, Pepys concurred in this statement of the question, and desired to have the "easy computation." Newton accordingly sent, on the 16th December, a table of eight progressions for making it. In returning thanks for the "easy computation," Pepys confessed that he did not understand how to make the full use of the table of progressions, and therefore put the question in a different form. This letter is dated December 21, 1693,[1] but Newton's answer to it has not been found. In perusing this correspondence, the mathematical reader will have no doubt of the consistency of Newton's mind, and of its fitness for the most profound research.

It is obvious from Newton's letter to Pepys, of the 13th September, that the subject of his receiving some favour from the Government had been a matter of anxiety with himself, and of discussion among his friends. Mr. Millington was no doubt referring to this anxiety, when he represents Newton as an honour to the nation, and expresses his surprise "that such a person should *lye so neglected by those in power*." We have already shown that the same subject was alluded to in his letters to Locke in 1692. In all these letters Newton no doubt referred to some appointment in London which he was solicitous to obtain, and which Mr. Montague and his other friends may have failed in procuring. This opinion is confirmed by the letter of Mr. Montague, announcing to him his appointment to the wardenship of the Mint, in which he says that

[1]The three first letters above-mentioned have been published by Lord Braybrooke in his *Memoirs of Samuel Pepys*, vol. ii. pp. 131-135: Lond. 1825. The fourth letter I have given in the APPENDIX, No. IX., in order to complete the published correspondence.

he is very glad he can *at last* give him good proof of his friendship.

In the same month in which Newton wrote to Mr. Pepys, we find him in correspondence with Mr. Locke. Displeased with his opinions respecting innate ideas, he had rashly stated that they struck at the root of all morality, and that he regarded the author of such doctrines as a Hobbist. Upon reconsidering these opinions, he addressed the following remarkable letter to Locke, written three days after his letter to Mr. Pepys, and consequently during the illness under which he then laboured :—

"SIR,—Being of opinion that you endeavoured to embroil me with women, and by other means, I was so much affected with it, as that when one told me you were sickly and would not live, I answered, 'twere better if you were dead. I desire you to forgive me this uncharitableness ; for I am now satisfied that what you have done is just, and I beg your pardon for my having hard thoughts of you for it, and for representing that you struck at the root of morality, in a principle you laid in your book of ideas, and designed to pursue in another book, and that I took you for a Hobbist.[1] I beg your pardon also for saying or thinking that there was a design to sell me an office, or to embroil me.—I am your most humble and unfortunate servant,

"IS. NEWTON.

"At the BULL, in Shoreditch, London,
Sept. 16*th*, 1693."

To this letter, characterized by Mr. Dugald Stewart as ingenuous and infantine in its simplicity, Locke returned the following answer, which, as the same author justly remarks, "is written with the magnanimity of a philosopher, and with the good-

[1] The system of Hobbes was at this time very prevalent. According to Dr. Bentley, 'the taverns and coffee-houses, nay, Westminster Hall, and the very churches, were full of it ;" and he was convinced, from personal observation, that "not one English infidel in a hundred was other than a Hobbist."—Monk's *Life of Bentley*, p. 31.

humoured forbearance of a man of the world, breathing throughout so tender and unaffected a veneration for the good as well as great qualities of the excellent person to whom it is addressed, as demonstrates at once the conscious integrity of the writer, and the superiority of his mind to little passions."[1]

"OATES, *Oct. 5th*, 1693.

"SIR,—I have been, ever since I first knew you, so entirely and sincerely your friend, and thought you so much mine, that I could not have believed what you tell me of yourself, had I had it from anybody else. And, though I cannot but be mightily troubled that you should have had so many wrong and unjust thoughts of me, yet next to the return of good offices, such as from a sincere good will I have ever done you, I receive your acknowledgment of the contrary as the kindest thing you have done me, since it gives me hopes I have not lost a friend I so much valued. After what your letter expresses, I shall not need to say anything to justify myself to you. I shall always think your own reflection on my carriage, both to you and all mankind, will sufficiently do that. Instead of that, give me leave to assure you that I am more ready to forgive you than you can be to desire it; and I do it so freely and fully, that I wish for nothing more than the opportunity to convince you that I truly love and esteem you, and that I have the same good will for you as if nothing of this had happened. To confirm this to you more fully, I should be glad to meet you

Newton and Locke occasionally corresponded on theological subjects. In the autumn of 1702, Newton visited Locke at Oates, and having read his Essay on the Corinthians, he promised to give him his observations and opinion upon it after a more careful perusal. Locke accordingly sent it to him before Christmas 1702; but in consequence of receiving no answer, he wrote to him again on the 30th April 1703, and received his observations in a letter dated May 15, 1703, published by Lord King. In this letter Newton tells him that he had purposed to pay him a visit at Oates, on his way to Cambridge in summer, but was "now uncertain of this journey." We believe they never met again. Locke died on the 28th October 1704, in the seventy-third year of his age; and it has been stated that Newton visited his tomb at High Laver, in Essex, in all probability when he paid his next visit to Cambridge.

anywhere, and the rather, because the conclusion of your letter makes me apprehend it would not be wholly useless to you. But whether you think it fit or not, I leave wholly to you. I shall always be ready to serve you to my utmost, in any way you shall like, and shall only need your commands or permission to do it.

"My book is going to press for a second edition; and, though I can answer for the design with which I write it, yet, since you have so opportunely given me notice of what you have said of it, I should take it as a favour if you would point out to me the places that gave occasion to that censure, that, by explaining myself better, I may avoid being mistaken by others, or unawares doing the least prejudice to truth or virtue. I am sure you are so much a friend to them both, that, were you none to me, I could expect this from you. But I cannot doubt but you would do a great deal more than this for my sake, who, after all, have all the concern of a friend for you, wish you extremely well, and am, without compliment, &c."[1]

To this letter Newton made the following reply:—

"Sir,—The last winter, by sleeping too often by my fire, I got an ill habit of sleeping; and a distemper, which this summer has been epidemical, put me farther out of order, so that when I wrote to you I had not slept an hour a night for a fortnight together, and for five days together not a wink. I remember I wrote to you, but what I said of your book I remember not. If you please to send me a transcript of that passage, I will give you an account of it if I can.—I am your most humble servant,

"Is. Newton.

"Cambridge, *Oct.* 15*th*, 1693."

Although the first of these letters evinces the existence of a nervous irritability which could not fail to arise from want of

[1] "The draft of this letter is indorsed J. L. to I. Newton." I have not found the original among Newton's papers.

appetite and of rest, yet it is obvious that its author was in the full possession of his mental powers. The answer of Mr. Locke, indeed, is written upon the supposition that Newton was then qualified to point out the objectionable passages in his book, that they might be corrected and better explained; and it deserves to be remarked, that Mr. Dugald Stewart, who first published a portion of these letters, never imagined that Newton was labouring under any mental alienation.

In the autumn of 1693, when Newton was suffering most severely from want of appetite and sleep, we find him deeply engaged in biblical research—collating ancient manuscripts of the New Testament—criticising the manuscript works of Dr. John Mill of Edmund Hall, Oxford, and communicating to him the results of his labours. Only two letters of this correspondence have been found, the letter from Dr. Mill to Newton, requesting the return of his manuscript with his observations, and Newton's reply, showing how busily he had been occupied in the task assigned to him by his friend.[1]

Among the other evidences of Newton's consistency of mind, in May 1694, when he is said to have been only beginning to understand the *Principia*, we may mention the visit paid to him in the beginning of that month by David Gregory, who went to Cambridge for the purpose of "consulting the divine author of the *Principia*" on certain errors which appeared to have crept into that work.[2] On the 7th of the same month, probably when Gregory was at Cambridge, we find Newton

[1] The letter of Dr. Mill, dated Nov. 7, 1693, I found among Newton's papers. That of Newton, dated Jan. 29, $169\frac{3}{4}$, is preserved in the library of Queen's College, Oxford, and is No. 26 of the printed Catalogue. Having been kindly favoured with a copy of this letter by Dr. Fox, I have given both of them in the APPENDIX, No. X., as they possess a peculiar interest.

[2] "Quoniam varii errores in propositiones 37 et 38 (Lib. 2) irrepsere, illos omnes restitutos hic apponam, prout in auctoris exemplari inveni, ineunte Maio 1694, dum Cantabrigiæ hærerem, consulendi divini auctoris gratia."—MS. of David Gregory, Rigaud, *Hist. Essay*, p. 100. Mr. Rigaud adds, that this is "the place in which Fatio says he convinced Newton of his mistakes." See *Edinburgh Transactions*, 1829, vol. xii. p. 71.

denouncing the imposture of the haunted house, and scolding the Fellows of Trinity and several of the scholars for their credulity.[1]

The erroneous opinion that Newton devoted his attention to theology only in the latter part of his life, may be considered as deriving some countenance from the fact, that the celebrated general scholium, at the end of the second edition of the *Principia*, published in 1713, did not appear in the first edition of that work. This argument has been ably controverted by the late Dr. J. C. Gregory of Edinburgh, on the authority of a manuscript of Newton, which seems to have been transmitted to his ancestor, Dr. David Gregory, between the years 1687 and 1698. This manuscript, which consists of twelve folio pages in Newton's handwriting, contains, in the form of additions, and scholia to some propositions in the third book of the *Principia*, an account of the opinions of the ancient philosophers on gravitation and motion, and on natural theology, with various quotations from their works. Attached to this manuscript are three very curious paragraphs. The two first appear to have been the original draught of the general scholium already referred to; and the third relates to the subject of an ethereal medium, respecting which he maintains an opinion diametrically opposite to that which he afterwards published at the end of his *Optics*.[2] The first paragraph expresses nearly

[1] The following account of this affair is given by Mr. Edleston from De la Pryme's Diary:—"On [the] Monday [night] likewise, there being a great number of people at the door [of the haunted house,—it was a house opposite St. John's College, in the occupation of Valentine Austin], there chanced to come by Mr. Newton, Fellow of Trinity College, a very learned man, and perceiving our Fellows to have gone in [three Fellows of St. John's, with a Fellow Commoner of that College, had rushed in armed with pistols], and seeing several scholars ab^{t} y^{e} door, 'Oh ye fools!' says he, 'will ye never have any wit? know ye not that all such things are mere cheats and impostures? fie, fie! go home for shame,' and so he left them, scorning to go in." In this Diary, to which we have already referred, there is a full account of the proceedings of the "spirit," which the writer of the Diary had received in a letter from Cambridge.—Edleston's *Correspondence*, &c., p. lxiv.

[2] Dr. Gregory concludes his account of this manuscript, which he kindly lent me, in the following words:—"I do not know whether it is true, as stated by Huygens, 'New-

the same idea as some sentences in the scholium beginning "Deus summus est ens æternum, infinitum, absolute perfectum;"[1] and it is remarkable that the second paragraph is found only in the third edition of the *Principia*, which appeared in 1726, the year before Newton's death.

In reviewing the details which we have now given respecting the health and occupations of Newton from the beginning of 1692 to 1694, it is impossible to draw any other conclusion than that he possessed a sound mind, and was perfectly capable of carrying on his mathematical, his physical, and his theological inquiries. His friend and admirer, Mr. Pepys, residing within fifty miles of Cambridge, had never heard of his being attacked with any illness till he inferred it from the letter to himself written in September 1693. Mr. Millington, who lived in the same University, had been equally unacquainted with any such attack, and, after a personal interview with Newton, for the express purpose of ascertaining the state of his health, he assures Mr. Pepys, "that he is very well; *that he fears he is under some small degree of melancholy*, but that there is no reason to suspect that it hath at all touched his understanding."

During this period of bodily indisposition, his mind, though in a state of nervous irritability, and disturbed by want of rest, was capable of puting forth its highest powers. At the request

tonum incidisse in Phrenitim;' but I think every gentleman who examines this manuscript will be of opinion that he must have thoroughly recovered from his phrenitis before he wrote either the Commentary on the Opinions of the Ancients, or the Sketch of his own Theological and Philosophical Opinions which it contains." An account of this manuscript, by Dr. J. Gregory, has been published in the *Edinburgh Transactions* for 1829, vol. xii. pp. 64-67.—See Rigaud's *Hist. Essay*, p. 99.

[1]This paragraph is as follows:—"Deum esse ens summe perfectum concedunt omnes. Entis autem summe perfecti Idea est ut sit substantia una, simplex, indivisibilis, viva et vivifica, ubique semper necessario existens, summe intelligens omnia, libere volens bona, voluntate efficiens possibilia, effectibus nobilioribus similitudinem propriam quantum fieri potest communicans, omnia in se continens tanquam eorum principium et locus, omnia per presentiam substantialem cernens et regens, et cum rebus omnibus, secundum leges accuratas ut naturæ totius fundamentum et causa constanter co-operans, nisi ubi aliter agere bonum est."

of Dr. Wallis he drew up examples of one of his propositions on the quadrature of curves in second fluxions. He composed, at the desire of Dr. Bentley, his profound and beautiful letters on the existence of the Deity. He was requested by Locke to reconsider his opinions on the subject of innate ideas. Dr. Mill engaged him in profound biblical researches, and we shall presently find him grappling with the difficulties of the lunar theory.

But with all these proofs of a vigorous mind, a diminution of his mental powers has been rashly inferred from the cessation of his great discoveries, and from his unwillingness to enter upon new investigations. The facts, however, here assumed, are as incorrect as the inference which is drawn from them. The ambition of fame is a youthful passion, which is softened, if not subdued, by age. Success diminishes its ardour, and early pre-eminence often extinguishes it. Before the middle period of his life Newton was invested with all the insignia of immortality; but endowed with a native humility of mind, and animated with those hopes which teach us to form a humble estimate of human greatness, he was satisfied with the laurels which he had won, and he sought only to perfect and complete his labours. Although his mind was principally bent on the improvement of the *Principia*, yet he occasionally diverged into new fields of scientific research—he created, as we shall see, his fine theory of astronomical refractions—he made great improvements on the lunar theory—he solved difficult problems, which had been proposed to try his strength,—he wrote a profound letter to Leibnitz,—he made valuable additions to his "Opticks," —he continued his chemical experiments,—and he devoted much of his time to profound inquiries in chronology and theological literature.

The powers of his mind were therefore in full requisition; and, when we consider that he was called to the discharge of high official functions which forced him into public life, and compelled him to direct his genius into new channels, we can scarcely be surprised that he ceased to produce any very original works on abstract science. In the direction of the affairs of the Mint, and of the Royal Society, to which we shall now follow him, he found ample occupation for his time; while the leisure of his declining years was devoted to those exalted studies in which philosophy yields to the supremacy of faith, and hope administers to the aspirations of genius.[1]

[1] Some light has been recently thrown on the illness of Newton by Dr. Dowson of Whitby, who, at a meeting of the Philosophical Society there, on the 3d of January 1856, read a paper "On the supposed Insanity of Sir Isaac Newton," in which he has shown that the malady with which he was afflicted in September 1693 was probably Influenza or Epidemic Catarrhal Fever, which prevailed in England, Ireland, France, Holland, and Flanders in the four last months of 1693. This distemper, which lasted from eight or ten days to a month, was so general, that "few or none escaped from it;" and it is therefore probable, as Dr. Dowson believes, that Newton's mental disorder was merely the delirium which frequently accompanies a severe attack of Influenza. See Dr. Theophilus Thomson's *Annals of Influenza or Epidemic Catarrh in Great Britain*, published in 1852 by the Sydenham Society. See also the *Philosophical Transactions* for 1694, vol. xviii. pp. 105-115.

CHAPTER XVIII.

Newton occupied with the Lunar Theory—His Correspondence with Flamsteed, the Astronomer-Royal—Newton's Letters to Flamsteed, published by Mr. Baily—Controversy which they occasioned—Flamsteed's Letters to Newton discovered recently—Character of Flamsteed, in reference to this Controversy—Of Newton, and of Halley—All of them engaged, with different objects, in studying the Lunar Theory—Newton applies to Flamsteed for Observations on the Moon, and on the Refraction of the Atmosphere, which Flamsteed transmits to him—Analysis of their Correspondence—Flamsteed's bitterness against Halley—Differences between Newton and Flamsteed—Flamsteed's ill health interferes with his supplying Newton with Observations—Newton's impatience and expostulation with Flamsteed—Justification of Flamsteed—Biot ascribes Newton's Letter to Mental Illness—Refutation of this view of the subject—Newton never afflicted with any mental disorder.

WHILE Newton was supposed to be incapable of understanding his *Principia*, we find him occupied with the difficult and profound subject of the lunar irregularities. He had resumed this inquiry in 1692,[1] and it was probably from the intense application of his mental powers which that subject demanded, that he was deprived of his appetite and sleep during that and the subsequent year. When Mr. Machin long afterwards was complimenting him upon his successful treatment of it, Sir Isaac told him that his head had never ached but when he was studying that subject; and Dr. Halley told Conduitt that he often pressed him to complete his theory of the moon, and that he always replied that it made his head ache, and *kept him awake so often, that he would think of it no more*. On a future occasion, however, he stated to Conduitt, that if he lived till Halley made six years' observations, "he would have another stroke at the moon."[2]

[1] Rigaud, *Hist. Essay*, p. 104.

[2] Conduitt's *Manuscript notes*.

In order to verify the equations which he had deduced from the theory of gravity, accurate observations on the moon were required; and, for the purpose of obtaining them, Newton had arranged, in the month of July 1691, to pay a visit to Flamsteed at the Royal Observatory of Greenwich. Learning, however, that Flamsteed was at that time from home, he postponed his visit, and intimated what had been his intention, in a letter of introduction which David Gregory delivered to the Astronomer-Royal in August 1691.[1] During this visit Gregory introduced the subject of the lunar irregularities, and, in a letter to Newton, gives him an account of the conversation which arose on this and other subjects. "Flamsteed," he says, "remembered you very kindly;" and, among other things, he said "that he did not believe the irregularity of the moon's motions in summer and winter is of that quantity your system would make it."[2] In the letter delivered by Gregory, Newton had advised Flamsteed to publish a catalogue of the correct places of such fixed stars of the first six magnitudes, as had been observed by others, and afterwards, by way of an appendix, those observed by himself alone,—an advice which, from causes perhaps not then known to Newton, struck a discordant key in the mind of Flamsteed. He believed that this advice was suggested by Halley, whom he considered as an enemy, who had misrepresented him to his friends as unwilling to print his observations. He enters, therefore, in a long letter,[3] into an explanation of his reasons for not printing his observations, and he concludes the letter with the severest animadversions upon Halley, which it is impossible to justify. "I have no esteem," he says, "of a man who has lost his reputation, both for skill, candour, and ingenuity, by silly tricks, ingratitude, and foolish prate; and that I value not all, or any of the shame of him and his infidel companions; being very well satisfied, that if

[1] Dated 10th August 1691, published in Baily's *Flamsteed*, p. 129.

[2] August 27, 1691, unpublished.

[3] February 24, 1692. Baily's *Flamsteed*, pp. 129-133.

Christ and his Apostles were to walk again upon the earth, they should not escape free from the calumnies of their venomous tongues. But I hate his ill manners, not the man. Were he either honest or but civil, there is none in whose company I could rather desire to be." Newton's reply to this letter, if he did reply, has not been found either among his own papers or those of Flamsteed.

Newton seems to have had no farther communication with Flamsteed till 1694,[1] when a correspondence took place between them, which was continued with little intermission for nearly two years, and with the nature of which the public was not till lately acquainted. The late Mr. Francis Baily having obtained access to the manuscripts of Flamsteed, in the possession of a private individual, and to other manuscripts and books of his which had been left in the Royal Observatory, found that they contained materials which he considered of inestimable value in the history of astronomy, and through the influence of the Duke of Sussex, the Lords Commissioners of the Admiralty were induced to print them at the public expense.[2]

The general effect of this publication, and of the sentiments expressed by Mr. Baily, was injurious to the memory of Newton; and as the work excited a high degree of interest in every part of the globe where science was cultivated, the friends of the injured philosopher were roused in his defence, and the

[1] In sending a copy of an unpublished letter on Earthquakes to a mutual friend, dated April 10, 1693, Flamsteed says, "Give my humble service to Mr. Newton, and let him know I owe him another concerning the present state of my labours, which I shall not fail to pay him now in a short time. It may satisfy him, that they go on successfully, and tend towards what they were designed for. I have thirty maps of the constellations drawn, having observed 2200 fixed stars visible by the naked eye, and having about as many left to observe, as will make them above 3000, which is above double the old catalogues."

[2] This work, with a preface and note by the editor, is entitled *An Account of the Rev. John Flamsteed, the first Astronomer-Royal, compiled from his own MSS., and other authentic Documents, never before published; to which is added, his British Catalogue of Stars.* By Francis Baily, Esq.: Lond. 1835. 4to. Pp. 671. A Supplement appeared in January 1837, in reply to criticisms by the friends of Newton.

scientific world is still divided on the subject. In justifying himself for publishing certain parts of the correspondence, Mr. Baily remarks, "that the personal motives for withholding them have long passed away, and now ceased to exist; and however unpleasant and painful it must be to an enlightened mind to find such eminent characters as Newton and Halley mixed up with subjects of the kind to which I shall presently allude, and pursuing a line of conduct towards Flamsteed which tends to make them appear less amiable in our eyes, yet a proper regard for truth and justice prevents any suppressions at the present day of the many curious and important (though often at the same time lamentable) facts which these manuscripts have, for the first time, brought to light. I have indeed," he continues, "in justice to the parties here alluded to, endeavoured to procure information of a contrary tendency from various sources, and sought for documents which might tend either to extenuate or explain the conduct of Newton and Halley in these proceedings, or to throw new light on the origin and nature of the quarrel that at a certain period of this history existed between Flamsteed and his two distinguished contemporaries, but, notwithstanding all my researches, I regret that it has been hitherto without success."[1]

In enumerating the repositories to which his researches extended in quest of information favourable to Newton and Halley, Mr. Baily mentions "the valuable collection of Newton's MSS. belonging to the Earl of Portsmouth," and states that he "found nothing in it to throw any light on the special object of his inquiries." From causes which I cannot explain, Mr. Baily had not lighted upon the letters of Flamsteed to Newton, which had been carefully preserved, and of which I have now before me nearly *forty*, which complete the correspondence[2] between these two distinguished individuals, and enable us to

[1] Baily's *Flamsteed*, pref., pp. xix. xx.

[2] Mr. Baily was able to publish only *eleven* of Flamsteed's letters to Newton, and these not correct copies of the originals.

form a more correct judgment on those delicate questions to which this controversy has given rise. Before proceeding, however, to give a general account of its history, the reader requires to have some knowledge of the position and character of the three distinguished men whose reputation is so deeply at stake.

Flamsteed, who was four years younger than Newton, held the high position of Astronomer-Royal, along with the small living of Burstow, in Surrey. His salary was only £100 a year, and he was allowed nothing from Government, either to provide or repair instruments, or to pay the expenses of a computer for reducing his observations. He was, therefore, obliged to purchase, or to construct with his own hands, the instruments which he used, and to pay the expenses of a servant capable of making the calculations which he required. His observations, consequently, were his own property, and no private individual was entitled to demand them. Flamsteed was, from his infancy, a person with a feeble constitution, and, when Astronomer-Royal, was afflicted with severe headaches, and with the stone and other painful distempers; but he bore these with Christian resignation, and never failed to exhibit in his conduct, and to express in his writings, the humblest submission to the Divine will. But with all his piety and virtues, there was a defect of character which it is necessary to state. He was prone to take an unfavourable view of the motives, as well as the conduct of those with whom he differed; and when such impressions were once made upon his mind, it was almost impossible to dislodge them. The following anecdote from the autobiography of William Molyneux, Esq., the friend of Locke, gives a view of Flamsteed, which is in every respect consistent with that which he displayed in his controversy with Newton and Halley:—"Mr. John Flamsteed, the King's astronomer at Greenwich, was formerly my constant correspondent for many years,[1] but

[1] Eighteen letters from William Molyneux to Flamsteed, written in the most affectionate terms, and dated between September 17, 1681, and May 17, 1690, inclusive, were published in the *General Dictionary*, Art. MOLYNEUX, vol. vii. p. 613.

upon publication of my *Dioptrics*, he took such offence at my placing a solution of his, of the 16, 17, and 18 propositions thereof, after, and not before, the solution I myself gave of the said propositions,[1] that he broke his friendship with me, and that, too, with so much inveteracy, that I could never bring him to a reconciliation, though I have often endeavoured it, so that at last I slighted the friendship of a man of so much ill nature and irreligion, how ingenious and learned soever."[2]

Mr. Newton was at this time Fellow of Trinity College, and Lucasian Professor at Cambridge, and though his *Principia* had been for six or seven years before the public, its value was known but to a few, and his great talents not sufficiently appreciated. He had been long acquainted with Flamsteed, and, in a correspondence which he had with him about the comet of 1680, Flamsteed had considered him as "magisterially ridiculing an unanswerable opinion of his," which turned out to be true.[3] In Newton's controversies with Hooke,[4] too, we have noticed some traces of personal feeling which might have been spared; but it is in his relations with Locke that some of those little imperfections of character are seen which slightly reappear in his communications with Flamsteed. We do not refer to the opinions which he expressed when under a nervous irritation, but to those occurrences which induced Locke, in 1703, to state confidentially to his cousin, Lord Chancellor King, "that Newton was a nice man to deal with, and a little too apt to raise in himself suspicions where there is no ground."[5]

Dr. Edmund Halley, who was twelve years younger than Flamsteed, was resident in London, and clerk and assistant secretary to the Royal Society during the correspondence between Newton and Flamsteed. He was a man of the world, and

[1] These propositions are referred to in his letter to Flamsteed, May 7, 1690.

[2] Molyneux's *Dioptrics* was published in 1692, and the Life, addressed to his brother in 1694. He died in 1698, at the age of forty-two. See *An Account of the Family and Descendants of Sir Thomas Molyneux, Bart.* Evesham, 1820. 4to.

[3] See Vol. i. p. 263.

[4] See Vol. i. p. 128.

[5] King's *Life of Locke*, vol. ii. p. 38.

much esteemed in society, but was generally supposed to entertain infidel opinions. Under this impression, Bishop Stillingfleet refused to recommend him to the Savilian Chair of Geometry in Oxford, when he was a candidate along with David Gregory; and Bishop Berkeley, on very imperfect information, rashly ventured to dedicate the Analyst to him as an "infidel mathematician." "Mr. Addison, as has been stated,[1] "had given Bishop Berkeley an account of their common friend Dr. Garth's behaviour in his last illness, which was equally unpleasing to both these advocates of revealed religion. For when Mr. Addison went to see the Doctor, and began to discourse with him seriously about preparing for his approaching dissolution, the other made answer, 'Surely, Addison, I have a good reason not to believe these trifles, since my friend Dr. Halley, who has dealt so much in demonstration, has assured me that the doctrines of Christianity are incomprehensible, and the religion itself an imposture.'" Flamsteed never scrupled to denounce Halley as a libertine and an infidel; and we regret to see that a modern writer has ventured to say that Halley was low and loose in his moral conduct, and an avowed and shameless infidel.[2] Had such been his character, he never would have been the friend and companion of Newton. It is quite true that Halley was sometimes checked by Newton when he had said anything that appeared disrespectful to religion, by the mild reproof, "I have studied these things—you have not;" and I have found a memorandum signed by Mrs. Conduitt, in which she says that Newton "could not bear to hear any one talk ludicrously of religion, and that he was often angry with Dr. Halley on that score, and lessened his affection for Bentley." Thus placed in the same category with Dr. Bentley, we have no doubt that Halley's speaking ludicrously of religion amounted to nothing more than his maintaining cer-

[1] *Biog. Brit.* vol. ii. p. 256, or, *The Works of* GEORGE BERKELEY, D.D., Bishop of Cloyne, p. viii. Lond. 1837.

[2] *Quarterly Review*, vol. lv. p. 112.

tain opinions about the existence of a pre-Adamite earth, and ridiculing vulgar errors which have been too frequently associated with religious truth.[1]

These three eminent individuals were, in the years 1694 and 1695, engaged in nearly the same researches. They were all intently studying the irregularities of the moon's orbit,[2] and had Halley not been a party, there is reason to believe that no difference would have arisen between Newton and Flamsteed. We have failed, like Mr. Baily, to discover the ground of Flamsteed's virulent antipathy to Halley, evincing a degree of hatred which no Christian could rightly cherish, and which no honourable man could avow, and still less record. The charge of infidelity and libertinism was, we fear, but the mask under which personal feelings were too readily expressed; and if David Gregory's memorandum of him be true, we have a satisfactory explanation of the origin of Flamsteed's enmity to Halley, in what Mr. Rigaud calls "his detected act of dishonesty." "Newton," says Gregory, "often told me, but especially in December 1698, that these tables (Flamsteed's lunar ones) were first made and computed by Edmund Halley, and communicated to Flamsteed, and published by him without the knowledge of Halley, and that this theft was the origin of the eternal quarrels between Halley and Flamsteed. Newton said that he had seen the handwriting of Halley."[3]

Under these circumstances, Newton and Halley were desirous

[1] We recommend to the reader the able *Defence of Halley against the Charge of Religious Infidelity*, by the Rev. S. J. RIGAUD, M.A., of Ipswich, Oxford, 1844. Professor Rigaud, the author's distinguished father, a man of genuine piety, entertained the same opinion of Halley.

[2] We owe to Halley the discovery of the secular equation of the moon.

[3] "The following curious memorandum," says Mr. Rigaud, "is written by Dr. Gregory in the margin of his annotations on the *Principia*, p. 162. The subject to which he has annexed it, is the mention of Flamsteed's lunar tables, derived from the hypothesis of Horrox (Schol. p. 462, first edit. of *Principia*), 'Newtonus mihi saepe dixit, nominatim Decembri 1698, Londini, tabulas hasce fuisse ab Ed. Halleio primum factas et supputatas, et cum Joh. Flamstedio communicatas, et ab illo, *inscio* Halleio, editas, et propter hoc factum æternas natas esse inter Halleium et Flamstedium rixas. Newtonus dixit se vidisse autographum Halleii.'"—*Defence of Halley*, p. 20.

of receiving from Flamsteed his observations on the moon,—observations of such value, that without them they could not proceed in their researches, and of such rarity that they could not obtain them from any other observatory in the world. In order to procure the observations which he required, Newton paid a visit to the Royal Observatory on the 1st of September 1694. Flamsteed showed him 150 places of the moon calculated from his own observations, either by himself or "his hired servants," with the differences, in three synopses, between these places and those in the common tables of the moon, "in order to correct the theory of her motions." These observations were given to Newton on two conditions, which he accepted, 1st, That he would not, without Flamsteed's consent, communicate them to anybody; and 2dly, That he would not, in the first instance, impart the result of what he derived from them to anybody but himself.[1]

[1] Flamsteed, who makes this statement in his autobiography, concludes it by saying, "All this he (Newton) approved, and by a letter of his dated . . . . . confessed. Nevertheless he imparted what he derived from them both to Dr. Gregory and Dr. Halley, *contra datam fidem*. The first of these conditions I believe he kept. The latter he *has forgot* or broke."

In defence of Newton, we may state, that in a few days after Flamsteed exacted the *first* of these conditions, he not only showed the same observations to Halley, but suffered him to take notes of part of them. With regard to the *second* condition, which he is said to have broken, we shall presently see, from an unpublished letter of Flamsteed, that he asks Newton certain questions about the moon's theory, and that Newton imparted to him his remarkable equation of the menstrual parallax. We shall find also that he imparted to him, *in return for his observations*, his theory and table of refractions, one of the finest productions of his genius, and of essential value to Flamsteed in the reduction of his observations, and subsequently his valuable tables of the moon's parallax, and the equations of the moon's apogee and the eccentricity of her orbit. It appears, too, from a letter of Newton of the 17th November 1694, that he asks Flamsteed to have but a little patience, and he will be the first man to whom it will be imparted, *when the theory is fit to be communicated without danger of error*. In consequence of the delay in getting Flamsteed's observations, he was not able to proceed any farther with the lunar theory, and his appointment to the Mint necessarily interfered with his scientific researches. His connexion with Flamsteed had ceased for many years, and therefore the brief notice of the lunar theory which he communicated to Gregory in June 1702, could not be considered as a breach of the condition under which Flamsteed brought him.

That the reader may be sufficiently aware of the rash charges which Flamsteed never

In a few days after this visit, Flamsteed addressed a letter [1] to him acknowledging the return of the two synopses of the moon's places, offering him more when he signifies his having occasion for them, and informing him that he "intends hereafter to cause his man to calculate them both from the observations and tables as soon as observed, whereby it will be soon evident whether the heavens will allow these new equations you introduce, and if they will, how they are to be limited." During his visit to the observatory, Newton must have expressed a wish for observations to test his theory of atmospherical refractions, for Flamsteed mentions that he has set himself to inquire what refractions can be got from his observations, and promises that whatever he gathers from them shall be freely imparted to him. "I shall never," he says, "refuse to impart either the observations themselves, or my deductions from them, to any person that will receive them with the same candour as you do. If I desire to have them withheld from others who make it their business to pick faults in them, and censure them, and asperse me no less unjustly than ungratefully, you will not blame me for so doing. When H. (Halley) shows himself as candid as other men, I shall be as free to him as I was the first seven years of our acquaintance, when I re-

scrupled to make against those who displeased him, we quote the following example contained in his own letters, which Mr. Rigaud has observed:—"In 1705, Abraham Sharpe communicated to the Royal Society his quadrature of the circle; and Flamsteed writes him an account in which Halley is accused of acting most unfairly, and with a view to his own credit, about printing the papers. This was on the 20th August, and on the 11th of the following month, Flamsteed found himself obliged to retract what he said on the subject, and yet in April 1715 he had forgotten everything but what accorded with his hostile feeling, and writes to the very same man to say, 'you remember how he served you about the quadrature of the circle; after such usage, you ought to be very cautious how you treat him.'"—*Defence of Halley*, pp. 20, 21, and Baily's *Flamsteed*, pp. 244, 246, and 313.

[1] Newton seems to have been unwell at the time of his visit to Greenwich, for Flamsteed begins this unpublished letter, dated September 7, 1694, with the intimation that he had sent him a receipt which Mr. Stanhope's sister makes use of with good effect, and wished he might find the same benefit from it. In his reply, Newton "thanks him heartily for the receipt."

fused him nothing that he desired. I am told by a friend of his that he is very busy calculating the moon's places on a sudden. Perhaps some hints he has got from you have set him to work anew, but except you have been as plain with him as you were with me, I am satisfied he will never be able to find out the parallactic equation,[1] nor limit it without a bigger store of observations than he is possessed of, though he have many of mine made between 1675 and 1682.

" Since you went home, I examined the observations I employed for determining the greatest equations of the earth's orbit, and considering the moon's places at the times of . . . , I find that (if, as you intimate, the earth inclines on that side the moon then is) you may abate ab^{t} 20″ from it, say y^{t} it may be only 1° 56′ 00″."

Flamsteed concludes this curious letter, the first of the series, with a request that Newton would acquaint him how the observations agree with his conception, and with an offer of more observations upon due notice.

To this letter[2] Newton returned an answer which must have been very agreeable to Flamsteed. After comparing the observations with his " conception," he was satisfied that by both together the moon's theory might be reduced to the exactness of 2 or 3 minutes, and that he believed he would be able to set it right this winter. For this purpose, however, he requested certain observations which he specified.

In a few days Flamsteed replied in a long letter of October 11, 1694, in which he mentions that Halley had applied for a sight of the lunar observations, and that he had come to Greenwich and taken notes from the synopses of the moon's places, which Newton had received,—an act of kindness which Flamsteed did not grant without " minding him of his disingenuous behaviour in several particulars." [3]

[1] The coefficient for this equation is the Sine of the sun's parallax divided by that of the moon's.

[2] Dated October 7, 1694.

[3] The original of this letter differs from that published by Mr. Baily in two points.

For the table of refractions near the horizon, Newton was particularly thankful.[1] He ascribed the differences of refraction at the same altitude to the different temperatures of the air, and suggested that Flamsteed should in all his observations note the state of the barometer and thermometer. He told him that he had dined with Halley, and had much discourse with him about the moon; that Halley had asked for a sight of the observations which made the parallactic equation between 8′ and 10′, but that he had refused on account of the engagement to communicate them to nobody without his consent. "I am glad," he adds, "that there is like to be a new correspondence between you, and hope it will end in friendship."

Flamsteed, in his reply,[2] is delighted to hear of the agreement between his observations and the theory. He offers to recalculate any of the observations that may appear incorrect, and promises a new synopsis of the moon's places, along with one of all the observations from which he drew the small empirical table of the refractions. "Yesterday at London," he says, "I had a great deal of talk with Mr. Halley about the moon's motion. He affirmed the moon's motion to have been swifter in the time of Albategni than at present, and that the cause of it was by reason that the bulk of the planets continually increased. I gave him the hearing, and at last told him that his notion was yours, he answered 'in truth you helpt him with that.'[3] He affirms farther, that the moon's apogee moves swifter in winter than in summer, and that the greatest equations of it are biggest when the sun is in perigee. That they are as big as Copernicus makes them, that is 13° 9′.

The "empirical small table of the differences of refraction of the sun and Venus in height" has been omitted in the published copy, and also the following postscript. "Mr. Halley is busy about the moon, has promised me his corrections, intends to print something about her system ere long, and affirms the moon's motion different in the times of Albategni from what it is now." I have given this table in the APPENDIX, No. XI., in order to justify the references to it in the letters of October 11 and 24, 1694.

[1] October 24, 1694.

[2] October 25, 1694, unpublished.

[3] See *Principia*, 2d Edit. p. 481.

This smells too, of your theories. I remember that you affirm all the equations biggest when the earth is nearest the sun. I should be glad to hear that you had found in what proportion the equations of the apogee and the eccentricity alter, and what are their greatest differences in the last, the quantity of the first, how the variations alter, and that you would please to impart it to me, that so hereafter I may calculate on sure grounds, and compare not an apparently erroneous, but a true theory with my observations, whereby its faults may be corrected."

In Newton's reply of the 1st November, he answers Flamsteed's questions, and explains to him the menstrual parallax of the sun, which he estimates at 16″ or 20″, an equation depending on the ratio of the masses of the earth and moon, and which, as M. Biot remarks, is one of the most delicate corrections in our modern tables, amounting only to 8″.

"Perceiving[1] that Newton was as yet only trying how his observations would consist with y^e emendations, and that he had not as yet limited them to his mind," Flamsteed would not urge him any farther for them, but trusts that when he has "determined what corrections or additions are to be made to that theory which it was his good fortune to meet with, and usher into the world, he doubts not but you will impart them as freely as he did the observations, where you limit or confirm them, to you."[2]

On the 17th November, Newton sends to Flamsteed his table of refractions, computed by applying a certain theorem to his observations; and he explains to him his plan of first obtaining a general notion of the lunar equations to be determined, and then by accurate observations to determine them, seeing "that there is a complication of small equations, which can never be determined till one sees the way of distinguishing

[1] November 3, 1694, unpublished.

[2] In this letter Flamsteed says, that the parallactic equation does not exceed a single vibration of the pendulum, and cannot be determined by the largest instruments.

them, and attributing to each their proper phenomena." He asks Flamsteed to have a little patience with him till he has brought the theory he ushered into the world to competent perfection, fit to be communicated to him; and he promises "to gratify him to his satisfaction for the trouble he is at in this business."

In replying to this letter, Flamsteed[1] says that "he was

[1] This letter of Flamsteed's, as published by Mr. Baily, differs entirely from the letter actually sent to Newton, and must have been a scroll, which he greatly altered and enlarged. *We cannot, therefore, place confidence in the abstracts of his letters to Newton, as printed by Mr. Baily.* The date of the letter is December 16th, not the 6th. In the original copy of this letter, and also in the scroll, Flamsteed introduces a new charge against Halley in the following words:—"I desired you in my last to let me know if you had not been presented some years agone with a geometrical tract of Viviani's, in quarto, Latin. You have given me no answer. Pray, be free with me, and let me have one, it will much oblige." In his letter of the 27th, he had said,—"I desire you to let me know whether Mr. Halley did not, five or six years ago, present you with a geometrical piece of Viviani's in quarto?" Newton made no reply to these requests. On the 31st December, Flamsteed thus recurs to the subject:—"I must beg your pardon for having urged you twice about Viviani's book. I shall tell you the occasion, and give you no further trouble. Mr. Rook being in Italy, received one of them directed to me by the author's own hand, which he sent to E. H. (Edmund Halley) with other things, who, I am told, presented it to you: and himself denies not that he sent it you. Now, I am not concerned for the book at all. If you had one from him, keep it either as his gift or mine; but because I have great reason to suspect a book of much greater value, directed to me, has been disposed of for advantage by a friend and acquaintance of his this last summer, and if the first had been brought to light, the latter might have been made evident; but I desire to concern you no farther with it, and therefore shall move you no more, nor expect any answer in this particular, being ever desirous to make my friends as easy as I can." To these applications Newton replied on the 26th Jan. 169$\frac{4}{5}$,—"About three or four months before Dr. Gregory was made Professor of Astronomy at Oxford, an Oxford gentleman, a student in mathematics (I think his name was Rook), called on me on his way from London, and showed me a new book published by Viviani. He offered to leave it with me to peruse; whereupon I turned over the leaves, and then returned it to him again, and he took it away with him, I think, to Oxford; and I saw it no more. I forbore to answer your first inquiries about it, because I feared it might tend to widen the breach between you and Mr. Halley, which I would rather reconcile if it were in my power. And now I hope that what I have told you will not be made use of to that purpose, lest it should also do me an injury."—See Baily's *Flamsteed*, pp. 144, 145, 148, 149. "I am very well satisfied," replies Flamsteed on the 29th January, "in what you tell me about Viviani's book; and you may conclude what you are to think of Mr. Halley from this, that he told me before a club of the Society that you had it. I find you understand him not so well as I do. I have had some years' experience of him, and a very fresh instance of his ingenuity, with which I shall not

displeased with him not a little for the offer, and ascribes it to the suggestion of some malicious friend, and assures Newton that he never did, and would scorn to receive money for any such service." Newton makes an apology[1] for the mistake he committed, and requests that Flamsteed will let it pass, and concur with him in the promotion of astronomy; and, in order to appease his friend, he sends him the beautiful theorem by which he computed his table of refractions,—a theorem which M. Biot justly characterizes as giving the true analytical expression of the differential of refraction, such as it is now employed, and which cannot but be regarded as one of the highest efforts of Newton's genius.[2]

In answering this letter on the 31st December, Flamsteed enlarges on the subject of refractions, and transmits a table of morning and evening refractions observed in June 1678, from 79° to 89° 50′ of zenith distance. He sends him two lunar observations, and explains why he has not sent him others that he had made. Newton informs him in return,[3] that the theorem on refractions which he had sent him was defective in making the refractive power of the atmosphere as great at the top as at the bottom, and that he had found another theorem which required consideration. "In the former theorem," he continues, "the areas are to be determined by the fifth lemma of the third book of the *Principia*." He then explains to him how the air being colder and more dense at sunrise, the refractions

trouble you. 'Tis enough that I suffer by him. I would not that my friends should, and therefore shall say no more, but that there needs nothing but that he show himself an honest man to make him and me perfect friends; so that if he were candid, there is nobody living in whose acquaintance I would take more pleasure; but his conversation is such that no modest man can bear it, and no good man but will shun it." The four obnoxious paragraphs in the draught of this letter, at the bottom of p. 150 of Baily's *Flamsteed*, do not exist in the original sent to Newton!

[1] December 20, 1694.

[2] See Biot's interesting observations on this theorem, and his admirable and elaborate *Analysis of Newton's Tables of Refraction, with an indication of the Numerical Processes by which he computed them in the Journal des Savans*, 1836, pp. 642, 735.

[3] January 15, 1695.

are then greater, and how from its being rarer in the evening, the refractive power is diminished. Flamsteed admits, in his reply,[1] that the change of temperature is the principal, but not the sole cause of the alterations in the morning and evening refractions. Sir Jonas Moore told him often, that when he lived in the fens, he often saw "the beasts raised to his sight very much by the fogs that lay betwixt him and them; and he has heard the late King Charles and old sea-captains talking together about the sea-air, and relating how, standing upon Dover beach at high-water, they saw the streets of Calais very plain, but whilst they stood, as the water sunk, these objects sunk and at last disappeared."

Considering a table of refractions as the foundation of astronomy, and very necessary for Flamsteed's great work, Newton[2] is anxious to present him with one in return for his observations, and, as he has found a new theorem which makes the calculation easy, he hopes, when he has recovered from a slight indisposition, to finish it. "Supposing," he says, "the atmosphere[3] to be of such a constitution as is described in the 22d Prop. of my second Book (which certainly is the truth), I have found, that if the horizontal refraction be 34′, the refraction in the apparent altitude of 3° will be 13′ 3″; and if the refraction in this apparent altitude of 3° be 14′, the horizontal refraction will be little more than 37′." On the 15th March, Newton sends him his table of refractions, in which, at 3° of altitude, the refraction is 13′ 20″, so that the table is the same as that published by Halley in the *Phil. Trans.* for 1721. "Newton," as M. Biot remarks, "is therefore the creator of the theory of astronomical refractions, as he is of that of the theory of gravity. But the first of these titles was hitherto unknown to us; and we can now see, that it is not one of his works which has given him the least trouble, on account of the number, the variety, and the dispersion of the physical elements

[1] January 18, 1695. [2] January 26, 1695. [3] February 16, 1695.

which he required to discover, to collect, and to combine in its establishment."[1]

In his letter of the 23d April 1695, Newton says, "When I set myself wholly to calculations (as I did for a time last autumn, and again since Christmas, in making the table of refractions), I can endure them, and go through them well enough; but when I am about other things (as at present), I can neither fix to them with patience, nor do them without errors, which makes me let the moon's theory alone at present, with a design to set to it again, and go through it at once. When I have your materials, I reckon it will prove a work of about three or four months; and when I have done it once, I would have done with it for ever."

After writing other two letters, in one of which he expresses his desire to get the naked observations on the R. ascension and meridional altitude of the moon, and have them calculated by "his servant Sir Collins," he addresses Flamsteed in the following manner:[2]—"After I had helped you where you had stuck in your three great works, that of *the theory of Jupiter's satellites*, that of your *catalogue of the fixed stars*, and that of *calculating the moon's places from observations*, and in all these things freely communicated to you what was perfect in its kinds (so far as I could make it), and *of more value than many observations*, and what (in one of them) contains more than two months' hard labour, which I should never have undertaken but upon your account, and which I told you I undertook that I might have something to return you for the observations you then gave me hopes of, and yet when I had done, saw no *prospect of obtaining them*, or of getting your *synopses rectified.*[3] I despaired of compassing the moon's theory, and had thought of giving it over as a thing impracticable, and occasionally told a friend so, who then made me a visit. But now you offer me these observations which you made before the year 1690, I

[1] *Journal des Savans*, November 1836, p. 655.

[2] July 9, 1695.

[3] These passages were underlined by Flamsteed.

thankfully accept of your offer, and will get as many of them computed as are sufficient for my purpose."

We cannot find in the seven unpublished letters which Flamsteed wrote to Newton from February 7th to July 2d, 1695, inclusive, anything to justify this letter. Flamsteed begins his letter of February 7th with a long tirade against Halley, and promises that when they meet he will tell him his history, which is too foul and large for a letter: He mentions two different reports from London of Newton's death, which he was able to contradict: He tells him that his servant, his computer, has run away, and that he is teaching another: He sends him observations on refractions and on the eclipses of the moon in 1678 and 1682, and he complains of a report which, at his request, Newton succeeds in putting down, that Flamsteed refused to impart his observations to him. This request is preferred in the following manner:[1] "You see how willing I am to accommodate you with what is necessary for clearing the motion of the moon, and how small a return I desire,—that is only to know what equations you use at present in the moon, and what limitations you give them. Not that I have any desire or design to meddle with the restitution of her motions myself, but only to satisfy my own curiosity, and not to be ignorant of the use you have made of what you imparted to me, as I told you before. Only I must desire you to acquaint Mr. Bentley (whom I know not), but who, I am told, complains that the second edition of the *Principia* will come out without the moon, because I do not impart my observations to you, that I *shall furnish you to your satisfaction in that particular.*[2] Had I heard of it from yourself, I had told you the contents of this letter some days since, and assured you the fault should not be laid to my charge. And he adds in a postscript, "What one friend may justly expect from another, you shall ever command from yours, J. F."

In his very short letter of July 13, 1695, Flamsteed takes

[1] July 2, 1695.

[2] The *italics* are in the original.

no notice of the attack of Newton. He promises places of the fixed stars and the nonagesimal table, and adds, "A report is industriously spread in town that I have refused to impart any more observations to you. I heard that he who spreads it intends you a visit ere long. I hope you will take notice of his disingenuity in this particular, since 'tis only my violent distemper and your own silence that were the cause of mine. I shall answer yours more fully next week."

On the 18th Flamsteed replies, as might have been expected, to the charge which had been made against him. "I have just cause," says he, "to complain of the style and expression of your last letter. They are not friendly, but that you may know me not to be of that quarrelsome humour I am represented by the Clerk of the Society (Halley), I shall waive all save this expression, *that what you communicated to me was of more value than many observations.* I grant it—as the wire is of more worth than the gold from which it was drawn.[1] I gathered the gold matter, and fined and presented it to you sometimes washed. I hope you value not my pains the less because they became yours so easily. I allow you to value your own as high as you please, and require no other reward for what assistance I sometimes afford you, but that I may now and then see some of the workmanship; and if that be not ready when I desire it, or if you think it not fit to favour me with it, I can easily be contented. Nor do I take it amiss that you often take no notice of some small particulars whereon I have desired to know what you have determined. Since I know very well that in things of their nature it is difficult to determine, and we often change what at first we thought would need no alteration or towards none. I have altered my solar numbers five times, and would not be ashamed to change again if I saw reason for it. If you answer me that you have not

[1] "Machin told me," says Conduitt, "that Flamsteed said 'Sir Isaac worked with the ore he had dug,' to which Sir Isaac replied, 'If he dug the ore, I made the gold ring.'"—*Conduitt's* MSS.

determined whether any other than the usual equations are to be used in the Syzigies, if you are not resolved how the moon's mean motion is to be corrected, you may say it. I shall urge you no farther, and nevertheless whenever you let me know that it lies in my power to serve you, I shall do it freely. But you will not complain of me to others without cause, and thereby add to the affliction I suffer from my obstinate distempers, and the calumnies of disingenuous and impudent people, if you have any value for your friend and humble servant."

This earnest remonstrance is acknowledged by Newton on the 20th of July, the day that he received it. "The report," he says, "was against his mind, and he has written to put a stop to it. . . . Such expostulations or expressions in your last and some other letters, as tend to a difference, I pass by. Pray take care of your health. Dr. Battely (chaplain to Archbishop Sancroft) was much troubled with violent headaches, and found it a certain cure to bind his head strait with a garter till the crown of his head was numbed; for thereby his head was cooled by retarding the circulation of the blood. 'Tis an easy remedy, if your pain be of the same kind."

Flamsteed was gratified with this letter. He thanked Newton[1] for Dr. Battely's remedy, as it "shows your friendly concern for my welfare." "Your letter," he says, "sets all right betwixt us. I have as great a stock of patience, and as good an one as I have of observations, and 'tis all ways drawn out on every occasion to serve my friends. My indisposition hindered me from serving you as I desired. You mistook the reason of my silence. I hope you will have the patience on my account that you demand of me on yours. . . . The next week I am going to my parsonage, but I shall take care to have you furnished with another sheet of observations before. If you would rather have any other than the remains of 1677, let me know it. I shall fit you according to your desires."[2]

[1] July 23, 1695.

[2] In this letter, Flamsteed "presents him, before he demands it," with "a nonagesi-

In replying to this letter in the following week,[1] Newton tells Flamsteed "that he had an excuse sent him (from Bentley or Halley) for what was said at London about your not communicating, and that the report should proceed no farther." He is glad all misunderstandings are composed. He thanks him for the nonagesimal table, "which he designed to make himself, as it saves him labour." And he adds, "that as the transcribing of these things gives your servant trouble, and, for encouraging him, I shall order Will. Martin, the Cambridge carrier, to pay him *two guineas if you please to let him call for it, or to pay it to his or your order in London, if you please to let me know where.*"[2]

Flamsteed was annoyed by this proposal to pay his servant. "I take it very kindly," he says,[3] "that you acquainted me with your intent to gratify him for his pains before you did it, but I must entreat you to forbear. He is paid already. A superfluity of moneys, I find, is always injurious to my servants. It makes them run into company, and waste their time idly, or worse. I take care he wants nothing. If you send

mary table" for every degree of right ascension, "as I would not have you want any thing that lies in my power to save you the trouble of calculation;" and he closes his letter thus: "By frequent trials and alterations of his contrivances, Kepler found out the true theory of the planetary motions. You must not be ashamed to own that you follow his example. When the inequalities are found, you will more easily find the reason of them than he could do when but little of the doctrine of gravity was known."

[1] July 27, 1695.

[2] In the copy of this letter in the British Museum, the words *two shillings* appear here with the following note:—"Mr. Flamsteed altered it so for the word *guineas*, which is in the original, as is evident from the erasure." Professor Rigaud, at Mr. Baily's desire, examined the original letter, and found the words *two guineas if you please to call for it* crossed out with the pen, but no substitution of *shillings* for *guineas*. Mr. Edleston, however, who has examined the original, observes that all the words following "pay him" (in the passage given in the text in *italics*) are crossed out in the manuscript, and the word "guineas" altered into "shillings" *apparently* by Flamsteed. The words after "for them" to the end of the passage are conjectural, the original writing being most skilfully blotted out. . . . What motive Flamsteed could have had for disguising any part of the above sentence, I do not pretend to divine. It is curious that Mr. Rigaud, who examined the manuscript in reference to this very point, should have overlooked the original "guineas."—Edleston's *Correspondence*, &c., p. lxviii. note 125.

[3] August 4, 1695.

him verbal acknowledgments of his pains, and commendations for his care and fidelity in copying, it will be a reward for him, and encouragement the best you can give him, and further I cannot allow. . . . Pray say nothing to anybody of your proposal."

In another letter on the 6th of August, he says, that during the last six years "he had done more towards the restitution of astronomy than has been done in some ages before;" and, after mentioning what he has accomplished in the nineteen years that he has been at Greenwich, he adds,—"I write this purposely to you, because I know a sparke (Halley) is with you, that complains much I have lived here twenty years and printed nothing. I do not intend to print a St. Helena catalogue, and for that reason I defer the printing of anything thus long, that when I do print it may be perfect, as by the grace of God it shall."

Newton closes this correspondence with a short letter, dated September 14, in which he intimates that Halley's determination of the orbit of the comet of 1683, by his theory, "answers all your observations and his own to a minute;" that he has just returned from a journey into Lincolnshire, and is going on another; and that "he has not got any time to think of the theory of the moon, or have leisure for it, for a month or above."

Flamsteed answered this letter on the 19th September,[1] complains of his ill health, considers the theory of gravity confirmed by its giving the orbit of the comet conformable to observation, and hopes, by travelling, to have some small share of health left wherewith to serve his friends, "and to supply you with what is wanting to finish the theory of the lunar motions, which I hear you doubt not now but to render very nearly agreeable to the heavens."

Having heard nothing from Newton for four months, Flam-

[1] Not on the 17th, as stated in Baily's *Flamsteed*, p. 160. Flamsteed's notes of his answer to Newton's letter, as usual, misrepresents its contents.

steed writes him on the 11th January 1696, and, after offering him "further observations of the moon," which may be of use to him, he says,—"But if what I hear be true, you will have little need of them, for I have been told, ever since I came out of Surrey, that you have finished the theory of the moon *on incontestable principles ;* that you have determined six general inequalities not formerly known ; and that nevertheless the calculations will not be much more troublesome or difficult than formerly. I am heartily glad to hear this, and should be more so to have it from yourself, for in truth I suspect you are scarce so forward ; and I flatter myself with the opinion, that if you were, you would have acquainted me with it, as you promised both when I imparted the three synopses of lunar calculations, and observed places to you, and in your letters since. Pray let me know how far you are proceeded, you will oblige me, and, if you please, the true reason *why I have had no letters from you this four months*."[1] Newton does not seem to have answered this letter. His appointment to the Mint, though not officially communicated to him, was well known ; and all his time must have been occupied in preparing for the discharge of its duties.

In reviewing the remarkable correspondence which terminates with the preceding letter, and which has been regarded in such different lights, we have no hesitation in saying, that the two charges against Flamsteed of ignorance of the importance of the theory of gravity, and of unwillingness to supply Newton with the observations he required for his lunar theory, have no sufficient foundation. With the exception of those occasional bursts

[1] In this letter, Flamsteed tells him that "some friends of his who live at a distance in the country, have made new tables for representing the motions of the two superior planets, Jupiter and Saturn," within ten or twelve minutes of observation. I find other four letters from Flamsteed to Newton, dated September 4, December 10, 1697, December 29, 1698, and January 9, 1699. The last of these letters is a long and curious reply to Newton on the subject of his letter of the 6th January 1699, blaming Flamsteed for mentioning his theory of the moon in a letter on the parallax of the fixed stars, sent to Dr. Wallis to be printed. The consideration of these letters belongs to another Chapter.

of spleen against Halley, which must have been annoying to his friend, his letters to Newton—though sometimes of an irritating tendency—are yet respectful, and even affectionate, and exhibit not only a willingness, but an anxious desire to supply him with every observation he possessed, and even to make and to reduce new observations expressly for his use. His ill health, which often required him to travel for its recovery—his severe headaches, a pulmonary affection, and sharp attacks of the stone and gravel, frequently unfitted him for observing and reducing his observations, while the want of computers, for whose labours he was obliged to pay, and the necessity of visiting his living at Burstow, often prevented him from communicating his observations as quickly as Newton wished, and as he himself desired. When his letters are published, and read along with those of his correspondent, his good name will not, from this cause, greatly suffer in the estimation of posterity. Flamsteed was not the less a great man that he has been confronted with the greatest.

But while we thus justify the Astronomer-Royal, we must make some apology for the philosopher. Newton was not in good health during the correspondence which we have been examining. The depths of his mind were stirred with the difficulties of the lunar problem. The new views which burst upon him in its solution could be tested only by observation; and they who have felt the impatience of spirit when a speculation waits for the verdict of an experiment or a fact, or who have started from their midnight couch to submit a happy idea to the ordeal of observation, will understand the sensitiveness of Newton when he waited whole weeks for the precious numbers which the Observatory of Greenwich only could supply. Newton certainly thought, as his letter of the 9th July shows, that the Astronomer-Royal had not been sufficiently active in his cause; and though he knew that he had no other right but that of courtesy to the observations he required, yet he had established another ground of right which he was entitled to urge,—the social right of reciprocal obligation. He had given Flamsteed

valuable tables of refraction, and had computed
nths of labour, the equations for the apogee and
d important tables of horizontal parallaxes, for
pose of making some return for the observations
which he required. He was, therefore, entitled to press these grounds of claim upon Flamsteed when he thought "he saw so little prospect of obtaining what he wanted," as to make him "despair of compassing the lunar theory," and "giving it over as a thing impracticable."

Regarding the letter of Newton in this light, we have been greatly surprised at the view taken of it, and indeed of all his letters, by M. Biot. No philosopher, either of our own or of Newton's day, has done more justice to his labours, or shown a deeper affection for his memory, than that distinguished philosopher; and there is no living writer, whose appreciation of the feats of science is more valuable than his; but the view which he has taken of the idle story of M. Colin and the dog Diamond, charged with fire-raising among Newton's manuscripts, and of the influence of this accident upon the mind of their author, is to us, and, we believe, to every Englishman, utterly incomprehensible. The story of the burning of the papers about 1691 or 1692, is entirely fabulous;[1] but even if it were true, it produced no effect upon Newton's mind. His illness at that time, the want of his usual consistency of mind, a condition which every deep thinker must have experienced, arose, as he himself distinctly declares, from want of sleep and appetite during the preceding year.

"Is it then," says M. Biot, "going too far to see in the *incoherence* of these letters (the letters to Flamsteed) a fatal resemblance to those which Newton wrote to Pepys and Locke *two years* before, and almost in the same months? Do we not equally discover in them the morbid irritability of a mind fatigued by the continuity of its meditations, and which, *according to the avowal of Newton himself, could no longer sustain*

[1] We have already shown that this accident happened before 1684.

such great efforts?[1] And if it be true that, at the end of 1692, the fire which destroyed a part of his works had already produced in him moral symptoms of the same kind, still more distressing, why should we be surprised to see him brought back to it by the renewal of researches as profound and as fatiguing on account of the vagueness of the data at his command, as were those which he executed and attempted on refractions and the lunar theory, from the months of October 1694 to September 1695, as we have already related. *This was his last spark.*"[2]

M. Biot has expressed his surprise at the sensitiveness of Englishmen, on the allegation that Newton "had fallen into phrenitis"—that is, was insane in 1692; but, however great that surprise may be, it cannot be equal to that which they feel at his persisting in the statement, and at the offensive aggravation of it, which is contained in the preceding extract. Before M. Biot had read the letters to Flamsteed, he had declared that Newton's intellect was permanently weakened by his illness of 1692, and yet he now finds, by the perusal of these letters, that Newton had put forth his highest powers *two years* after that event! But what surprises and offends us, and what must offend every friend of truth and of genius, is his new allegation, that Newton's great intellectual efforts in 1694 and 1695 brought him back into his phrenitis of 1692, from which he never recovered his usual powers of invention and discovery. His letters to Flamsteed exhibit no such symptoms, no incoherence of mind, and no failure of a mental or moral nature. In 1696, when he exchanged the daily pursuit of science for the active and engrossing duties of official life, he was capable of developing the highest powers of his genius. He displayed them in the preparation of the second edition of

[1] Newton has made no such avowal. Biot quotes, in support of his allegation, Newton's declaration to Locke, that "he had not his former consistency of mind,"—a mere temporary state, from which he completely recovered.

[2] *Journal des Savans*, November 1836, p. 657.

the *Principia*, and in his *Optics.* They appeared fresh and vigorous during the fluxionary controversy. They shone with a more subdued light in the discharge of his duties at the Mint ; and no period of his life can be named when his intellectual arm was shortened, or his mental eye was dim. Even in extreme old age, his robust frame protected from decay the bright spirit which it enclosed, and, ripe for the spiritual world which he had ever contemplated as his home, he adorned the last years of his long and honoured life with the humility of the sage and the graces of the Christian.

CHAPTER XIX.

No mark of National Gratitude conferred upon Newton—Friendship between him and Charles Montague, afterwards Earl of Halifax—Montague appointed Chancellor of the Exchequer in 1694—He resolves upon a Re-coinage—His Letter nominating Newton Warden of the Mint in 1696—Newton appointed Master of the Mint when Montague was First Lord of the Treasury—His Report on the Coinage—Anecdote of his Integrity when offered a Bribe—He obtains for Halley the Deputy-Comptrollership of the Mint at Chester—Quarrels among the Officers there—Disturbances in the London Mint—New Misunderstanding with Flamsteed—Remarkable Letter to him from Newton—Newton's conduct defended—The French Academy of Sciences remodelled—Newton elected one of the Eight Foreign Associates—M. Geoffroy describes to Dr. Sloane the change in the Academy—Newton resigns his Professorship and Fellowship at Cambridge—Whiston appointed his Successor—Newton elected Member for the University in 1701, and President of the Royal Society in 1703—Queen Anne confers upon him the honour of Knighthood in 1705—Love-Letter to Lady Norris—His Letter to his Niece, Miss C. Barton—Account of Sir William and Lady Norris—Letters of Newton about standing for the University in 1705—Letters of Halifax to Newton on that occasion—Newton and Godolphin defeated.

Hitherto we have viewed Newton chiefly as a philosopher, leading a life of seclusion within the walls of a college, and either engaged in the duties of the Lucasian Chair, or constantly occupied in mathematical and scientific inquiries. He had now reached the fifty-third year of his age, and though his friends had exerted themselves to procure him some permanent appointment, they had failed in the attempt. An event, however, now occurred which relieved him from his labours at Cambridge, and placed him in a situation of affluence and honour.

Among his friends at Cambridge, Newton had the good fortune to number Charles Montague, fourth son of George Montague, Esq. of Harton in Northamptonshire, whose father was Henry, first Earl of Manchester. He was born on the 16th April 1661, and exhibited early indications of genius and talent. From Westminster school, where he was elected King's Scholar, he entered Trinity College, Cambridge, as a Fellow Commoner on the 19th November 1679, and received the degree of M. A. by royal mandate on the 6th October 1681. Here he became acquainted with Newton; and though devoted to literary pursuits, an ardent friendship arose between them which various causes contributed to strengthen and maintain. In the year 1685, when Montague was only twenty-three years of age, we find him co-operating with Newton and others in establishing a Philosophical Society at Cambridge; but though both of them had made personal application to different individuals to become members, yet the plan failed from the want of persons willing to try experiments, and from the refusal of one individual on whom they relied for that species of assistance.

While yet at college, Mr. Montague was brought into notice by a poem which he wrote on the death of Charles II. in 1685. The Earl of Dorset, who happened to admire it, invited him to London, where an incident occurred which "led him on to fortune." Having, in conjunction with Matthew Prior, published a poem entitled "The Hind and the Panther, transversed to the Story of the Country Mouse, and the City Mouse," his patron the Earl of Dorset introduced him to King William in the following manner: "May it please your Majesty, I have brought *a mouse* to have the honour of kissing your hand," and having learned the reason why Mr. Montague was so called, he smiled and replied, "You will do well to put me in the way of making *a man* of him," and he immediately gave orders that a pension of five hundred pounds per annum should be paid to him out of the privy purse till an opportunity should occur of giving him an appointment. When Prior learned the good fortune of the more favoured mouse, he wittily exclaimed—

"My friend Charles Montague's preferred,
Nor could I have it long observed
That one mouse eats, while t'other's starved."

In 1687, when Newton was occupied with the completion of his *Principia*, he was in correspondence with Montague, whom he characterizes as his "intimate friend,"[1] and notwithstanding the contrariety of their pursuits, and the great difference of their age, the young statesman cherished for the philosopher all the veneration of a disciple, and his affection for him gathered new strength as he rose to the highest offices and honours of the state.

Mr. Montague sat along with Newton in the Convention Parliament, and such were his habits of business, and the powers which he displayed as a public speaker, that he was appointed a Commissioner of the Treasury, and soon afterwards a Privy Councillor. In 1694 he was elevated to the Chancellorship of the Exchequer; and as the current coin of the realm had been adulterated and debased, one of his earliest designs was to re-coin it and restore it to its original value. In 1698 he was appointed First Commissioner of the Treasury, and one of the Lords Justices of England during the absence of the king in Holland; and in 1700 he was raised to the peerage with the title of Baron of Halifax, in the county of York.

The scheme of the re-coinage, like all measures of reform, encountered great opposition. It was characterized as a wild project, unsuitable to a period of war, as highly injurious to the interests of commerce, and as likely to sap the foundation of the government. The Chancellor of the Exchequer, however, was not influenced by the cries of faction. He had studied the subject with the deepest attention, and had entrenched himself behind opinions too impartial and too well founded, to be driven from a measure which the best interests of his country seemed to require. Having consulted Newton, Locke, and Halley, he immediately took measures to carry his plan into effect. The

[1] See Vol. i. APPENDIX No. VIII., p. 413.

advantage of having proper officers for superintending the re-coinage must have presented itself to the minds of his advisers, and we have no doubt that Locke and Halley warmly seconded his own desire to place Newton in one of the principal offices in the Mint. We have already seen that the Comptrollership had been, some years before, mentioned as a suitable office for Newton, and so early as November 1695, Dr. Wallis, in a letter to Halley,[1] mentions a rumour at Oxford that he had been actually appointed to the Mastership of the Mint. This, however, was a mistake, as there was no vacancy in the Mint till the beginning of $169\frac{5}{6}$. Mr. Overton, the Warden, was then made a Commissioner of Customs, and Mr. Montague embraced the opportunity thus offered to him of serving his friend and his country by recommending Newton to that important situation. This appointment was notified to him by the following letter, addressed to him at Cambridge :—

"*19th March* 1695.[2]

"SIR,—I am very glad that at last I can give you a good proof of my friendship, and the esteem the king has of your merits. Mr. Overton, the Warden of the Mint, is made one of the Commissioners of the Customs, and the king has promised me to make Mr. Newton Warden of the Mint. The office is the most proper for you. 'Tis the chief officer in the Mint. 'Tis worth five or six hundred pounds per annum, and has not too much business to require more attendance than you may spare. I desire you will come up as soon as you can, and I will take care of your warrant in the meantime. Pray give my humble services to John Lawton.[3] I am sorry I have not been able to

[1] November 26th. See Edleston's *Correspondence*, &c. lxviii., note, 126, and p. 302, Appendix.

[2] The date of this letter should have been $169\frac{5}{6}$.

[2] Mr. Lawton, or Laughton, was a great personal friend of Sir Isaac Newton and Charles Montague. He was afterwards Librarian and Chaplain of Trinity. He subsequently became Canon of Worcester and Lichfield, and gave to the Library of Trinity College a valuable collection of books. See p. 51, and Monk's *Life of Bentley*, pp. 226, 243.

assist him hitherto, but I hope he will be provided for ere long, and tell him that the session is near ending, and I expect to have his company when I am able to enjoy it. Let me see you as soon as you come to town, that I may carry you to kiss the king's hand. I believe you may have a lodging near me.[1]—I am, Sir, your most obedient servant,

"Chas. Montague."

This letter must have been the occasion of much surprise to Newton and his friends; for only five days previous to its date, namely, on the 14th of March, he had intimated to Halley that "if the rumour of preferment for me in the Mint should hereafter, upon the death of Mr. Hoare, or any other occasion, be revived, I pray that you would endeavour to obviate it by acquainting your friends that I neither put in for any place in the Mint, nor would meddle with Mr. Hoare's place[2] were it offered me." About three months before Newton's appointment, Mr. Montague had been placed at the head of the Royal Society,[3] and it must have been very gratifying to the Fellows, that their most distinguished member had been promoted by their new president. When it was stated "that Mr. Montague gave Newton employment before he wanted it or asked it," either Montague or some one else replied, "*that he would not suffer the lamp which gave so much light to want oil.*"[4]

Thus refreshed, the lamp continued to burn, and with no flickering light. Its asbestos torch, though kept at a high temperature for a quarter of a century, was unconsumed, and required only the gaseous material to make it continue its brilliant though chastened light; and, as if to give a prophetic reply to the allegation that his mind had been injuriously overwrought by study and enervated by office, he solved, about a

[1] Copied from the original.

[2] Mr. Hoare was Comptroller of the Mint.

[3] He was elected on the 30th of November 1695, and resigned at the same date in 1699.

[4] Conduitt's MSS.

year after his appointment, the celebrated problems with which John Bernoulli challenged "the acutest mathematicians in the world." When the great geometer of Basle saw the anonymous solution, he recognised the intellectual lion by the grandeur of his claw; and in their future contests on the fluxionary controversy, both he and Leibnitz had reason to feel that the sovereign of the forest, though assailed by invisible marksmen, had neither lost a tooth nor broken a claw.

In the new and responsible situation to which Newton was elevated, his chemical knowledge was of great use to the country; and, in effecting the re-coinage, which was completed towards the close of 1699, his services were so highly appreciated, that the Chancellor of the Exchequer declared that he could not have carried it on without his assistance. In the year 1699, when the situation of master and worker of the Mint became vacant, Mr. Montague was First Lord of the Treasury, and through his influence Newton was promoted to that high office, which was worth from twelve to fifteen hundred pounds per annum, and which he held during the remainder of his life.[1] In this situation he drew up an official report on the coinage in 1717, and Mr. Conduitt says, "that he behaved himself with an universal character of integrity and disinterestedness, and had frequent opportunities of employing his skill in numbers, particularly in his Table of Assays of Foreign Coins, which is printed in the Book of Coins lately published by Dr. Arbuthnot."[2]

[1] Among Newton's papers, I found the following list of his securities, which, I presume, must be those which were required when he was elevated to the Mastership of the Mint:—

Mr. Newton, ...	£2000
And Bondsmen,	
Rt. Honble. Charles Montague, . . .	1000
Thomas Hall, Esq., . . .	1000
—— Flayer, Esq., . . .	1000
Thos. Pilkington, gent., . . .	1000
	£6000

[2] Conduitt's MSS. Dr. Arbuthnot's work was published in 4to, in 1727, under the title

A very remarkable proof of Newton's integrity is given in the following interesting extract of a letter from the Rev. Dr. Derham to Mr. Conduitt :[1]—" The last thing, Sir, that I shall trouble you with, shall be a passage relating to the coinage of the copper money some years ago, which pleased me much in setting forth the integrity of my friend Sir Isaac. The occasion of our discourse was, the great inconveniences which many underwent by the delay of the coinage of this sort of money. The occasion of which delay, Sir Isaac told me, was from the numerous petitions that were presented to them, in most of which some person or other of quality was concerned. Amongst others, he told me that an agent of one had made him an offer of above £6000, which Sir Isaac refusing on account of its being a bribe, the agent said he saw no dishonesty in the acceptance of the offer, and that Sir Isaac understood not his own interest. To which Sir Isaac replied, that he knew well enough what was his duty, and that no bribes should corrupt him. The agent then told him, that he came from a great Dutchesse, and pleaded her quality and interest. To which Sir Isaac roughly answered, 'I desire you to tell the lady, that if she was here herself, and had made me this offer, I would have desired her to go out of my house ; and so I desire you, or you shall be turned out.' Afterwards he learned who the Dutchesse was."

The elevation of Newton to the Mint led to the promotion of his friend, Dr. Halley, to an office in the same establishment. He was made Deputy-Comptroller of the Mint at Chester, in 1696, the office of Comptroller being at that time held by Mr. Thomas Molyneux. Soon after his appointment, disturbances of a very serious kind arose among the officers. Mr. Halley, and Mr. Woodall the Warden, feeling it their duty to see the

of Tables of Ancient Coins, Weights, and Measures, Explained and Exemplified in several Dissertations. It was reprinted in 1754, with observations by Dr. Benjamin Langworth.

[1] This letter, dated Upminster, 18th July 1733, was written when Mr. Conduitt requested information regarding Newton from Dr. Derham, who had been intimately acquainted with him for about thirty years.

King's business well and faithfully performed, had insisted upon correcting certain irregularities in the proceedings of Bowles and Lewis, two clerks in the establishment. The Master of the Mint, a Mr. Clark, espoused the cause of the clerks, and, "pretending to take offence at something that nobody else had observed in the company, went and borrowed Bowles his sword, to waylay the Warden as he went home." He did not, however, fulfil his threat, but some time after he sent a challenge to the Warden, which was accepted. "He appeared, however, on the ground," says Halley, "before the hour, with his man and horses, and staid not after it, by which means they fought not, and I demonstrated the folly of such decisions that went no farther."[1] In the same spirit, Lewis, the clerk of the Warden, threw a standish at Mr. Woodall, and he and the Master brought forward all sorts of charges against Halley and Woodall. Halley was accused of showing a preference to individuals in the purchase of silver, and of committing professional blunders in adding an alloy to what is called *schissell*, and thus diminishing the purity of the coin; while the Warden was charged with having used expressions of a treasonable nature, dangerous to the Government. Halley was at first greatly annoyed by these dissensions, and, in requesting Newton to interfere for his protection, he expressed a hope "that his potent friend, Mr. Montague, would not forget him, if there should be occasion."[2] When Parliament had voted the continuance of the five country mints, Halley desires "that Lewis may appear face to face with him before the Lords, there to answer to his throwing the standish at Mr. Woodall, the giving the undue preference to Palford, and some other accusations of that nature, I am prepared to lay before their Lordships. I came to town purposely to charge that proud, insolent fellow, whom I humbly beg you to believe the principal author of all the disturbance we have had at our mint, whom if you please to see removed all will be easy; and on that condition I am content to submit to all you shall pre-

[1] Letter to Molyneux, August 25, 1697.

[2] August 2, 1697.

scribe to me. Nevertheless, as I have often wrote you, I would urge you to nothing but what your great prudence shall think proper, since it is to your particular favour I owe this post, which it is my chiefest ambition to maintain worthily, and next to that to approve myself in all things."[1] In the same letter he speaks of his resignation, but as he is unwilling "that Lewis and Clark should interpret it to be any other than a voluntary cession," he thinks it necessary to prosecute the charges against them.[2]

Before these dissensions had come to this crisis, Newton had offered, in February 1697, to procure for Halley an "engineer's place," through a Mr. Samuel Newton. Halley[3] expressed his willingness to accept of this kind offer, provided Sir Martin Beckman was of opinion that the post was likely to be durable; but two days before the date of Halley's letter, Newton[4] had offered him a situation worth ten shillings a week, to teach the mathematical grounds of engineering two hours a day to the engineers and officers of the army; but he seems to have declined both these situations. When the five country mints were discontinued in 1698, Halley, at his own desire, was appointed by the King to the command of the Paramour Pink, which sailed in November 1698, in order that he might study the variation of the needle in different parts of the globe.[5]

[1] Letter to Newton, dated December 30, 1697.

[2] These facts are gleaned from four unpublished letters to Newton, and three to Molyneux.

[3] February 1697.

[4] *Macclesfield Correspondence*, vol. ii. p. 420.

[5] Halley was one of the most distinguished and accomplished philosophers of the seventeenth and eighteenth centuries. On the death of Dr. Wallis, in 1703, he was appointed Savilian Professor of Geometry in Oxford. In 1703, he was chosen Secretary to the Royal Society, and, in 1719, in the sixty-third year of his age, he succeeded Flamsteed as Astronomer-Royal. In 1729, he was elected a corresponding Member of the Academy of Sciences at Paris, and he died on the 14th January 1742, in the eighty-sixth year of his age. In his *Eloge* upon Halley, M. Mairan thus speaks of him:—"While we thought the eulogium of an astronomer, a naturalist, a scholar, and a philosopher, comprehended our whole subject, we have been insensibly surprised with the history of an excellent mariner, an illustrious traveller, an able engineer, and almost a statesman."—*Mém. Acad. Par.* 1742.

While Newton was thus disturbed by the quarrels of the Chester Mint, in which he had personally no share, his tranquillity was more seriously compromised by disputes which arose under his own eye, and in which his character was concerned.

In the year 1697, a person of the name of William Chaloner, who is stated to have made experiments connected with the Mint "for the Parliament," pretended that he had discovered certain abuses in that establishment, and was sent for by a committee of the House of Commons to give information on the subject. Dreading, however, the personal consequences to which he might be exposed, he obtained a promise of protection from the committee, and he then disclosed several abuses alleged to have been committed in that department, and pointed out the methods by which false money was coined, and the mode of effectually preventing it. Some of the functionaries of the Mint having heard of these disclosures, and of Chaloner's having promised to "write a book on the present state of the Mint," are said to have threatened to take away his life before the next sitting of Parliament. Hearing of this threat, a Member of the House was, by its direction, appointed to represent his case to the King, who promised "that he should suffer no damage for the discoveries he had made, and that he would provide for him for the service he had rendered." Notwithstanding these proceedings, the officers of the Mint, as Chaloner states, committed him to Newgate, and, after keeping him in irons for seven weeks, they preferred against him a Bill of Indictment; but having no evidence to produce, they laid a plot to induce him "to coin false money," and thus to destroy his testimony against themselves. With this view Richard Morris, a messenger of the Mint, having apprehended for high treason John Peers, a clockmaker in the city, together with his wife, kept them prisoners in his own house, and told them that they were in great danger of being hanged, "unless they would undertake to do service to the Government." This service, according to the affidavit of

Peers, was to engage Chaloner "to be concerned in coining with them." Peers undertook the task, and arranged with Captain Harris, the Mint engraver, that a place in the establishment would be the reward of his success. Peers and his wife were with this view "bailed before Mr. Justice Negus." But though they used every means in their power, they could not succeed in alluring Mr. Chaloner. Peers then renewed the attempt with Holloway, a turner, and one Prince. For this purpose they went to the country provided with tools, and coined several plated shillings; but before they had applied to Chaloner, they availed themselves of their position, and circulated some of these shillings as legal coin.

In this state of matters, Sir Isaac, as Warden of the Mint, granted a warrant for the apprehension of Peers, but having sent for him, and learned that he was at work with Holloway and Prince, "to get Mr. Chaloner to be concerned in coining with them," Sir Isaac is said to have highly approved of the plan as one well-contrived, and to have given Peers five shillings and liberty to coin money, in order to promote the object they had in view. Upon being questioned by Sir Isaac concerning Chaloner, "whom he had assisted in experiments for Parliament," Peers told him that he knew nothing against Chaloner, excepting that one Moore had prevailed upon himself to make for him a tool to edge or mill money, when he was working with Chaloner; that Chaloner told him he would be hanged if he did such a thing, and was indignant at the idea of its being done with his tools. Chaloner, however, afterwards told Peers, that if Moore should force him to make the tool by threats, "he should make it of iron, *that it might not answer the end to mill money with it*." When Sir Isaac heard the amount of this charge against Chaloner, he is said to have told Peers "that these advices were not material against him;" and to have given through Morris, four pounds, to carry on the design of entrapping Chaloner. Holloway, however, having heard that Peers had been with the Warden, suspected that he himself would be taken up, and gave Peers and Prince in charge to a constable, who committed them to Newgate for high treason. Sir Isaac hearing of this, went to Newgate, and, having been assured by Peers that he had done nothing in coining excepting what he had been told to do by himself and Captain Harris and Morris, "in order to draw in Mr. Chaloner," he agreed to admit him to bail the next day.

Holloway also made an affidavit, and swore "that he heard Isaac Newton, Esq., Warden of the Mint, and his clerk and Morris, all say, that the said Chaloner should not be tried until the last day of the Sessions, for then he was sure to be tried by the Recorder, they being sensible that the Recorder was Chaloner's enemy;" or, as Peers expressed it in his affidavit, "that as the Recorder had a prejudice against Chaloner, he would certainly do his business." By the evidence of Morris, and the story of the tool made for Moore, a bill for high treason was found by the grand jury against Chaloner.

Under these circumstances, Chaloner presented a petition to Parliament on the 18th July 1697-8, "praying that his sufferings and ruined condition might be considered and redressed." The petition was referred to a large committee, one of whom was Charles Montague, with instructions to send for any information against Chaloner, and report to the House. New members were added to the committee on the 2d of March: The committee got leave to sit on the 8th, and other members were added on the 28th, but it does not appear from the journals of the House that any report was given in, or any farther proceedings taken in the matter.[1]

As this singular story involves charges deeply affecting the character of Sir Isaac, and as these are contained in printed papers, and probably in unpublished records of the House of Commons, which might some day come to light when there was

[1] The preceding statement is taken from a printed copy of the petition of Chaloner, with which Mr. Edleston has kindly favoured me. The affidavits of Holloway and Peers, annexed to the petition, are dated in November and December 1697.

no opportunity of defending him, and perhaps no means of defence, I felt it a sacred duty to inquire into their origin and history. Had they rested on any foundation, and been the subject of public or private discussion, Charles Montague, then Chancellor of the Exchequer, against whom the spirit of party ran high, would doubtless have been questioned in the House of Commons; and Flamsteed, in his private correspondence with Abraham Sharpe, would not have failed to record the failings of his friend. It was therefore probable, both from the character of Newton and the silence of his contemporaries, that some palliation of his conduct, or some exposure of the calumny, might yet be discovered.

Through the kindness of Lord Brougham, to whom I submitted the case, inquiries were made in the House of Commons, the Mint, and the British Museum; but it is only from the latter that any useful information has been obtained. Mr. Panizzi found three printed papers by Chaloner, one of which was a proposal, dated February 11, 1694, and addressed to the House of Commons, that they should pass an Act to prevent the clipping and counterfeiting of money; another containing reasons against the resolutions of the committee appointed to revise these proposals; and a third, pointing out the defects in the constitution of the Mint. Mr. Panizzi likewise found a tract, containing an account of the life and execution of Chaloner, which completely exculpates Newton from the charges brought forward in the petition to the House of Commons.[1] Chaloner seems to have been a man of extraordinary talent, who, in order to conceal his own criminality, brought false accusations against the officers of the Mint. "He scorned," says his biographer, "to fly at low matters. He pretended his commitment to be malicious, and accused that

[1] Entitled, *Guzmanus Redivivus. A Short View of the Life of William Chaloner, the Notorious Coyner, who was executed at Tyburn, on Wednesday, the* 22d *of March* $169\frac{8}{9}$, *with a brief Account of his Tryal, Behaviour, and Last Speech.* London: J. Haynes. 12mo, 1700; pp. 12.

worthy gentleman, Isaac Newton, Esq., Warden of His Majesty's Mint, with several other officers thereof, as connivers (at least) at many abuses and cheats there committed. This accusation he impudently put into Parliament, and a committee was appointed to examine the same, who, upon a full hearing of the matter, dismissed the same gentleman with the honour due to his merit, and Chaloner with the character he deserved."[1]

While Newton was thus distracted by the quarrel among the functionaries of the Mint at Chester, and by the charges against himself, his tranquillity was disturbed by another misunderstanding with Flamsteed. He had now resumed his inquiry into the lunar irregularities; and "on Sunday the 4th December 1698, in the time of evening service," he went to Greenwich to obtain twelve computed places of the moon, which Flamsteed had corrected for him, in consequence of the places formerly given him not having been correct. On the 29th December, Flamsteed sent him a correction of the time in one of the observations; and having afterwards discovered that the results required to be still farther improved, he waited upon Newton on the 30th or 31st December, to acquaint him with the fact. According to Flamsteed, Newton was "reserved to him contrary to his promise;" that is, he was reserved, as Mr. Baily interprets it, in not imparting to him the particulars of his lunar theory; or, as Mr. Edleston thinks, reserved in his manner from being at that time displeased with Flamsteed.

When Dr. Wallis was preparing the third volume of his works, he requested from the Astronomer-Royal his observations on the parallax of the earth's annual orbit. Flamsteed complied with his request; but without supposing that it would be offensive to Newton, he made the following reference

[1] Chaloner had been three times under prosecution before he petitioned the House of Commons. He was finally apprehended for forging Malt Tickets; but when tried for coining, he feigned madness to avoid pleading. He was however found guilty of high treason by "a cloud of witnesses," and executed,—abusing the Judge and the Jury, and declaring to the last that the witnesses, particularly Holloway, had perjured themselves.

to his lunar theory :—"I had become intimate with Mr. Newton, then the most learned man of the day, and Professor of Mathematics in the University of Cambridge, to whom I had given 150 places of the moon deduced from my observations, and at the same time her places as computed from my tables, and I promised him similar ones in future as I obtained them, along with the elements of my calculations for the improvement of the Horroxian theory of the moon, in which matter I hope he will have all the success which he expects."[1] Dr. Gregory having heard from Wallis of this allusion to the lunar theory, mentioned it to Newton, who took it very much amiss, and begged that Gregory would request Wallis to suppress the clause. When Flamsteed heard of this request from Wallis, he wrote to Newton a long letter, dated January 2, 1699, transmitting to him the offensive paragraph,—reminding him that it contained nothing but what Newton himself had acknowledged to many of his friends, and proposing to leave out the word *Horroxianæ*, which was put in because Newton "allowed that theory as far as it goes."[2] As Newton did not reply to this letter, Flamsteed wrote to him again on the 5th, and drew from him the following expostulation :—

"Jermyn Street, *January* 6, 1698-9.

"Sir,—Upon hearing occasionally that you had sent a letter to Dr. Wallis about the parallax of the fixed stars to be printed, and what you had mentioned therein with respect to the theory of the moon, I was concerned to be publicly brought upon the stage about what, perhaps, will never be fitted for the public, and thereby the world put into an expectation of what, perhaps, they are never like to have. I do not love to be

[1] In the Latin version of this passage, given by Baily in p. 668, for *similium* read *similia*, for *posteriore* read *posterum*, for *enarrare* read *qua in re;* for *cum* [*eorum ?*] read *eum ;* and for *censeas harum* read *consecuturum.*

[2] As this letter derives a peculiar interest from its connexion with the remarkable letter of Newton of January 6th, which has been the subject of so much discussion, we have printed it in the Appendix, No. XII.

printed on every occasion, much less to be dunned and teased by foreigners about mathematical things, or to be thought by our own people to be trifling away my time about them, when I should be about the King's business. And, therefore, I desired Dr. Gregory to write to Dr. Wallis, against printing that clause which related to that theory, and mentioned me about it. You may let the world know, if you please, how well you are stored with observations of all sorts, and what calculations you have made towards rectifying the theories of the heavenly motions. But there may be cases wherein your friends should not be published without their leave. And, therefore, I hope you will so order the matter, that I may not on this occasion be brought upon the stage.—I am, your humble servant,

"Is. Newton."

This letter has been characterized by Mr. Baily as a "most extraordinary"[1] production; and another writer represents it as unworthy of Newton's transcendent genius, and Newton as "indignant," and taking fire at the paragraph sent to Wallis, which he says "was obviously written without the slightest intention to give offence." If Newton had written this letter as a simple expression of his feelings, upon hearing that his lunar theory had been mentioned by Flamsteed, as these writers, without any authority, assume, we should have regarded it as unseemly, and as a display of unnecessary feeling. But this was not the case. Newton did nothing more than request Dr. Wallis to leave out the paragraph; and Mr. Baily knew, and the other writer ought to have known,[1] that the letter was extracted from Newton by the two letters of Flamsteed, which we have already mentioned, and which, for anything they knew, might have been written in such a tone as to make Newton's letter appear an amiable, in place of an extraordinary production. Mr. Flamsteed's first letter of January 2d was a provoking letter, and yet Newton did not reply to it; and it is

[1] See Baily's *Flamsteed*, p. 164.

very probable that Flamsteed's second letter, which extorted an answer, was still more annoying; for it is quite clear, from his own note, that he was greatly offended at Newton for delaying to answer it.[1]

But independently of these circumstances, Newton was entitled to express his feelings at being "brought upon the stage," and thus exposed to being dunned and teased by foreigners; and what is still more in his favour, he had peculiar reasons at that very time to prevent the belief that he was occupied with anything else than "the King's business." The great recoinage of silver was now going on. Some of the provincial mints were in a state of anarchy; and the Mint itself was charged before Parliament with the toleration of grave abuses, which might have been attributed to Newton as its Master. In thus justifying Newton, we do not mean to attach much blame to Flamsteed. He should have asked Newton's permission to print the obnoxious paragraph, and, when it was printed, Newton should have requested its suppression from Flamsteed himself, and ought to have returned an answer to the letter of explanation which had been sent him.

Notwithstanding these differences, Flamsteed continued to visit Newton when he went to London, to promise him his observations when he required them, and to converse upon the tender subject of the printing of the Greenwich observations. On the 3d of May 1700, Flamsteed paid one of these visits, and has given such a graphic account of it in a letter to Lowthorp[2] a week after, that it gives us some insight into the peculiarities of both these great men. Flamsteed went before Newton was up, and "waited his rising." He found a Bible in his room, which he seems to have read, "and meeting," he says, "with a sheet of paper, I wrote upon it this distich, which I remembered from a late satire,—

[1] Flamsteed answered Newton's letter on the 10th of January, in a very contrite spirit, and sent him the paragraph as altered by Wallis.

[2] Baily's *Flamsteed*, pp. 174, 175.

A bantering spirit has our men possessed,
And Wisdom is become a standing jest.
Read Jeremiah, chap. ix. to the 10th verse.

I do not know whether he has read it, but I think he cannot take it amiss if he has; and if he reflects a little on it, he will find I have given him a reasonable caution against his credulity, and showed him the way of the world much better than his politics or a play could do." When the subject of printing his observations was started, and Flamsteed had explained the order in which they were to be given, he added, "that the book of tables would follow." At this Newton started, and asked him, "what tables?" and "if I would publish any for the moon?" "My answer was, that she was in his hands, and if he would finish her, I would lend him my assistance; if not, I would fall upon her myself when I had leisure." During "the discourse," Newton complained of his friend's reserve, which Flamsteed denied, and said, that if he would come down some morning with Sir Christopher Wren and take his dinner with him, "he should then see in what forwardness his work was, and we would consider how to forward it to the press."

The reputation of Newton had been gradually extending itself on the Continent, as his philosophy became better known. James Cassini, the celebrated French astronomer, came to England, after the peace of Ryswick,[1] to pay Newton a visit, and is said to have offered him a large pension from the French king, which he refused;[2] and it was probably on the suggestion of Cassini that he was appointed one of the eight foreign

[1] The Treaty of Ryswick was signed in 1697.

[2] I have given this anecdote in the words of Conduitt, which cannot be correct. James Cassini, the younger, paid a visit to London in the early part of 1698, as appears from the following short note, in which he communicates from his father the periodic times of the five satellites of Saturn, slightly different from those published in the 2d Edit. of the *Principia*, p. 960.

"Clarissimo viro Domino Isak Newton, Jacobus Cassini, S.P.D.

"Cum e Londino reversurus in Galliam huc pervenissem, accepi a patre meo epistolam una cum maximis satellitum Saturni digressionibus quas a me expostulaveras. Has tibi mandare et gratitudinem meam tuorum erga me beneficiorum simul exhibere associates of the Academy of Sciences, who were created on the remodelling of the Academy in 1699.[1]

A short time after this election, namely, on the 7th of March, M. Geoffroy, one of the members of the Academy, transmitted to Dr. Sloane, the Secretary of the Royal Society, a list of the eight foreign associates, containing the name of Newton; and gave him the following account of the new organization of the Academy: "I shall here give you an account of the great splendour that the Académie des Sciences has received by the regulation, increase, encouragement, and order, M. l'Abbé Bignon has obtained to it from the king. That Academy is now composed of ten honorary academicians, which are chosen learned and eminent gentlemen, of eight strangers associates, each of which is distinguished by his learning—twenty pensioners fellows; twenty élèves; twelve French associates. Between the honorary academicians, two are elected every year, one for president, the other for vice-president. Twenty pensioners have every year 1500 French livres; and after the death of one pensioner, the Académie will propose to the king three persons, associates or élèves, or sometimes others, and his Majesty will call one of the three for pensioner."[2]

While Newton held the inferior office of Warden of the

mihi liceat. Tuam domum adivi ut te inviserem, sed mala usus fortuna cum nunc abfuisses. Vale vir clarissime, et sic habeas me tibi semper esse addictissimum. Dover, die 6 Aprilis, 1698, St. N."

[1] The eight foreign Associates created on this occasion were—

1. Leibnitz. 2. Guglielmini. 3. Hartsoecker. 4. Tschirnhausen.	February 4.
5. James Bernoulli. 6. John Bernoulli.	February 14.
7. Newton. 8. Roemer.	February 21.

Newton and Roemer, and the two Bernoullis, were nominated by the Academy, and the other four by the King.—Edleston's *Correspondence*, &c., p. lxix.

[2] Mr. Weld has published this letter from the Letter-Book of the Royal Society, "as marking the different manner in which the great learned societies of England and France were treated by their respective sovereigns. In the latter country, science was thus

Mint, he retained the Lucasian Chair ; but upon his promotion to the Mastership in 1699, he appointed Mr. Whiston his deputy at Cambridge, with "the full profits of the place." Whiston began his astronomical lectures on the 27th January 1701 ; and when Newton resigned the chair on the 10th December 1701, he succeeded in getting Whiston appointed his successor. When he resigned his fellowship, which he did soon after, he stood tenth on the list ; and had he remained a fellow till August 1702, he would have been elected a senior.

We have not been able to discover why Newton did not represent the University in the Parliament which met in 1690. When a vacancy took place in November 1692, by the death of Sir Robert Sawyer, Newton's health would probably not permit him to aspire to the office. But at the next election for King William's sixth Parliament, he was chosen one of the members for the University. The other successful candidate was Mr. Henry Boyle, afterwards Lord Carleton ;[1] "so that on this occasion Trinity College had the honour of supplying the University with both its representatives, and Dr. Bentley had the satisfaction of assisting in the return of his illustrious friend."[2]

Newton's honours were now gathering thick around him. On the 30th November 1703, he was elected, on the retirement of Lord Somers, President of the Royal Society ; and he was annually re-elected during the remaining twenty-five years of his life, having held the office for a longer time than any of his predecessors, and longer too than any of his successors, excepting Sir Joseph Banks.

In this new position, Newton was brought into personal

early fostered and rewarded, while in England the Royal Society was left to struggle with poverty."—*History of the Royal Society*, vol. i pp. 355, 356. See vol. i. p. 89, &c.

1 Mr. Hammond was the opponent of Newton on this occasion. The votes stood thus—

Mr. Henry Boyle,	. .	180
Mr. Newton,	. . .	161
Mr. Hammond,	. .	64

2 Monk's *Life of Bentley*, p. 122.

communication with Prince George of Denmark (the consort of Queen Anne), who had been elected a fellow of the Royal Society. The Prince was anxious to promote the interests of science, and, on Newton's recommendation, had offered to be at the expense of printing Flamsteed's observations, and particularly his catalogue of the stars. Newton's high merits then became known to the Queen, who resolved to take the first opportunity of showing her respect for his genius. In the month of April 1705, which her Majesty was spending at her royal residence of Newmarket, she went on the 16th, accompanied by Prince George of Denmark and her whole court, to visit the University of Cambridge, where she was to be the guest of Dr. Bentley, at Trinity Lodge. "Alighting at the Regent Walk," says Dr. Monk, "before the schools, she was received by the Duke of Somerset, the Chancellor, the head of the University, and addressed in a speech by Dr. Ayloffe, the public orator. From thence her Majesty went in procession to the Regent House, where, agreeably to ancient custom, was held the congregation of the senate, termed *Regia Comitia*, at which the University conferred degrees upon all persons nominated by the Royal command—the presence of the sovereign dispensing with statutable qualifications and exercises. Afterwards, the Queen held a court at Trinity Lodge, where she rendered this day memorable, by conferring knighthood upon the most illustrious of her subjects, Sir Isaac Newton.[1] A sumptuous dinner was then given to the royal visitor and her suite in the Hall of Trinity College, which had been newly fitted up and decorated. Whoever is acquainted with the large sums which *Alma Mater* has since expended on public objects, will be surprised to learn, that she was then so poor as to be com-

[1] The two persons who had the honour of being knighted along with Sir Isaac were Sir John Ellis, Master of Caius College and Vice-chancellor, and Sir James Montague, the University Counsel, afterwards Lord Chief Baron. Sir James, who was of Trinity College, was a younger brother of Lord Halifax, and, along with others, received on this occasion the degree of LL D. At the same time the celebrated Dr. Arbuthnot, physician to the Queen, received the degree of M.D.

pelled to borrow £500 for the purpose of this entertainment. The royal party, after attending evening service, at the magnificent chapel at King's College, took leave of the University, and returned the same night to Newmarket."

It must have been at this period of Newton's life, that he wrote a love-letter, of which a copy was found among the Portsmouth papers; but we have no means of ascertaining whether it was for himself or a friend that he composed this remarkable epistle. It is in the handwriting of Mr. Conduitt, who, doubtless, intended to publish it, and is entitled, in the same hand, "Copy of a letter to Lady Norris, by ——," while on the back is written in another hand, "A Letter from Sir I. N. to ——." It has no date, but, as we shall presently see, it must have been written in 1703 or 1704:—

"MADAM,—Your ladyship's great grief at the loss of Sir William, shows that if he had returned safe home, your ladyship could have been glad to have lived still with a husband, and therefore your aversion at present from marrying again can proceed from nothing else than the memory of him whom you have lost. To be always thinking on the dead, is to live a melancholy life among sepulchres, and how much grief is an enemy to your health is very manifest by the sickness it brought when you received the first news of your widowhood: And can your ladyship resolve to spend the rest of your days in grief and sickness? Can you resolve to wear a widow's habit perpetually,—a habit which is less acceptable to company, a habit which will be always putting you in mind of your lost husband, and thereby promote your grief and indisposition till you leave it off. The proper remedy for all these mischiefs is a new husband, and whether your ladyship should admit of a proper remedy for such maladies, is a question which I hope will not need much time to consider of. Whether your ladyship should go constantly in the melancholy dress of a widow, or flourish once more among the ladies; whether you should spend the rest of your days cheerfully or in sadness, in health or in sickness, are questions which need not much consideration to decide them. Besides that your ladyship will be better able to live according to your quality by the assistance of a husband than upon your own estate alone ; and therefore since your ladyship likes the person proposed, I doubt not but in a little time to have notice of your ladyship's inclinations to marry, at least that you will give him leave to discourse with you about it.

I am, Madam, your ladyship's most humble,
and most obedient servant."

The words at the close of this letter might lead us to suppose that the writer and the lover were different persons ; but as no name is mentioned, nor any reference made to the qualifications of a third party, it is probable that the title, "person proposed," is a quaint and not uncommon form of expression to avoid the use of the first person. It is not probable that any gentleman aspiring to Lady Norris's hand would intrust his cause to a friend, and still less probable is it that that friend would be Sir Isaac Newton. It could only have been for a very particular friend that Newton's modesty would have permitted him to undertake such a task, and not one of his acquaintances can be named who was unmarried, and who was likely to call in the aid of a philosopher in an affair of matrimony. Newton had been acquainted with Lady Norris for some years, and from the following letter to his niece, Miss Catherine Barton, which we found among his papers, there is some ground for supposing that he was then intimately acquainted with her :[1]—

[1] This letter, which had on the back of it calculations about the Mint, is bound up near the beginning of the second volume of the large folio volumes containing papers about the Mint.

"To Mrs. Catherine Barton,
at Mr. Gyre's at Pudlicot,
near Woodstock, in Oxfordshire.

LONDON, *Aug.* 5, 1700.

"DEAR NIECE,—I had your two letters, and am glad the air agrees with you; and though the fever is loth to leave you, yet I hope it abates, and that the remains of the small-pox are dropping off apace. Sir Joseph Tilley is leaving Mr. Toll's house, and it's probable I may succeed him. I intend to send you some wine by the next carrier, which I beg the favour of Mr. Gyre and his lady to accept. *My Lady Norris thinks you forget your promise to write her, and wants a letter from you.* Pray let me know by the next how your face is, and if the fever be going. Perhaps warm milk from the cow may help to abate it.—I am your very loving uncle,

"IS. NEWTON."

Lady Norris was the widow of Sir William Norris, Bart. of Speke, near Liverpool. Sir William took his degree of B.A. in 1679. He became one of the Lay-Fellows of Trinity College, and was succeeded in his Fellowship by Charles Montague. He sat for Liverpool in the third, fourth, and fifth parliaments of William III., in the proceedings of which he took an active part. He was created a baronet on the 3d December 1698, was minister at the Porte, and subsequently went out to Delhi as ambassador to the Great Mogul. Sir William arrived at the Mogul's camp, near Purnella, in April 1701, and appears to have conducted himself in "an imprudent and expensive" manner. The object of his mission seems to have been to solicit the favour of the Mogul to the English Company, in opposition to the London Company; and it so far succeeded that the Mogul seized the property and servants of the last of these establishments. Sir William embarked on board the Scipio from Surat on the 29th of April 1702, and his brother, who was Secretary to the Embassy, went on board the China Merchant, one of the Company's ships, the cargo of which amounted to 60,000 rupees on the Company's account, and 987,200 rupees on Sir William Norris's. The two vessels sailed for England on the 5th of May ; Sir William was seized with dysentery, and died on the 10th of October 1702, between the Mauritius and St. Helena, which the Scipio reached on the 31st October. Sir William left no family, and therefore his widow must have succeeded to his fortune.[1]

Lady Norris, whose name was Elizabeth Read, was daughter and heiress of Robert Read of Bristol, and had been twice married before her union with Sir William, first to Isaac Meynell of Lombard Street, goldsmith, and, secondly, to Nicholas Pollexfen, a merchant in London.

As Mr. Norris resided at Trinity College while Newton held the Lucasian Chair, he must have been personally acquainted with him at that time, and their acquaintance must have been renewed when both of them had their residence in London. If our interpretation of the letter to Lady Norris be correct, the desire of Sir Isaac to marry at the age of sixty, has a remarkable coincidence with that of Leibnitz, who made proposals to a lady when he was fifty. "The lady," says Fontenelle, "asked for time to take the matter into consideration, and as Leibnitz thus obtained leisure to consider the matter again, he was never married."[2]

The Parliament had just been dissolved when Newton was knighted, and he seems to have been urged by his friends to stand for the University. He had visited Cambridge about a fortnight before, as Mr. Edleston supposes, on business connected with the election ; but it would appear from the following letter[3] that he had no desire to contest the University again :—

"SIR,—I wrote lately to Mr. Vice-chancellor, that by reason of my present occasions here, I could very ill come down to

[1] See Bruce's *Annals of the Honourable East India Company,* vol. iii. pp. 261, 461, 472, &c.

[2] Fontenelle's Eloge of Leibnitz, *Mém. Acad.* Par. 1718, p. 126.

[3] There is no address on this letter, of which I have found two rough copies.

your University to visit my friends in order to be chosen your burgess. I would have it understood that I do not refuse to serve you (I would not be so ungrateful to my Alma Mater, to whom I owe my education, nor so disobliging to my friends), but by reason of my business here I desist from soliciting, and without that, I see no reason to expect being chosen. And now I have served you in this Parliament, other gentlemen may expect their turn in the next. To solicit and miss for want of doing it sufficiently, would be a reflection upon me, and it's better to sit still. And tho' I reckon that all one as to desist absolutely, yet I leave you and the rest of the gentlemen to do with all manner of prudence what you think best for yourselves, and what pleases you shall please—Your most humble and most obedt. servant."

Although we might suppose from this letter that Newton was unwilling to canvass personally for a seat in the new Parliament, yet it appears from the following interesting communication to him from Lord Halifax, that he had resolved to be a candidate in the middle of March, and before the dissolution :[1]—

"Sir,—I send you the address of the House of Lords, to which the Queen made so favourable an answer, that the enemy are quite enraged. The paragraph in her speech against the Tackers provokes them still more than this. And whatever the ministers may think, they will never forgive them for either. I believe they begin to think so, and will take measures to make other friends. I was in hopes by this post to have sent you an account of several alterations that would have pleased you, but they are not yet made, tho' you may expect to hear of them in a very little time. Among other expectations we have,

[1] This appears also from a letter of Flamsteed's written on the 5th April 1705, the day of the dissolution, in which he wishes Newton " good success in his affairs, health, and a happy return."—Baily's *Flamsteed*, p. 238. This letter (marked " not sent as he returned too soon") is given by Baily as probably addressed to Mr. Hodgson ; but as Mr. Edleston first suggested, it was to Newton.—*Correspondence*, &c., p. lxxiii, note 151.

we do depend upon a good Bishop, Dr. Wake is likely to be the man. We are sure Sir William Dawes will not. I think this will have great influence in the place where you are, and therefore I think you may mention it among your friends as a thing very probable, tho' it be not actually settled. He is to hold St. James's in commendam, and Dr. Younger will be Dean of Exeter. Mr. Godolphin will go down to Cambridge next week, and if the Queen goes to Newmarket, and from thence to Cambridge, she will give you great assistance. The Tories say she makes that tour on purpose to turn Mr. Ansley out. He is so afraid of being thrown out, that Lord Gower has promised to bring him in at Preston, which they should know at Cambridge. If you have any commands for me, I desire you would send them to me, who shall be very ready to obey them.—I am your most humble, and most obedient servt., HALIFAX.

"17 *March*," [1705.]

It appears from this letter that Newton had resolved to become a candidate. He seems, however, to have been very undecided, and very unwilling to take active steps in the matter, as appears from the following letter without a date and address.[1]

"I understand that Mr. Patrick is putting in to be your representative in the next Parliament, and believe that Mr. Godolphin, my Lord High Treasurer's son, will also stand. I do not intend to oppose either of them, they being my friends, but being moved by some friends of very good note to write for myself, I beg the favour of you and the rest of my friends in the University to reserve a vote for me till I either write to you again, or make you a visit, which will be in a very short time, and you will thereby very much oblige yours, &c."

Lord Halifax exerted his influence for Newton and Mr. Godolphin, as might have been expected, but, as the following

[1] This letter is among the MSS. of Newton, in the possession of the Rev. Jeffrey Ekins, who kindly communicated it to me. It was probably written shortly before his visit to Cambridge in March.

letter shows, anticipated their defeat from the opposition of the Court.

"SIR,—I have sent to my Lord Manchester to engage Mr. Gale for Mr. Godolphin, but I am afraid his letter will not come time enough. There can be no doubt of Lord Manchester's sentiments in this affair. Mr. Gale may be sure he will oblige him and all his friends by appearing for Mr. Godolphin, and he can do you no good any other ways. I am sorry you mention nothing of the election. It does not look well, but I hope you still keep your resolution of not being disturbed at the event, since there has been no fault of yours in the management, and then there is no great matter in it. I could tell you more stories where the conduct of the Court has been the same, but complaining is to no purpose; and now the die is cast, we shall have a good Parliament.—I am your most humble and most obedient servant, HALIFAX.

"*5th May* 1705."

In order to promote his election, Newton went to Cambridge on the 24th or 25th of April. The Tory election cry on this occasion was "the Church in danger;" and, on the polling day, the 17th of May, "hundreds of young students hollowed, like schoolboys and porters, crying, No Fanatic, no occasional Conformity, against two worthy gentlemen that stood candidates."[1] Newton and Godolphin were defeated, and Annesley and Windsor elected.[2] Mr. Mansfield mentioned to Mr. William Bankes, that his father, Sir James Mansfield, knew an old man at Cambridge who remembered this election, and who said that all the residents voted for Newton, but that they were outnumbered by the non-resident voters.

[1] Cobbett's *Parliamentary History*, vol. vi. p. 496. Flamsteed thought Newton's success doubtful, "by reason he put in too late."—Baily's *Flamsteed*, p. 239.

[2] The following was the state of the poll:—

Hon. Arthur Annesley,	(*Magd.*), . .	182
Hon. Dixie Windsor,	(*Trinity*), . .	170
Hon. Fra. Godolphin,	(*King's*), . .	162
Sir Isaac Newton,	(*Trinity*), . .	117

Dr. Bentley voted for Sir Isaac.—Edleston's *Correspondence*, &c., p. lxxiv., note 153.

CHAPTER XX.

Sir Isaac is anxious to have the Greenwich Observations published—Flamsteed agrees, provided his expenses are paid—Prince George offers to pay the expense of publishing them—He appoints Sir Isaac and others Referees to manage the matter—Articles agreed upon between Flamsteed and the Referees—Differences arise, and delays in Printing—The Prince offers to publish Tycho's Observations along with Flamsteed's—Newton writes to Olaus Roemer about Tycho's Manuscripts—To prevent delay the Referees propose to appoint another Corrector of the Press—Flamsteed opposes this in a Letter to Sir C. Wren—Prince George dies—The Work is stopped for three years—Flamsteed's Charges against Newton—Sanctioned by Mr. Baily—Defence of Newton—Flamsteed inserts in his Autobiography a false copy of his Letter to Wren—The Queen appoints a Board of Visitors to superintend the Observatory—Flamsteed's Correspondence with Dr. Arbuthnot—A scene between Newton and Flamsteed—Halley publishes the Observations printed at the expense of the Prince and the Public—Flamsteed publishes at his own expense the Historia Celestis—Observations on the Controversy.

Elevated to the Chair of the Royal Society, and enjoying the confidence of the Prince Consort, Sir Isaac had it in his power to do something for the promotion of Science. He had long cherished the desire of having the observations of Flamsteed published; and Halley and his other friends had frequently urged their publication with a degree of pertinacity which a personal interest in them could alone explain. It was a very natural wish on the part of physical astronomers to possess the best observations then made, by which they could test their speculations and their theories; and it was not an unreasonable expectation that the Astronomer-Royal, the author and the custodier of these observations, should impart such of them to his friends as their researches might require, and as his leisure would permit him to reduce. This, however, was a very different thing from the systematic publication of an immense mass of observations accumulated by an astronomer who had a salary of only £100 per annum, and no allowance either for assistants or computers. Flamsteed had laid down a plan for reviewing the heavens, making a catalogue of the fixed stars, and instituting regular observations on the moon and the other planets. He again and again explained to Newton and others the reasons why he could not comply with their wishes, and, regardless of the clamours which were raised against him, and which he should have despised more than he did, he went steadily onward pursuing his own plan, till it was nearly ripe for execution.

In 1701 he had finished the greater number of the constellations, but it was not till the commencement of 1703 that his catalogue was so complete that he wished it to be made known, publicly, that he was ready and willing to publish it "at his own charge," provided the public would defray the expense "of copying his papers and books for the press." He had already employed calculators from the country, and made great progress in the preparation of his manuscripts, when Sir Isaac Newton paid him a visit on the 11th of April 1704. When dinner was over, Sir Isaac asked to see the state of his work, and, having been shown the catalogue of the fixed stars, the maps of the constellations, "his new lunar numbers fitted to his corrections," and the observations on the planets, he told Flamsteed that he would recommend them to the Prince *privately*. To this Flamsteed objected, and insisted that it should be done *publicly*;—a request which Newton did not seem to think reasonable.[1] In order to have a proper document for the Prince's consideration, Flamsteed found that the papers would occupy 1400 folio pages, and, having "drawn up an estimate of them," he sent them to the Royal Society, where it was proposed that the work should be "recommended to the Prince." Sir Isaac concurred in this opinion, and, on the 7th of December,

[1] An account of this interview by Flamsteed will be found in Baily's *Flamsteed*, pp. 69, 217.

he waited on the Prince, and gave him a copy of Flamsteed's estimate of his observations. The Prince lost no time in coming to a decision on the subject. After perusing the estimate, he intimated to Newton on the 11th, through his secretary, Sir George Clark, his persuasion of Flamsteed's fitness for the work, and desired that Newton, Mr. Robartes, Sir C. Wren, Dr. Gregory, Dr. Arbuthnot, and any other members of the Society Sir Isaac thought qualified, should consider what papers were fit for the press. Newton communicated this intelligence to Flamsteed on the 18th December 1704, and asked him to meet "the referees" at dinner next day, and bring his papers with him in the morning. Flamsteed attended the meeting; but as the referees had not time to examine the papers which he brought, Newton went to dine with him at the Observatory on the 29th, and made himself acquainted with the papers which it was proposed to publish. He accordingly, on the 23d January 1705, drew up the report of the referees, which was submitted to the Prince, and received his approbation.[1]

During the rest of the year 1705, the printing of the work advanced slowly, on account of the ill health of the Astronomer-Royal, his distance from town, and differences of opinion which arose between him and Newton about the order of the observations. The Greenwich ones had been placed before the Derby ones, contrary to Sir Isaac's wishes; and, on the 25th of October, Flamsteed defends this arrangement, and adduces, as a sufficient warrant for his plan, that Albert Curtius, in publishing Tycho's Observations, began in 1582 with the most accurate. Along with this explanation he transmits the title of the work for Newton's approbation. The articles of agreement between Flamsteed and the referees were signed at

[1] In this Report, the original of which I have found in Sir Isaac's handwriting, the expense of printing 400 copies is £683, with £180 to pay the charges of two calculators, &c. "This set of observations," the reporters say, "we repute the fullest and completest that has ever yet been made, and as it leads to the perfection of astronomy and navigation, so, if it should be lost, the loss would be irreparable." The Report is published in Baily's *Flamsteed*, p. 234.

Newton's house on the 17th November 1705;[1] and in a day or two after their signature, we find Flamsteed writing to his friend, Mr. Sharp,[2] "that Newton had at last forced him to enter into articles for printing his works, with a bookseller, very disadvantageous to himself;" that "he has thereby injured him;" and that he does "not see that they are nearer the press than before."

The referees had found it necessary, as the dispensers of the Prince's bounty, and as acting for public interests, to draw up articles binding themselves, as well as Flamsteed and the printer, to perform their relative obligations. It is therefore of importance to know what these articles were, before we can rightly judge of the conduct of the parties. Mr. Baily has seen "four copies or draughts of these articles," so "similar to each other," that he "cannot ascertain *which* was the one actually agreed upon." He has overlooked the very title of these copies, and Flamsteed's note,[3] written upon one of them, which prove that they are only articles *proposed* by Flamsteed,[4] and not *the articles which he signed*. Of these he has left no copy, because he had wilfully violated them. From the very first he seems to have resolved not to perform his part of the agreement, and to have thrown difficulties in the way, in order to procure more money from the referees. After signing the second agreement, he followed the same course, lamenting constantly the hardness of his bargain, because he had made the instruments, and paid his assistants out of his own funds,—facts which had nothing whatever to do with the agreement,

[1] I have found three rough copies of these articles, all in Sir Isaac's handwriting, and obviously drawn up by himself. The very receipts granted by Flamsteed were written by Newton.

[2] November 20, 1705. Baily's *Flamsteed*, p. 256.

[3] In this note he offers *immediately* to put the first volume into the hands of the referees.

[4] Flamsteed says that he himself had drawn up articles which "were not to Newton's purpose;" and he refers to certain topics in "the articles," which are not mentioned in what Mr. Baily has ventured to consider as the genuine articles. See pages 80 and 81 of his *Autobiography*.

and which, though well known, were never pleaded before the agreement was made. He complains, too, in one place, that the £125 owing to him, was not paid till *above two months after* it was due; and in another he says, "it was *some months* after (March 20, 1707-8) ere I could get the £125; and I am apt to think, had it not been for Dr. Arbuthnot, I should never have received it."[1] Now these statements he must have known to be false. I find by the agreement of the 20th March, that the £125 was due on the re-delivery of the Catalogue of stars to Sir Isaac, which took place on the 20th of March. The order upon Newton for the £125 was signed by the referees on the 26th March, and Flamsteed received his money on the 12th April![2] It is strange how trivial writings are often preserved for the defence of innocence, and the establishment of truth.

I have not found a copy of the articles which were actually signed by the parties. I have before me, however, three drafts of them in Newton's handwriting, and I regret to say that they are essentially different from those published by Mr. Baily. In the latter, Flamsteed is brought under no obligation whatever, and he is made the custodier of all the copies of the work. In the former, he is brought under the most stringent obligation to produce "fair and correct copies" of his Catalogue, and of all his other tables, within a specified time; and there is no obligation to give him the custody of the printed work.[3] The discovery of these drafts of the articles, which cannot be very different from those really signed, justifies the anticipation of Mr. Edleston, that they would throw light upon the controversy.

[1] Baily's *Flamsteed*, pp. 86 and 320.

[2] I have now before me the originals of the order upon Newton, of the 26th March, the order of Flamsteed of the 10th April, to pay the money to Mr. Hodgson, and Hodgson's receipt of the 12th April, all carefully preserved by Sir Isaac.

[3] In Newton's drafts of these articles, two different modes of paying Flamsteed are mentioned. One of these provides that he shall receive £50 for copying and correcting the press of each volume; and also 1s. 6d. per place, for computing the longitudes and latitudes of the planets, the places not exceeding 100, and the same sum for the places of the moon. The other mode is to pay two hundred and . . . pounds for both volumes.

Halley has distinctly stated, that it was agreed to prefix the Catalogue of stars to the first volume of the work; and Mr. Baily, without any evidence, has denied this statement, and charged its author with many misrepresentations and misstatements. Flamsteed, indeed, has asserted that "he signed the articles, but *covenanted* that the Catalogue of the fixed stars *mentioned* to make a part of the first volume, should not be printed, but with the last;" but this is an express declaration that the articles provided otherwise; and Flamsteed's covenant had this strange character, that after signing articles, he either said to himself, or wrote upon the document, that he "covenanted" something different from them. In the articles of March 20, 1708, for example, after he had got a copy of them, he writes, "underneath it," that he covenanted certain things which the articles did not contain. In the draft of the original articles which I have mentioned, the contents of the two volumes are distinctly written in Newton's hand; and it is not only stated in the contents, but it is the very first of the articles, that the first part of the first volume is to be the Catalogue of the fixed stars.[1]

The allusion in Flamsteed's letter to the observations of Tycho, seems to have drawn the attention of the referees to that subject; and they appear to have suggested to his Royal Highness the idea of having the unpublished observations of the Danish astronomer, which had been left in the King of Denmark's library, written in Tycho's own hand, printed at his expense, and published at the same time with Flamsteed's work. The Prince agreed to the suggestion; and in communicating his secretary's letter to Newton, Dr. Arbuthnot, one of his Royal Highness's physicians,[2] requests him to inform the referees and Mr. Halley, but not to let Flamsteed know that Halley was

[1] This draft of the articles is given in APPENDIX, No. XIII.

[2] In an unpublished letter, dated Windsor, July 30, 1706. On the 8th of January 1707, Sir Isaac was requested by the Royal Society to endeavour to procure Tycho's MSS., to be printed with Mr. Flamsteed's observations, and on the 27th he stated that he would endeavour to procure them. Tycho's observations on the comets of 1585,

consulted. As the Prince was "mighty desirous to have the eight volumes of Tycho's observations in his possession," Dr. Arbuthnot suggested, that as they were sent into France by Olaus Roemer, the Danish astronomer, the referees should write to him, giving an account of the substance of Flamsteed's observations, and requesting an abstract of the eight volumes of those of Tycho. Sir Isaac accordingly drew up a letter in the name of the referees, and addressed it to Roemer; but whether or not it was sent, and what was the result of the application, if it was made, I have not been able to discover.

Notwithstanding these impediments, the first volume, containing the Sextant observations, was finished in December 1707; and preparations were made for printing the second volume, which was to contain the observations with the mural arc. On the 20th of March 1708, Flamsteed deposited the materials for this volume in the hands of the referees, copied out in 175 sheets of paper; and he soon after amended the catalogue which had been previously lodged in their hands under a new agreement.

At a meeting of the referees on the 13th July, it was agreed "that the press should go on without farther delay;" and "that if Mr. Flamsteed do not take care that the proofs be well corrected, and go on with dispatch, another corrector be employed." In order "to prevent the designed effect of this malicious order," Flamsteed wrote a long and temperate letter of remonstrance to Sir Christopher Wren, defending himself against the charge of delay, and protesting against anything being printed without his corrections. No answer was returned to this letter: The press was stopped; and before any arrangements could be made, Prince George died on the 28th October 1708, and the printing of the work was suspended for three years.[1]

1590, and 1596, were given to the Royal Society by Newton, October 5, 1722.—*Miscellaneous MSS.* lvii.

[1] The agreement with the Prince was considered as cancelled by his death. His treasurer had advanced £375; and as £25 of this had not been expended, it was returned to his administrators. See APPENDIX, No. XV.

During this long interval, no communication passed between Flamsteed and any of the referees. Newton had in his possession the synopses of lunar observations which it is said were given him "with an express understanding that they were not to be published;" and also the uncompleted Catalogue of the stars, which, it is said, was sealed up at his own request. The obligation thus imposed, and the trust thus confided to him, he is charged with having violated. Had this charge appeared but in the letters and manuscripts in which it has slumbered for more than a century, few astronomers would have heard of it, and it might have been neutralized by the high character of the great and good man whose character it affected. But after being repeated in a variety of shapes, in the letters, and diary, and autobiography of its author, the calumny has been presented to the world in all its original bitterness, and in a more attractive form, by Mr. Baily; and the public money[1] has been expended in printing the volume which contains it, and in circulating it among all the distinguished astronomers and institutions throughout the world. I have felt it therefore a sacred duty to investigate the subject, and to defend an illustrious name, embalmed in the affections of his disciples and of his countrymen.

When Mr. Baily had seen the effect produced by his *Life of Flamsteed*, he found it necessary to publish a Supplement, in its explanation and defence; and from his preliminary observations, the reader will see the necessity of the task we have undertaken.

"It cannot be disguised," says Mr. Baily, "that the quarrel between Newton and Flamsteed, relative to the printing of the Greenwich observations, has arrested a much greater portion of the public attention than any other incident recorded in Flamsteed's Life, and indeed greater than its relative importance

[1] Mr. Baily's *Life of Flamsteed* was printed by order of the Lords Commissioners of the Admiralty in 1835, and copies of it presented by them to numerous individuals and institutions.

seems to merit; and Newton's admirers have, as might have been expected, shown a natural desire to remove from him every appearance of misconduct arising out of that dispute. In doing this, however, it seems to me that, in some instances at least, the tendency of their remarks has been to exculpate Newton, not so much by a direct refutation of the charges adduced by Flamsteed, as by attempting to lower the moral and scientific character of Flamsteed himself in public opinion, and thus to show that Newton was most probably right in the line of conduct which he pursued. This course, however, can scarcely be tolerated at the present day: neither is it just to the character of Flamsteed (nor indeed to that of Newton, which stands too high in the general opinion of mankind to need such support), that the decision should rest on such grounds. The mere fact of mental superiority, which no one is disposed to deny, ought not to weigh one feather in the scale of justice, and the case must be decided solely on its own merits."

After this explanation, we may reasonably expect that the charge against Newton, when preferred by Mr. Baily, will be couched, as it is, in less exceptionable terms than in the vulgar and offensive phraseology of Flamsteed. We shall give it, therefore, in his own words, in order to make the charge and the answers to it perfectly intelligible.

"At the end of that period," says Mr. Baily (the interval of three years), "namely, in March 1710-11, Flamsteed learned, for the first time (no communication having been had with him on the subject during the interval), that this packet containing the Catalogue had been *broken open*, and that *not only* the Catalogue itself was at press, but also that the observations (copied out on the 175 sheets of paper as above-mentioned) were likewise in the course of being printed in a *garbled and mutilated state.*

"Flamsteed was of course *very much annoyed and irritated at this unexpected* piece of intelligence: he saw at once that his favourite plan of printing his observations in detail in the order in which they were made, and the only way indeed in which they could be essentially useful to the future astronomer, was, *without his knowledge or consent*, about to be sacrificed to a scheme that would render them of little or no practical utility, and compromise his own character as an observer. He likewise found that the places of the moon, which he had from time to time communicated to Newton, with an express understanding that they were not to be published, because they were deduced from an imperfect catalogue of the stars, were annexed to the work. He was convinced that this scheme had been long in agitation, since it must have taken the referees a considerable time to dissect and arrange the observations in the manner in which they were then prepared and sent to the press.[1] Upon what grounds *this clandestine and improper conduct can be justified, I have ever been at a loss to imagine ; and I have always regretted* (in common I am sure with every other reader), *to find Newton's name mixed up with a transaction of this kind ;* since it is, in my opinion, the only portion of the series of disputes recorded in this volume that is worthy of a serious refutation ; all the other *sudden ebullitions of temper and apparent perversity of conduct* being mere venial offences of our common nature. But I suspect it was in that day as at the present hour, that individuals of high and honourable character (when acting in concert with others having interested objects in view, and not quite so scrupulously austere in their conduct as themselves) may sometimes be led to countenance and sanction certain acts which, as private persons, and on their sole responsibility, they would cautiously avoid."[2]

[1] "The same remark may perhaps be applied to the Catalogue; and therefore Flamsteed's assertion that the Queen's order (to open the packet), if obtained at all, had been obtained after the offence was committed, is probably correct ; as that order would not have been given prior to February, and the Catalogue containing the additional stars by Halley, was at press in the following month, and actually finished by the month of June." See page 174.

[2] Baily's *Flamsteed*. Supplement, pp. 727, 728.

Had Mr. Baily told us how Flamsteed first heard of the ill usage and clandestine proceedings so forcibly described in the preceding passage, and *how he received the intelligence*, we should have been better able to form an opinion of the nature of the offence. The whole statement of Mr. Baily, that he was annoyed and irritated at the piece of *unexpected intelligence*, is an entire fiction. The intelligence was received in *March* with perfect composure of mind, and the alleged irritation was not shown till *October*, *seven* months afterwards, and then too, not at the intelligence, but during a personal altercation with Sir Isaac Newton, in which Flamsteed was the aggressor! This important fact is proved by the correspondence which was begun by Dr. Arbuthnot on the 14th March 1710-11, and continued till the 23d of May. The fact of the Catalogue being in the press, and consequently of the packet having been opened, if it ever was sealed, is obvious from the very first letters of Arbuthnot; and in the five answers returned by Flamsteed, there is not the slightest allusion made to the irritating event![1]

Mr. Baily asserts on Flamsteed's authority,[2] that it was in March 1711 that he first learned that the sealed Catalogue was broken open, but the incorrectness of this statement, which Mr. Baily ought to have known, is proved by the very letters of Flamsteed himself. In his petition to the Queen, April 16, 1712, he distinctly states that "some time after (March 1706), he was told that the copy of the Catalogue was *opened and unsealed*;" and in a letter to Sharp, May 15, 1711, he tells him, "we met on March 20, 1707-8 (the date of the

[1] Flamsteed tells us in his autobiography, written long afterwards, that in March 1711 he was "privately told that his Catalogue was in the press" (p. 93); and in his letter to Sharp, dated May 15, 1711, he says, "March 25th last past I was informed by a friend that my Catalogue was in the press, and some sheets of it printed off;" but this was no secret, for on the 21st February, at a meeting of the Royal Society, Dr. Sloane was ordered "to write a letter to him, desiring him to furnish the deficient part of his Catalogue of the Fixed Stars, *now printing by order of the Queen.*"

[2] Baily's *Flamsteed*, p. 93.

second agreement), and then Sir Isaac had *opened the Catalogue*, and desired me to insert the magnitude of the stars to their places, for they had not always been inserted in it." Now it is here placed beyond a doubt, that Flamsteed knew in March 1708, that the Catalogue was *open*—that he found no fault whatever with its being open, and did not *at the time* charge Newton with having opened it. Nay, he is so well pleased with this second agreement, and the payment to himself of £125, to which he had no claim by the original articles, that he tells Sharp, on the 19th April 1708, of this "*change in his affairs* which it will not be displeasing to him to hear," and he finds no fault with the Catalogue being open, though he adds that it was part of the new agreement that the magnitudes were to be inserted in it. In the whole of his correspondence with Sharp, the depositary of his afflictions and his calumnies, from March 1708, when he knew that the Catalogue was opened, till the end of November 1712, he makes no charge against Newton or any other person for having unsealed the Catalogue. At that date, however, when the arrangement between him and the referees was at an end, he tells his correspondent for the first time that "he was forced to trust in the hands of Sir Isaac Newton an imperfect copy of the Catalogue, which he *very treacherously broke open*, though it was at his own desire *sealed* up and so delivered into his hands."[1]

The next charge which Mr. Baily makes against Newton and his colleagues is, that *without Flamsteed's knowledge and consent* they sacrificed "his *favourite* plan of printing his observations in detail in the order in which they were made," to "a scheme of little or no practical utility, and compromising his character as an observer." To this charge it is sufficient to reply, that the scheme here condemned is that which forms the first article of the agreement signed by Flamsteed himself! Of the same character is the charge that, in "annexing to the

[1] Baily's *Flamsteed*, p. 298.

work" the places of the moon, Newton had violated an express understanding that they were not to be published. Now, Mr. Baily ought to have known that this understanding was imposed upon Newton in 1694, when he received these observations for his lunar theory. By the articles of agreement, these lunar observations were to form part of the *Historia Cœlestis*, and for the purposes of collation the referees were authorized to call for *all the original papers* in Flamsteed's custody. These observations, whether in Newton's possession or anywhere else, had thus become the property of the referees for publication, and they were guilty of no clandestine conduct in annexing them to the work. In a note, which we have not quoted, Mr. Baily says, "that no demand was ever made by the referees for any observations subsequent to the year 1705," whereas it is expressly stipulated in the first article, "that the observations made with the wall quadrant telescope and micrometer," shall be those "made in and after 1689, *until the finishing of the impression !*"

After making these injurious attacks upon Newton, which we trust have been satisfactorily repelled, Mr. Baily "imagines that it may now be left to the candid and unbiassed judgment of the public to decide whether there is the slightest foundation for the opinion that Flamsteed opposed any impediment to the publication of his astronomical observations, or whether, on the other hand, Newton exhibited any great anxiety for their speedy appearance, in order to complete his Theory of Gravitation." A brief notice of the conduct of the two parties thus placed at the bar of the public, will enable it to give the unbiassed decision which Mr. Baily solicits.

Previous to the 10th of April 1704,[1] the Prince, whose "help to print had been craved by Flamsteed,"[2] had expressed a willingness to bear *the expense of printing* his Observations. At the above date, Newton saw the Book of Observations, the Catalogue, so far as finished, and the Maps of the Constellations;

[1] Baily's *Flamsteed*, pp. 73 and 219. [2] Ibid. p. 76.

and an estimate by Flamsteed of the number of pages or extent of the work was laid before the Royal Society, who recommended the publication of it. The referees appointed by the Prince inspected the papers, and on the 23d January 1705, they reported that the expense of printing 1200 pages, "all which was ready for the press," would be £683, including £100 for copying the papers and correcting the press. At the end of the report, the referees observe, "that it *may be very proper* to print the places of the moon, planets, and comets, 600 being computed, and 1400 not, and that the charges of two calculators to finish them, and of paper, press-work, and printing, will be about £180; so that the whole charge will be about £863."[1]

It will be seen from these arrangements, that the idea of Flamsteed's receiving any recompense for his own labours was never contemplated by the Prince or the referees; but in about a month after the date of the report, he suggested to Newton that he should have *an honourable recompense for his pains.*[2] No notice being taken of the suggestion, he again, on the 15th June 1705, complains that on that occasion Newton did not say *a word of any recompense for thirty years' pains*, though he said *it would be for the committee's honour to provide for that first;* and, on the 29th August 1705, he pronounces it "extremely unjust that no care should be taken to secure him the reimbursement of his large expenses for above thirty years,"

[1] It is here important to notice that the printing of the places of the planets, &c., is not a *necessary* part of the arrangement, and that if it is thought proper to adopt it, it is to be paid for by a separate sum. In two copies of this report, found among Flamsteed's MSS., this £180 is not mentioned—Baily's *Flamsteed*, p. 76, note. But in giving in his autobiography a copy of the estimated expense, Flamsteed not only inserts the £180 along with the other sums, but he gives it as the sum to be paid *for two calculators*, thus making it appear that £280 out of the £863 is to be at his disposal. After his statement of the charges of printing, &c., Flamsteed adds, "But the last particular of the charge (£180 for two calculators) was not mentioned *in* it (the Report), but added in a note *under* it, for what reason those know best who drew it up." The Report states distinctly the *reason*. It is strange that an editor like Mr. Baily, who has given the *real* Report as possessed by Flamsteed, should have allowed these misstatements to pass unreproved.

[2] Feb. 28, 1705.

adding, "that it was a great dishonour to the Queen, the Prince, and the nation, that no reward was proposed."

Previous to these expressions of his views,[1] Flamsteed had communicated, in a letter to his nephew, Mr. Hodgson, a plan of doubtful honesty, for making money out of the "Prince's Bounty"—a plan which he never could have meant for the public eye, and which Mr. Baily ought not to have published.[2] It is obvious, indeed, that before and after he had signed the articles in 1705, the grand object of the Queen's Astronomer was to secure a sum of money for himself, and that to obtain this he threw every obstruction in the way of completing the work.

On the 13th July 1708, nearly *three years* after the work had begun, and when it ought to have been finished, the delay on Flamsteed's part was so great, that the referees, as we have previously stated, agreed, that if he "did not go on with despatch, another corrector would be employed."

In order to thwart this resolution, Flamsteed immediately addressed a letter to Sir Christopher Wren,[3] in which he lays the whole of the blame upon Newton; and, in order to give authenticity to the copy of it which he preserved, he tells us that "he took a copy of it himself to show any acquaintance, friends, and some gentlemen that had an opinion of Sir Isaac Newton before, and could not think he could be guilty of such collusion as this order and my letter proved upon him."[4] This

[1] March 22, 1705.

[2] "I think to be very plain with Mr. Aston, and desire that he, I, and Mr. Churchill, may understand one another fully, and know what each shall advantage themselves by my pains; for his and Mr. Churchill's will be little or nothing, but to accept their shares, and this will be no equal bargain for me that must be at all the labour and trouble here, nor for Mr. Newton, who saves us the labour of soliciting for the Prince's bounty at Court. And therefore I think he too ought to be acquainted with what advantage every one of us shall make, and go and share with us. I shall say this to him when he returns from Cambridge."—March 22, 1705. It may be conjectured, from the postscript to this letter, that the parties were, according to this plan, to divide the profits arising from the sale of the 400 copies of the work.

[3] Dated July 19, 1708, and sent by Wren to Newton.

[4] Baily's *Flamsteed*, p. 87.

copy, which exists in Flamsteed's handwriting,[1] was transferred to his autobiography for the avowed purpose of proving Newton's guilt, and correcting the good opinion entertained of him by the friends of the Queen's Astronomer and others. The letter certainly has not such a tendency, but in order to give it efficacy, Flamsteed cancelled a paragraph in the original sent to Wren, and substituted another in the incorrect copy, which he submitted as evidence to the contemporary jury that was to try Newton, and to the more solemn judgment of posterity. Sir Isaac had fortunately preserved the original letter, which, after slumbering for a century and a half, and eluding the search of Mr. Baily, has reappeared to defend Newton, and cast a doubt on every document Flamsteed left behind him that is not authenticated by other evidence than his own.[2]

In the original, or cancelled paragraph, Flamsteed declares his willingness, and even his desire, to finish the work. He instructs his nephew to correct the proofs: He leaves six sheets to be added: He authorizes Newton to go on with the 175 sheets of the second volume, that the press may proceed while he is completing the Catalogue, so that there should be no stop on his account, as there never was, and never should be.[3] Nothing could be more satisfactory to the referees than this communication. Whatever misunderstandings had occurred, the Queen's Astronomer here bound himself anew to complete the Catalogue, and avoid all further delay; but after the Prince's death, when he had refused to complete the Catalogue, and, in 1716, when he came to write his autobiography, he was willing to forget the obligations in the original paragraph, and he therefore falsified the document by the substitution of a paragraph in which he abjures hurrying on the work in his absence, and limits his former promise, that there shall be no stop on his account, by the condition that "heed should be

[1] Baily's *Flamsteed*, p. 87, note.

[2] See pages 121, note; 128, note; and 129, note.

[3] This paragraph, and the one substituted for it, are given in APPENDIX, No. XIV.

given to his advice;" or, in other words, that he should have his own way, which he took in spite of all his written promises and sealed obligations.[1]

Previous to Flamsteed's correspondence with Arbuthnot, the Royal Society, anxious to make the Greenwich Observatory useful for the promotion of astronomy and navigation, applied to the Queen to place it under the superintendence of a Board. An order was accordingly issued on the 12th December 1710, appointing the President, and such other members of the Royal Society as it should name, to suggest observations to be made,—to repair and renew the instruments in the Observatory, and to receive from the Astronomer-Royal the annual observations which he has recorded. Armed with this authority, and by an order from the Queen to print the observations, in the hands of the referees, the Society requested Dr. Arbuthnot to apply to Flamsteed, as we have seen he did, in March 1711, for the rest of his Catalogue, the part of it in their hands having been already in the press. The sheets were sent to Flamsteed, who asserted that they contained many errors and unnecessary alterations, while Halley[2] declared that he had corrected numerous errors

[1] Nearly three years after this letter to Wren was written, on the 26th April 1711, Flamsteed desired Dr. Arbuthnot "to peruse his letter to Sir C. Wren, of which he had given him a copy, and *particularly the last paragraph*, whereby he would be satisfied that he had done all that lay in his power to expedite his work, and *had taken great care of the Catalogue of the fixed stars.*" Now it is only in the original letter actually sent to Wren that these matters occur in the last paragraph, so that Flamsteed referred to the real letter, of which he had taken a *correct* copy for Arbuthnot. The incorrect copy was, therefore, manufactured at a later date for the purposes we have mentioned.

[2] This letter of Halley's to Flamsteed, dated June 23, 1711, is the only appearance he makes in person in this multifarious correspondence. When we consider the innumerable and coarse attacks made upon his character, and the vulgar abuse of him which almost every letter contains, the following advice to Flamsteed at the close of his epistle will not be thought unfriendly:—"Pray govern your passion, and when you have seen and considered what I have done for you, you may perhaps think I deserve at your hands a much better treatment than you have for a long time been pleased to bestow on your quondam friend, and not yet profligate enemy (as you call me)." This advice is not so severe as that of Flamsteed's own particular friend Dr. Smith. "My advice is, that you represent your case nakedly, clearly, and without any flourish, or without any kind of resentment, as you are a philosopher and a mathematician, *and above all, as you are a clergyman.*"—Baily's *Flamsteed*, pp. 293 and 747.

in the original Catalogue—that he had asked Flamsteed for any corrections he thought necessary, and that he offered to make them and reprint the whole sheet if required.[1]

While matters were in this state, Sir Isaac requested Flamsteed to meet him at the Royal Society's house on the 26th October 1711. Flamsteed accordingly went, and found there Dr. Sloane and Dr. Mead along with Newton. Flamsteed has given *three*[2] accounts of this meeting, which are not very consistent with one another. According to him, Newton asked what instruments he wanted, and what repairs. Upon which Flamsteed said that he would not suffer any one to concern themselves about repairing his own instruments. To this Newton replied, "As good have no observatory as no instruments." Flamsteed then complained that he had been *robbed of the fruit of his labours.* "At this," says Flamsteed, "the impetuous man grew outrageous, and said, 'We are then robbers of your labours?' I answered, 'I was sorry that they owned themselves to be so.' After which all he said was in a rage. He called me many hard names—*puppy* was the most innocent of them."

Such is Flamsteed's account of an altercation which he did not make known at the time it happened, in order to allow the other three parties concerned to give their account of what actually took place. We have Flamsteed's own authority for stating that Dr. Mead ran into the same passion, and charged him with having insulted the President. If it be true that Newton lost his temper and called Flamsteed a puppy, we leave it to those who have perused the correspondence, and studied the character of Flamsteed as gathered from the preceding pages, to determine the amount of provocation which Newton seems to have received. How simple-minded must he have been in

[1] On the 18th March 1712, when Halley visited the Observatory, "He offered," says Flamsteed, "to burn his Catalogue if I would print mine." Dr. Arbuthnot had previously offered to "reprint, change, or alter anything Flamsteed allowed."

[2] In his Autobiography and Diary, and in a letter to Sharp.

whose vocabulary of vituperation the epithet given to Flamsteed was the most prominent !

The referees, by orders from the Queen, proceeded to print the copy of the Catalogue when they could procure no other, and therefore they, and not Newton, must have broken open the seal if it was sealed. In violation of the promise contained in his letter to Wren, Flamsteed had refused to go on with it, and we find him telling Sharp, what he durst not insinuate to the referees, that "*he shall not urge the press forward again till he sees a good fund settled and secured.*"[1] No sooner, however, does he find that his Catalogue is printing, and that the *press is urged forward* by the referees, than he assails them with the most violent language. Halley is called a *malicious* thief. His property, which he gave to Newton, and got money for it, is said to have been surreptitiously forced out of his hands by his profligate enemies, and under the influence of these feelings he determined to print his observations at his own expense, thus violating two solemn obligations, and frustrating the liberal arrangements of Prince George, after he had received £125 of his money, and caused £250 more to be expended in printing the work, and in paying Machin for correcting his own calculations.

Under these circumstances the referees, with the assistance of Dr. Halley as its editor, published in 1712, under the title of *Historia Cœlestis*, the part of the work which had been executed at the expense of the Prince and the Government.[2] Of the 400 copies that were printed, nearly 100, including 30 reserved by the Treasury, were presented to eminent individuals and public bodies, and the remaining 300 were given to Flamsteed by Sir Robert Walpole, when First Lord of the Treasury. Flamsteed committed them to the flames, preserving only what is now the first 97 sheets of the *Historia Cœlestis*, which he left

[1] Baily's *Flamsteed*, p. 270; March 24, 1709.

[2] In Appendix No. XV., I have given an account of the expense incurred by the Prince and the Government in printing the work.

almost ready for publication at the time of his death, on the 31st December 1719. The work was published in 1725 by his executors, in three vols. folio, and dedicated by them to the King.[1]

In taking a general view of this painful controversy, Mr. Baily has remarked, that the friends of Newton have defended him by attempting to lower the moral and scientific character of Flamsteed ;[2] a course which he thinks can scarcely be toler-

[1] The correspondence between Newton and Flamsteed seems to have terminated with Flamsteed's letter of September 14, 1706. I have found, however, among the Portsmouth papers, a draft of a letter from Newton to Flamsteed, without a date, and certainly written about the 24th of March 1711. It shows his great anxiety to get on with the printing of the work, in place of stopping it, as Flamsteed maintained. It will be found in APPENDIX, No. XVI. There is also a short one from Flamsteed, dated April 23, 1716, wishing Newton to return some of his manuscripts.

It may be proper here to notice an observation made by Professor De Morgan, in reference to the omission of Flamsteed's name from the second edition of the *Principia*. "Shortly afterwards," he says, "the second edition of the *Principia* appeared. Flamsteed, whose observations had been of more service to Newton than those of any other individual, and to whom proper acknowledgment had been made in the first edition, and who had increased the obligation in the interval, had his name erased in all the passages in which it appeared (we have verified for this occasion eight or nine places ourselves). To such a pitch is this petty resentment carried, that whereas in one place of the first edition (prop. 18, book iii.) there is in a parenthesis 'by the observations of Cassini and Flamsteed,' the corresponding place of the second is 'by the consent of the observations of astronomers.'"—*Sketch of the Life of Newton, Cabinet Portrait Gallery*, vol. xi. p. 101. Lond. 1846. In reply to this statement, Mr. Edleston observes, "The name, however, will be found in pages 441, 443, 445, 458, 465, 478, and 479 : The last two references occur in some additional matter on comets, which was put into Cotes's hand in October 1712. (See p 141 of this work.) I question very much whether the suppression of Flamsteed's name in several places where it had appeared in the first edition, was not such as was necessary in the process of improving the work."—*Correspondence*, &c. p. lxxv., note 162. In thus correcting the numerical oversight of Professor De Morgan, we must admit that his criticism is substantially correct. Mr. Edleston's explanation is not applicable to the omission of the joint names of Cassini and Flamsteed ; but even if it had an application to them, it would not justify the omission. Newton owed to Flamsteed substantial obligations, and we do not think that these obligations are sufficiently acknowledged in the *Principia*, even if his name had in every case been retained in the second edition.

[2] The following opinion of the *Principia*, given by Flamsteed in 1713, might have either justified an attempt on the part of Newton's friends, to lower his scientific character, or rendered it unnecessary. "I think his new *Principia* worse than the old, save in the moon !"—Baily's *Flamsteed*, p. 307.

ated in the present day. Attainments in science have certainly nothing to do with the present question; but after Flamsteed has charged Newton with illegal, unjust, and immoral acts, upon no evidence but his own, and has sullied that venerable name with vulgar and offensive abuse,—it is a strange position to maintain, that we are not to inquire into the temper and character of the accuser.[1] In the revolting correspondence which Flamsteed has bequeathed to posterity, he has delineated his own character in sharp outline and glaring tints; and Newton requires no other Ægis to defend him than one whose compartments are emblazoned with the scurrilous invectives against himself, and garnished with pious appeals to God and to Providence. We have hesitated, however, to associate the sacred character of the accuser with systematic calumny; and we hasten to forget that there may be an astronomer without principle, and a divine without charity.

[1] The injurious tendency of Mr. Baily's work is strikingly exhibited in the notices of it in our two leading reviews. Both the *Edinburgh* and the *Quarterly Review* took the part of Flamsteed, and made no attempt to defend Newton against his charges. It never seems to have occurred to the writers of these articles, that the charges are supported by no other evidence than that of the choleric individual by whom they are preferred; and neither of them has been at the trouble of cross-questioning their solitary witness. The Quarterly Reviewer goes so far, as "charitably to attribute Newton's letter of the 6th of January 1699, to the effect of that distressing malady which overwhelmed Newton for a time in 1692—a malady rashly ascribed by some to mental aberration!"—See *Edinburgh Review*, vol. lxii. p. 359, June 1836; and *Quarterly Review*, vol. lv. p. 96, December 1835.

CHAPTER XXI.

Dissensions in the Royal Society—Dr. Sloane and Dr. Woodward—Letter to Newton on the subject—Dr. Woodward removed from the Council—Second edition of the *Principia*—Dr. Bentley's letter to Newton about it—Delay of the work—Bentley's second letter—Newton's residences in London—Bentley announces to Newton the completion of the second edition—The Duke D'Aumont elected F.R.S.—Deslandes' account of a dinner party at Newton's—Origin of the Royal Observatory at Greenwich—Prince Menzikoff elected F.R.S.—Petition to Parliament for a Bill to promote the discovery of the Longitude—Evidence of Newton—His conduct misrepresented by Whiston and Biot—The Bill passes both Houses of Parliament—Dissensions in the Government—Offer of a pension to Newton—Death of Queen Anne—Accession of George I.—Lord Halifax Prime Minister—Death of Halifax—His will—His affection for Miss Catherine Barton, Newton's niece—Her intimacy with Swift—Her character defended.

While Sir Isaac and his friends were striving with Flamsteed to complete the printing of the Greenwich Observations, his tranquillity was disturbed by an exciting dispute which took place in the Council of the Royal Society, between Dr. Sloane and Dr. Woodward. So early as 1700, before Newton was President of that body, the conduct of its Secretary, Dr. Sloane, in furnishing "unfit entertainment" at their meetings, and in conducting the publication of the Philosophical Transactions, had been the subject of animadversion. In a pamphlet, entitled *The Transactioneer, with some of his Philosophical Fancies*, the Royal Society, and particularly Dr. Sloane, were severely satirized. The Council made great exertions to discover the author of this silly production,[1] "and Dr. Sloane, and his friend Mr.

[1] Dr. Johnson says that it was written in 1700 by Dr. William King, "a man of shallowness;" and Mr. Weld, who has looked into the copy of it in the British Museum, characterizes it as "of so low and ridiculous a nature, that it is surprising the Council

Pettiver, caused it to be set abroad, that Dr. Woodward was either the author, or at least concerned in its production." Dr. Woodward indignantly denied the charge. "I am sorry," he says, "to find two or three members of the Society, and my particular friends, ill treated in it: The writer of it is but meanly qualified for what he undertakes; though whether there was not occasion given, may be worth your consideration. This I am sure, the world has been now for some time past very loud upon that subject: and there were those who laid the charges so much wrong, that I have but too often occasion to vindicate the Society itself, and that in public company too." This homologation of the charges in the pamphlet, by a distinguished member of the body, could not fail to irritate the Secretary; and we need not wonder that a more public quarrel arose between Dr. Woodward and Dr. Sloane. At the anniversary meeting of the Society held on the 30th November 1709, Dr. Sloane was re-elected to the office of Secretary; and Mr. Richard Waller, who had been the other Secretary since 1687, was replaced by the Rev. John Harris, D.D., a friend of Dr. Woodward and his party.[1] Soon after this election, and at one of its ordinary meetings,[2] Dr. Sloane "entertained" the Society with a translation from the Memoirs of the Academy of Sciences, in which it was "maintained *that the Bezoar is a gall stone*," and the Doctor himself asserted "*that the stones in the gall-bladder were the cause* of colic." Dr. Woodward denied the truth and probability of these opinions; and when his adversary "was not able to maintain what he had asserted by words, he had recourse to grimaces very strange and surprising, and such as were enough to provoke any ingenuous sensible man to a warmth at least equal to that which Dr. Woodward

should have thought it worth their while to notice it."—*History of the Royal Society*, vol. i. pp. 352-355.

[1] Mr. Waller was reinstated in place of Dr. Harris at the next election on the 30th November 1710.

[2] The following account of the quarrel I find in an anonymous letter addressed to Sir Isaac Newton, and dated March 28, 1710.

used. His words were, *no man that understands anatomy, can assert that the stones in the gall-bladder are the cause of the colic.* When Dr. Sloane averred that all medical writers were of that opinion, Dr. Woodward replied, *none, unless the writer of the History of Jamaica;* challenging him to assign any one man, which he did not. But appealing to Dr. Mead,—which was only a small mean shift, the Doctor was forced to give it against him. Those recited were the very words Dr. Woodward used; and whether they are unfit, you are a proper judge. That they were not spoken till after Dr. Sloane had made his grimaces twice or thrice, you were assured by Mr. Clavel, and Mr. Knight is ready to confirm the same if you please to ask him. He is a gentleman, as modest, impartial, and creditable, and indeed, with Mr. Clavel and Dr. Harris,[1] sate so fronting Dr. Sloane, as to be able to see his face and grimaces. The rest, which were but few, sate out of fair view. In particular, Mr. Moreland, that with so much solemn formality, made asseveration, that to the very best of his memory the words preceded the grimaces, sate directly behind Dr. Sloane, so that he neither did, nor possibly could, see one of those grimaces."

In defence of the language used by Woodward, the author of this letter reminds Sir Isaac, that he himself had on a previous occasion employed still stronger terms against Sloane. "You had complained," he says, "of Dr. Sloane's artifices in surprising you with things at the Council, frequently very unfit, without having given you any previous account. As upon others, you had declared to more than one friend, how little qualified he was for the post of Secretary, so upon these occasions you as freely declared him *a tricking fellow;* nay, *a villain* and *rascal*,[2]

[1] Dr. Harris was the author of a work published in 1697, in defence of Woodward's "Essay towards a Natural History of the Earth." It was entitled, "Remarks on some Late Papers, relating to the Universal Deluge, and to the Natural History of the Earth." —Ward's *Lives of the Gresham Professors*, p. 286.

[2] Without better evidence than that of a partisan, we cannot believe that these words were in Newton's vocabulary. When he was irritated at the conduct of Flamsteed, he could not command a harsher term than that of *Puppy*. See p. 180. The letter, how-

for his deceitful and ill usage of you in the affair of Dr. Wall. Such expressions do not fall forth of the mouth of a gentleman of your truly good sense and breeding, without cause. Indeed, all allow you had very great and just cause; and though Dr. Woodward has not used any such expressions, he has had causes as great and just, long and often, of which I have heard the particulars, but shall not trouble you with them here."

This appeal to Sir Isaac does not seem to have advanced the objects of Dr. Woodward and his party. The grimaces of Sloane, and the uncivil language of Woodward, were brought under the notice of the Council on the 10th of May 1710. Sloane denied the grimaces, and in such a way as to induce Woodward to say, "Speak sense, or English, and we shall understand you." The consideration of this new attack upon the Secretary came before the Council on 24th May; and as the Doctor refused to make an apology, it was resolved, "that Dr. Woodward be removed from the Council for creating a disturbance by the said reflecting words." A resolution was, at the same time, passed, thanking Dr. Sloane for his pains and fidelity in serving the Society as Secretary. Dr. Woodward brought an action at law against the Council in order to reinstate him as a member of it, but he was unsuccessful. Dr. Sloane resigned the office of Secretary in 1713, and on the 30th November 1727, he re-appeared in the Council with the rank of a baronet, —in the more dignified position of its President, and the successor of Sir Isaac Newton.[1]

ever, is well written, and contains many useful and temperate suggestions for improving the Society. The author, too, seems not at all disposed to maintain his incognito, as he expresses a willingness to have a personal interview with Sir Isaac.

[1] Sir Hans Sloane and Dr. Woodward were both of them distinguished men, and great national benefactors. Dr. Woodward was Professor of Physic in Gresham College. He not only collected much valuable information respecting the geological structure of the earth, but so early as 1695, he began to form a collection of fossils, which after arranging and cataloguing it, he bequeathed to the University of Cambridge, of which he was a member, with the sum of £150, "for the maintenance of a lecturer to read there on the subject of the Doctor's Natural History of the Earth," &c. He was born May 1, 1665, and died April 25, 1728. His expulsion from the Council of the Royal Society

We have already seen, in the history of the *Principia*,[1] that Newton had been occupied during many years in preparing for the press a second edition of the work. His disputes with Flamsteed, however, and his duties at the Mint, rendered more anxious by the disturbances which had arisen in that establishment, interfered greatly with its progress; and it was with much difficulty that Dr. Bentley persuaded him to intrust the publication to him. He accordingly sent him, in June 1708, or earlier, a portion of the copy of the work, with his corrections and additions; and on the 10th of that month Bentley sent him a proof of the first sheet for his approbation, accompanied with the following letter:—

"TRIN. COL., *June* 10, 1708.

"DEAR SIR,—By this time I hope you have made some progress towards finishing your great work, which is now ex-

does not seem to have alienated him from Newton, as in 1714 he dedicated to him his *Naturalis Historia Telluris*, of which he says, "It is wholly owing to you, it being begun, carried on, and finished at your request."—*Fossils of all Kinds*, 1728. Letter I.

Sir Hans Sloane, who was of Scotch extraction, was born in Ireland on the 16th April 1660. In the year 1705, he published the first volume of his *Natural History of Jamaica*, and the second volume in 1725. He wrote also twenty-four Papers for the *Phil. Transactions*. He was created a Baronet in 1716, and died on the 11th January 1753. On the condition of his family receiving £20,000, he bequeathed his museum to the public, with his library of 50,000 volumes, and 3566 manuscripts. The original cost of his museum was £50,000. Parliament accepted the trust, and these valuable collections form the nucleus of the British Museum.—Weld's *History of the Royal Society*, vol. i. p. 456.

During the time of the dispute, however, in the Royal Society, Newton is said to have remarked, "that Dr. Woodward might be a good natural philosopher, but that he was not a good moral one."

In consequence of some difference of opinion on medical subjects, Woodward and Dr. Mead fought a duel under the gate of Gresham College. Woodward's foot slipped, and he fell. "Take your life," exclaimed Mead: "Anything," replied Woodward, "but your physic." An amusing account of this duel, by Dr. Woodward, will be found in the *Weekly Journal* of June 20, 1719, and in Nichol's *Literary Anecdotes of the Eighteenth Century*, vol. vi. p. 641.

In writing to Abraham Sharp on the 14th July 1710, Flamsteed says, "Sir Isaac Newton has hurt our Royal Society by his partiality for E. Halley and Dr. Sloane, upon a small and inconsiderable occasion; so that they have broke up some few weeks before their time. Dr. Harris has lost all his reputation by actions not fit for me to tell you."—Baily's *Flamsteed*, p. 276, note.

[1] See vol. i. p. 273.

pected here with great impatience, and the prospect of it has already lowered the price of the former edition above half of what it once was. I have here sent you a specimen of the first sheet, of which I printed about a quire; so that the whole will not be wrought off before it have your approbation. I bought this week a hundred reams of this paper you see; it being impossible to have got so good in a year or two (for it comes from Geneva), if I had not taken this opportunity with my friend Sir Theodore Jansen, the great paper merchant of Britain. I hope you will like it, and the letter too, which upon trials we found here to be more suitable to the volume than a greater, and more pleasant to the eye. I have sent you likewise the proof-sheet, that you may see what changes of pointing, putting letters, capitals, &c., I have made; and I hope much to the better. This proof-sheet was printed from your former edition, adjusted by your own corrections and additions. The alterations afterwards are mine, which will show and justify themselves, if you compare nicely the proof-sheet with the finished one. The old one was without a running title upon each page, which is deformed. The sections only made with Def. 1. Def. 2., which are now made full and in capitals—DEFINITIO I., &c. Pray look on Hugenius de Oscillatione, which is a book very masterly printed, and you'll see that it is done like this. Compare any period of the old and new, and you'll discern in the letter, by the change in the points and capitals, a clearness and emphasis that the other has not; as all that have seen this specimen acknowledge. Our English compositors are ignorant, and print Latin books as they are used to do English ones, if they are not set right by one used to observe the beauties of the best printing abroad. In a few places I have taken the liberty to change some words, either for the sake of the Latin, or the thought itself; as that in page 4, *motrices, acceleratrices et absolutas*, I placed so, because you explain them afterwards in that order.

"But all these alterations are submitted to your better judgment; nothing being to be wrought off finally without your approbation. I hope to see you in about a fortnight, and by that time you will have examined this proof, and thought of what's to come next. My wife has brought me a son lately, who, I thank God, is a true healthful child.—I am, yours,

"RI. BENTLEY.

"Note that the print will look much better when a book is bound and beaten."

I have not been able to discover any reason why the printing of the second edition, thus fairly begun, and for which paper was purchased, should have been discontinued, and why the duty of editing it had passed from the hands of Bentley into those of Cotes.

Newton was at this time occupied as one of the referees with the publication of the Greenwich Observations, and with his business in the Mint and at the Royal Society; and we may ascribe, as Mr. Edleston has done, the delay which took place, when the assistance of Cotes had been obtained in 1709, to the political excitement of the times, and to the occupation of Bentley with his quarrels with the seniors of his College.

It does not appear at what date Mr. Whiston delivered to Cotes "the greatest part of the copy of the *Principia*." Newton intimates the transmission of it, in a letter dated October 11, 1709. Cotes was then in the country, where he had been for about a month, and Newton's letter to him was acknowledged by Bentley on the 20th in the following terms:—

"TRIN. COLL., *Octob.* 20, 1709.

"DEAR SIR,—Mr. Cotes, who had been in the country for about a month, returned hither the very day Dr. Clarke[1] brought your letter, in which, I perceive, you think we have

[1] Dr. Clarke had probably come up to perform some exercises for the degree of D.D. which he took in 1710.

not yet begun your book; but I must acquaint you that five sheets are finely printed off already, and had not we staid for two cuts that Rowley carried to town to be mended by Lightbody, which we have not yet received, you had had sent you six sheets by this time. I am sure you'll be pleased with them when you see them. Besides the general running title at the head of every leaf, PHILOSOPHIÆ NATURALIS PRINCIPIA MATHEMATICA, I have added the subdivisions of the book (like Hugenii de Oscillatione), first, DEFINITIONES, then AXIOMATA SIVE LEGES MOTUS, then DE MOTU CORPORUM LIBER PRIMUS. Next will come SECUNDUS, and lastly, DE MUNDI SYSTEMATE LIBER TERTIUS. All these stand in the top of the margin of the several leaves. Your new corollary, which you would have inserted, came just in time, for we had printed to the fiftieth page of your former edition, and that very place where the insertion was to be was in the compositors' hands. The correction in the first sheet which you would have, *plusquam duplo, et plusquam decuplo*, was provided for before; for we printed it *quasi duplo* and *quasi decuplo*, which, you know, amounts to the same thing, for *quasi* denotes either the excess or the defect, and, in my opinion, since in that place you add no reason why it will be *plusquam*, 'tis neater to put it *quasi*, undetermined, and leave the reader to find it out. In the old edition, p. 34, lines 20 and 21, for *infinite major*, you had twice mended it *minor*. This, we thought, you did in haste; for it was right before, and so we have printed it *major*. I proposed to our master printer to have Lightbody come down and compose, which at first he agreed to; but the next day he had a character of his being a mere sot, and having played such pranks that nobody will take him into any print-house in London or Oxford; and so he fears he'll debauch all his men. So we must let him alone, and I daresay we shall adjust the cuts very well without him. You need not be so shy of giving Mr. Cotes too much trouble. He has more esteem for you, and obligations to you, than to think the trouble too grievous; but, however, he does it at my orders, to whom he owes more than that, and so pray you be easy as to that. We will take care that no little slip in a calculation shall pass this fine edition. Dr. Clarke tells me you are thinking for Chelsea, where I wish you all satisfaction. I hope my picture at Thornhill's will have your last sitting, before you leave the town.[1] The time you set under your hand is already lapsed. When the two cuts are sent us we shall print faster than you are aware of—therefore, pray take care to be ready for us.—I am, Sir, your very obedient humble servant,

"RI. BENTLEY.

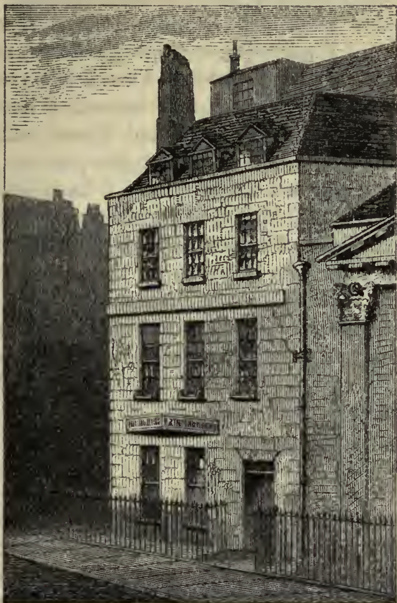
"To SIR ISAAC NEWTON,
at his house in Jermin Street,
near St. James's Church, London."

Newton received this letter when he was removing from Jermyn Street to Chelsea, where he had a house "near the College." On the 1st of July 1697, Dr. Wallis addressed letters to him at the Tower, as if he had lived at the Mint. That he had no official residence there, may be inferred from the observation of Charles Montague, in 1695, that he might have a lodging near him when he came to town to kiss the King's hand.[2] Towards the close of 1697, he occupied a house in Jermyn Street, near St. James's Church, where he remained thirteen years, till he went to Chelsea in October 1709. About the end of September 1710, he removed to Martin Street, near Leicester Fields, where he resided during the rest of his life. This house, which we have represented in the adjoining sketch, from a photographic picture, is the first house on the left hand, or east side of Martin Street, as you enter it from Leicester Square. It stands at the corner of Long's Court, beside a chapel, and is surmounted by a wooden erection, said to have been Newton's private observatory. The house, which

[1] This picture was bequeathed by Bentley to Trinity College.

[2] I find it stated in Conduitt's MSS., that Halley once dined with Newton at the Mint.

is now occupied as a printing-office, is described by Mr. Heneage as one "of good size, and formerly perhaps of some pretensions."[1]



Nearly four years elapsed before the second edition of the *Principia* was completed;[2] and, about the beginning of July 1713, this happy event was intimated to Sir Isaac in the following letter from Bentley, without date, but bearing the post-mark of July 1st:—

"DEAR SIR,—At last your book is happily brought forth,

[1] *Literary and Historical Memorials of London*, 2 vols. Lond. 1847. Mr. Croker, in his edition of Boswell's *Life of Johnson*, mentions a plan of converting Newton's house into a lecture-room.

[2] See vol. i. pp. 274-279.

and I thank you anew that you did me the honour to be its conveyer to the world. You will receive by the carrier, according to your order, six copies; but pray be so free as to command what more you shall want. We have no binders here that either work well or quick, so you must accept of them in quires. I gave Roger (Cotes) a dozen, who presents one to Dr. Clarke and Whiston. This I tell you, that you may not give double; and on that account I tell you that I have sent one to the Treasurer, Lord Trevor, and Bishop of Ely. We thought it was properest for you to present Dr. Halley—so you will not forget him. I have sent (though at great abatement) 200 already to France and Holland. The edition in England to the last buyer is 15s. in quires, and we shall take care to keep it up so for the honour of the book. I can think of nothing more at present, but shall expect your commands, if you have anything to order me.—I am, with all respect and esteem, your affectionate and humble servant,

"RI. BENTLEY.

"*Tuesday*, TRIN. COLL.
"To SIR ISAAC NEWTON,
at his house in Martin Street,
near Leicester Fields, London."

During the years 1712 and 1713, when Newton was occupied with this work, he was obliged to devote much of his leisure to the fluxionary controversy which had now begun to divide the mathematical world. The publication of the differential method of Leibnitz in 1684, before Newton had made public his method of fluxions, rendered it necessary that he should establish, by authentic documents, his prior claim to that great discovery. The Royal Society had, indeed, in 1712, appointed a committee of their body to examine the letters and papers which related to the question, but all the labour of research fell upon Newton, and the *Commercium Epistolicum*, which contained the documents and the report of the committee, though not written by him, in the ordinary sense of the term, was yet virtually his production. A controversy then arose between the English and continental mathematicians, which harassed him during the rest of his life ; and though he seldom appeared in the front of the battle, yet he supplied the munitions of war, and guided the army of his disciples with all the prudence and skill of a leader.

Owing to the interest excited by this controversy, of which we have given an ample history in a former chapter, the Royal Society and its distinguished members became better known on the Continent, and foreigners of distinction sought for admission among its Fellows.[1] Among these was the Duke D'Aumont, who came to England as Ambassador Extraordinary from France in January 1713.[2] He was elected a Fellow of the Royal Society on the 21st of May, and he afterwards addressed a letter to the Society[3] of such a kind, that Newton "returns him their thanks for the great humanity and civility with which he has treated them ;" tells him "that his letter was read in a full meeting of the Society to the great satisfaction and pleasure of all the members present," and assures him "that when anything comes to their knowledge which they may think acceptable to his Grace, they will take care to communicate it."[4]

The Duke D'Aumont was accompanied to England by Mr. Deslandes, the author of a work entitled *A Critical History of Philosophy.*[5] Deslandes dined at Newton's house in company with Halley, Demoivre, and Mr. Craig, and has given the following interesting account of his visit :—" I may be permitted," he says, "to mention here an anecdote, not for the honour which may attach to me from having been familiar with the

[1] A very large number of foreign ambassadors and persons of distinction were chosen Fellows of the Society at this period.

[2] Swift's *Works*, January 2d and 4th, 1713, vol. xiv. pp. 333, 335. Edit. 1784.

[3] This letter, dated February 25, 1714, and an English version of it, are preserved in the Royal Society, A 55, 56.

[4] This letter, dated May 27, 1714, is published in the *Macclesfield Correspondence*, vol. ii. p. 420.

[5] *Histoire Critique de la Philosophie*, par Mr. D. [Deslandes], 4 vols. 12mo. Amst. 1737. Vol. ii. pp. 264, 265.

greatest men of the age, but from the bearing which it may have on the history of philosophy. Having gone to England with the late Duke D'Aumont, who united with the highest talents a generosity almost unknown in our times, I was invited to dine with the illustrious Mr. Newton. And as it is the custom in England, after dinner, to drink to the health of kings and princes whom philosophers generally do not know, and seldom associate with, Mr. Newton more judiciously proposed to me to drink the health of all honest persons, to whatever country they belonged. 'We are all friends,' he added to me, 'because we unanimously aim at the only object worthy of man, which is the knowledge of truth. We are also all of the same religion, because, leading a simple life, we conform ourselves to what is right, and we endeavour sincerely to give to the Supreme Being that worship which, according to our feeble lights, we are persuaded will please him most.' The witnesses to this speech were Mr. Halley, Mr. De Moivre, and Mr. C— (Craig), all mathematicians of the first order."

In the following year, Prince Alexander Menzikoff addressed a letter to Newton, expressing his admiration of the wisdom, bravery, and rare talents of the English nation, and soliciting admission into the illustrious Society of which he was the President.[1] He was accordingly elected on the 29th July 1714; and it appears from Newton's answer, that the English merchants had requested this honour for the Prince on account of his humanity, his love of science, and his affection for the English.[2]

The great problem of the determination of the longitude at sea, to which the discoveries of Newton so greatly contributed, had begun, at this time, to attract the notice of English mathematicians. At an earlier period indeed, the subject had been

[1] This letter, dated Petersburg, Aug. 23, 1714, has been preserved. The Prince's signature, as if written with a paralytic hand, is illegible.

[2] Three drafts have been preserved of Newton's letter, written in Latin, and dated October 25, 1714

brought before the leading members of the Royal Society under very singular circumstances. Towards the close of 1674, Le Sieur de St. Pierre, a French charlatan, who commanded the interest of the Duchess of Portsmouth, had procured from the King a commission for examining a scheme for the discovery of the longitude. This commission, among other names, included those of Lord Brouncker, Dr. Ward, Sir C. Wren, Sir Jonas Moore, and Dr. Hooke. In February 1675, Flamsteed was on a visit to Sir Jonas Moore, and having accompanied him to a meeting of the commissioners, his name was added to their list. By his assistance the ignorance and presumption of the Frenchman were soon exposed; and the result, though mortifying to his patrons at court, proved highly advantageous to the interests of astronomy. Flamsteed had written a letter to the commissioners, and another to St. Pierre, explanatory of his views, and thus describes the origin of the Royal Observatory of Greenwich :—" I heard," he says, " no more of the Frenchman after this; but was told that my letters being shown King Charles, he, startled at the assertion of the fixed stars' places being false in the catalogue (of Tycho), and said with some vehemence, 'he must have them anew observed, examined, and corrected, for the use of his seamen;' and further (when it was urged to him how necessary it was to have a good stock of observations taken for correcting the motions of the moon and planets), with the same earnestness, 'he must have it done.' And when he was asked who could or who should do it? 'The person,' says he, 'that informs you of them.' Whereupon I was appointed to it."[1] In the royal warrant for the payment of Flamsteed's salary, the astronomical observator, as he was then called, was commanded " to apply himself forthwith, with the utmost care and diligence, to rectify the tables of the motions of the heavens, and the places of the fixed stars, so as to find out the so much desired longitude of places for the perfecting the art of navigation."[2]

[1] Baily's *Flamsteed*, pp. 37, 38.

[2] Ibid. pp. 111, 112.

No further steps seem to have been taken in this important matter till the 25th May 1714, when several captains of her Majesty's ships, merchants of London, and commanders of merchantmen, presented a petition to the House of Commons, setting forth "that the discovery of longitude is of such consequence to Great Britain, for safety of the navy, for merchant ships, as well as of improvement of trade, that for want thereof many ships have been retarded in their voyages, and many lost; but if due encouragement were proposed by the public, for such as shall discover the same, some persons would offer themselves to prove the same before the most proper judges, in order to their entire satisfaction, for the safety of men's lives, her Majesty's navy, the increase of trade, and the shipping of these islands, and *the lasting honour of the British nation*."

This sagacious petition, which proved to be a grand step in the advancement of astronomy,[1] was submitted to a large committee, whose report was laid on the table of the House on the 7th of June, and taken into consideration on the 11th. The following is the report and resolution of the committee, which, as we shall see, forms an important event in the life of Newton:—

"Mr. Ditton and Mr. Whiston being examined, did inform the committee that they had made a discovery of the longitude, and were very certain that the same was true in the theory, and did not doubt but that, upon due trial made, it would prove certain and practicable at sea.

"That they had communicated the whole history of their proceedings towards the said discovery to Sir Isaac Newton, Dr. Clarke, Mr. Halley, and Mr. Cotes, who all seemed to allow of the truth of the proposition as to the theory, but doubted of several difficulties that would arise in the practice."

Sir Isaac Newton, attending the committee, said,—

"That for determining the longitude at sea there have been several projects, true in theory, but difficult to execute.

[1] See vol. i. p. 306.

"1. One is by a watch to keep time exactly; but, by reason of the motion of the ship, the variation of heat and cold, wet and dry, and the difference of gravity in different latitudes, such a watch hath not yet been made.

"2. Another is by the eclipses of Jupiter's satellites; but, by reason of the length of telescopes requisite to observe them, and the motion of a ship at sea, those eclipses cannot yet be there observed.

"3. A third is by the place of the moon; but her theory is not yet exact enough for that purpose. It is exact enough to determine the longitude within two or three degrees, but not within a degree.

"4. A fourth is Mr. Ditton's project: And this is rather for keeping an account of the longitude at sea, than for finding it, if at any time it should be lost, as it may easily be in cloudy weather. How far this is practicable, and with what charge, they that are skilled in sea affairs are best able to judge. In sailing by this method, whenever they are to pass over very deep seas, they must sail due east or west, without varying their latitude; and if their way over such a sea doth not lie due east or west, they must first sail into the latitude of the next place to which they are going beyond it, and then keep due east or west, till they come at that place.

"In the three first ways there must be a watch regulated by a spring, and rectified every visible sunrise and sunset, to tell the hour of the day or night. In the fourth way such a watch is not necessary. In the first way there must be two watches, this and the other above-mentioned.

"In any of the three first ways, it may be of some service to find the longitude within a degree, and of much more service to find it within forty minutes, or half a degree if it may, and the success may deserve rewards accordingly.

"In the fourth way, it is easier to enable seamen to know their distance and bearing from the shore, forty, sixty, or eighty miles off, than to cross the seas; and some part of the reward may be given, when the first is performed on the coast of Great

Britain, for the safety of ships coming home; and the rest, when seamen shall be enabled to sail to an assigned remote harbour without losing their longitude if it may be.

"Dr. Clarke said that there could no discredit arise to the Government in promising a reward in general, without respect to any particular project, to such person or persons who should discover the longitude at sea.

"Mr. Halley said, that Mr. Ditton's method for finding the longitude did seem to him to consist of many particulars which first ought to be experimented before he could give his opinion; and that it would cost a considerable sum to make the experiments, but what the expense would amount to he could not tell.

"Mr. Whiston affirmed that the undoubted benefit which would arise in the land, and near the shore, would vastly surmount the charges of experiments.

"Mr. Cotes said that the project was right in the theory near the shore, and the practical part ought to be experimented.

"And, upon the whole, the committee came to these resolutions: 'That it is the opinion of this committee that a reward be settled by Parliament upon such person or persons as shall discover a more certain and practicable method of ascertaining the longitude, than any yet in practice; and the said reward be proportioned to the degree of exactness to which the said method shall reach.'"

This resolution was unanimously adopted by the House.

The bill passed through the House of Commons on the 3d of July,[1] and the House of Lords on the 8th of that month.[2]

This important bill, which, as predicted by British captains and merchants, has in various ways contributed "to the lasting honour of the British nation," contributes in no slight degree to the honour of Newton. Had the evidence of the different witnesses in Parliament been recorded without their names, it

[1] Journals of the House of Commons, vol. xvii. pp. 641, 671, 677, and 716.

[2] In consequence of this Act, Henry Gully, an Englishman, devoted himself to the improvement of timekeepers. He settled in Paris, made various improvements upon watches, and had for his pupil the celebrated Julien le Roy, to whom, and to his son, M. Berthoud, the art of watchmaking is under great obligations.

would not have required the sagacity of Bernouilli to have discovered the testimony of Newton,—the "lion from his claw." The most distinguished of his successors, with all the lights of a century and a half, could not have stated more correctly the true and the only methods of finding the longitude at sea. The method by chronometers has been brought to the highest perfection, and is doubtless the most correct and infallible. The method "by the place of the moon," has, by means of his own lunar theory, perfected by his successors, become second only to that of "the watch."

So early as 1696, a report was spread among the members of the Royal Society that Newton was occupied with the problem of finding the longitude at sea; but the report having no foundation, he requested Halley to acquaint the members "that he was not about it."[1] Long after this, however, his attention was directed to the invention of an instrument for finding the longitude by the place of the moon; and, in the year 1700, he communicated to Dr. Halley the description of a reflecting sextant, for observing the moon's distance from the fixed stars at sea.[2]

The bill which had been enacted for rewarding the discovery of the longitude, seems to have stimulated the inventive powers of Sir Christopher Wren, then in his eighty-third year. He communicated the results of his study to the Royal Society, as indicated by the following curious document, which I found among the manuscripts of Newton:—

"Sir Christopher Wrenn's Cypher, describing three Instruments proper for discovering the Longitude at Sea, delivered to the Society Novemb. 30, 1714, by Mr. Wren:
OZVCVAYINIXDNCVOCWEDCNMALNABECIRTE WNGRAMHHCCAW.
ZEIYEINOIEBIVTXESCIOCPSDEDMNANHSEFPR PIWHDRAEHHXCIF.
EZKAVEBIMOXRFCSLCEEDHWMGNNIVEOMRE WWERRCSHEPCIP.—Vera Copia,

"EDM. HALLEY."

[1] *Macclesfield Correspondence*, vol. ii. p. 419. [2] See vol. i. p. 209.

We presume that each of these paragraphs of letters is the description of a separate instrument. If it be true that every cypher can be decyphered, these mysterious paragraphs, which their author did not live to expound, may disclose something interesting to science.

Since the first edition of this work was published, the above cypher has been decyphered by Mr. Francis Williams of Grange Court, Chigwell. Each paragraph, according to Mr. Williams, is written backwards. If then the order of the letters be reversed, and every third letter omitted, the sentences will stand thus—

WACH MAGNETIC BALANCE WOUND IN VACUO.
FIX HEAD HIPPES HANDES POISE TUBE ON EYE.
PIPE SCREW MOVING WHEELS FROM BEAKE.

The *omitted* letters, in each of the three sentences, make—

CHR WREN MDCCXIIII or MDCCXIV.

In the first sentence there is a misprint,—a Y for a V.

The three last omitted Z's occur in the first part of each cypher, to show that that part must be taken last.

The three inventions contained in the cypher seem to refer to the 1st, 2d, and 4th methods of finding the longitude, mentioned in page 199.

1. A watch going in vacuo.

2. A method of poising a telescope on shipboard to observe the eclipses of Jupiter's satellites; and

3. Some machinery at the beak of the ship for "keeping an account of the longitude at sea," as proposed by Mr. Ditton.

After the death of Newton, the problem of finding the longitude at sea became a subject of general interest throughout Europe. Various acts relating to it were passed in England. In 1726, our countryman, John Harrison, produced a timepiece of singular accuracy, and, after many trials, in one of which it gave the longitude within 10′ 45″ of the truth, £10,000, half the reward offered in Queen Anne's Act, was adjudged to him;

and the other half promised when an equally good timepiece, upon the same principle, should be made by himself or others. Mr. Kendal, who was appointed by the Board to make such a watch, succeeded so completely, that after it had been round the world with Captain Cook in the years 1772-1775, the second £10,000 was given to Mr. Harrison. In order still farther to encourage inventions for the discovery of the longitude, a new Act was passed in 1774, offering a reward of £5000 for a chronometer or timepiece that would determine the longitude within a degree, or sixty geographical miles ;—of £7000 for determining it within two-thirds of a degree, or forty miles ; and £10,000 for determining it within half a degree, or thirty miles. The very same rewards were offered for any other method by which the same accuracy was obtained ; and a special reward of £5000 was promised to the author of such solar and lunar tables as were sufficiently exact to show the distance of the moon from the sun and stars, within *fifteen* seconds of a degree, "*such tables being constructed entirely upon the principles of gravitation laid down by Sir Isaac Newton*, except with respect to those elements which must necessarily be taken from astronomical observations." In terms of this Act, the widow of Tobias Mayer received £3000 for his lunar tables, and Euler £300 for the theorems on which they were founded.[1]

The Board of Longitude in France, established to promote the same object as the English Board, rewarded Euler for the new tables which he published in 1771, and, during the rest of the eighteenth century, and the first quarter of the nineteenth, these two Boards exerted themselves in the promotion of all those scientific objects which were calculated to improve the instruments and methods for determining the longitude at sea. The French Board, composed of the most distinguished astronomers in France, exists in all its original activity and usefulness ; but, as if we had ceased to be a maritime nation, the British

[1] See vol. i. pp. 305-307.

Board was abolished in 1828,—the only scientific Board in the kingdom which afforded salaries for scientific men.

Such is the official account of the part which Newton took in promoting this important measure, and a more clear and satisfactory testimony than his was never given before a committee of the House of Commons. Mr. Whiston, however, has left behind him an account of what took place in the committee, which has been interpreted by M. Biot in a way very offensive to the friends of Newton. "As soon as the committee was set," says Whiston,[1] "which was a very large one, Newton, Halley, Clarke, and Cotes appeared, a chair was placed for Sir Isaac near the chairman,[2] and I stood at the back of it. What the rest had to say they delivered by word of mouth, but Sir I. Newton delivered what he had to say in a paper. Upon the reading of this paper, the committee were at a loss, as not well understanding its contents, Sir I. Newton sitting still and saying nothing by way of explication. This gave the chairman an opportunity which it was perceived he wanted, of trying to stop the bill; which he did by declaring his own opinion to be, that 'unless Sir I. Newton would say that the method now proposed was likely to be useful for the discovery of the longitude, he was against making a bill in general for a reward for such a discovery,' as Dr. Clarke had particularly proposed to the committee. Upon this opinion of his, not contradicted by any other member of the committee, and upon Sir Isaac Newton's silence all the while, I saw the whole design was in the utmost danger of miscarrying. I thought it therefore absolutely necessary to speak myself, which I did nearly in these words:—'Mr. Chairman, the occasion of the puzzle you are now in, is nothing but Sir I. Newton's caution. He knows the usefulness of the present method near the shores (which are the places of greatest danger).' Whereupon Sir Isaac stood up and said,

[1] Historical Preface to some of the copies of his "Longitude Discovered, Lond. 1738," p. v., dated, as Mr. Edleston conjectures, in 1742.—*Correspondence*, &c., p. lxxvi.

[2] Mr. Clayton, M.P. for Liverpool.

that 'he thought this bill ought to pass, because of the present method's usefulness near the shores.' Which declaration of his was much the same with what he had said in his own paper, but which was not understood by the committee, and determined them unanimously to agree to such a bill." The effect of Newton's opinion upon the committee must have been highly gratifying to himself and his friends; and when he simply paused in repeating orally what he had so distinctly read from his paper, he little thought that a future biographer would ascribe an interval of silence to "puerility of conduct," to "an inexplicable timidity of mind, and to the consequences of a previous mental aberration."[1]

During the dissensions which prevailed in the ministry before the death of Queen Anne, the Tories were desirous of securing for their friends some of the more valuable offices in the patronage of Government. The Mastership of the Mint was one of those which they thought within their reach, and the scheme of releasing Sir Isaac from the labours of his office, and

[1] "Les trois derniers (Halley, Cotes, and Clarke) exprimèrent leur avis verbalement; mais Newton lut le sien, sur un papier écrit qu'il avait apporté, et *qui ne fut compris de personne;* puis il se rassit, et garda obstinément le silence, *quelque instance* qu'on lui fit de s'expliquer plus ouvertement. Enfin Whiston voyant que le bill allait être retiré, prit sur lui de dire que si M. Newton ne voulait pas s'expliquer davantage, c'était par crainte de se compromettre; mais qu'au fond, il trouvait le projet utile: Alors M. Newton répéta presque mot à mot ce qu'avait dit Whiston, et le projet du bill fut accepté. Cette conduite presque puérile, dans une circonstance si solennelle pourrait prêter aux plus étranges conséquences, surtout si on la rapporte au fatal accident que Newton aurait éprouvé en 1695." Biot, *Biog. Univ.* Art. Newton, pp. 192, 193.

Mr. Edleston justly remarks, that "*this is not a model of accurate condensation,*" and he leaves it to the reader, who will, of course, make the requisite allowance for the forwardness and vanity of the reporter, to judge whether M. Biot's term "presque puérile" be a proper epithet to apply to the part that Newton took on the occasion."—*Correspondence,* &c., p. lxxvi., note 167.

A more correct view of Newton's conduct was taken by my distinguished friend the late Professor Rigaud. "What kind of persons," he says, "the committee must have consisted of, that such a plain statement as Newton's should not have been understood by any one of them, I cannot tell. The whole story is evidently tinctured by Whiston's spleen and disappointment."—MS. letter, Oct. 21, 1830. M. Biot is mistaken in saying that the act of 1714 *is still in force.* It was repealed, along with various Longitude Acts, in 1774.

thus giving him more leisure for the prosecution of his studies, seemed to be one that was likely to meet with general approbation. It was resolved, therefore, to offer him a pension of £2000 on the condition that he resigned the Mastership of the Mint, the salary of which was only about half of that sum; and Dr. Swift was commissioned by Bolingbroke, one of the Secretaries of State, to propose the plan to Mrs. Catherine Barton,[1] the particular friend of Swift, and the favourite niece of Newton. The liberality of the offer might have tempted ordinary functionaries, but it met with a very different reception from Newton, who saw at once the character and object of the proposal. When Mrs. Barton communicated to him the message from Bolingbroke, Sir Isaac replied, "My place is at their service, but I will have no pension."[2]

Although the character and talents of Newton had been appreciated at Court during the life of Prince George of Denmark, yet, in the latter years of Queen Anne, the political party to which he belonged had ceased to be in the ascendant. His patron, Lord Halifax, the supporter of every liberal measure in the Upper House, had thus excited the hostility of the Tories, and had rendered himself doubly obnoxious by his attachment to the House of Hanover. When the Act for the naturalization of the Hanoverian family, and the better securing the crown in the Protestant line, had passed, Lord Halifax was selected to carry the act and the insignia of the Order of the Garter to the Electoral Prince; and in 1714 he succeeded in procuring a writ to call the Elector of Hanover as Duke of Cambridge to the House of Peers. These proofs of his devotion endeared him to the House of Hanover; and on the death of the Queen on the 1st of August 1714, and the accession of George the First, he was nominated one of the Lords of the Regency in his Majesty's absence. He discharged this trust with such zeal and fidelity, that he was admitted into the most

[1] The name *Mrs.* was then given to unmarried women

[2] Conduitt's MSS.

secret counsels of the King, appointed First Lord of the Treasury, created Earl of Halifax, and admitted a Knight Companion of the Order of the Garter. From the elevated position to which his friend had now attained, and the ascendency of those opinions which he had never ceased to advocate, Newton naturally became an object of interest at court. His high situation under Government, his European reputation, his spotless character, and, above all, his unaffected piety, attracted the attention of the Princess of Wales, afterwards Queen Consort of George the Second. This lady, who possessed a highly cultivated mind, derived the greatest pleasure from conversing with Newton and corresponding with Leibnitz. In all her difficulties she received from Sir Isaac that information and assistance which she had from other quarters sought in vain; and she had been often heard to declare, that she was fortunate in living at a time when she could enjoy the conversation of so great a genius.

Though a man of robust health and a sound constitution, the Earl of Halifax did not long survive the honours which had been conferred upon him, and Newton had to mourn the loss of his earliest and best friend. When on a visit at the house of Mynheer Duvenvoord, one of the Dutch ambassadors, he was seized with inflammation in the lungs, of which he died on the 19th May 1715, in the 47th year of his age.[1]

This accomplished nobleman, to whom Sir Isaac owed his appointment in the Mint, was distinguished as the patron of literature as well as of science. He was the intimate friend of Addison, Congreve, Prior, Tickell, Steele, and Pope, and as the author of the *Battle of the Boyne*, the *Man of Honour*, and the greater part of the "*Country Mouse* and the *City Mouse*,"

[1] In writing to Sir John Newton of Westly, on the 23d May 1715, four days after the death of Lord Halifax, Sir Isaac Newton says, "I am concerned that I must send an excuse for not waiting upon you before your going into Lincolnshire. The concern I am in for the loss of my Lord Halifax, and the circumstances in which I stand related to his family, will not suffer me to go abroad till his funeral is over."

he was ranked even by Addison among the poets of the day.[1]

> I'm tired with rhyming, and would fain give o'er,
> But justice still demands one labour more—
> The noble Montague remains unnamed,
> For wit, for humour, and for judgment famed.

Like Locke and Bentley, he was very desirous of understanding the *Principia*, and he one day asked Sir Isaac if there was no way of making himself master of his discoveries without learning mathematics. Sir Isaac replied that it was impossible, but Mr. Maine having recommended to his Lordship Mr. Machin, a friend of Newton's, and Professor of Astronomy in Gresham College, as a proper person to give him instructions in mathematics, he presented him with fifty guineas to encourage him. The task, however, proved more difficult than either party had expected, and Machin told Mr. Conduitt that, after trying various schemes, they gave it up in despair.[2]

As a frequent visitor at the house of Sir Isaac, Lord Halifax became acquainted with his niece, Mrs. Catherine Barton, a lady of wit, beauty, and accomplishments.[3] She was the daughter of Robert Barton, Esq. of Brigstock, in Northamptonshire, and Hannah Smith, Newton's half-sister ; and so great an impression had she made upon Lord Halifax, that in a codicil to his will in 1706, he bequeathed to her all the jewels he should have at the time of his death, and three thousand pounds, "as a small token of the great love [4] and affection he had long had

[1] The *Poetical Works* of the late Right Hon. CHARLES EARL OF HALIFAX. London, 1716, 2d edit.

[2] Conduitt's MSS.

[3] Born 1679, married August 26, 1717, died 20th January 1739.

[4] The words *love* and *affection* had not, in Halifax's day, the same meaning which they have now. Swift, for example, writes to Stella that he "*loves* Mrs. Barton better than any one here." Speaking of the Duke of Argyle, he says, "I *love that Duke mightily*. Lady Mountjoy is a little body I *love very well*." Speaking of the pictures of Lady Orkney, Lord Bolingbroke, and Lady Masham, he says, "I shall have the pictures of those I *really love* here." In like manner, Pope writes to H. Cromwell, "I should be glad to tell all the world that I have an *extreme affection and esteem* for you."

for her." Mrs. Catherine Barton was only in her twenty-seventh year, and, under ordinary circumstances, a marriage might have been expected as the result of so ardent an attachment. On the death, however, of his first wife, the Countess of Manchester, Halifax is said to have resolved to lead a single life, though it has been asserted, on the authority of his rival,[1] that he was disappointed in gaining the affections of a lady of great birth and fortune, to whose hand he had aspired. But however this may be, his attachment to Miss Barton continued unabated, and, at his death in 1715, it was found that, by another codicil, dated February 1, 1712, he had greatly increased the bequest which he had made in 1706. He left to Sir Isaac Newton the sum of one hundred pounds, as "a mark of the great honour and esteem he had for so great a man;" and he "bequeathed to his niece, Mrs. Catherine Barton, the sum of five thousand pounds," with "a grant from the Crown, during her life, of the Rangership and Lodge of Bushy Park, with all the household goods and furniture;" and, to enable her to keep the house and garden in good order, he bequeathed his manor of Apscourt, in Surrey. "These gifts and legacies," he adds, "I leave to her as a token of the sincere love, affection, and esteem I have long had for her person, and as a small recompense for the pleasure and happiness I have had in her conversation." He charges also his executor to "transfer to her an annuity of two hundred pounds per annum, purchased in Sir Isaac Newton's name, and which he (Lord Halifax) held in trust for her."

When the contents of this will became known after the death of Halifax, Miss Barton did not escape the censure of the world, though she was regarded by all who knew her as a woman of strict honour and virtue. During his lordship's life, and when a frequent visitor at the house of Newton, his affection for Miss Barton, and his delight in her society, never

[1] The Earl of Shaftesbury. See his *Letters to Robert Molesworth, Esq.* Edit. 1750, lett. iii. pp. 70-72.

once excited the criticism of his contemporaries; and there is not the slightest reason to believe that it exceeded that love and admiration which married men, and men of all ages, ever feel in the presence of physical and intellectual beauty. Halifax was not a libertine, and the very terms of affection in which he accounts for his liberality to Miss Barton are the most satisfactory proof that his love was virtuous and her conduct pure. If there is one hour in man's life more solemn than another, it is that hour when he is preparing for his death.

Venit summa Dies, et ineluctabile fatum,

were the words which Halifax prefixed to the codicil, which evinces his affection and liberality to Miss Barton; and he little thought that the language of the heart, dictated at such an hour, would be regarded as a record of her shame. Nor is it a slight testimony to the purity of his affection for Miss Barton, that he introduces his liberality to her by a legacy to her pious uncle, Sir Isaac Newton, his earliest and best friend, "as a mark of the great honour and esteem he had for so great a man;" and that he records the fact of his holding for her in trust an annuity of two hundred pounds per annum, purchased in Sir Isaac Newton's name.

Although it is stated that Miss Barton did not escape from censure, yet calumny, with her many tongues, does not seem to have left upon record the slightest charge against her character. Flamsteed, who never scrupled to calumniate Newton in language applicable only to the most abandoned of mankind, would have gloated over a charge so destructive of the character of his *friend*. He mentions, however, merely the fact of Lord Halifax's bequest, and he has limited his malice, if he meant it to be malicious, to the simple act of placing the words *excellent conversation* in italics.[1]

[1] Baily's *Flamsteed*, Letter to Sharp, July 9, 1715. He adds, "Sir I. Newton loses his support in him (Halifax), and having been in with Lord Oxford, Bolingbroke, and

The only contemporary document which really bears upon this question, is the following passage in an anonymous Life of the Earl of Halifax, published in 1715.[1]

"I am likewise to account," says the author, "for another omission in the course of this history, which is that of the death of Lord Halifax's lady;[2] upon whose decease his Lordship took a resolution of living single thenceforward, and cast his eye upon the widow of one Colonel Barton, and niece to the famous Sir Isaac Newton, to be superintendent of his domestic affairs. But as this lady was young, beautiful, and gay, so those that were given to censure, passed a judgment upon her which she no ways merited, since she was a woman of strict honour and virtue; and though she might be agreeable to his Lordship in every particular, that noble peer's complaisance to her proceeded wholly from the great esteem he had for her wit and most exquisite understanding,[3] as will appear from what relates to her in his will at the close of these Memoirs."[4]

With the exception of the mistake that the lady was the widow of Colonel Barton, we may admit the truth of the preceding passage. We shall therefore adopt it as the foundation of our argument, and we may admit that it was known to

Dr. Arbuthnot, is not now looked upon as he was formerly," p. 314. See also pp. 73 and 317, where the great intimacy of Newton and Halifax is mentioned.

[1] This Life of Halifax, written by some literary hack of the disreputable house of Curll and Co., cannot be regarded as a work of any authority upon the statements of which we can safely rely. The anonymous author obviously received no information from the family of Halifax, and therefore any fact which he did not derive from public documents, must be considered as resting upon vulgar rumour. The author himself says, in his Dedication to George Earl of Halifax, that "he is sensible that he has been guilty of many omissions through want of intelligence from persons who might have obliged him with proper information." In a copy of the first edition of the Life of Halifax, in the University Library of Cambridge, the author is said to be William Pittis.

[2] The Countess Dowager of Manchester, whom Charles Montague married "some time before the Revolution in 1688."—*Life of Halifax*, p. 3.

[3] Oldisworth, in "The British Court," says—

"Give Cowper wit, still Barton will have sense."

[4] *Life of Halifax*, pp. 195, 196, 2d edit. Lond. 1716.

Newton and his friends. After the death of Halifax, Miss Barton continued to reside, as she always did, in so far as there is any evidence on the subject, with her uncle Sir Isaac. He gave splendid entertainments at his house in Martin Street, where the most distinguished foreigners were occasionally assembled, and where doubtless the best company in London was to be found. Miss Barton presided at her uncle's table, and by her "excellent conversation," excited the love and affection even of some of her married friends. M. Montmort, a married man and a distinguished mathematician, had heard of her wit and beauty before he had visited England, and after he had met with her as a friend of Sir Isaac's, his admiration knew no bounds. In a letter addressed to Montmort by the celebrated Brook Taylor, another of Newton's friends, Miss Barton had sent her compliments to him, and he is thus led to express in the warmest terms, compared with which those of Halifax are cold, the great admiration with which Miss Barton had inspired him.[1]

Among the other admirers of Miss Barton, we must mention Dean Swift, who frequently visited her, and on one occasion "at her lodgings," that is, we presume, in the house of Sir Isaac Newton, with whom, as Mr. Conduitt distinctly tells us, "no other person ever lived." Thus loved and admired by politicians, wits, and philosophers, she remained in Newton's house till the 24th of August 1717, when she married John Conduitt, Esq.,[2] M.P., of Cranbury in Hampshire, a gentleman of independent circumstances, and much esteemed by Sir Isaac. The result of this marriage was an only daughter, Catherine Conduitt, who was born in 1718, and who was married in 1740 to the Honourable John Wallop, afterwards Lord Viscount Lymington. She died in 1750, at the early age of thirty-two, leaving one daughter and four sons, from the eldest of whom the Portsmouth family are descended.

During the century and a half which has passed away since

[1] See Appendix, No. XVII.

[2] Born 1688; died May 20, 1737, æt. 49.

the death of Halifax, no stain has been cast on the memory of Mrs. Conduitt, and the very writer whose ambiguous words have been misinterpreted to her injury, has himself declared that *she was a woman of strict honour and virtue*, and that *the complaisance to her of the noble peer proceeded wholly from the great esteem he had for her wit and most exquisite understanding.*[1] On such authority the biographers of Newton, while they recounted with pride the beauty and accomplishments of his niece, could not but feel another interest in one who had been the ornament of his domestic circle, and the solace of his declining years. They did not attempt to conceal the warmth of Halifax's attachment to her, or omit to record the liberality with which it was marked; but they never imagined that the affection breathed in the solemn pages of a will would be viewed as the expression of unhallowed love, and that a bequest to a female friend would be regarded as the wages of iniquity.

As every event in Newton's life, and every topic with which his name is associated, possess the deepest interest, it is desirable that those which affect the character of so great a man should be examined and discussed when it is possible to find materials by which we may explain what is ambiguous, or refute what is false. Viewed in this light, we are disposed to welcome the discussion which has lately been raised by Mr. De Morgan in reference to the nature of the attachment which subsisted between Miss Barton and the Earl of Halifax.[2]

[1] The sneer of Voltaire in ascribing Newton's promotion to the Mint to the beauty of his niece, scarcely deserves our notice. Miss Barton was only sixteen when he received the appointment, and Montague could not then have seen her. Voltaire, however, makes no insinuation against the character of Miss Barton. "J'avais cru, dans ma jeunesse," says he, "que Newton avait fait sa fortune par son extrême mérite. Je m'étais imaginé que la cour, et la ville de Londres l'avait nommé par acclamation grand maître des monnaies du royaume. Point du tout. Isaac Newton avait une nièce assez aimable nommé Madame Conduitt, elle plut beaucoup au grand Trésorier Halifax. Le calcul infinitésimal et le gravitation ne lui auraient servi de rien sans une jolie nièce."—*Dict. Philos.* tom. iv. p. 61.

[2] This discussion will be found under the title of *Lord Halifax and Mrs. Catherine Barton*, in *Notes and Queries*, No. 210, November 5, 1853, pp. 429, 433, in an elaborate article marked by the usual acuteness of that distinguished writer.

Assuming it as proved, by the single testimony of the biographer of Halifax, that Miss Barton "*was received by Montague into his house* as superintendent of his domestic affairs," and that she left the house of her uncle Sir Isaac Newton to cohabit with that nobleman, and believing it *to be impossible that Newton could be ignorant that his niece was regarded by the world as the mistress of his friend and political patron*, Mr. De Morgan "takes it to be established that she was *either the wife or the mistress of Halifax*;" and on various grounds, which it is unnecessary to repeat, he prefers the alternative of a private marriage. In coming to this conclusion, the most favourable certainly to Newton's reputation, Mr. De Morgan finds it difficult to explain why the marriage was concealed in the lifetime of Halifax. He ascribes it to the inferior station of Miss Barton as the *grand-daughter* of a country clergyman, which would have given the marriage the character of a *mésalliance*, from which Halifax would have been weak enough to shrink. In opposition to this estimate of Mrs. Barton's social position, we have to state that the "Bartons of Brigstock possessed estates in Northamptonshire for several hundred years, and were *nearly related* to the Earl of Rockingham, Lord Griffin, Sir Jeffrey Palmer, and other honourable families in that neighbourhood."[1] But Mr. De Morgan finds it a still greater difficulty, and entirely fails in surmounting it, to explain how there was no record of the marriage, and what could induce Sir Isaac, Mrs. Conduitt, and her husband, to conceal it, after Halifax's death, and thus to leave it as the most probable conclusion, that the niece of the one and the wife of the other had been the mistress, instead of the wife of Halifax. If there was no marriage, some kind friend might have propagated a rumour that there was; but no such rumour was ever heard, and no

[1] Conduitt's MSS. I find it stated in the handwriting of Mrs. Catherine Barton, upon the back of a drawing of the arms of the Swinfords of Stamford, that "the Bartons were descended from the Swinfords," from Catherine Swinford, the wife of Sir Hugh Swinford, who became the mistress of John of Gaunt.

attempt has ever been made to obtain such a solution of this mysterious connexion. To infer a marriage, when the parties themselves have never acknowledged it—when no trace of a record can be found—and when no friend or relation has ever attempted even to make it the subject of conjecture, is to violate every principle of sound reasoning ; and we are disposed to think that Mr. De Morgan's respect for the memory of Newton has led him to what he regards as the only conclusion which is compatible with the character of a man so great and pure.

In denying the marriage, we do not admit one of the grounds upon which it has been maintained. We deny that Miss Barton ever lived a single night under the roof of Lord Halifax. His biographer makes no such statement. The passage which has given rise to the discussion contains three distinct propositions,—

1. That Halifax had resolved never to marry.

2. That he cast his eye upon Miss Barton to be the superintendent of his domestic affairs ; and,

3. That she was a woman of strict honour and virtue.

The first of these propositions overturns the theory of a marriage ; and the second merely proves a *plan or a wish* on the part of Halifax that Miss Barton should superintend his household,—a wish, too, which was never expressed amid the gossip of contemporary correspondence, or in the hearing of any witness. It rests, indeed, upon no other evidence than that of the anonymous biographer. Where, then, is the proof, or even its shadow, that Miss Barton occupied such a situation, or was ever once seen seated at Halifax's table ? In 1710, Swift visited Miss Barton frequently, and once "at her lodgings." He dined with Halifax on the 28th November, and with Miss Barton on the 30th ; and though he mentions this fact to Stella, he never alludes to any connexion whatever between his two friends.[1] But independent of these facts, there is no evidence

[1] Swift's great admiration of Miss Barton, notwithstanding her Whig politics, is no slight proof of the purity of her social position. I have placed in APPENDIX, No. XVIII.,

whatever that Miss Barton ever slept out of her uncle's house; and we are distinctly told by Mr. Conduitt, that "nobody ever lived with Sir Isaac but his wife, who was with him near twenty years, before and after her marriage." It is not known at what time Miss Barton took up her residence with her uncle, or during what periods she may have been absent before and after her marriage, either on visits to her friends in the one case, or when living with her husband in the other;[1] but whatever may be its amount, its addition to the twenty years of her residence with Newton, before and after her marriage, will not allow us to assign any period during which, under Halifax's roof, that love and affection which, previous to 1706, he *had long had for her*, could have been developed.

Mr. De Morgan lays great stress upon the admitted fact, that the statement in the "Life of Halifax" was "left uncontradicted by herself (Mrs. Conduitt),—by her husband,—by her daughter,—by Lord Lymington, her son-in-law,—and by the uncle (Sir Isaac Newton) who had stood to her in the place of a father. It is impossible," he adds, "that Newton could have been ignorant that his niece *was living in Montague's House*,—enjoyed an annuity[2] bought in his own name,—and was re-

a letter from Mrs. Conduitt to himself, and all the passages in which she and Halifax are mentioned in his journal to Stella.

[1] I find letters addressed to Mr. Conduitt at Cranbury, his country house in Hampshire, where it is probable he and his family frequently resided, when he was not attending his duty in the House of Commons. During Newton's illness in 1726, Dr. Mead addressed several letters to him "at his house near Winchester." Miss Barton, as we have already seen (p. 158), was boarded in Oxfordshire, where she had an attack of the small-pox, in August 1700. There is no evidence that she lived with Newton before this date, and we have not been able to determine at what time she took up her residence under his roof. If we suppose it to have been in 1701, we obtain sixteen years as the period of her residence in Newton's house before her marriage, and four years for her residence with him after her marriage in 1717—the other six years having been spent with her husband.

[2] Mr. De Morgan says, that Halifax bought this annuity for Miss Barton in Newton's name; but this is a conjecture, and not a fact; and we consider it quite certain, from a fair interpretation of the words, that Newton purchased this annuity, and, being nearly twenty years older than Halifax, made him the trustee. He is simply the trustee, and not the granter of the annuity. Had he granted the annuity, he would have mentioned

garded by the world as the mistress of his friend and political patron." Now, the very fact that such respectable parties, so deeply interested in the character of their accomplished relative, contradicted neither the fact, if it was a fact, nor the rumour, if it was a rumour, is a proof that there was neither fact nor rumour to contradict. How could any person contradict the *cast of an eye*,—the only act ascribed to Halifax by his biographer? How could they contradict the statement, made only in 1853, that Miss Barton lived in Montague's House, when no person in their own lifetime ever made such a

it as one of the "gifts and legacies" which he left her. An annuity purchased in Sir Isaac Newton's name can mean nothing else than an annuity purchased by Sir Isaac Newton. I find among Newton's papers a scroll of the beginning of the act of transference from the executor, George Lord Halifax, in which the date of the trust is stated to be October 26, 1706. Mr. De Morgan remarks, that if "the annuity had been bought by Newton, Conduitt would have mentioned it in his list of the benefactions which Newton's relatives received from him." But the annuity was not a benefaction like those contained in Conduitt's list. It was virtually a debt due to his favourite niece whom he had educated, and who had for twenty years kept his house; and if she had not received it from Sir Isaac, his conduct would have been very unjust, as, owing to his not having made a will, she got only the eighth part of his personal estate along with his four nephews and nieces. Mr. De Morgan makes other statements which it is necessary to examine. After mentioning the important fact, that though "Swift writes to Stella of every kind of small talk, he never mentions Halifax and Miss Barton together, —never makes the slightest allusion to either in connexion with the other, though in one and the same letter he minutes his having dined with Halifax on the 28th, and with Miss Barton on the 30th (September 1710)," he adds, "*there must have been intentional suppression in this.*" Certainly, if Swift knew or believed that Miss Barton lived with Halifax; but the true inference is, that she not only did not live with him, but that it was never even reported that she did. Mr. De Morgan, however, adds, "*All the world knew that there was some liaison between the two.*" On the contrary, we maintain that not one person in the world knew this, or could know it, in 1710. There is not a single fact to prove that the codicil of 1706 was known to any individual. Mr. De Morgan goes on to say, as if in proof of "intentional suppression," for which we can see no motive, that when Swift (November 20, 1711) records his having been "teased with Whiggish discourse" by Miss Barton, "*he does not even drop a sarcasm about her politics having been learnt from Halifax.*" Why make Miss Barton the political pupil of Halifax, seeing that her own uncle, Sir Isaac Newton, with whom she had spent the greater part of her life, and from beneath whose roof she never strayed, was one of the most decided Whigs of the day? This Whig conversation took place in the house of Lady Betty Germain, from which "it appears," as Mr. De Morgan has justly observed, "that she (Miss Barton) was regarded as a respectable woman,"—a fact of which there are abundant indications.

statement? How could they express their indignation at the charge, that she was the mistress of Halifax, when calumny had never breathed that she was, and when the very biographer, whose words are in every other respect admitted as true, declares that "she was a woman of strict honour and virtue?" However different may have been the state of public morals in the reign of George I., it would require substantial evidence to prove that the Earl of Halifax, the First Minister of the Crown, and a great favourite of the royal family, was (unknown to any contemporary writer) living in open concubinage with Miss Barton,—one of the beauties and toasts of the day,[1]—the friend of Swift and Lady Betty Germain, and the accomplished and favourite niece of Sir Isaac Newton,—himself the religious instructor of the Princess of Wales,—the personal friend of the dignitaries of the Church,—and a man universally esteemed for his piety and virtue.

[1] "In a poem called the Toasters, where all the distinguished beauties at that time are celebrated in distinct epigrams, these two appear in honour of Miss Barton:—

Stampt with her reigning charms this brittle glass
Will safely through the realms of Bacchus pass;
Full fraught with beauty, will new flames impart,
And mount her shining image in the heart.

Another—

Beauty and Wit strive each in vain,
To vanquish Bacchus and his train;
But Barton, with successful charms,
From both their quivers drew her arms,
The roving god his sway resigns,
And cheerfully submits his vines."

Art. MONTAGUE, *Biographia Britannica*, vol. v. p. 3156, note.

CHAPTER XXII.

Leibnitz attacks Newton's Philosophy—Newton's Reply—Leibnitz attacks the English Philosophy as irreligious, in Letters to the Princess of Wales—The King requests Newton to defend himself—He claims the Invention of Fluxions—Dr. Clarke defends the English Philosophy—The Dispute carried on through the Princess of Wales—Insincerity of Leibnitz—His Death—His Eloge by Fontenelle, who apologizes to Chamberlayne for a mistake adverse to Newton—Newton's Observations on the Eloge—Varignon reconciles Newton and John Bernoulli—Newton's Correspondence with Varignon, whose views are favourable to Leibnitz—Newton asks Varignon's Opinion on the Commercium—His Criticisms upon it—His Death—Correspondence between Newton and John Bernoulli—Montmort's views on the Fluxionary Controversy—Nicolas Bernoulli's Letter to Newton—Letters of Dr. Smith, Dr. Derham, and Fontenelle, referred to.

Before Newton had taken an open part in the fluxionary controversy, and before the publication even of the *Commercium Epistolicum*, Leibnitz had begun to challenge the soundness of the Newtonian philosophy, and to excite against it the prejudices of continental philosophers. In his *Théodicée*, published in 1710,[1] he attacks the theory of gravity, and accuses Newton of introducing occult qualities and miracles into philosophy; and, in a controversy which he had in 1711 with Hartsoeker,[2] who maintained that all things arose from certain atoms floating in a perfect fluid without cohesion, he took occasion to renew his attack upon the English philosophy. In this dispute with the Dutch physician, the name of Newton is not mentioned by Leibnitz, but he was so obviously the person whose opinions were assailed, that he addressed a very able reply to the editor,

[1] *Essais de Théodicée sur la Bonté de Dieu, la Liberté de l'Homme, et l'Origine du Mal.*
[2] *Journal de Trevoux*, May 1712.

in which neither his own name nor any of his writings are referred to. "In your weekly paper,"[1] he says, "dated May 5, 1712, I meet with two letters, one written by Mr. Leibnitz to Mr. Hartsoeker, the other by Mr. Hartsoeker to Mr. Leibnitz, in answer to the former. And in the letter of Mr. Leibnitz, meeting with some things reflecting upon the English, I hope you will do them the justice to publish this vindication as you have printed the reflection." He then proceeds to show that the theory of gravity is "proved by mathematical demonstration, grounded upon experiments and the phenomena of nature; and that to understand the motions of the planets under the influence of gravity, without knowing the cause of gravity, is as good a progress in philosophy as to understand the frame of a clock, and the dependence of the wheels upon one another, without knowing the cause of the gravity of the weight which moves the machine, is in the philosophy of clockwork; or the understanding the frame of the bones and muscles, and their connexion in the body of an animal, and how the bones are moved by the contracting or dilating of the muscles, without knowing how the muscles are contracted and dilated by the power of the mind, is in the philosophy of animal motion."[2]

The pertinacity with which Leibnitz reiterated his attacks upon the doctrine of gravity, has no parallel in the history of science, and it is difficult to believe that the love of truth was the only motive by which he was actuated. We have already seen[3] how he indulged in the same criticisms in the postscript of his letter to the Abbé Conti in November 1715, and we shall now find him in the climax of his hostility to Newton, when in the very same month he endeavoured to misrepresent and malign his philosophy, in his correspondence with the Princess of Wales. He had no doubt learned from her Royal

[1] *Memoirs of Literature*, No. XVIII. p. 137. See Cotes' letter to Newton in Edleston's *Correspondence*, &c., p. 153.

[2] The scroll of this letter, which occupies two folio pages, has no date. It does not appear in the *Memoirs of Literature* for which it was written.

[3] Vol. ii. p 22.

Highness the regard which she entertained for Newton, and the pleasure and instruction which she derived from his conversation; and, under such circumstances, it might have been expected that a man of high principle would have kept in subordination his feelings as a rival, without abjuring his opinions as a philosopher. He might have taught the Princess his doctrine of pre-established harmony as incompatible with Newton's opinions respecting certain irregularities in the planetary system, or he might have whispered into the royal ear that gravity was an occult quality, and a miracle; but when he represented the Newtonian philosophy and the opinions of Locke as subversive of natural, and inferentially of revealed religion, he yielded to an ignoble impulse, and did violence to the dignity of philosophy.

In a letter which Leibnitz addressed to the Princess in the month of November 1715, the following charges were made against the English:—

"1. *Natural religion itself* seems to decay [in England[1]] very much. Many will have human *souls* to be material; others make *God himself* a corporeal being.

"2. *Mr. Locke* and his followers are *uncertain* at least whether the *soul* be not *material* and naturally perishable.

"3. *Sir Isaac Newton* says that *space* is an *organ*, which God makes use of to perceive things by;—it will follow that they do not depend altogether upon Him, nor were produced by Him.

"4. Sir *Isaac Newton* and his followers have also a very odd opinion concerning the Work of God. According to their doctrine, God Almighty wants to *wind up* his watch from time to time, otherwise it would cease to move. He had not, it seems, sufficient foresight to make it a perpetual motion. Nay, the machine of God's making is so imperfect according to these

[1] The words *in England* are not in the original paragraph, but they were added either by the Princess or Dr. Clarke, and, as we shall presently see, were meant to be understood by Leibnitz himself.

gentlemen, that he is obliged to clean it now and then by an extraordinary concourse, and even to *mend* it as a clockmaker mends his work; who must consequently be so much the more unskilful a workman, as he is oftener obliged to mend his work, and to set it right. According to *my* opinion the same *force* and vigour remains always in the world, and only passes from one part of matter to another, agreeably to the laws of nature and the beautiful *pre-established* order. And I hold that when God works miracles, he does not do it in order to supply the wants of nature, but those of grace. Whoever thinks otherwise, must needs have a very mean notion of the wisdom and power of God."

These views of Leibnitz having become the subject of conversation at court, where Newton and Locke were in high esteem, the king, who never seems to have had much affection for his countryman, expressed a wish that Sir Isaac Newton would draw up a reply in defence of his philosophy, as well as of his claim to be the original inventor of Fluxions. It was accordingly arranged that Newton should undertake the mathematical part of the controversy, while Dr. Clarke was intrusted with the defence of the English philosophy. The Princess of Wales, therefore, communicated to the Dr. the preceding extracts from Leibnitz's letter, and Dr. Clarke's reply was transmitted to Leibnitz through her Royal Highness. Leibnitz replied to this communication; and after Dr. Clarke had returned his fifth answer to the fifth paper of Leibnitz, the death of the latter, on the 14th November 1716, put an end to the controversy.[1]

While this dispute was going on, Leibnitz sent an account of it to John Bernoulli, in a letter dated June 7, 1716. After abusing Brook Taylor's Method of Increments in language which

[1] All these papers, which passed through the hands of the Princess, were published at Amsterdam in 1720, under the title of *Recueil de Diverses Pièces sur la Philosophie, la Religion Naturelle, l'Histoire, les Mathématiques*, &c., par Messrs. LEIBNIZ, CLARKE, NEWTON, &c. They were published also in French and English in 1738 in Dr. Clarke's *Works*, vol. iv. pp. 580-710.

the editor has struck out, he tells his correspondent that he is engaged in a philosophical dispute with Newton, or rather with his defender Clarke,—that he had written to the Princess of Wales, who took an interest in such subjects,—"that philosophy or rather natural religion, was degenerating among the English," —that the Princess had transmitted extracts of his letter to Clarke,—that her Royal Highness had sent him his answer, and that he had replied four times to the communications of his opponent. He tells him that *space* is now the *idol* of Englishmen; and "that whatever is inexplicable from the nature of things, such as the Newtonian general attraction of matter, and other things of the same kind, is either miraculous or absurd." He expects that the contest, from which everything offensive is excluded, will be continued, and he concludes the paragraph with the following singular sentiment, the conclusion of which may be inferred from its being struck out by the editor:[1]—

"Hujusmodi enim collationes mihi *ludus jocusque* sunt,[2] quia in philosophia,

[1] We hope that those who possess the originals of the *Commercium Epistolicum* of Leibnitz and Bernoulli, will supply the *numerous elisions* which the editor had not the courage to insert, as they would throw much light on the temper with which the Fluxionary controversy was carried on by these eminent mathematicians. No such eliminations have been made in the letters of Newton or his friends.

[2] It has been supposed by many persons that the *Théodicée* of Leibnitz, which was written for the information of the Queen of Prussia, with the view of counteracting the sceptical opinions of Bayle, did not express his own sentiments, and that Leibnitz really believed the doctrines which he impugned. Professor Pfaff of Tübingen, whose opinion of the *Théodicée* Leibnitz had requested, thus replied to him:—"It seems to me that you have invented that theological system *only in jest*, while at the bottom you receive the doctrines of Bayle; but it is necessary that some one give the dangerous principles of Bayle *a serious and thorough refutation.*" To this letter Leibnitz answered, "You are right, venerable sir, in what you say respecting the *Theodicea.* You have hit the nail on the head; and I wonder that no one before has taken this view of my intentions, for it is not the business of philosophers always to treat of subjects *seriously;* they who, as you correctly observe, so tax the powers of their mind in the invention of hypotheses. You who are a theologian, will pursue the theological course in the refutation of errors." This letter was, of course, understood in its natural meaning; but the biographer of Leibnitz, Dr. Gurhauer, maintains it to be an *ironical answer to the presumptuous Professor!* We do not venture to say, though he has himself said it, that Leibnitz's real opinions were not expressed in his *Théodicée*, and in his letter to the Princess of Wales,

Omnia precepi atque animo mecum ante peregi."[1]

* * * * * *

It is very obvious from the notes on Dr. Clarke's replies to Leibnitz, that he had received assistance on several astronomical points from Newton himself.[2] Sir Isaac's attention indeed had been called to the subject by the postscript to Conti's letter, and in his reply to it, on the 26th February 1716, he devotes a page to a defence of his views and a criticism upon those of his rival. Satisfied no doubt with the ample discussion which the subject was undergoing between Clarke and Leibnitz, he takes no notice of this portion of Leibnitz's rejoinder[3] in his celebrated "Remarks,"[4] which were written in May 1716. On a subsequent occasion, when M. Des Maizeaux requested from him some new observations on the subject, he declined to renew the discussion, and assigned the following reason for his silence :—

"You know," he says, "that when Mr. L'Abbé Conti had received a letter from Mr. Leibnitz with a large postscript against me, full of accusations foreign to the question, and the postscript was showed to the King, and I was pressed for an answer, to be also showed to his Majesty,[5] and the same was afterwards sent to Mr. Leibnitz ;[6] he sent it with his answer to Paris, declining to make good his charge, and pretending that I

but we call the attention of the reader to the *ludus et jocus*, with which our metaphysical gladiator carried on his contest with Dr. Clarke, and pointed out the decay of natural religion in England.

[1] *Comm. Epistol. Leibnitii et Bernoullii*, tom. ii. pp. 381, 382.

[2] I have found, among Sir Isaac's papers, many folio pages of manuscript containing the same views as those given by Dr. Clarke.

[3] Letter to Conti, April 14, 1716.

[4] Raphson's *Fluxions*, p. 111.

[5] "By the contrivance of some of the court of Hanover, I was prevailed with to write an answer to the postscript of a letter written by Mr. Leibnitz to Mr. L'Abbé Conti, that both might be shewed to the King. I did it with reluctancy; and by the letters which Mr. Leibnitz thereupon wrote to several at court, I found that he was at the bottom of the design. It is now about forty years since I left off all correspondence by letters about mathematics and philosophy, and therefore I say nothing farther to you about these matters."—*Scroll of a letter to the Abbé Varignon in* 1718.

[6] This was Newton's letter to Conti of the 26th February 1716.

was the aggressor, and saying that he sent those letters to Paris that he might have neutral and intelligent witnesses of what passed between us. I looked upon this as an indirect practice, and forbore writing an answer in the form of a letter to be sent to him, and only wrote some observations[1] on his letter to satisfy my friends here that it was easy to have answered him had I thought fit to let him go on with his politicks. As soon as I heard that he was dead, I caused the letters and observations to be printed, lest they should at any time come abroad imperfectly in France. You are now upon a design of reprinting them with some other letters written at the same time, whose originals have been left in your hands for that purpose by Mr. L'Abbé Conti, for making that controversy complete, and I see no necessity of adding anything more to what has been said, especially now Mr. Leibnitz is dead."[2]

After the death of Leibnitz, the fluxionary controversy was almost in abeyance. The attention of mathematicians, however, was again called to the subject by the Eloge of Leibnitz, from the pen of Fontenelle, which was published in the Memoirs of the Academy of Sciences for 1716, and by another Eloge which appeared in the *Acta Eruditorum* for 1717. The friends of Newton were not pleased with the observations of Fontenelle, and Mr. Chamberlayne, who had previously interfered between the rival analysts, did not scruple to complain of them in his "Lives of the French Philosophers." Fontenelle received this criticism with his usual urbanity, and wrote the following note to Mr. Chamberlayne :—"You complain of me, but after so civil a manner, that I think myself obliged to return you an

[1] Published in Raphson's *Fluxions*, p. 111.

[2] In this scroll, of which there is a duplicate, another page is added, giving the usual history of his discovery of fluxions. In the duplicate, apparently the first written, there is added after the word *dead,* "For I have always industriously avoided disputes. If any thing more were to be added, it should be what follows the following declaration." The pen is drawn through this last sentence, and the declaration is not mentioned. This paper was probably drawn up for the use of M. Des Maizeaux, in writing his preface to his *Recueil,* &c., which contains a good account of the Fluxionary dispute. The Preface is dated October 27, 1719.

answer. I confess to you sincerely that till we had seen the *Commercium Epistolicum*, it was commonly believed here that Leibnitz was the first inventor of the Differential Calculus, or at least the first publisher of it, though it was as well known that Sir Isaac Newton was master of the secret at the same time; but as he did not challenge it, we could not be undeceived, and what I said concerning it was upon the credit of the common belief, which I did not find contradicted. But since it is so now, I promise you I will change my language whenever there is an opportunity, for I do assure you that it has been my study all my lifetime, to keep myself free from any partiality, whether national or personal, nothing being my concern but truth."[1]

When Newton himself perused the Eloge of Leibnitz, which was sent to him by Varignon, he did not scruple to express his dissatisfaction with it. In thanking Varignon for his "kind present of the Elogia of the Academicians," he says, "in that of Mr. Leibnitz Mr. Fontenelle has been very candid. There are some mistakes in matter of fact, but not by design. I rèckon that Mr. Fontenelle was not sufficiently informed." He then proceeds to point out the mistakes to which he refers, criticising at the same time the Elogium of Leibnitz in the *Acta Eruditorum*, and repeating many of the leading facts which we have already given in the history of the controversy.[2] As no notice is taken of these criticisms in the letters of Varignon, it is probable that they were never sent to him, and this is the more likely, as I have found three copies of a more elaborate paper entitled *Historical Annotations on the Elogium of Mr. Leibnitz*, which, in so far as I have been able to ascertain, has not been published.[3]

1 This extract from Fontenelle's letter, dated February 5, 1717, is in Mrs. Barton's handwriting, and seems to have been sent by Chamberlayne to Newton.

2 This scroll occupies nearly two closely written folio pages, and one part of it is almost obliterated with alterations.

3 These annotations occupy about ten closely written folio pages.

But though the leaders in this controversy had ceased to take a public or active part in it, yet some of them looked back with uneasiness to the part which they had played. John Bernoulli, who had been dragged into it by the importunities of Leibnitz, and whose character had been compromised by the disclosure of secrets which ought to have been concealed, was anxious for a reconciliation with Newton; and the Abbé Varignon, to whom he had communicated his desire, succeeded in the task. We have already [1] given a brief account of the correspondence which took place on this occasion, in so far as it forms a part of the fluxionary controversy. There are, however other points of interest in these letters which throw some light on the personal character of their authors, and we have therefore given the most interesting of them in the Appendix.[2] The letters of Varignon relate chiefly to the French translation of Newton's *Optics*, by M. Coste, the publication of which had been delayed by the improper conduct of the bookseller who undertook it; while those of Newton, which we possess only in scrolls, are occupied with details respecting his controversy with Leibnitz, and his nominal reconciliation with Bernoulli. That Newton never forgave Bernoulli is very distinctly shown in the following paragraph of a letter to Varignon:[3]—

"Demoivre told me that Bernoulli wished to have my picture; but he has not yet acknowledged publicly that I possessed the method of fluxions and moments in 1672, as is confessed in the Eloge on Leibnitz, published in the History of your Academy. He has not yet acknowledged that I gave, in the first proposition of the Book of Quadratures, published in 1693 by Wallis, and in 1686 in Lem. 2, book ii. of the *Principia*, synthetically demonstrated, the true rule for differentiating differences; and that I had in the year 1672 the rule for determining the curvature of curves. He has not yet acknowledged that, in the year 1669, when I wrote the Analysis by series, I

[1] Pages 32-36. [2] See APPENDIX, No. XIX.
[3] Sept. 26, 1721, O. S.

had a method of accurately squaring curve lines when it could be done, and which is explained in my letter to Oldenburg, dated 24th October 1676, and in the Fifth Prop. of the Book of Quadratures ; and also that Tables of Curvatures, which could be compared with the Conic Sections, were composed by me at that time. If these things were admitted, it would put an end to all disputes, and I could not then easily refuse him my picture."

In replying to this letter, Varignon says,[1] "I sent to Bernoulli, on the 21st October, the portions of your letter relating to his complaints, with the addition that you prohibited me from publishing them ; but I took no notice of the conditions which you considered necessary before you granted him your picture, lest they should have annoyed him. In order, however, that you might still appear friendly to him, I informed him that three copies of your *Optics*, now in the press, were destined for himself, his son, and his nephew ; and, indeed, in his last letter from Basle of the 22d November, he desires me to present to you, in his name and theirs, their best thanks for the many gifts you intend for them. But the answer which he has made to the parts which I transcribed from your last letter, I dare not communicate to you. I have deemed it better to transcribe it for Demoivre, who will tell it to you, that you may say to him what perhaps you would not wish to write."

From the high character of Varignon, both as a mathematician and an individual, Newton and his rivals were equally anxious to obtain his judgment in their favour. Leibnitz had expressed to Bernoulli his great anxiety that Varignon would do nothing in France that would be injurious to his cause ;[2] and Bernoulli, in his reply,[3] sends him an extract from a letter of

[1] Dated December 9, 1721.

[2] Spero dominum Varignonium curaturum, te presertim hortatore, ne quid in Gallia fiat de quo queri possim. Aug. 19, 1713, *Com. Epist. Leib. et Bern.* tom. ii. p. 321.

[3] September 9, 1713.

Varignon's, in which, while he concedes to Newton an early knowledge of the doctrine of infinitesimals, he gives to Leibnitz the discovery of the differential calculus. Varignon has nowhere given an opposite opinion in his letters to Newton, though he could scarcely have avoided it had any favourable impression been made upon his mind by the information communicated to him, and by his subsequent perusal of the second edition of the *Commercium Epistolicum*, and the *Recensio*. Previous to the publication of this edition, Newton sent him a copy of the second, and, in the following letter, requested his opinion of the preface, and of the annotation at the end of the work.[1]

"To the celebrated M. Abbé Varignon,
Prof. of Mathematics in the College Mazarin.

"REVEREND SIR,—I send you a copy of the *Commercium Epistolicum*, reprinted here along with the account of it turned into Latin, and the Judgment of the 'Primary Mathematician.' All these were printed long before the death of Leibnitz, but this *Commercium* has not yet been offered for sale in the booksellers' shops. A preface to the reader is prefixed, and an annotation of which two parts are new, but taken from ancient writings. I earnestly entreat you to read these two, and if you find anything said which ought not to be said, or anything which ought to be said otherwise, and mention it to me, I will take care that it shall be corrected, if necessary, before the book is published. The object of the book is, not that disputes may be revived, but that questions may be rejected which have nothing to do with the subject, and that what has been said respecting the first inventor of the method, either of fluxions, or moments, or differences, may be handed down to posterity, and quietly referred to their judgment. I am getting well slowly, and hope that I shall soon enjoy my usual health.—Farewell, IS. NEWTON."

[1] Varignon had lost his copy of the first edition, from having lent it to a friend. The date of Newton's letter must have been in June or July 1722.

In replying to this letter, the Abbé wisely avoids the question at issue between his friends, and contents himself with making the following observations on the two points to which Newton had called his attention :—" I lately requested M. Demoivre to mention to you that I had some hesitation about two places in the first preface to the reader, which you begged me to consider, along with the notes on the anonymous epistle, dated 7th June 1713, in which I see nothing calculated to give offence. But it is otherwise in the above-mentioned preface, for, in page 5, there are two things which I think may be offensive." These two criticisms, which we have given in a note, though very trivial, were attended to in the edition of 1725.[1] This letter, which announced to Newton that the French translation of his *Optics* was completed on the last day of July 1722, was the last which he received from Varignon, who died a few months afterwards, on the 23d December 1722.[2]

In a preceding chapter we have mentioned the short corre-

[1] "1°. Lin. 12, 13. Legitur *Jam velo sublato, ut militem in hac rixa pro se inducere:* Mallem simpliciter *Jam in hac rixa pro se inducere,* ne quis sub illo *velo* prius *latitantem* putet Dum. Bernoullium, cui Leibnitius epistolam predictam ascripsit. Adde quod *ut Militem* vilior est denominatio quam ut eundem Dum. Bernoullium non offendat.

"2°. Ibidem, Lin. 29. Legitur de Do. Des Maizeaux *et in lucem edidit:* Mallem *et me non consulto in lucem edidit* ut nimirum hæc loquendi ratio concilietur cum Epistola quam ad Dum. Bernoullium, tua cum venia nuper scripsi.

"Hæc sunt quæ te lubente notavi in prædicti libri Præfatione prima ad Lectorem. At in notis ad Epistolam sine nomine datam die 7 Junii 1713, nihil mihi visum est quod sic paci noxium esse possit, ut Jam dixi." This letter is dated Paris, 4 Aug. 1722.

[2] Pierre Varignon was born at Caen in 1654. In 1687 he published his *Projet d'une Nouvelle Méchanique,* in consequence of which he was elected a Member of the Academy of Sciences, and appointed Professor of Mathematics in the College Mazarin. Though of a robust constitution, his habits of severe study made such an impression upon it, that, in 1705, his life was for six months in great danger, and during the three following years, he was in a state of constant languor and fever, during the attacks of which, as he told Fontenelle, he believed that he was in the middle of a forest, where he saw the leaves of all the trees covered with algebraic calculations. After teaching his class at the College Mazarin, on the 22d December 1722, he was seized with an illness which carried him off on the following night. Newton contributed the plate for the portrait of Varignon to the edition of his *Méchanique,* republished in 1725, as a present to the friends of Varignon.

spondence which took place between Newton and John Bernoulli, and have quoted those portions of it which bear upon the Fluxionary controversy.[1] It commenced on the part of Bernoulli with a letter of thanks[2] for the copy of Newton's *Optics*, which he had received through Varignon, and for the copy of the Latin edition which was promised him, with apologies for the delay that has taken place in writing him, which he hopes he will not impute to any insensibility to his divine and unrivalled genius. He appeals to his correspondence with Montmort and Demoivre for a proof of his admiration of his talents and his affection for his character, and he cannot understand how it has happened that—after the torch had been lighted of that deadly war, which, to the disgrace of mathematical science, has raged for three years between the geometers of Britain and Germany—he, neither a Briton nor a German, but a Swiss, who belonged to no party, and would have done anything rather than voluntarily intermeddle with the disputes of others, should have fallen, as was reported, in his esteem. If such should be the case, which he cannot believe, he must ascribe it to a combination of sycophants who seek to advance their own reputation and that of their friends, by destroying the good name of others, and proscribing all who are not English, the innocent and the guilty, unless they are willing to applaud them in everything. He believes, therefore, that many falsehoods have been told which have sunk him in his esteem, and, in defence of himself, he appeals to his writings, and declares that in these as well as in his letters, his conversations, his orations, and his lectures, he has always extolled him and his inventions with the highest praise. Nor can he doubt that such sincere appreciation of his talents will be more agreeable to posterity "than that immoderate ardour (not of praising you, for you cannot be too much praised) of arrogating to you what you do not claim, and leaving nothing to foreigners." This extravagant praise, which could not but be offensive to Newton, is

[1] Pages 32, 33.

[2] Dated July 5, 1719. See APPENDIX, No. XX.

followed by the solemn denial (the substance of which we have already quoted)[1] that he was the author of the celebrated letter in the *Charta Volans*, which he understood Newton had, on the authority of Leibnitz, ascribed to him in Raphson's *Fluxions*. He makes the best apology he can for Leibnitz's disregard of his feelings in ascribing the letter to him, and he concludes with an ardent expression of his gratitude to Newton for his splendid presents, and for his admission into the Royal Society, begging that he will regard him as "a most zealous worshipper of his immortal merits."

In Newton's reply to this letter,[2] he assures Bernoulli, as we have already seen,[3] that as soon as he learned that he was not the author of the obnoxious letter, he wished to cultivate his friendship. He thanks him for his kind reception of his *Optics*, and will endeavour to repay his politeness by mutual friendship. He explains how he suspected him to be the writer of the letter of the 7th June, but hopes, as he is not the author of it, that this will do him no injury. He assures him that the addition to Cor. 1, Prop. xiii. Book I. of the *Principia* was made at the suggestion of Cotes, and was printed in 1709, before the commencement of these disputes, and he concludes with the promise that he will exert himself to put an end to the controversy between his friends and him.

To this letter Bernoulli replied on the 21st December 1719. After referring to the obnoxious letter in the manner we have elsewhere mentioned,[4] and to the publication of some of Leibnitz's letters, he wishes Newton's countrymen would consider, if the controversy was to be carried on by the testimony of mathematicians, whether or not it would be better that other letters should be produced than those of Leibnitz, who cannot be regarded as a proper witness in his own cause. "I have letters," he adds, "from some learned men from countries

[1] Page 17, note 3.

[2] I have found the scroll of this letter, but without a date.

[3] See p. 33.

[4] See pp. 33, 34.

which have taken no part in this national contest, and which, if I were to make public, I doubt if such of your countrymen as rate me with so much warmth, proceeding even to gross insults, would have much reason to boast. I have, among other authentic documents, a letter from M. Montmort, a very learned mathematician, lately dead, who, as you know, was, while he lived, attached to no party, being a Frenchman. I have, I say, a copy of a certain letter sent to me by him, which he addressed, on the 18th December 1718, to the celebrated Brook Taylor,[1] and which even of itself might put an end to the greatest part of the controversy, but not according to the views of Taylor and his foreign disciples. I willingly abstain, however, from publishing these letters, provided your countrymen will cease to provoke our patience, which I wish for the sake of peace." Bernoulli then expresses his satisfaction with Newton's statement respecting the corollary in the *Principia.* He explains that he had only spoken against the form of Newton's assertion in the first edition of the work, and he claims to be the first who gave the analysis of the inverse truth, without supposing the direct one to be already known. He then mentions a report brought by a friend of his from England, that he had been expelled from the Royal Society,[2] and he begs that Newton will let him know whether he was expelled by a decision of the Society, or by the single authority

[1] I have found among Newton's papers a copy of this very interesting letter. Montmort was the particular friend of Brook Taylor, and was much attached to Newton, to whom he sent in 1716 a present of fifty bottles of champagne. That Montmort was, as Bernoulli says, an impartial judge in this matter, can hardly be doubted, and as his letter expresses the opinion of continental mathematicians on the Fluxionary controversy, in a manner at once precise and intelligible, I consider it a duty to give it a place in the Appendix. In consequence of Bernoulli's reference to it in his correspondence with Newton, it has acquired a historical interest. See APPENDIX, No. XXI., where I have prefixed to it Brook Taylor's letter to Sir Isaac, dated 22d April, 1716, in which Montmort's regard for Newton is specially mentioned.

[2] This friend had seen in the list of Fellows for 1718 the name of Bernoulli; but in a work entitled *Magnæ Britanniæ Notitia*, by John Chamberlayne, the friend of Newton, published in 1718, p. 144, he saw a catalogue of the Fellows containing the name of Bernoulli's nephew, but not his own.

of the Secretary, whom at that time he suspects to be Brook Taylor.

The answer of Sir Isaac to this letter has not been found, but there can be no doubt that he explained to Bernoulli, as I find he did to another foreign member of the Royal Society, who made a similar complaint, that the omission of his name from the list of the Fellows was merely an error of the person who copied it. No farther correspondence seems to have taken place between Newton and Bernoulli till 1723, when the latter acknowledged the receipt of three splendidly bound copies of the French edition of the *Optics*, for himself, his son, and his nephew. In this letter Bernoulli characterizes Newton's theory of light and colours as a discovery which will be more admired by posterity than it was then. He tells him that Hartsoeker had claimed for himself the discovery of the different refrangibility of light, and attacked his theory of the planetary system ; and he expresses his surprise that no Englishman was at hand to defend their illustrious countryman against a "fellow so rude and barbarous." After giving an account of Hartsoeker's attack upon himself founded upon a letter of Newton's, and requesting his assistance in protecting him against the charge,[1] he concludes with thanking him in the name of the celebrated Scheuchzer for the kindness he had shown to his son when in London, and giving him the privilege of conversing with the greatest of philosophers and mathematicians.[2]

It does not appear that Newton returned any answer to this letter, or that he carried on any correspondence with the other distinguished members of the Bernoulli family. Nicolas, the nephew of John, to whom, as we have seen, Newton presented

[1] See p. 35.

[2] John Bernoulli was born at Basle on the 7th August 1667, and died there on the 1st of January 1748, in the 81st year of his age. He was one of the most distinguished mathematicians of the last century. He was Professor of Mathematics at Basle, and one of the eight Associates of the Academy of Sciences. Two of his sons, Daniel and Nicolas, to the last of whom Newton sent copies of his *Optics*, were eminent mathematicians. His works were published in 1742 at Lausanne and Geneva, in 4 vols. 4to.

copies of several of his works, had pointed out a mistake in the 10th Prop. of the 2d Book of the *Principia*.[1] He went to London in the summer of 1712, where he met with the kindest reception from Newton and Halley, a circumstance which he speaks of with much gratitude in a letter in which he thanks Newton for a copy of the second edition of the *Principia*.[2] In the fluxionary controversy he was attacked by Keill, as one of Newton's enemies, but it appears that he denied the imputation in an explanatory letter to Newton, to which he received no answer.[3]

In the latter part of Newton's life his correspondence was very limited, and with the exception of a few letters from Dr. Robert Smith of Cambridge, Fontenelle,[4] Dr. Derham,[5] and others, his other letters possess very little interest. We are informed by Conduitt that he destroyed many of his papers before his death, and it is probable that some of them were letters which he deemed of no importance.

[1] Page 263 of the 1st edit. and p. 232 of the 2d edit. In his letter to the Abbé Varignon, in the autumn of 1719, Newton mentions that N. Bernoulli had pointed out this mistake, and adds, " constructionem propositionis correxi, et correctam ei ostendi, et imprimi curavi non subdole, sed eo cognoscente."—*Macclesfield Correspondence*, vol. ii. p. 437. John Bernoulli had previously shown in 1710, that Newton's result was erroneous when the curve was a circle, and he resumed the subject in the Leipsic Acts for February and March 1703. " It is remarkable," says Mr. Edleston, " that both of these mathematicians mistook the source of the error. They imagined that Newton had taken the coefficients of the successive powers of h in the expansion of $(x + h)_n$ for the successive fluxions of x^x."—See *Comm. Epist. Leib. et Bern.* tom. ii. p. 229 ; Bernoullii *Opera*, tom. i. pp. 489, 509 ; and Edleston's *Correspondence*, &c., pp. 142, 145, 156, 170.

[2] Dated Padua, May 31, 1717.

[3] I find this fact stated in a letter to Newton from the Scotch mathematician James Stirling, who met with Nicolas Bernoulli when he was at Venice in 1719. The postscript to the letter containing a message from Bernoulli to Newton is interesting. I have given it in APPENDIX, No. XXII.

[4] See APPENDIX, No. XXIII.

[5] See APPENDIX, No. XXIV.

CHAPTER XXIII.

The Princess of Wales obtains from Newton a manuscript Abstract of his System of Chronology—The Abbé Conti, at her request, is allowed to take a Copy of it under promise of Secrecy—He gives a Copy to M. Freret of the French Academy, who writes a Refutation of it, and gives it to a Bookseller, who asks Newton's permission to print it—Newton neglects to answer two Letters on the subject—The Abstract and the Refutation of it printed—Newton reprobates the conduct of Conti, and defends his system—It is attacked by Father Souciet, and is defended by Halley—Sir Isaac's larger work on Chronology published after his Death, and dedicated to the Queen by Mr. Conduitt—Pope assists in writing the Dedication—Opinions respecting the Chronology—Sir Isaac's Paper on the form of the most ancient Year—His unpublished Papers on the Julian Year, and the Reformation of the Calendar.

When Sir Isaac Newton was one day conversing with the Princess of Wales on some points of ancient history, in reference to the education of the royal family, he was led to mention to her, and to explain, a new system of chronology which he composed during his residence at Cambridge, where he was in the habit, as he expresses it, "of refreshing himself with history and chronology when he was weary of other studies." The Princess was so much pleased with the ingenuity of his system, that she sent a message by the Abbé Conti, when in England, desiring Sir Isaac to speak with her, and on this occasion she requested a copy of the work which contained his system of chronology. Sir Isaac informed her that it existed only in separate papers, which were not merely in a state of confusion, but contained a very imperfect view of the subject; and he promised in a few days to draw up an abstract of it for her Royal Highness's perusal, and on the condition that it should, not be communicated to any other person.[1] Some time after the Princess received the manuscript, she requested that the Abbé Conti might be permitted to have a copy of it. Sir Isaac granted her request, and the Abbé was distinctly informed that the manuscript was given to him at the request of the Princess, and with Sir Isaac's leave, and that he was to keep it a secret. It was entitled "A Short Chronicle from the First Memory of Things in Europe to the Conquest of Persia by Alexander the Great." It occupies only twenty-four quarto pages, with an introduction of four pages, in which Sir Isaac states that he "does not pretend to be exact to a year, and that there may be errors of five or ten years, and sometimes twenty, but not much above."

During his residence in England, the Abbé Conti kept his promise of secrecy, but he no sooner reached Paris than he communicated the manuscript to several persons, and among others to M. Freret, a learned antiquary, who not only translated it into French, but added observations of his own, for the purpose of refuting some of its leading results. Sir Isaac knew nothing of this transaction till the month of May 1724, when he received a respectful letter from G. Cavelier, a bookseller in Paris, informing him that a small manuscript had fallen into his hands, which he was assured came from his pen, and that as his name was very highly esteemed throughout Europe, he wished to print it. He had learned, however, that it contained

[1] In order to enjoy the conversation of the most distinguished literary men at that time in England, the "Princess of Wales appointed a particular day in the week, when they were invited to attend her Royal Highness in the evening; a practice which she continued after her accession to the throne. Of this company were Drs. Clarke, Hoadley, Berkeley, and Sherlock. Clarke and Berkeley were generally considered as principals in the debates that arose upon those occasions, and Hoadley adhered to the former as Sherlock did to the latter. Hoadley was no friend to Berkeley: he affected to consider his philosophy and his Bermuda project as the reveries of a visionary. Sherlock (who was afterwards Bishop of London), on the other hand, warmly espoused his cause, and particularly when the 'Minute Philosopher' came out, he carried a copy of it to the Queen, and left it to her Majesty to determine whether such a work could be the production of a disordered understanding."—*Works* of George Berkeley, D.D., Bishop of Cloyne, p. vii. Lond. 1837.

some errors, and as Sir Isaac would probably not wish it to appear under his name, he begged, as the manuscript which he had was of little value, that he would give him a correct copy of *His Chronology*. He added, that several persons who had defective copies would be glad to have correct ones, and as he was a bookseller who desired only to publish good articles, he was persuaded there could be nothing better than what came from his pen.

Sir Isaac took no notice of this letter, the object of which he probably thought was to get money for the manuscript, for he could hardly suppose that a mere pamphlet on a subject by no means popular, and supposed to contain errors, would repay the expenses of publication. After waiting nearly ten months for an answer, Cavelier addressed another letter to Newton, dated March 20, 1725, in which he asks him if he has additions or corrections to make, as some errors have been committed by the translator. He requests an immediate answer, and adds, that if he does not receive one, he will consider his silence as his consent to the publication of the work, "with remarks."[1]

As Sir Isaac paid no attention to this second letter, Cavelier requested a friend in London to procure an answer, which he at last received in the following terms :—

"I remember that I wrote a chronological index for a particular friend, on condition that it should not be communicated. As I have not seen the MS. which you have under my name, I know not whether it be the same. That which I wrote was not at all done with design to publish it. I intend not to meddle with that which hath been given you under my name, nor to give any consent to the publishing of it.—I am, your very humble servant, IS. NEWTON.

"*London, May* 27, 1725. St. Vet."

Before Cavelier received this letter the work was printed,[2]

[1] These two letters of Cavelier have been preserved by Sir Isaac.

[2] It was entitled *Abrégé de Chronologie de M. Le Chevalier Newton, fait par lui-même, et traduit sur le manuscript Anglois.* Paris, 1725.

and a copy of it was sent to Newton as a present, on the 11th November 1725.[1] The pamphlet was accompanied by Freret's observations, and, in an advertisement prefixed to it, Cavelier defends himself for printing it against the author's wishes, on the ground that he had written three letters to obtain his permission, and, in order to insure an answer, had intimated to him that he would take his silence for consent. When Sir Isaac received this work, he drew up a paper entitled, *Remarks on the Observations made on a Chronological Index of Sir Isaac Newton, translated into French by the Observator, and published at Paris*, which was printed in the *Philosophical Transactions*.[2] In this paper Sir Isaac gives a history of the transaction, charges

[1] The existence of this manuscript in Paris was generally known, and was the subject of conversation before the date of Cavelier's first letter to Newton (May 11, 1724), as appears from the following extract of a letter from M. Montmort (or perhaps from Conti) to Brook Taylor, dated Paris, January 15, 1724 :—

" On m'a dit aussi que Mr. Newton imprime la Chronologie Raisonnée. Tout le monde l'attend avec bien de l'impatience. Faites luy mes complimens, je vous en prie ; voicy une petit Sonnet que vous luy communiquerez ; j'espère qu'il en sera content ; car il verra l'attraction désigné par l'amour, qui règle le sistême de M. Descartes désigné par Phaeton. Dans le Mémoires de Leipsique, il aura vu si je suis du parti des Allemands.

' Lasciar mi il curro Governar del giorno,'
Disse à Febo l'Amor, ' e tosto sia
Rectificata in Ciel l'alta armonia
Che Fetonte turbó con suo gran scorno
Io diedi sede al cancro ed al capricorno
Ed al corpo lunar l'obliqua via
Io sterno al par del Caos ; ed Io con lumeor
Forzo al mondo l'equilibro ; ed Io l'adorno.
Disse :' e le Briglie imperioso stese
E corresse l'Aurora, ed agli infinite
Fonti del lume il corso antico rese
Ritornó i Pianet' ai primi siti
Il Solar Orbe a perni scai s'apese
E tal fu poi qual' O Newton l'additi.

Par L'ABBOT CONTI."

Contemplatio Philosophica, and Life of Brook Taylor. Lond. 1793, p. 141.

M. Conti is supposed to be the Abbot who corresponded with Lady Mary Wortley Montague. See her *Letters and Works*, vol. i. p. 358, and vol. ii. pp. 11, 21, 119, and 128. See also p. 375.

[2] *Phil. Trans.* 1725, Vol. xxxiii. No. 389, p. 315. I have found seven distinctly written copies of this paper in Sir Isaac's handwriting.

the Abbé Conti with a breach of promise,[1] and blames the publisher for having asked his leave to print the translation without sending him a copy for his perusal,—without acquainting him with the name of the translator,—and without announcing his intention of printing along with it a refutation of the original. The observations made by the translator against the conclusions deduced by the author, were founded on an imperfect knowledge of Sir Isaac's system; and they are so specious, that Halley himself confesses that he was at first prejudiced in favour of the Observations, taking the calculations for granted, and not having seen Sir Isaac's work.

To all the observations of M. Freret Sir Isaac returned a triumphant answer. This celebrated writer had ventured to assert, "that he believed he had stated enough concerning the epoch of the Argonauts, and the length of generations, to make people cautious about the rest; for these are the two foundations of all this new system of chronology." He founds his arguments against the epochs of the Argonauts, as fixed by our author, on the supposition that Sir Isaac places the vernal equinox at the time of the Argonautic expedition *in the middle of the sign of Aries*, whereas Sir Isaac places it *in the middle of the constellation*,—a point corresponding with the middle of the back of Aries, or 8° from the first star of Aries. This position of the colure is assigned on the authority of Eudoxus, as given by Hipparchus, who says that the colure passed over the back of Aries. Setting out with this mistake, M. Freret concludes that the Argonautic expedition took place 532 years earlier than Sir Isaac made it. His second objection to the new system relates to the length of generations, which he says is made only eighteen or twenty years. Sir Isaac, on the contrary, reckons a generation at thirty-three years, or three generations at 100; and it was the lengths of the reigns of kings that he made eighteen or twenty years. This deduction he

[1] Conti is said to have defended himself with much moderation, and with many expressions of esteem for Newton. See *Biog. Univ.* tom. ix. p. 517.

founds on the reigns of sixty-four French kings. Now, the ancient Greeks and Egyptians reckoned the length of a reign equal to that of a generation; and it was by correcting this mistake, and adopting a measure founded on fact, that Sir Isaac placed the Argonautic expedition forty-four years after the death of Solomon, and fixed some of the other points of his system.

Sir Isaac concludes his remarks with the following passage:—"Abbé Conti[1] came into England in spring 1715, and, while he staid in England, he pretended to be my friend, but assisted Mr. Leibnitz in engaging me in new disputes. The part he acted here may be understood by the character given of him in the *Acta Eruditorum* for 1721. . . . . And how Mr. Leibnitz, by his mediation, endeavoured to engage me against my will in new disputes about occult qualities, universal gravity, the sensorium of God, space, time, vacuum, atoms, the perfection of the world, supramundane intelligence, and mathematical problems, is mentioned in the second edition of the *Commercium Epistolicum.* And what he hath been doing in Italy may be understood by the disputes raised there by one of his friends,[2] who denies many of my optical experiments, though they have been all tried in France with success; but I hope that these things, and the perpetual motion, will be the last efforts of this kind—*will be the last efforts of those friends of Mr. Leibnitz to embroil me.*"[3]

[1] In the passage from the *Acta Eruditorum,* Conti is described as carrying letters of Newton to Leibnitz, and communicating Leibnitz's letters to Newton. Conti was a very excellent and accomplished person, distinguished as a poet and a man of very considerable acquirements. He was a great favourite of the King, and acted as interpreter when Dr. Clarke, who could speak only Latin and English, was explaining to his Majesty the discoveries of Newton. It was at the King's request that he interfered in the dispute between Newton and Leibnitz, and we see no reason to blame him for the part which he acted in that matter.

[2] Signior Rizzetti, who afterwards published his attack upon Newton in a book entitled *De Luminis Affectionibus Specimen Physico-Mathematicum.* Venet. 1727.—See Desaguliers' Defence of Newton in the *Phil. Trans.* 1728, p. 596.

[3] The words in *italics* are in another copy. I find also from one of these copies that Conti is charged with "sending Mr. Stirling to Italy, a person then unknown to me, to be ready to defend me there, if I would have contributed to his maintenance;" and in

This answer of Sir Isaac's to the objections of Freret called into the field a fresh antagonist, Father Souciet, who published five Dissertations on the new chronology. These Dissertations were written in a tone so highly reprehensible, that Mr. Conduitt being apprehensive that the manner in which his system was attacked would affect Sir Isaac more than the arguments themselves, prevailed upon a friend to draw up, for his perusal, an abstract of Souciet's objections, stripped of the "extraordinary ornaments with which they were clothed." The perusal of these objections had no other effect upon him than to convince him of the ignorance of their author; and he was induced to read the entire work, which produced no change in his opinion.

In consequence of these discussions, Sir Isaac was prevailed upon to prepare his larger work for the press. After the publication of Freret's Observations, he had resolved to print it "as privately as possible, and keep the copies in his own possession," but it was not ready till nearly the time of his death. It did not therefore appear till 1728, when it was published by Mr. Conduitt under the title of the *Chronology of Ancient Kingdoms amended, to which is prefixed a short Chronicle, from the First Memory of Things in Europe to the Conquest of Persia by Alexander the Great.*[1] It consists of six chapters:—1. On the Chronology of the Greeks;[2] 2. Of the Empire of Egypt; 3. Of the Assyrian Empire; 4. Of the two contem-

another, Conti is said to have "softened the business, by lately writing a poem upon him, and in the colour of a friend." This poem is probably that mentioned by Bolingbroke in a letter to Brook Taylor, Dec. 26, 1723. "He has begun a philosophical poem which will be finished, I believe, long before the Anti-Lucretius of the Cardinal de Polignac. Sir I. Newton's system will make the principal beauty in it. He recited the Exorde to me, which I thought very fine. I need not tell you that he writes in Italian." —*Life of Brook Taylor*, p. 136.

[1] The work is dedicated to the Queen by Mr. Conduitt, in an address of twelve quarto pages, in composing which he sought the assistance of Pope. We have given Pope's letter, containing his criticisms, in the APPENDIX, No. XXV.

[2] According to Whiston, Sir Isaac wrote out eighteen copies of this chapter with his own hand, differing little from one another.—*Whiston's Life*, p. 39.

porary Empires of the Babylonians and Medes; 5. A Description of the Temple of Solomon; 6. Of the Empire of the Persians. The sixth chapter was not copied out with the other five, which makes it doubtful whether or not it was intended for publication; but as it was found among his papers, and appeared to be a continuation of the same work, it was thought right to add it to the other five chapters.[1]

After the death of Newton, Dr. Halley, who had not yet seen the larger work, felt himself called upon, both as Astronomer-Royal and as the friend of the author, to reply to the first and last dissertations of Father Souciet, which were chiefly astronomical; and in two papers[2] he has done this in a most convincing and learned argument.

Among the supporters of the views of Newton, we may enumerate Dr. Reid, Nauze, and some other writers; and among its opponents, M. Freret, who left behind him a posthumous work on the subject,[3] M. Fourmond, Mr. A. Bedford, Dr. Shuckford, Dr. Middleton, Whiston,[4] and the late M. Delambre. The object of M. Fourmond is to show the uncertainty of the astronomical argument, arising on the one hand from the vague account of the ancient sphere as given by Hipparchus; and, on the other, from the extreme rudeness of ancient astronomical observations. Delambre has taken a similar view of the subject: he regards the observations of ancient astronomers as too incorrect to form the basis of a system of chronology; and he maintains, that if we admit the accuracy of the details in the sphere of Eudoxus, and suppose them all to belong to the same epoch, all the stars which it contains ought at that epoch to be

[1] This work forms the first article in the fifth volume of Dr. Horsley's edition of Newton's works, and is accompanied with copious notes. The next article in the volume is entitled, "A Short Chronicle from a MS., the property of the Reverend Dr. Ekins, Dean of Carlisle," which is nothing more than an abstract of the chronology already printed in the same volume. We cannot even conjecture the reasons for publishing it especially as it is less perfect than the abstract, two or three dates being wanting.

[2] *Phil. Trans.*, 1727, vol. xxxiv. pp. 205, 296.

[3] *Défense de la Chronologie contre le Système de M. Newton.* Paris, 1758, 4to.

[4] Collection of *Authentic Records,* Part II. No. 24. 1727.

found in the place where they are marked, and we might thence verify the accuracy, and ascertain the state of the observations. It follows, however, from such an examination, that the sphere would indicate almost as many different epochs as it contains stars. Some of them even had not, in the time of Eudoxus, arrived at the position which had been for a long time attributed to them, and will not even reach it for 300 years to come, and on this account he considers it impossible to deduce any chronological conclusions from such a rude mass of errors.

But, however well founded these observations may be, we agree in opinion with M. Daunou,[1] "that they are not sufficient to establish a new system, and we must regard the system of Newton as a great fact in the history of chronological science, and as confirming the observation of Varro, that the stage of history does not commence till the first Olympiad."

Among the chronological writings of Sir Isaac Newton, we must enumerate his *Letter to a person of distinction who had desired his opinion of the learned Bishop Lloyd's Hypothesis concerning the Form of the most Ancient Year.* This hypothesis was sent by the Bishop of Worcester to Dr. Prideaux. Sir Isaac remarks, that it is filled with many excellent observations on the ancient year; but he does not "find it proved that any ancient nations used a year of twelve months and 360 days without correcting it from time to time by the luminaries to make the months keep to the course of the moon, and the year to the course of the sun, and returns of the seasons and fruits of the earth." After examining the years of all the nations of antiquity, he concludes, "that no other years are to be met with among the ancients but such as were either luni-solar, or solar or lunar, or the calendars of these years." A practical year, he adds, of 360 days, is none of these. The beginning of such a year would have run round the four seasons in seventy years, and such a notable revolution would

[1] See an excellent view of this controversy in an able note by M. Daunou, attached to Biot's Life of Newton in the *Biog. Universelle*, tom. xxxi. p. 180.

have been mentioned in history, and is not to be asserted without proving it.[1]

When the public attention was called to the reformation of the Kalendar, Sir Isaac seems to have been consulted on the subject. Among his papers he has left two copies, one distinctly written out as if for publication, entitled *Considerations about rectifying the Julian Kalendar.*[2] After giving an account of the Egyptian Kalendar—the improvements introduced by Julius Cæsar and the Roman senate, and the correction made by Gregory XIII.—he describes what in another manuscript he calls the best form of the solar year. "The best form of the solar year," he says, "is to divide it by the four cardinal periods of the equinoxes and solstices, so that the quarters of the year may begin at the equinoxes and solstices as they ought to do, and then to divide every quarter into three equal months, which will be done by making the six winter months to consist of thirty days each, and the six summer months of thirty-one days each, excepting one of them, suppose the last, which in the leap years shall have thirty-one days, in the other years only thirty days. At the end of every 100 years, omit the intercalary day in the leap year, excepting at the end of every 500 years. For this rule is exacter than the Gregorian, of omitting it at the end of every 100 years, excepting at the end of every 400 years, and thus reckoning by 500ds and thousands of years is rounder than the other by 400, 800, and 1200ds. And this I take to be the simplest, and in all respects the best form of the civil year that can be thought of."

In the paper entitled *Considerations*, &c., in which the

[1] This letter was first published without any date in the *Gentleman's Magazine* for 1755, vol. xxv. p. 3. I have found two copies of it among Sir Isaac's papers. Mr. Edleston informs us that the original is in the British Museum, presented by Mrs. Sharp. I have found also two copies of the communication he made to the Bishop of Worcester, which is published by Mr. Edleston in his *Correspondence*, &c., Appendix, p. 314. One of these copies is much fuller than that which is printed by Mr. Edleston.

[2] I infer that this paper was written in 1699, from the statement in it that Pope Gregory's corrections "were made 118 years ago."

above form of the civil year is stated less fully, he goes on to consider the best method of introducing a change of style.

"But without the consent of a good part of Europe," he says, "I do not think it advisable to alter the number of days in the month. The question is now whether the old style should be retained in conformity with antiquity, or the new received in conformity with the nations abroad. I press neither opinion; but whenever the latter shall be resolved upon, I believe the best way may be, to receive the new style without the Gregorian Kalendar by an Act of Parliament, to some such purpose as that which follows."[1]

[1] I find two copies of another paper in Latin, entitled *Regulæ pro determinatione Paschæ*. The subject of the Kalendar is touched upon in Newton's *Chronology*, p. 71, and in his *Observations on the Prophecies of Daniel*, p. 137, note.

CHAPTER XXIV.

Theological Writings of Newton—Their importance to Christianity—Motives to which they have been ascribed—Biot's opinion disproved—The date of Newton's theological writings fixed—His Letters to Locke on these subjects—History of his Account of Two Corruptions of the Scriptures—His observations on the Prophecies of Daniel, and on the Apocalypse—Abstract of his Historical Account of Two Corruptions of Scripture—His views adopted by the ablest Biblical critics of modern times—His unpublished theological writings—Paradoxical questions concerning Athanasius—His Irenicum or Ecclesiastical Polity tending to Peace—His views on points of Trinitarian doctrine—His Articles of Faith—His Plan for correcting the Romish tendencies of the Church of England—Coincidence of his opinions with those of Locke—His views on the future residence of the Blest—Opinions of Voltaire and others—Napier, Boyle, Milton, and Locke, students of the Scriptures—Analogy between the Book of Nature and that of Revelation—Letter of Dr. Morland to Newton.

If Sir Isaac Newton had not been distinguished as a mathematician and a natural philosopher, he would have enjoyed a high reputation as a theologian. The occupation of his time, however, with those profound studies, for which his genius was so peculiarly adapted, and in the prosecution of which he was so eminently successful, prevented him from preparing for the press the theological works which he had begun at a very early period of life, and to which he devoted much of his time even when he mixed with the world, and was occupied with the affairs of the Mint. The history of Sir Isaac's theological writings cannot fail to be regarded as an interesting portion of his life, and much anxiety has been expressed for a more precise account than has yet been given of his religious opinions. That the greatest philosopher of which any age can boast was a sincere and humble believer in the leading doctrines of our religion, and lived conformably to its precepts, has been justly regarded as a proud triumph of the Christian faith. Had he exhibited only an outward respect for the forms and duties of religion, or left merely in his dying words an acknowledgment of his belief, his piety might have been regarded as a prudent submission to popular feeling, or as a proof of the decay or the extinction of his transcendent powers; but he had been a searcher of the Scriptures from his youth, and he found it no abrupt transition to pass from the study of the material universe to an investigation of the profoundest truths, and the most obscure predictions, of holy writ.

The religious opinions of great men—of those especially who, by force of genius and patient thought, have discovered new and commanding truths—possess an interest of various kinds. The apostle of infidelity cowers beneath the implied rebuke. The timid and the wavering stand firmer in the faith, and the man of the world treats the institutions of religion with more respect and forbearance. Nor are such opinions less influential when they emanate from men who follow truth through her labyrinth, neither impelled by professional ambition, nor alarmed by articles which they have to sign, or creeds which they have to believe. Though often solicited by its highest dignitaries, Newton never thought of entering the Church. He had, therefore, no beacons to dread, and no false lights to mislead him. He was free to range through the volume of inspiration, and to gather from the Sibylline pages of its prophets and apostles, its historians and its poets, the insulated truths which they reveal, and to combine them into a broader faith, and embalm them in a higher toleration.

To the friends and countrymen of Newton it has been no inconsiderable source of pain that some foreign writers have referred to extraordinary causes his religious opinions and theological writings. While some have ascribed them to the habits of the age in which he lived, and to a desire of promoting civil liberty by turning against the abettors of irresponsible power the sharp weapons which the Scriptures supply, others have endeavoured to show that they were composed at a late period of life when his mind was in its dotage, or had suffered from that supposed mental aberration to which so many acts of his life have been erroneously ascribed. In answer to such allegations, we may adduce the testimony of one of his most distinguished friends, John Craig, an eminent mathematician, who, in the very year in which Newton died, gave the following account of his theological writings:[1]—

"I shall not tell you what great improvements he made in geometry and algebra, but it is proper to acquaint you that his great application in his inquiries into nature did not make him unmindful of the Great Author of nature. They were little acquainted with him who imagine that he was so intent upon his studies of geometry and philosophy as to neglect that of religion and other things subservient to it. And this I know, that he was much more solicitous in his inquiries into religion than into natural philosophy, and that the reason of his showing the errors of Cartes' philosophy was, because he thought it was made on purpose to be the foundation of infidelity. And Sir Isaac Newton, to make his inquiries into the Christian religion more successful, had read the ancient writers and ecclesiastical historians with great exactness, and had drawn up in writing great collections out of both; and to show how earnest he was in religion, he had written a long explication of remarkable parts of the Old and New Testament, *while his understanding was in its greatest perfection, lest the infidels might pretend that his applying himself to the study of religion was the effect of dotage.* That he would not publish these writings in his own time, because they showed that his thoughts were sometimes different from those which are commonly received, which would engage him in disputes; and this was a thing which he avoided as much as possible. But now it's hoped that the worthy and ingenious Mr. Conduitt will take care that they be published, that the world may see that Sir Isaac New-

[1] Letter to Conduitt, dated 7th April 1727. See vol. i. Appendix, p. 422.

ton was as good a Christian as he was a mathematician and philosopher."

The anxiety to refer the religious writings of Newton to a late period of his life, seems to have been particularly felt by M. Biot, who goes so far as to fix the date of one of his most important works,[1] and to associate his religious tendencies with the effects of what he calls "the fatal epoch of 1693."

"From the nature of the subject," says he, "and from certain indications which Newton seems to give at the beginning of his dissertation, we may conjecture with probability that he composed it at the time when the errors of Whiston and a work of Clarke on the same subject, drew upon them the attacks of all the theologians of England, which would place the date between the years 1712 and 1719. It would then be a prodigy to remark, that a man of from seventy-two to seventy-five years of age was able to compose, *rapidly* as he leads us to believe, so extensive a piece of sacred criticism, of literary history, and even of bibliography, where an erudition the most vast, the most varied, and the most ready, always supports an argument well arranged and powerfully combined. . . . . At this epoch of the life of Newton, the reading of religious books had become one of his most habitual occupations; and after he had performed the duties of his office, they formed, along with the conversation of his friends, his only amusement. He had then almost ceased to care for the sciences, and, as we have already remarked, since the fatal epoch of 1693, he gave to the world only three really new scientific productions, of which one had probably been long ready, while the others required from him only a very little time."[2]

Notwithstanding the prodigy which it involves, M. Biot has adopted 1712-1719 as the date of this critical dissertation ;—

[1] *Historical Account of Two Notable Corruptions of the Scriptures*, 50 pp. quarto.

[2] The papers here alluded to were one on the Scale of Heat, his Reflecting Sextant, and his Solution of the Problem of Quickest Descent. See *Bibl. Univ.*, tom. xxxi. p. 190; and *ante*, vol. i. pp. 208, 349.

it is regarded as the composition of a man of seventy-two or seventy-five ;—the reading of religious works is stated to have *become* one of his most habitual occupations, and such reading is said to have been his only amusements ; and all this is associated with "the fatal epoch of 1693," as if his illness at that time had been the cause of his abandoning science and betaking himself to theology.

The incorrectness of these opinions we are fortunately able to prove. It appears from Mr. Pryme's manuscript,[1] that previous to 1692, when a shade is supposed to have passed over his gifted mind, Newton was well known by the appellation of an "excellent divine,"[2] a character which could not have been acquired without the devotion of many years to theological researches ; but, important as this argument would have been, we are not left to so general a defence. The correspondence of Newton with Locke, places it beyond a doubt that he had begun his researches respecting the prophecies before the year 1691,—before the forty-ninth year of his age, and before the "fatal epoch of 1693." The following letter shows, that he had previously discussed this subject with his friend.[3]

"CAMBRIDGE, *Feb.* 7, 1690-1.

"SIR,—I am sorry your journey proved to so little purpose, though it delivered you from the trouble of the company the day after. You have obliged me by mentioning me to my friends at London, and I must thank both you and my Lady Masham for your civilities at Oates, and for not thinking that I made a long stay there. I hope we shall meet again in due time, and then I should be glad to have your judgment upon some of my mystical fancies. The Son of Man (Dan. vii.), I take to be the same with the Word of God upon the White

[1] See p. 90.

[2] In a book called "Newton's Waste Book," containing his discoveries in mathematics in the years 1664 and 1665, there are many extracts which prove that he had in these years prosecuted the study of theology.

[3] Lord King's *Life of Locke*, vol. i. p. 402, 2d edit. Lond. 1830.

Horse in Heaven (Apoc. xii.), for both are to rule the nations with a rod of iron; but whence are you certain that the Ancient of Days is Christ? Does Christ anywhere sit upon the throne? —If Sir Francis Masham be at Oates, present, I pray, my service to him, with his lady, Mrs. Cudworth, and Mrs. Masham. Dr. Covel is not in Cambridge.—I am your affectionate and humble servant, IS. NEWTON.

"Know you the meaning of Dan. x. 21? *There is none that holdeth with me in these things but Mich. your Prince.*"

In replying to this letter, Locke does not seem to have distinctly noticed Newton's question, why he thought that Christ was the Ancient of Days, for in another letter[1] addressed to Locke, he says, "Concerning the *Ancient of Days*, Dan. vii., there seems to be a mistake either in my last letter or in yours, because you wrote in your former letter that the Ancient of Days is Christ; and in my last I either did, or should have asked how you knew that. But these discourses may be done with more freedom at our next meeting."

It is obvious from these facts, that Locke and Newton had corresponded on the prophecies of Daniel so early as 1691, and that these subjects were discussed by them when they met. In replying to some questions of Locke on the subject of miracles, Newton tells him[2] that "miracles of good credit continued in the Church for about two or three hundred years. Gregorius Thaumaturgus had his name from them, and was one of the latest who was eminent for that gift, but of their number and frequency I am not able to give you a just account;" and he resumes the subject in the following interesting letter:—

"CAMBRIDGE, *May* 3, 1692.

"SIR,—Now the churlish weather is almost over, I was thinking within a post or two, to put you in mind of my desire to see you here, where you shall be as welcome as I can make

[1] Dated Cambridge, June 30, 1691.

[2] Cambridge, Feb. 16, 1691-2.

you. I am glad you have prevented me, because I hope now to see you the sooner. You may lodge conveniently either at the Rose Tavern or Queen's Arms Inn. I am glad the edition is stopped, but do not perceive that you had mine, and therefore have sent you a transcript of what concerned miracles, if it come not now too late ; for it happens that I have a copy of it by me. Concerning miracles, there is a notable passage or two in Irenæus, L. 22, c. 56, recited by Eusebius, I. 5, c. 17. The miraculous refection of the Roman army by rain, at the prayers of a Christian legion (thence called fulminatrix), is mentioned by Ziphilina apud Dionam. in Marco Imp., and by Tertullian, Apolog. c. 5, and ad Scap. c. 4, and by Eusebius, I. 5, c. 5, Hist. Eccl., and in Chronico, and acknowledged by the Emperor Marcus in a letter, as Tertullian mentions. The same Tertullian somewhere challenges the heathens to produce a demoniac, and he shall produce a man who shall cast out the demon. For this was the language of the ancients for curing lunatics. I am told that Sir Henry Yelverton, in a book about the truth of Christianity, has writ well of the ancient miracles, but the book I never saw. Concerning Gregory Thaumaturgus, see Gregory Nystra in ejus vita, and Basil, de Spiritu Sancto, c. 29. My humble service to Sir Francis and his lady. I am, your most humble servant, Is. NEWTON.

"I know of nothing that will call me from home this month."

In the early part of 1703, Locke sent to Newton the manuscript of his Commentary on the Epistles of St. Paul to the Corinthians, which have been published among his posthumous works, and in the following letter he gave him his opinion of the work, with a criticism upon his interpretation of a particular passage :[1]—

"LONDON, *May* 15, 1703.

"SIR,—Upon my first receiving your papers, I read over

[1] "The words of Locke," says Lord King, "stand unaltered in the printed copy," vol. ii. p. 420.

those concerning the First Epistle of the Corinthians, but by so many intermissions, that I resolved to go over them again, so soon as I could get leisure to do it with more attention. I have now read it over a second time, and gone over also your papers on the Second Epistle. Some faults, which seemed to be faults of the scribe, I mended with my pen as I read the papers; some others I have noted in the enclosed papers. In your paraphrase on 1 Cor. vii. 14, you say, 'the unbelieving husband is sanctified or made a Christian in his wife.' I doubt this interpretation, because the unbelieving is not capable of baptism, as all Christians are. The Jews looked upon themselves as clean, holy, or separate to God, and other nations as unclean, unholy, or common, and accordingly it was unlawful for a man that was a Jew to keep company with, or come unto one of another nation; Acts x. 28. But when the propagation of the gospel made it necessary for the Jews, who preached the gospel, to go unto and keep company with the Gentiles, God showed Peter by a vision, in the case of Cornelius, that he had cleansed those of other nations, so that Peter should not any longer call any man common or unclean, and on that account forbear their company; and thereupon Peter went in unto Cornelius and his companions, who were uncircumcised, and did eat with them; Acts x. 27, 28, and xi. 3. Sanctifying, therefore, and cleansing, signify here, not the making a man a Jew or Christian, but the dispensing with the law whereby the people of God were to avoid the company of the rest of the world as unholy or unclean. And if this sense be applied to St. Paul's words, they will signify, that although believers are a people holy to God, and ought to avoid the company of unbelievers as unholy or unclean, yet this law is dispensed with in some cases, and particularly in the case of marriage. The believing wife must not separate from the unbelieving husband as unholy or unclean, nor the believing husband from the unbelieving wife; for the unbeliever is sanctified or cleansed by marriage with the believer, the law of avoiding the company of unbelievers being, in this case, dispensed with. I should therefore interpret St. Paul's words after the following manner :

" 'For the unbelieving husband is sanctified or cleansed by the believing wife, so that it is lawful to keep him company, and the unbelieving wife is sanctified by the husband ; else were the children of such parents to be separated from you, and avoided as unclean, but now, by nursing and educating them in your families, you allow that they are holy.'

" This interpretation I propose as easy and suiting well to the words and design of St. Paul, but submit it wholly to your judgment.

" I had thoughts of going to Cambridge this summer, and calling at Oates in my way, but am now uncertain of this journey. Present, I pray, my humble service to Sir Francis Masham and his lady. I think your paraphrase and commentary on these two Epistles is done with very great care and judgment.—I am, your most humble and obedient servant,

" IS. NEWTON."

It is obvious from these letters that Newton had carried on his theological studies, and particularly those relating to the Prophecies, long before the epoch of 1693, and there is no reason to believe that any part of his principal theological work on the Prophecies and the Apocalypse was composed after that date. If any further evidence were required for this fact, it may be derived from his folio *Commonplace Book*, written in his early hand, and containing copious extracts and observations on theological subjects of every kind.

The other work of Newton, entitled *Historical Account of two Notable Corruptions of the Scriptures, in a Letter to a Friend*, is certainly an early production. In 1690, or perhaps earlier, he had corresponded on the subject of it with Locke, who requested a sight of the manuscript. In reply to this request, Newton writes to him,[1] " that he would have answered

[1] Cambridge, Sept. 28, 1690.

his letter sooner, but that he stayed to revise and send the papers which he desired; but the consulting of authors, proving more tedious than he expected, made him defer sending them till next week." In the following letter to Locke, which accompanies the manuscript, he mentions part of it as something that he "had by him," and it was therefore in all probability written long before 1690:—

"*November* 14, 1690.

"SIR,—I send you now by the carrier, Martin, the papers I promised. I fear I have not only made you stay too long for them, but also made them too long by an addition; for, upon the receipt of your letter reviewing what I had by me concerning the text of 1 John v. 7, and examining authors a little further about it, I met with something new concerning that other of 1 Tim. iii. 16, which I thought would be as acceptable to inquisitive men, and might be set down in a little room; but by searching further into authors to find out the bottom of it, is swelled to the bigness you see. I fear the length of what I say on both texts may occasion you too much trouble, and therefore if at present you get only what concerns the first done into French, that of the other may stay till we see what success the first will have. I have no entire copy besides that I send you, and therefore would not have it lost, because I may, perhaps, after it has gone abroad long enough in French, put it forth in English. What charge you are at about it (for I am sure it will put you to some) you must let me know, for the trouble alone is enough for you. Pray present my most humble service and thanks to my Lord and Lady Monmouth, for their so kind remembrance of me, for their favour is such that I can never sufficiently acknowledge it. If your voyage hold, I wish you a prosperous one, and happy return. I should be glad of a line from you to know that you have these papers, and how far you have recovered your health, for you told me nothing of that.—I am, Sir, your most faithful and most humble servant,

IS. NEWTON."

When this correspondence was going on, Mr. Locke meditated a journey to Holland, and undertook, in compliance with the wishes of his friend, to have the *Historical Account*, &c., translated into French, and published in Holland. Dreading the intolerance of the divines of his own country, he was anxious to have the opinions of foreign biblical writers before he "put it forth in English." Having abandoned his design of visiting Holland, Locke transmitted the manuscript, in his own handwriting,[1] and without Newton's name, to his friend M. Le Clerc in Holland, with a request to have it translated into French and published. Sir Isaac was not aware of the step that Locke had taken; and knowing that he had not left England, he believed that the manuscript was still in his possession. It had reached M. Le Clerc, however, previous to the 11th April 1691, for, in a letter to Locke of that date, he tells him that he will translate, either into Latin or French, the small *Historical Account*, &c., which deserves to be published. "I believe, however," he adds, "that it would be better if the author had read with care what M. Simon has said on the subject, of which he speaks in his Criticism of the New Testament."[2] In a subsequent letter, Le Clerc tells Locke that he has been prevented, by various occupations, from doing anything with the manuscript, but that he hopes to have an opportunity of publishing it along with some other dissertations, as it is too small to appear alone. In reply to a letter which he had received from Locke, Le Clerc says, "that he will take care to insert in the dissertation on the passage in St. John, the addition which he had sent him, and translate the other, to publish both in Latin."

Locke seems to have intimated the intentions of Le Clerc to Sir Isaac, who lost no time in addressing to him the following letter:—

"CAMBRIDGE, *Feb.* 16, 1691-2.

"SIR,—Your former letters came not to my hand, but this

[1] Edition of 1754, pp. 122, 123.

[2] *Hist. Critique du Texte du Nouveau Testament.* Rotterdam, 1689.

I have. I was of opinion my papers had lain still, and am sorry to hear there is news about them. Let me entreat you to stop their translation and impression so soon as you can, for I design to suppress them. If your friend hath been at any pains and charge, I will repay it and gratify him. . . .

"Your most affectionate and humble servant,

"IS. NEWTON."

From these facts it is obvious that this celebrated treatise, which Biot alleges to have been written between 1712 and 1719, *was actually written in* 1690, or probably much earlier, and was in the hands of Le Clerc on the 11th April 1691, previous to the time of the supposed insanity of its author. Locke lost no time in communicating to his friend the wishes of Newton, and the publication of the *Historical Account* was therefore stopped.

Although we are not acquainted with the reasons which induced Newton to take this step, they may to a certain extent be inferred from Le Clerc's answer to Locke.[1] "It is a pity," he says, "that these two dissertations should be suppressed. I do not think that any person could find out that they were translated, unless it were said so. In a matter of this kind, where I would not fail to seize the meaning of the author, I would have given it an original air which would not have savoured of a translation." And, in another letter,[2] he says, "I will keep carefully the two dissertations, till you tell me what the author wishes me to do with them."

No information concerning these dissertations is contained either in the correspondence of Locke with Newton, or with Le Clerc. We are told by the editor of the edition of 1754, that Le Clerc deposited the manuscript in the Library of the Remonstrants, and that he received, through a friend, the copy of it which he published, under the title of *Two Letters from Sir Isaac Newton to M. Le Clerc, the former containing a Disserta-*

[1] April 11, 1692. [2] July 15, 1692.

tion upon the Reading of the Greek Testament, 1 *John* v. 7, *the latter upon* 1 *Timothy* iii. 16;—a form which had never been given to it by its author. The copy thus published was a very imperfect one, wanting both the beginning[1] and the end, and erroneous in many places; but Dr. Horsley has published a genuine edition, which has the form of a single letter to a friend, and was copied from a manuscript in Sir Isaac Newton's handwriting, now in the possession of the Reverend Jeffrey Ekins, Rector of Sampford.[2]

Having thus determined, as accurately as possible, the dates of the principal theological writings of Sir Isaac, we shall now proceed to give some account of their contents.

The work entitled *Observations upon the Prophecies of Daniel and the Apocalypse of St. John,*[3] is divided into two parts, the first of which treats of the Prophecies of Daniel, and the second of the Apocalypse of St. John. It begins with an account of the different books which compose the Old Testament; and, as the author considers Daniel to be the most distinct in the order of time, and the easiest to be understood, he makes him the key to all the prophetic books in those matters which relate to the "last time." He next considers the figurative language of the prophets, which he regards as taken "from the analogy between the world natural, and an empire or kingdom considered as a world politic;" the heavens, and the things therein, representing thrones and dynasties; the earth, with the things therein, the inferior people; and the lowest parts of the earth the most miserable of the people. The sun is put for the whole race of kings, the moon for the body of the common people, and the stars for subordinate princes and

[1] The editor supplied the beginning down to the 13th page, where he mentions in a note, that "*thus far is not Sir Isaac's.*"

[2] I have not found any copy of this *manuscript,* or any letters relating to it, among the manuscripts of Newton. In his list of the MSS., Dr. Horsley mentions a Latin translation of the *Historical Account,* and a paper book entitled *Sancti Johannis Apostoli Vindiciæ contra Novaticos et Falcarios.*

[3] Lond. 1733. 4to. Pp. 323.

rulers. In the earth, the dry land and the waters are put for the people of several nations. Animals and vegetables are also put for the people of several regions. When a beast or man is put for a kingdom, his parts and qualities are put for the analogous parts and qualities of the kingdom; and when a man is taken in a mystical sense, his qualities are often signified by his actions, and by the circumstances and things about him. In applying these principles he begins with the vision of the image composed of four different metals. This image he considers as representing a body of four great nations which should reign in succession over the earth, viz., the people of Babylonia, the Persians, the Greeks, and the Romans, while the stone cut out without hands is a new kingdom which should arise after the four, conquer all those nations, become very great, and endure to the end of time.

The vision of the four beasts is the prophecy of the four empires repeated, with several new additions. The lion with eagles' wings was the kingdom of Babylon and Media, which overthrew the Assyrian power. The beast like a bear was the Persian empire, and its three ribs were the kingdoms of Sardis, Babylon, and Egypt. The third beast, like a leopard, was the Greek empire, and its four heads and four wings were the kingdoms of Cassander, Lysimachus, Ptolemy, and Seleucus. The fourth beast, with its great iron teeth, was the Roman empire, and its ten horns were the ten kingdoms into which it was broken in the reign of Theodosius the Great.

In the fifth chapter Sir Isaac treats of the kingdoms represented by the feet of the image composed of iron and clay which did not stick to one another, and which were of different strength. These were the Gothic tribes called Ostrogoths, Visigoths, Vandals, Gepidæ, Lombards, Burgundians, Alans, &c., all of whom had the same manners and customs, and spoke the same language, and who, about the year 416 A.C., were all quietly settled in several kingdoms within the empire, not only by conquest, but by grants of the Emperor.

In the sixth chapter he treats of the *ten* kingdoms represented by the ten horns of the fourth beast, into which the Western empire became divided about the time when Rome was besieged and taken by the Goths. These kingdoms were,—

1. The kingdom of the Vandals and Alans in Spain and Africa.
2. The kingdom of Suevians in Spain.
3. The kingdom of the Visigoths.
4. The kingdom of the Alans in Gaul.
5. The kingdom of the Burgundians.
6. The kingdom of the Franks.
7. The kingdom of the Britains.
8. The kingdom of the Huns.
9. The kingdom of the Lombards.
10. The kingdom of Ravenna.

Some of these kingdoms at length fell, and new ones sprung up; but whatever was their subsequent number, they still retain the name of the ten kings from their first number.

The eleventh horn of Daniel's fourth beast is shown in chapter vii. to be the Church of Rome in its triple character of a seer, a prophet, and a king; and its power to change times and laws is copiously illustrated in chapter viii.

In the ninth chapter our author treats of the kingdom represented in Daniel by the ram and he-goat, the ram indicating the kingdom of the Medes and Persians from the beginning of the four empires, and the he-goat the kingdom of the Greeks to the end of them.

The prophecy of the seventy weeks, which had hitherto been restricted to the first coming of our Saviour, is shown to be a prediction of all the main periods relating to the coming of the Messiah, the times of his birth and death, the time of his rejection by the Jews, the duration of the Jewish war, by which he caused the city and sanctuary to be destroyed, and th etime of his second coming.

In the eleventh chapter Sir Isaac treats with great sagacity and acuteness of the time of our Saviour's birth and passion, —a subject which had perplexed all preceding commentators.

After explaining, in the twelfth chapter, the last prophecy of Daniel, namely, that of the scripture of truth, which he considers as a commentary on the vision of the ram and he-goat, he proceeds, in the thirteenth chapter, to the prophecy of the king who did according to his will, and magnified himself above every god, and honoured Mahuzzims, and regarded not the desire of women. He shows that the Greek empire, after the division of the Roman empire into the Greek and Latin empires, became the king who, in matters of religion, did according to his will, and in legislation exalted and magnified himself above every god.

In the second part of his work, entitled *Observations on the Apocalypse of St. John*, consisting of three chapters, Sir Isaac treats in the *first* or introductory chapter, "concerning the time when the Apocalypse was written," which he conceives to have been during John's exile in Patmos, and before the Epistle to the Hebrews and the Epistles of Peter were written, which in his opinion have a reference to the Apocalypse. In the *second* he treats "of the relation which the Apocalypse has to the book of the law of Moses, and to the worship of God in the temple;" and in the *third*, "of the relation which the prophecy of John hath to those of Daniel, and of the subject of the prophecy."

Sir Isaac regards the prophecies of the Old and New Testament not as given to gratify men's curiosities, by enabling them to foreknow things, but that, after they were fulfilled, they might be interpreted by the event, and afford convincing arguments that the world is governed by Providence. He considers that there is so much of this prophecy already fulfilled, as to afford to the diligent student sufficient instances of God's Providence; and he adds, that "amongst the interpreters of the last age, there is scarce one of note who hath not made some discovery worth knowing, and thence it seems one may gather that God is about opening these mysteries. The success of others," he continues, "put me upon considering it, and if I have done anything which may be useful to following writers, I have my design." Such is a brief notice of this ingenious work, which is characterized by great learning, and marked with the sagacity of its distinguished author.[1]

The same qualities of Sir Isaac's mind are equally conspicuous in his *Historical Account of Two Notable Corruptions of Scripture.* This celebrated treatise relates to two texts in the Epistles of St. John and St. Paul. The first of these is in 1 John v. 7, "For there are three that bear record in heaven, the Father, the Word, and the Holy Ghost, and these three are one." This text he considers as a gross corruption of Scripture, which had its origin among the Latins, who interpreted the Spirit, Water, and Blood, to be the Father, Son, and Holy Ghost, in order to prove them one. With the same view Jerome inserted the Trinity in express words in his version. The Latins marked his variations in the margins of their books; and in the twelfth and following centuries, when the disputations of the schoolmen were at their height, the variation began to creep into the text in transcribing. After the invention of printing, it crept out of the Latin into the printed Greek, contrary to the authority of all the Greek manuscripts and ancient versions; and from the Venetian press it went soon after into Greece. After proving these positions, Sir Isaac gives the following paraphrase of this remarkable passage, which is printed in italics.

"*Who is he that overcometh the world, but he that believeth that Jesus is the Son of God*, that Son spoken of in the Psalms, where he saith, 'Thou art my Son; this day have I begotten thee.' *This is he that*, after the Jews had long expected him, *came*, first in a mortal body, *by* baptism of *water*, *and* then in an immortal one, by shedding his *blood* upon the cross, and rising again from the dead; *not by water only, but by water and blood;* being the Son of God, as well by his resurrection

[1] Voltaire, who probably never read this work, has erroneously stated that Sir Isaac explained the Revelations in the same manner as all those that went before him.

from the dead (Acts xiii. 33) as by his supernatural birth of the virgin (Luke i. 35). *And it is the Spirit* also *that*, together with the water and blood, *beareth witness* of the truth of his coming; *because the Spirit is truth;* and so a fit and unexceptionable witness. *For there are three that bear record* of his coming; *the Spirit* which he promised to send, and which was since shed forth upon us in the form of cloven tongues, and in various gifts; *the* baptism of *water*, wherein God testified 'this is my beloved Son;' *and the* shedding of his *blood*, accompanied with his resurrection, whereby he became the most faithful martyr, or witness, of this truth. *And these three*, the spirit, the baptism, and passion of Christ, *agree in* witnessing *one* and the same thing (namely, that the Son of God is come); and, therefore, their evidence is strong; for the law requires but two consenting witnesses, and here we have three: *and if we receive the witness of men, the* threefold *witness of God*, which he bare of his Son, by declaring at his baptism, 'this is my beloved Son,' by raising him from the dead, and by pouring out his Spirit on us, *is greater;* and, therefore, ought to be more readily received."

It appears from the introduction to this letter, that Locke, to whom it was addressed, had been reading the "discourses of some *late* writers on the subject,"[1] and had expressed to Newton a desire "to know the truth of that text of Scripture concerning the testimony of the three in heaven." Without noticing the views of his predecessors, Sir Isaac contents himself with referring to Luther, Erasmus, Bullinger, and Grotius, and some others, as "the more learned and quick-sighted men, who would not dissemble their knowledge" (of the corruption of this text), and to "the generality who were fond of the place for its making against heresy." In the last edition of his Bible,

[1] Among the writers here referred to, Father Simon was doubtless the most important. In his *Hist. Crit. du Texte du Nouv. Test.* chap. xviii. p. 203; and in his *Hist. Crit. des Versions du Nouv. Test.* chap. xiv., Rott. 1690, he has given the same opinion of the text as Newton.

published by himself, Luther had expunged the text as spurious, but in deference to popular opinion it was restored by his followers. Erasmus too, omitted it in his edition of the New Testament, published in 1516 and 1519,[1] but, as Porson informs us, having promised Lee that he would insert the passage in his text if it was found in a single Greek MS., he accordingly inserted it in his edition of 1522, after learning that it existed in a MS. which is now in Trinity College, Dublin. Dr. Clarke came to the conclusion, "that much stress ought not to be laid upon the passage in any question, because the sense of the Epistle was complete without it,"[2] and because it was not found in any MS. before the invention of printing, nor cited by any of the numerous writers in the Arian controversy; and Dr. Bentley read a public lecture to prove that the verse in question was spurious. Gibbon, in the third volume of his History, expressed the general opinion of biblical critics upon the subject; and Wetstein and Griesbach adopted the same views. In reply to these authors, Archdeacon Travis entered the field by attacking Gibbon in 1782, and subsequently Newton and Griesbach in 1786.[3] Michaelis considered it a sufficient answer to the English divine to say, that "he was indisputably half a century behindhand in critical knowledge;" and Porson, indignant at the presumption of his countryman, exposed his ignorance and errors in the celebrated letters which he addressed to him in 1788, 1789, and 1790.[4] In referring to these able letters, Sir Charles Lyell remarks, that "by them the question was for ever set at rest."[5] Had it been a question in science, it might

[1] In stating this fact, Sir Charles Lyell omits to mention the re-insertion of the text in the edition of 1522. He is mistaken in saying, after Porson, that Newton's Dissertation was written between 1690 and 1760 (a typographical error for 1700), as it was written in 1690, or much earlier, as we have shown.

[2] Clarke's *Works*, vol. iv. p. 121.

[3] In letters in the *Gent. Magazine*, reprinted and enlarged in 1784 and 1786.

[4] Five of these letters appeared in the *Gent. Magazine* for 1788, and were reprinted with some others, and entitled "*Letters to Mr. Archdeacon Travis*," &c. By R. Porson. Lond. 1790. 8vo. Pp. 406.

[5] *Second Visit to the United States*, vol. i. p. 122.

have been expected that presumptuous error, when once sternly refuted, would not dare to reappear ; but theological questions are never set at rest, and the very corruption of the sacred text which Sir Charles characterizes as having been "given up by every one who has the least pretension to scholarship and candour," has been defended in our own day by Dr. Burgess, Bishop of St. David's, and afterwards of Salisbury, with a boldness of assumption, and a severity of intolerance, unworthy of a Christian divine.[1]

The other notable corruption of Scripture discussed by Sir Isaac, is that which he charges the Greeks with having perpetrated in the text of St. Paul,[2] *Great is the mystery of godliness, God manifest in the flesh.* According to him this reading was effected by changing ὅ into ΘC, the abbreviation of Θεος, . . . whereas all the churches for the first four or five hundred years, and the authors of all the ancient versions, Jerome as well as the rest, read, 'Great is the mystery of godliness which was manifested in the flesh.' For this is the common reading of the Ethiopic, Syriac, and Latin versions to this day, Jerome's manuscripts having given him no occasion to correct the old vulgar Latin in this place."

After showing that the corruption in question took place in the sixth century, Sir Isaac thus sums up his arguments :—"The difference between the Greek and the ancient version puts it past dispute that either the Greeks have corrupted their MSS., or the Latins, Syrians, and Ethiopians their versions ; and it is more reasonable to lay the fault upon the Greeks than upon the other three, for these considerations :—It was easier for one nation to do it than for three to conspire,—it was easier to change a letter or two in the Greek than six words in the Latin. In the Greek the sense is obscure,—in the versions

[1] *Tracts on the Divinity of Christ*, pp. xc. 371, 372, Lond. 1820 ; and *Introduction to the Controversy on the disputed verse in St. John*, Salisbury, 1835, &c. An able reply to Dr. Burgess, said to be written by the Bishop of Ely, appeared in the *Quarterly Review*, March 1826, vol. xxxiii. p. 64. See *Notes and Queries*, vol. i. pp. 399 and 453.

[2] 1 Timothy iii. 16.

clear. It was agreeable to the interest of the Greeks to make the change, but against the interest of other nations to do it, and men are never false to their own interest. The Greek reading was unknown in the times of the Arian controversy, but that of the versions was then in use both among Greeks and Latins. Some Greek MSS. render the Greek reading dubious, but those of the versions, hitherto collated, agree. There are no signs of corruption in the versions, hitherto discovered, but in the Greek we have showed you particularly when, on what occasion, and by whom the text was corrupted."[1]

The view taken of this text by Sir Isaac has been defended by Dr. Clarke,[2] Whiston,[3] Semler,[4] Griesbach,[5] Wetstein, and others. In our own day it has been controverted, with much ability and learning, in an elaborate dissertation by Dr. Henderson,[6] who has not justified its retention as a portion of revealed truth.[7]

As the tendency of the *Historical Account*, &c., was to deprive the defenders of the doctrine of the Trinity of the aid of two leading texts, Sir Isaac Newton has been regarded by the Socinians and Arians, and even by some orthodox divines, as an Antitrinitarian; but this opinion is not warranted by anything which he has published.[8] "In the Eastern nations,"

[1] *Historical Account*, &c., Art. I. and XXIV., Newtoni *Opera*, tom. v. pp. 531, 548.

[2] *Works*, vol. iv. p. 47.

[3] *Memoirs*, p. 365.

[4] *Historical Collections* cited by Michaelis, vol. iv. p. 425.

[5] *Symbolæ Criticæ*, vol. i. p. 8.

[6] *The Great Mystery of Godliness incontrovertible, or Sir Isaac Newton and the Socinians Foiled*, &c. By E. Henderson, Professor of Divinity in Highbury College. Lond. 1730.

[7] The latest writers on the subject, although not Unitarian, namely, Dr. Davidson in his *Treatise on Biblical Criticism*, vol. ii. p. 382, Edin. 1852, and Dr. Tregelles in his *Account of the Printed Text of the Greek New Testament*, p. 226, Lond. 1854, have adopted the views of Sir Isaac.

[8] There are certainly, as Professor De Morgan has shown, two or three expressions in the Dissertation which a believer in the doctrine of the Trinity is not likely to have used; but while I freely make this admission, I think Mr. De Morgan will also admit that they would not justify us in considering Newton as an Antitrinitarian. They warrant us only to *suspect* his orthodoxy. See Professor De Morgan's *Life of Newton*, p. 113, note.

he says, "and for a long time in the Western, the faith subsisted without this text, and it is rather a danger to religion than an advantage to make it now lean upon a bruised reed. There cannot be better service done to the truth than to purge it of things spurious; and, therefore, knowing your prudence and calmness of temper, I am confident I shall not offend you by telling you my mind plainly, especially since it is no article of faith, no point of discipline, nothing but a criticism concerning a text of Scripture, which I am going to write about."

Although it is obvious that, in allowing his Dissertation to be published in Holland, Sir Isaac did not consider himself as supporting the Socinians or the Arians, yet it cannot be doubted that he was afraid of being known as the author of the work, and of holding the opinions which it advocates. The name of the author was never communicated to Le Clerc, but he no doubt learned it from the writings of Whiston,[1] who, after Newton's death, mentioned the Dissertation as his production. After the death of Le Clerc, Wetstein[2] placed Locke's copy of it in the Library of the Remonstrants, and endeavoured in vain to procure, from Newton's heirs, the parts that were deficient in the original.

It does not appear that Newton was charged with being an Arian during his lifetime. Whiston indeed tells us, that he "afterwards[3] found that Sir Isaac Newton was so hearty for the Baptists, as well as for the Eusebians or Arians, that he sometimes suspected these two were the two witnesses in the Revelations;" and Hopton Haynes, who was employed in the Mint, and who was himself a Humanitarian,[4] mentioned to Richard Baron,[5] that Newton held the same doctrine as him-

[1] *Authentic Records*, p. 1077. Lond. 1728.

[2] Prolegomena to his edition of the New Testament, p. 185. Amst. 1751.

[3] After 1712.—*Memoirs*, &c., p. 206.

[4] The Humanitarians believe in the humanity of our Saviour, and that he was not an object of prayer.

[5] "The Unitarian minister, Richard Baron," says Professor De Morgan, "who was a friend of Haynes, states the preceding as having passed in conversation between him and Haynes. The statement is made in the preface of the first volume of his collection of

self.[1] In so far as the opinions of Newton, Locke, and Clarke, all of whom were suspected of Arian tendencies, were hostile to the doctrine of the Trinity, they had substantial reasons for keeping them secret. In the Toleration Act passed in 1688,[2] before Newton had sent his Dissertation to Locke, an exception was made of those who wrote against "the doctrine of the blessed Trinity;" and in the Act for the Suppression of Blasphemy and Profaneness,[3] it was provided, that whoever "by printing, teaching, or advisedly speaking, denied any one of the persons of the Holy Trinity to be God," should, "for the first offence, be disabled to have any office or employment, or any profit appertaining thereunto." The expulsion of Whiston from the University of Cambridge in 1711, for holding Arian tenets, though the Queen did not confirm the censure passed by the Convocation,[4] was yet a warning to Antitrinitarians of every class who either held office, or were desirous of holding it, to refrain from the public expression of their opinions; and we have no doubt that Newton was influenced by motives of this kind when he desired Locke "to stop the translation and impression of his papers," and mentioned "his design to suppress them."[5]

tracts, called 'A Cordial for Low Spirits' (three vols. Lond. 1763, edit. 3d, 12mo), published under the name of Thomas Gordon. This is not primary evidence like that of Whiston, and it loses force by the circumstance, that in the posthumous work which Mr. Haynes left on the disputed points (and which was twice printed), there is no allusion to it."—*Life of Newton*, p. 110, note.

[1] The author of the *Life of Newton*, in the *Biographia Britannica*, vol. v. p. 3241, says that Newton would not suffer Whiston to be a member of the Royal Society, because he had represented him as an Arian, and, as if to prove this, he refers to Whiston's *Memoirs*, which contain no such statement. Whiston himself assigns another "reason of Sir Isaac Newton's unwillingness to have him a member," namely, "that he was afraid of him the last thirteen years of his life;" but the reason which Whiston assigned to Halley, who asked him "why he was not a member of the Society," was, "Because they durst not choose a heretick."—See Whiston's *Memoirs*, edit. 1749, pp. 206, 292, 293.

[2] Act, 1 William and Mary, 1688, chap. xviii., sect. 17.

[3] Act, 9 & 10 William III., 1698, chap. xxxii.

[4] Burnet's *History of his own Times*, vol. vi. p. 53, 8vo. 1833.

[5] In suppressing these papers, Sir Isaac certainly did not "deliberately suppress his

Although a traditionary belief has long prevailed that Newton was an Arian,[1] yet the Trinitarians claimed him as a friend, while the Socinians, by republishing his *Historical Account*, &c., under the title of "Sir Isaac Newton on the Trinitarian Corruptions of Scripture,"[2] wished it to be believed that he was a supporter of their views. That he was not a Socinian is proved by his avowed belief that our Saviour was the object of "worship among the primitive Christians," and that he was "the Son of God, as well by his Resurrection from the dead, as by his supernatural birth of the Virgin." "He animadverts, indeed," as Dr. Henderson observes,[3] "with great freedom, and sometimes with considerable asperity, on the orthodox; but it does not appear that this arose from any hostility to their views respecting the doctrine of the Trinity, or that it was opposed to anything beside the unfair mode in which he conceived they had treated one or two passages of Scripture, with a view to the support of that doctrine."

Influenced by similar views, and in the absence of all direct evidence, I had no hesitation, when writing the Life of Sir Isaac Newton in 1830, in coming to the conclusion that he was a believer in the Trinity;[4] and in giving this opinion on the creed of so great a man, and so indefatigable a student of Scripture, I was well aware that there are various forms of Trinitarian truth, and various modes of expressing it, which have been received as orthodox in the purest societies of the

opinions," as Dr. Burgess has stated. See Professor De Morgan's *Life of Newton*, p. 115. There is abundance of evidence that he never abandoned the opinions maintained in these papers.

[1] "Newton's religious opinions," says Dr. Thomson, "were not orthodox; for example, he did not believe in the Trinity. This gives us the reason why Horsley, the champion of the Trinity, found Newton's papers unfit for publication; but it is much to be regretted that they have never seen the light."—*Hist. Royal Society*, p. 284.

[2] Dr. Henderson's *Great Mystery of Godliness*, &c., p. 3.

[3] *Ibid.* p. 2.

[4] M. Biot had previously arrived at the same opinion. "There is absolutely nothing," he says, "in the writings of Newton which can justify, or even authorize the conjecture that he was an Antitrinitarian."—*Biog. Univ.* tom. xxxi. p. 190.

Christian Church. It may be an ecclesiastical privilege to burrow for heresy among the obscurities of thought, and the ambiguities of language, but in the charity which thinketh no evil, we are bound to believe that our neighbour is not a heretic till the charge against him has been distinctly proved. Truth has no greater enemy than its unwise defenders, and no warmer friends than those who, receiving it in a meek and tolerant spirit, respect the conscientious convictions of others, and seek, in study and in prayer, for the best solution of mysterious and incomprehensible revelations. If the HIGHEST authority has assured us "*that no man knoweth the Son but the Father*," the pretenders to such knowledge impiously presume to be *more than man*.[1]

When I examined in 1836 the manuscripts of Sir Isaac Newton at Hurtsbourne Park, I found various theological papers, some of which were so carefully written, and others so frequently copied, that they must have been intended for publication. We have already seen[2] that Craig, the friend of Newton, urged Conduitt to give these writings to the world. His own niece, Mrs. Conduitt, resolved to publish them herself, "if God granted her life," but "as she might be snatched away before she had leisure to undertake so great a work," she made a codicil to her will,[3] charging her executors to submit "them

[1] In order to correct a very grave misrepresentation by Dr. Burgess, Bishop of Salisbury, of the way in which this subject was treated in my former Life of Newton, I am obliged to insert in APPENDIX, No. XXVI. two letters from the Bishop.

[2] See page 250 of this volume.

[3] The following is a copy of the codicil which the Rev. Jeffery Ekins has been so kind as to communicate to me:—"I, Catherine Conduitt, do make and appoint this a Codicil to my last Will and Testament. Whereas, I have in my custody severall Tracts written by Sir Is. Newton, and which I propose to print if God grant me life; but as I may be snatched away before I can have leisure to undertake so great a work, towards publishing of which I design to ask the help of learned men, I will and appoint, and ordain, that my Executor do lay all the tracts relating to Divinity before Dr. Sykes, and in hopes he will prepare them for the press. There are two critical pieces, one on the three *that bear Record in Heaven*, and another upon the Text who thought it *not robbery*, &c., which I will have printed, and there's a piece called Paradoxical Questions concerning Athanasius, another the History of the Creed, or criticism on it, and a Church History

to Dr. Sykes, in hopes that he will prepare them for the press." The manuscripts referred to are—

1. The *Historical Account*, &c., already published.
2. Paradoxical Questions concerning Athanasius.
3. A History of the Creed.
4. A Church History complete.[1]
5. Many Divinity tracts.

Mr. Conduitt died a few months after the date of this codicil, and Mrs. Conduitt in January 1739, and there is reason to believe that the papers were never put into the hands of Dr. Sykes. After the marriage of Miss Conduitt to Mr. Wallop, afterwards Lord Lymington, the manuscripts went into their possession, and some of them, including the *Historical Account*, were given by Lady Lymington to her executor, Mr. Jeffery Ekins, from whom they passed successively into the hands of the Dean of Carlisle, the Rector of Morpeth, and the Rev. Jeffery Ekins, rector of Sampford, who now possesses them.

The most complete of the manuscripts above enumerated, is the one entitled *Paradoxical Questions concerning the morals and actions of Athanasius and his Followers.*[2] It consists of sixteen questions, and possesses a very considerable interest.

compleat, and many more Divinity Tracts, all of them I ordain shall be printed and published, so as they be done with care and exactness; and whatever profit may arise from the same, my dear Mr. Conduitt has given a bond of £2000, to be responsible to the seven nearest of kin to Sir Is. Newton. Therefore the papers must be carefully kept, that no copys may be taken and printed, and Dr. Sykes desired to peruse them here, otherways if any accident comes to them the penalty of the Bond will be levy'd. As the labour and sincere search of so good a Christian and so great a genius, may not be lost to the world, I do charge my Executor to do as I hereby ordain. Witness my hand and seal, the 26 of Jan. 1737. "CATHERINE CONDUITT."

[1] In a "Catalogue taken of Sir Isaac Newton's MSS., October 15th and 16th, in the year 1777, by William Mann Godschall, Esq., and the Rev. Dr. Horsley," no such manuscript is mentioned. The only MS. of this kind is one of two pages distinctly written and entitled CHAP. VII. of the Rise of the Roman Catholic Church or Ecclesiastical Dominion.

[2] The manuscript of this work, now before me, is beautifully written in Sir Isaac's own hand, and extends to sixty-two folio pages. It wants the last leaf. I have seen at Hurtsbourne Park a copy in another hand, distinctly written as if for publication. In the Catalogue above mentioned of Newton's MSS. two copies of this MS. are mentioned

"QUEST. 1. Whether the ignominious death of Arius in a boghouse was not a story feigned and put about by Athanasius above twenty years after his death?"

In answer to this question, Newton shows that though Athanasius pretended to have received this account of Arius's death, and of his dying out of communion, from Macarius, yet he invented it himself, and circulated it, "that the miracle of his death being known, it will no longer be doubted whether the Arian heresy be odious to God or not."

"QUEST. 2. Whether the Meletians deserved that ill character which Athanasius gave them?"

The charge against the Meletians that they were excommunicated for crimes, Sir Isaac considers to be a fiction invented by Athanasius in retaliation for his having been tried at the instance of Inschyras, a Meletian presbyter, and condemned by the Council of Tyre for having broken the communion cup of Inschyras, demolished his church, and afterwards killed Arsenius, the successor of Meletus.

"QUEST. 3. Whether the Council of Tyre and Jerusalem was not an orthodox authentic council bigger than that of Nice?"

Although this council received Arius into communion after he had "disowned the things for which he had been condemned at Nice, and excommunicated Athanasius," Sir Isaac endeavours to show, with great ingenuity and force of argument, that it was not an Arian council—that it did not profess Arianism, and that it was a full council, and "as authentic as any Greek council ever was or could be since the Apostles' days, they being in communion with the Church Catholic, and legally convened by the letters of Constantine the Great."

"QUEST. 4. Whether it was a dead man's hand in a bag, or the dead body of Arsenius, which was laid before the Council at Tyre to prove that Arsenius was dead?"

"QUEST. 5. Whether it was Arsenius alive, or only his

in one place, and in another part of the Catalogue another copy is mentioned as *complete*, showing that the other two were not so.

letter which Athanasius produced in the Council of Tyre, to prove that he was not dead?"

"QUEST. 6. Whether the story of producing the dead man's hand, and the living Arsenius, in the council of Tyre, was not feigned by Athanasius about twenty-five years after the time of the council?"

In answering these three questions together, Sir Isaac shows that the dead body of Arsenius was, after exhumation, produced before the Council of Tyre, to prove that he was murdered by Athanasius, who was found guilty and banished as the murderer. In defence of himself, Athanasius invented the story that it was only a dead man's hand that was produced before the council, and that he refuted the charge by producing Arsenius alive.

"QUEST. 7. Whether the letter of Pinnes for proving Arsenius to be alive was not feigned by Athanasius at the same time with the story of the dead man's hand?"

In order to defend Athanasius, a monk confessed that Arsenius had been concealed at Hypseles, and had been sent out of the way to the lower parts of Egypt. Sir Isaac endeavours to show the incorrectness of this story.

"QUEST. 8. Whether the letter of Arsenius was not feigned by Athanasius before the convening of the Council of Tyre?"

After an ingenious criticism on Arsenius' letter, Sir Isaac concludes that it is a forgery.

"QUEST. 9. Whether the letter of Inschyras was not feigned by Athanasius?"

This penitential letter, for having prosecuted Athanasius, addressed to the Blessed Pope Athanasius, is suspected, on very ingenious grounds, to be a forgery.

"QUEST. 10. Whether the recantation of Valens and Ursatius was not feigned by the friends of Athanasius?"

These recantations are supposed with good reason to be forgeries.

"QUEST. 11. Whether Athanasius was falsely accused, or did falsely accuse Eusebius of adultery before the Council of Tyre ?"

Athanasius is said to have sent a woman to accuse Eusebius of adultery, in the hope of such a tumult being raised that he might escape being tried. But when Eusebius asked her if she knew the man, she answered that she would not be so senseless as to accuse such men. The friends of Athanasius afterwards inverted this story, as if the woman had been hired by the Eusebians to accuse Athanasius.

"QUEST. 12. Whether Athanasius did sincerely acquit himself of the crime of breaking the communion cup of Inschyras ?"

This question is answered in the negative, and Athanasius' ingenious artifice to explain away the charge is well exposed.

"QUEST. 13. Whether Athanasius was not made Bishop of Alexandria by sedition and violence against the Canons of that Church ?"

The Bishops who ordained him, after resisting his importunities "for many days together," and having been kept prisoner in a church by a mob of Athanasius's party, were obliged to ordain him. He was only twenty-five years of age, so that "the Meletians used to cry, O wickedness ! he a bishop or he a boy ?"

"QUEST. 14. Whether Athanasius was not justly deposed by the Council of Tyre ?"

The justice of the sentence is proved by seven different arguments.

"QUEST. 15. Whether Athanasius was not seditious ?"

This question is answered in the affirmative by an examination of his "Epistle to the Orthodox of all Regions," and a letter entitled "The People of Alexandria to the Catholic Church, which is under Athanasius the most reverend Bishop."

"QUEST. 16. Whether Constantius persecuted the Athanasians for religion, or only punished them for immorality ?"

In answering this question Sir Isaac shows that Constantius and his Bishops, in place of persecuting the Athanasians, treated them with the greatest moderation, and that their martyrs "perished by the sword in resisting the higher powers." He shows that Hilary, who courted martyrdom by insulting Constantius, and was thus guilty of the capital crime of *Læsa Majestas*, was released from banishment by the Emperor, and allowed to return to his own country. After quoting the favourable opinions of the Emperor given by his enemies, he concludes with the following character of him :—"In short, the virtues of this Emperor were so illustrious, that I do not find a better character given of any Prince for clemency, temperance, chastity, contempt of popular fame, affection to Christianity, justice, prudence, princely carriage, and good government, than is given to him even by his very enemies. He kept up the imperial dignity of his person to the height, and yet reigned in the hearts of his people, and swayed the world by their love to him, so that no Prince could be farther from deserving the name of a persecutor."

Among the other theological manuscripts of Sir Isaac, there are none so distinctly written as the *Paradoxical Questions;* but there are so many copies of some of them, that it can scarcely be doubted that they were thus repeatedly corrected for publication. The fact, indeed, of Sir Isaac having, previous to his death, burned many of his letters and papers, and left these theological writings behind him, makes it more than probable that he had no desire to suppress his opinions.

The most remarkable of these MSS. is one entitled *Irenicum, or Ecclesiastical Polity tending to Peace.*[1] It consists of twenty *Positions*, or *Theses*, in which the doctrines of Christianity, the government of the Church, and its relations to the State, are described in a few brief and intelligible paragraphs. As the production of a great and good man who had studied the Scriptures and the history of the Church without any sectarian predilections, it cannot but be interesting to the Christian student.[2]

[1] There are four copies of this MS. with the title *Irenicum*, but only one with the full title given in the text.

[2] See APPENDIX, No. XXVII.

In a paper of a few pages, entitled *A Short Scheme of the True Religion*, in which religion is described as partly fundamental and immutable, and partly circumstantial and mutable, he treats of *Godliness*, *Atheism*, *Idolatry*, and *Humanity*, or our duty to man. "Opposite to godliness," he says, "is Atheism in profession, and idolatry in practice. Atheism is so senseless and odious to mankind, that it never had many professors. Can it be by accident that all birds, beasts, and men have their right side and left side alike shaped (except in their bowels), and just two eyes, and no more, on either side of the face; and just two ears on either side the head, and a nose with two holes; and either two fore-legs, or two wings, or two arms on the shoulders, and two legs on the hips, and no more? Whence arises this uniformity in all their outward shapes but from the counsel and contrivance of an Author? Whence is it that the eyes of all sorts of living creatures are transparent to the very bottom, and the only transparent members in the body, having on the outside a hard transparent skin, and within transparent humours, with a crystalline lens in the middle, and a pupil before the lens, all of them so finely shaped and fitted for vision, that no artist can mend them? Did blind chance know that there was light, and what was its refraction, and fit the eyes of all creatures, after the most curious manner, to make use of it? These, and suchlike considerations, always have, and ever will prevail with mankind, to believe that there is a Being who made all things, and has all things in his power, and who is therefore to be feared."

The section on idolatry is concluded with the following summary:—"We are, therefore, to acknowledge one God, infinite, eternal, omnipresent, omniscient, omnipotent, the Creator of all things, most wise, most just, most good, most holy. We must love him, fear him, honour him, trust in him, pray to him, give him thanks, praise him, hallow his name, obey his commandments, and set times apart for his service, as we are directed in the Third and Fourth Commandments, for this is the love of God that we keep his commandments, and his commandments are not grievous, 1 John v. 3. And these things we must do not to any mediators between him and us, but to him alone, that he may give his angels charge over us, who, being our fellow-servants, are pleased with the worship which we give to their God. And this is the first and the principal part of religion. This always was, and always will be, the religion of all God's people, from the beginning to the end of the world."

In another manuscript, *On our Religion to God, to Christ, and the Church*, he treats more fully of some of the theses in the *Irenicum*, but his doctrinal opinions are more conspicuous in the following twelve articles, which have no title :—

ART. 1. There is one God the Father, ever living, omnipresent, omniscient, almighty, the maker of heaven and earth, and one Mediator between God and man, the man Christ Jesus.

ART. 2. The Father is the invisible God whom no eye hath seen, or can see. All other beings are sometimes visible.

ART. 3. The Father hath life in himself, and hath given the Son to have life in himself.

ART. 4. The Father is omniscient, and hath all knowledge originally in his own breast, and communicates knowledge of future things to Jesus Christ; and none in heaven or earth, or under the earth, is worthy to receive knowledge of future things immediately from the Father but the Lamb. And, therefore, the testimony of Jesus is the spirit of prophecy, and Jesus is the Word or prophet of God.[1]

ART. 5. The Father is immovable, no place being capable of becoming emptier or fuller of him than it is by the eternal ne-

[1] In the Catalogue of Newton's MSS. by Dr. Horsley, he mentions a paper "of twelve short paragraphs in English, which seems to have been the beginning of a treatise on the divinity of our Saviour." In the *fourth* paragraph he adds, "The Arian interpretation of the word *Logos*, in St. John's Gospel, is sustained, but the Socinian doctrine is denied." This was probably another copy of the articles given in the text.

cessity of nature. All other beings are movable from place to place.

Art. 6. All the worship (whether of prayer, praise, or thanksgiving) which was due to the Father before the coming of Christ, is still due to him. Christ came not to diminish the worship of his Father.

Art. 7. Prayers are most prevalent when directed to the Father in the name of the Son.

Art. 8. We are to return thanks to the Father alone for creating us, and giving us food and raiment and other blessings of this life, and whatsoever we are to thank him for, or desire that he would do for us, we ask of him immediately in the name of Christ.

Art. 9. We need not pray to Christ to intercede for us. If we pray the Father aright, he will intercede.

Art. 10. It is not necessary to salvation to direct our prayers to any other than the Father in the name of the Son.

Art. 11. To give the name of God to angels or kings, is not against the First Commandment. To give the worship of the God of the Jews to angels or kings, is against it. The meaning of the commandment is, Thou shalt worship no other God but me.

Art. 12. To us there is but one God, the Father, of whom are all things, and one Lord Jesus Christ, by whom are all things, and we by him.—That is, we are to worship the Father alone as God Almighty, and Jesus alone as the Lord, the Messiah, the Great King, the Lamb of God who was slain, and hath redeemed us with his blood, and made us kings and priests.

On the subject of the Trinitarian controversy, I have found a manuscript of fourteen queries, which may throw some light on the opinions of its author, and which I have, therefore, given in the Appendix.[1]

Although Sir Isaac, in his observations on the Prophecies of

[1] See Appendix, No. XXVIII.

Daniel, has shown how the Church of Rome, as the eleventh horn of the fourth beast, rooted up three of his first horns, the Exarchate of Ravenna, the kingdom of the Lombards, and the dukedom of Rome, and thus rose up as a temporal power, he has not given any account of the steps by which the Bishop of Rome obtained the rank of the Universal Bishop. In a paper of eight queries, containing his views on this subject, he states, that after the death of Constantius in A.D. 341, he began to usurp the universal Bishopric; that the Emperor Constantius abolished Popery in A.D. 361; and that the Emperor Gratian, in 379, restored, by his edict, the universal Bishopric of Rome over all the West.

The tendency of the Church of England to relapse into Romish superstition seems to have shown itself in the time of Newton, and to have induced him to take steps to counteract it. It is probable that he had been requested by influential persons, both in the Church and in the State, to suggest a legislative measure for correcting an evil which at that time was as dangerous to the State as it was hostile to the articles of the Church and the fundamental truths of Christianity. This proceeding must have taken place at the accession of the House of Hanover in 1714, as will appear from the following draught of an Act of Parliament drawn up by Sir Isaac, and in his own handwriting:—

"Whereas of late years, some opinions have been propagated by superstitious men among the Christians of the Church of England, to break all communion and friendship with the Protestant churches abroad, and to return into the communion of the Church of Rome; such as are the opinions, that the Church of Rome is a true church, without allowing her to be a false church in any respect, and that the Protestant churches abroad are false churches, and that they have no baptism, and by consequence are no Christians, and that the Church of England is in danger, meaning, by the succession of the House of Hanover. For preventing the mischiefs which may ensue upon such dangerous, uncharitable, and unchristian principles, be it enacted,—

"That the following declaration shall be made and subscribed in open court in the Quarter Sessions next after . . . . by all persons.

"We, whose names are underwritten, do solemnly, and without all equivocation or mental reservation, acknowledge and declare that we do sincerely believe that the Church of Rome is, in doctrine and worship, a false, uncharitable, and idolatrous church, with whom it is not lawful to communicate; and that the churches of the Lutherans and Calvinists abroad are true churches, with whom we may lawfully communicate, and that their baptism is valid and authentic; and that the Church of England is in no danger by the succession of the House of Hanover in the throne of the kingdom of Great Britain."

It is interesting to observe the coincidence of the religious views of Sir Isaac Newton with those of John Locke, his illustrious contemporary and friend. Though, like Newton, he lived in communion with the Church of England, "yet it is obvious," as Lord King says, "from an unpublished reply to a work of Dr. Stillingfleet's, that he entertained a strong opinion that the exclusive doctrines of the Church of England were very objectionable—that he thought them much too narrow and confined, and that he wished for a much larger and easier comprehension of Protestants." In a paper dated 1688, and apparently drawn up for the guidance of a religious society when he was in Holland,[1] we find the following noble article, which Newton would have countersigned, and which, without having adopted the peculiar opinions of these distinguished men, we regard as at once the essence and the bulwark of Protestant truth.

"If any one find any doctrinal parts of Scripture difficult to be understood, we recommend him, 1*st*, The study of the

[1] This paper, entitled *Pacific Christians*, and containing eleven articles, is published in King's *Life of Locke*, vol. ii. pp. 63-67. Edit. 1830.

Scriptures in humility and singleness of heart. 2*d*, Prayer to the Father of lights to enlighten him. 3*d*, Obedience to what is already revealed to him, remembering that the practice of what we do know is the surest way to more knowledge; our infallible guide having told us, if any man will do the will of him that sent me [his will], he shall know of the doctrine, John vii. 17. 4*th*, We leave him to the advice and assistance of those whom he thinks best able to instruct him; no men, or society of men, having any authority to impose their opinions or interpretations on any other, the meanest Christian; since, in matters of religion, every man must know and believe and give an account for himself."

Interesting as any opinion of Newton's must be, on every subject to which he has directed his transcendent powers, there is one prophetic of the future destiny of man which has a peculiar value, and with which we may appropriately close our notice of his theological writings.[1] Although Sir Isaac believed in a plurality of worlds, he has nowhere given it as his opinion that the worlds beyond our own are to be the residence of the blessed. This opinion, however, resting on Scripture and science, and combining what is revealed with what is demonstrated, he has distinctly developed in the following passage:—

"God made and governs the world invisibly, and hath commanded us to love and worship him, and no other God; to honour our parents and masters, and love our neighbours as ourselves; and to be temperate, just, and peaceable, and to be merciful even to brute beasts. And by the same power by which he gave life at first to every species of animals, he is able to revive the dead, and hath revived Jesus Christ our Redeemer, who hath gone into the heavens to receive a king-

[1] The writer of the Life of Newton in the *Biographia Britannica* mentions an unfinished work entitled *Lexicon Propheticum*, to which was subjoined a Latin dissertation *On the Sacred Cubit of the Jews*, translated and printed in 1737, by Dr. Birch, in vol. ii. of the Miscellaneous Works of Mr. John Greaves. I have not seen any such MS., and it is not mentioned in Dr. Horsley's Catalogue. The paper on the Cubit may be included in "Latin Papers relating to the Jewish Temple," noticed by Dr. Horsley.

dom, and prepare a place for us, and is next in dignity to God, and may be worshipped as the Lamb of God, and hath sent the Holy Ghost to comfort us in his absence, and will at length return and reign over us, invisibly to mortals, till he hath raised up and judged all the dead, and then he will give up his kingdom to the Father, and carry the blessed to the place he is now preparing for them, and send the rest to other places suitable to their merits. *For in God's house (which is the universe), are many mansions, and he governs them by agents which can pass through the heavens from one mansion to another. For if all places to which we have access are filled with living creatures, why should all these immense spaces of the heavens above the clouds be incapable of inhabitants?*"[1]

Such is a brief view of the theological manuscripts of Sir Isaac Newton. With the exception of the "Paradoxical Questions concerning Athanasius," none of them were prepared for the press, and there can be no doubt that his representatives, and also Dr. Horsley, exercised a wise discretion in not giving them formally to the world. Had Sir Isaac found leisure to complete the works of which we have but imperfect fragments, they would have displayed his sagacity and varied erudition, and would have exhibited more correctly and fully than the specimens we have given, his opinions on the great questions of Christian doctrine and ecclesiastical polity.

It is scarcely a matter of surprise that sceptical writers should have spoken disrespectfully of the theological writings of a mathematician and philosopher, but it has surprised us that other authors should have regarded the study of the Scriptures as incompatible with scientific research. When Voltaire asserted that Sir Isaac explained the Prophecies in the same manner as those who went before him, he only exhibited his ignorance of what Newton wrote, and of what others had

[1] I have ventured to state and illustrate views similar to these in the last chapter "On the Future of the Universe," of a little volume entitled *More Worlds than One.* 1854.

written; and when he stated that Newton composed his Commentaries on the Apocalypse to console mankind for the great superiority which he had over them, he showed but the emptiness of the consolation to which scepticism aspires.

We have few examples, indeed, of truly great men pursuing simultaneously their own peculiar studies and the critical examination of the Scriptures. The most illustrious have been the ornaments of our own land, and England may well be proud of having had Napier, and Milton, and Locke, and Newton, for the champions both of its faith and its Protestantism. From the study of the material universe—the revelation of God's wisdom, to the study of his holy word—the revelation of his will, the transition is neither difficult nor startling. From the homes of planetary life to the homes of its future destiny the mind passes with a firm and joyous step, and it is only when scepticism or intellectual pride has obstructed the path, that the pilgrim falters in his journey, or faints by the way.

When a philosopher like Newton first directs his energies to the study of the material universe, no indications of order attract his notice, and no proofs of design call forth his admiration. In the starry firmament he sees no bodies of stupendous magnitude, and no distances of immeasurable span. The two great luminaries appear vastly inferior in magnitude to many objects around him, and the greatest distances in the heavens seem even inferior to those which his own eye can embrace on the surface of the earth. The planets, when observed with care, are seen to have a motion among the fixed stars, and to vary in their magnitude and distances, but these changes appear to follow no law. Sometimes they move to the east, sometimes to the west, passing the meridian sometimes near and sometimes far from the horizon, while at other times they are absolutely stationary in their path. No system, in short, appears, and no general law seems to direct their motions. By the observations and inquiries of astronomers, however, during successive ages, a regular system has been recognised in this chaos of moving bodies, and the magnitudes, distances, and revolutions of every planet which composes it have been determined with the most extraordinary accuracy. Minds fitted and prepared for this species of inquiry are capable of appreciating the great variety of evidence by which the truths of the planetary system are established ; but thousands of individuals, and many who are highly distinguished in other branches of knowledge, are incapable of understanding such researches, and view with a sceptical eye the great and irrefragable truths of astronomy.

That the sun is stationary in the centre of our system—that the earth moves round the sun, and round its own axis—that the diameter of the earth is 8000 miles, and that of the sun *one hundred and ten* times as great ; that the earth's orbit is 190 millions of miles in breadth ; and that, if this immense space were filled with light, it would appear only like a luminous point at the nearest fixed star—are positions absolutely unintelligible and incredible to all who have not carefully studied the subject. To millions of our species, then, the Great Book of Nature is absolutely sealed, though it is in the power of all to unfold its pages, and to peruse those glowing passages which proclaim the power and wisdom of its Author.

The Book of Revelation exhibits to us the same peculiarities as that of Nature. To the ordinary eye it presents no immediate indications of its divine origin. Events apparently insignificant—supernatural interferences seemingly unnecessary—doctrines almost contradictory—and prophecies nearly unintelligible, occupy its pages. The history of the fall of man—of the introduction of moral and physical evil—the prediction of a Messiah—the advent of our Saviour—his precepts—his miracles—his death—his resurrection—the gift of tongues—and the subsequent propagation of his religion by the unlettered fishermen of Galilee, are each a stumbling-block to the wisdom of this world. The youthful and vigorous mind, when summoned from its early studies to the perusal of the Scriptures, turns from them with disappointment. It recognises in the sacred page no profound science—no secular wisdom—no disclosures of Nature's secrets—no palpable impress of an Almighty hand. But, though the system of revealed truth which the Scriptures contain is like that of the universe concealed from common observation, yet the labours of centuries have established its divine origin, and developed in all its order and beauty the great plan of human restoration. In the chaos of its incidents, we discover the whole history of our species, whether it is delineated in events that are past, or shadowed forth in those which are to come,—from the creation of man and the origin of evil, to the extinction of his earthly dynasty, and the commencement of his immortal career.

The antiquity and authenticity of the books which compose the sacred canon—the fulfilment of its prophecies—the miraculous propagation of the gospel—have been demonstrated to all who are capable of appreciating the force of historical evidence; and in the poetical and prose compositions of the inspired authors, we discover a system of doctrine, and a code of morality, traced in characters as distinct and legible as the most unerring truths in the material world.—False systems of religion have indeed been deduced from the sacred record—as false systems of the universe have sprung from the study of the book of nature; but the very prevalence of a false system proves the existence of one that is true; and though the two classes of facts necessarily depend on different kinds of evidence, yet we scruple not to say that the Copernican system is not more demonstrably true than the system of theological truth contained in the Bible. If men of high powers, then, are still found, who are insensible to the evidence which has established the system of the universe, need we wonder that there are others who resist the effulgent evidence which sustains the strongholds of our faith?

If such be the character of Christian truth, we need not be surprised that it was embraced and expounded by such a genius as Sir Isaac Newton. Cherishing its doctrines, and leaning on its promises, he felt it his duty, as it was his delight, to apply to it that intellectual strength which had successfully surmounted the difficulties of the material universe. The fame which that success procured him he could not but feel to be the breath of popular applause, which administered only to his personal feelings ; but the investigation of the sacred mysteries, while it prepared his own mind for its final destiny, was calculated to promote the spiritual interests of thousands. This noble impulse he did not hesitate to obey, and by thus uniting philosophy with religion, he dissolved the league which genius had formed with scepticism, and added to the cloud of witnesses the brightest name of ancient or of modern times.[1]

> What wonder then that his devotion swelled
> Responsive to his knowledge ! for could he,
> Whose piercing mental eye diffusive saw
> The finished university of things,
> In all its order, magnitude, and parts,
> Forbear incessant to adore that power
> Who fills, sustains, and actuates the whole ?
>
> THOMSON.

[1] The piety of Newton was so well known and appreciated by his friends, that he was occasionally consulted about their spiritual state. We have already seen, in page 2 of this volume, that an eminent mathematician "thanked God that his soul was extremely quiet, in which Newton had the chief share ;" and, in the following letter from Dr. Morland (the brother, we believe, of Sir Samuel), who was elected a Fellow of the Royal Society in 1703, we find him acting the same benevolent part :—

"SIR,—I have done, and will do my best while I live, to follow your advice, to repent and believe. I pray often as I am able, that God would make me sincere and change my heart. Pray write me your opinion whether, upon the whole, I may die with comfort. This can do you no harm—written without your name. God knows I am very low and uneasy, and have but little strength.

"Your most humble servant,

"JOS. MORLAND.

"Pray favour me with one line, because when I parted I had not your last word to me, you being in haste.

"Direct for Dr. MORLAND, in Epsom, Surrey."

CHAPTER XXV.

Sir Isaac's early study of Chemistry—And of Alchemy, as shown in his letter to Mr. Aston—His Experiments on the Metal for Reflecting Telescopes—His chemical pursuits between 1683 and 1687—His Researches on the Quantities and Degrees of Heat, written after his illness in 1693—His Experiments on the Rarefaction of Air, Water, and Lintseed Oil—His paper on the nature of Acids—The results of his Chemical Researches, published among his queries in his Optics—His opinion on Fire and Flame—On Elective Attractions—Manuscript works on Alchemy left among Sir Isaac's papers—A belief in Alchemy prevalent in Newton's time—Boyle, Locke, and Newton studied Alchemy as a science—Others for fraudulent purposes.

Although Sir Isaac had directed his attention to chemistry at various periods of his life, yet his name has not been associated with any striking discovery in the science. I have therefore reserved an account of such of his chemical researches as have any real value, for the same chapter in which it is necessary to speak of his labours as an alchemist. It was doubtless during his residence with Mr. Clark, the apothecary at Grantham, that he first witnessed, and acquired a taste for, the practical operations of chemistry. In his earliest note-books there are copious extracts from Boyle and other chemical writers, and in 1669, when he wrote his interesting letter to Francis Aston,[1] we see very distinctly the great interest he felt in chemistry, and the peculiar bent of his mind to a belief in the doctrines of alchemy. He requests his young friend to observe the extraction of metals out of their ores, and the processes for refining them, and to notice as "the most luciferous, and many times lucriferous experiments in philosophy," "any transmutations out of their own species into another—of iron

[1] See Vol. I. Appendix No. I. pp. 365, 366.

into copper, and any metal into quicksilver, or of one salt into another, or into an insipid body, &c." He returns to the same topic as he proceeds, and asks him to inquire if at the gold, copper, and iron mines at Schemnitz they change iron into copper by a particular process which he describes as done in Italy and other places. He refers also to a method used in various places in Germany of obtaining gold from its solution in the water and rivers by laying mercury in the stream, and straining the mercury through leather so as to leave the gold behind. He concludes this remarkable letter by asking his friend to inquire when in Holland about one Borry, who always went clothed in green, and who had escaped from prison, into which he had been thrown by the Pope, in order to "extort secrets of great worth both as to medicine and profit."

At the time when this letter was written, Newton was occupied with the construction of his reflecting telescope, and he was therefore led to institute new experiments on the alloys of metals, and the changes which they underwent by their union with other bodies. In his letter to Oldenburg in 1671-2,[1] he has mentioned the general results of these experiments, which to a great extent have been the guide of all who have followed him in the construction of metallic specula for reflecting telescopes. He has left behind him, however, a full account of the composition of his specula, and of the method of founding them, in a paper carefully written in his own hand, and entitled *De Metallo ad conficiendum speculum componendo et fundendo*.[2]

During the four years, from 1683 to 1687, the period in which the *Principia* was composed, he never abandoned his chemical experiments. Dr. H. Newton, who was his amanuensis during that time, tells us that during six weeks in spring and autumn he was so constantly occupied in his laboratory

[1] Jan. 18th and 19th, 1671-2, Newtoni *Opera*, tom. iv. pp. 273, 274. I find records of experiments in Dec. 10-19, 1678, and also in 1679, 1680.

[2] See APPENDIX, No. XXIX.

that he was scarcely out of it either night or day—that the fire in it was almost always burning—that it was well furnished with chemical materials and apparatus, and that the transmuting of metals was his chief design.

At a later period, in 1692, he was engaged in chemical experiments, as appears from his correspondence with Locke;[1] and at the very time[2] at which Biot places the mental illness of Newton, I find a carefully drawn up record of chemical experiments made in that very month on the properties and action of barm, and on the distillation of the salts of metals.[3] They were resumed in April 1695, and continued to February 1696, when he was called to London upon his appointment to the Wardenship of the Mint.

The only chemical paper of importance published by Sir Isaac, was read at the Royal Society on the 28th of May 1701, and printed in the *Philosophical Transactions*[4] without his name, under the title of *Scala graduum Caloris.* The following are the principal points of the Scale:—

Degrees of Heat.	Equal parts of Heat.	
0	0	Heat of the winter air when water begins to freeze.
1	12	The greatest heat at the surface of the human body, and that at which eggs are hatched.
2	24	Heat of melting wax.
3	48	The lowest heat at which equal parts of tin and bismuth melt.
4	96	The lowest heat at which lead melts.
5	192	The heat of a small coal fire not urged by bellows. The heat of a wood fire is from 200 to 210.

In the original table eleven intermediate points of the scale are accurately determined, and the temperature of other parts of the scale less accurately indicated.

The first column of this table contains the degrees of heat in arithmetical progression; and the second contains the degrees

[1] See pages 76 and 77 of this volume.

[2] Between the 10th and 30th December 1692. See *Journal des Savans,* 1832, p. 332.

[3] Entitled "Experiments and Observations, Dec. 1692, April and June 1693."

[4] *Phil. Trans.* for March and April 1701. p. 824.

of heat in geometrical progression, the second degree being twice as great as the first, and so on. It is obvious from this table, that the heat at which equal parts of tin and bismuth melt is *four* times greater than that of blood heat; the heat of melting lead *eight* times greater; and the heat of a small coal fire *sixteen* times greater.

This table was constructed by the help of a thermometer and of red-hot iron. By the former he measured all heats as far as that of melting tin; and by the latter he measured all the higher heats. The heat which heated iron loses in a given time is as the total heat of the iron; and, therefore, if the times of cooling are taken equal, the heats will be in a geometrical progression, and may therefore be easily found by a table of logarithms.

He found by a thermometer constructed with lintseed oil, that if the oil, when the thermometer was placed in melting snow, occupied a space of 10,000 parts, the same oil, rarefied with *one* degree of heat, or that of the human body, occupied a space of 10,256; in the heat of water beginning to boil, a space of 10,705; in the heat of water boiling violently, 10,725; in the heat of melted tin beginning to cool, and putting on the consistency of an amalgam, 11,516, and when the tin had become solid, 11,496. Hence the oil was rarefied in the ratio of 40 to 39 by the heat of the human body; of 15 to 14 by the heat of boiling water; of 15 to 13 in the heat of melting tin beginning to solidify; and of 23 to 20 in the same tin when solid. The rarefaction of air was, with the same heat, *ten* times greater than that of oil; and the rarefaction of oil *fifteen* times greater than that of spirits of wine. By making the heats of oil proportional to its rarefaction, and by calling the heat of the human body 12 parts, we obtain the heat of water beginning to boil, 33; of water boiling violently, 34; of melted tin beginning to solidify, 72; and of the same become solid, 70.

Sir Isaac then heated a sufficiently thick piece of iron till it was red-hot; and having fixed it in a cold place, where the wind blew uniformly, he put upon it particles of different metals and other fusible bodies, and noted the times of cooling, till all the particles having lost their fluidity grew cold, and the heat of the iron was equal to that of the human body. Then, by assuming that the excesses of the heats of the iron and of the solidified particles of metal, above the heat of the atmosphere, were in geometrical progression when the times were in arithmetical progression, all the heats were obtained. The iron was placed in a current of air, in order that the air heated by the iron might always be carried away by the wind, and that cold air might replace it with a uniform motion; for thus equal parts of the air were heated in equal times, and received a heat proportional to that of the iron. But the heats thus found had the same ratio to one another with the heats found by the thermometer; and hence he was right in assuming that the rarefactions of the oil were proportional to its heats.

In giving a notice of this paper, M. Biot justly observes, that it contains three important discoveries, one of which is the method of making thermometers comparable by determining the extreme terms of their graduation from the phenomena of constant temperature; the second is the determination of the law of cooling in solid bodies at moderate temperatures; and the third is the observation of the constancy of temperature in the phenomena of fusion and ebullition—a constancy which has become one of the foundations of the theory of heat. This capital fact is established by numerous and varied experiments made not only upon compound bodies, and upon simple metals, but also upon various metallic alloys, which shows that Newton had felt the importance of it. "We may believe," M. Biot adds, "with great probability, that this work was one of those which he had finished before the fire in his laboratory."[1]

[1] This constant recurrence to the fatal attack of 1693, which is synonymous with the fire in the laboratory, in order to fix the date of Newton's writings and discoveries, is equally painful and unjust. The date of the fire itself is actually unknown.

This method of determining the date of important discoveries is certainly new in the history of science. Newton himself communicated this paper to the Royal Society in 1701, and, having no other evidence to guide us, we might have reasonably supposed that the experiments on which it was founded, and the important deductions which they authorized, were made a short time previous to its communication. M. Biot, however, follows a different rule. "The paper," he says, "contains three important discoveries, and therefore they must have been made previous to 1692-3," because, after the mental calamity which he believes befell him at that date, he was fit to write nothing but theology! Having already shown that, in every case in which he has thus reasoned, M. Biot has been incorrect in his decision, I was desirous of ascertaining the probable date of these experiments by an examination of Newton's note-books. In one of the oldest of these I found the following paragraphs written in a fresher ink, and in the handwriting of his later years :—

"The sealed thermometer, or another wholly like it, but made with oil, with the heat of my body (to which I equal that of a bird hatching her eggs), stands at the degree of $17\frac{3}{4}$. —*March* 10, $169\frac{2}{3}$. When water begins to freeze, it stands at the degree . . . . ; when water begins to boil, at the degree . . . . . ; when water boils vehemently, at the degree . . . . ; when water is as hot as the hand can endure to stay long in, at the degree . . . . ; when tin begins to melt, at the degree . . . . ; when wax begins to melt, at the degree . . . . ; when molten tin sets, at the degree . . . . ; when molten lead sets, at the degree . . . . ; when melted wax sets, at the degree . . . .

"By dipping a bolt-head with a short neck into hot water, and holding it with its neck under water for six or eight minutes till the glass be as hot as the water—then stopping the glass with my finger, inverting it into a vessel of cold water, taking away my finger, letting it stand for an hour to cool; putting my hand into the cold water and stopping it again with my finger, when the . . . . water within and without the glass, taking it out and weighing the water drawn up into the glass, and the water which will fill the glass, and making allowance for the ascent and descent of the barometer, I found how much the air was rarefied by the heat of the water; and by a barometer of lintseed oil, I found also how much the oil was rarefied by the same heat. The experiment I made twice, and found the first time that the rarefaction of air was to the rarefaction of water at equal heats as $10\frac{1}{9}$ to 1—the second time as $9\frac{14}{15}$ to 1. 'Tis, therefore, in round numbers, as 10 to 1. By another way of reckoning, I found that the rarefaction of this oil was to the rarefaction of spirit of wine in equal heats, as 15 to 1, or thereabouts, for I did not measure this proportion accurately. From these the rarefaction of air was to that of 🜈 in equal heats as 150 to 1.

"The space which lintseed oil took up with such heat as I could give to a little bolt-head with my body, was to the space which it took up in such a degree of coldness as made water begin to freeze, as 41 to 40; and, therefore, the spaces which air took up in the same degrees of heat and cold, were as 50 to 40, or 5 to 4."

From this manuscript it is obvious that Newton was engaged in his experiments on the scale of heat at the very time that he was supposed incapable of such an effort; and as he had not then completed the inquiry, it follows that the discoveries which it contains were made at a later date. The historian of science has not a more painful duty to discharge than that of fixing the date of discoveries, but it is a duty which he is never called to perform unless there are conflicting claims submitted to his judgment. It is a singular obligation which a biographer imposes upon himself to fix the date of a discovery by the alleged insanity of its author.

The only other chemical paper of Sir Isaac's that has been published, is one of about two pages, entitled *De Natura Acidorum.* It is followed by other two pages, entitled *Cogitationes*

Variæ, containing a number of brief opinions on chemical subjects, which have been more distinctly and fully reproduced in the Queries at the end of his Treatise on Optics. This paper must have been written subsequently to 1687, as it contains a reference to the *Principia*.

In the note-books and loose manuscripts of Sir Isaac, many chemical experiments and observations are recorded, but it is sometimes difficult to distinguish what is his own from what he has copied from other writers. As he seems to have considered his paper on the scale of heat the only one fit for publication, it is probable that he collected from his note-books the most important of the results at which he had arrived, and published them among the queries in his *Optics*.

The most interesting of these chemical queries relate to fire, flame, vapour, heat, and elective attractions,[1] and as they were revised in 1716 and 1717, we may regard them as containing the most mature opinions of their author. He considers fire as a body heated so hot as to emit light copiously, red-hot iron being nothing else than fire, and a burning coal red-hot wood. In one of his note-books "he concludes that flame is nothing but exhalations set on fire, and that *a burning coal and a burning flame differ only in rarity and density*," that is, that flame consists of particles of carbon brought to a white heat—an opinion of Sir Humphry Davy's. "Flame," he adds, "is nothing but a company of burning little coals dispersed about in the air, flame and vapour differing only as bodies red hot, and not red hot, by cold." He considers the "sun and fixed stars as great earths vehemently hot, whose heat is conceived by the greatness of the bodies and the material action and reaction between them and the light which they emit, and whose parts are kept from burning away, not only by their fixity, but also by the vast weight and density of the atmospheres incumbent upon them, and very strongly compressing them, and condensing the vapours and exhalations which arise from them.

[1] These queries are Nos. 6, 7, 8, 9, 10, 11, and 31.

In his long query on elective attractions, he considers the small particles of bodies as acting upon one another at distances so minute as to escape observation. When salt of tartar deliquesces, he supposes that this arises from an attraction between the saline particles and the aqueous particles held in solution in the atmosphere, and to the same attraction he ascribes it that the water will not distil from the salt of tartar without great heat. For the same reason sulphuric acid attracts water powerfully, and parts with it with great difficulty. When this attractive force becomes very powerful, as in the union between sulphuric acid and water, so as to make the particles "coalesce with violence," and rush towards one another with an accelerated motion, heat is produced by the mixture of the two fluids. In like manner, he explains the production of flame from the mixture of cold fluids,—the action of fulminating powders,—the combination of iron filings with sulphur,—and all the other chemical phenomena of precipitation, combination, solution, and crystallization, and the mechanical phenomena of cohesion and capillary attraction. He ascribes hot springs, volcanoes, fire-damps, mineral coruscations, earthquakes, hot suffocating exhalations, hurricanes, lightning, thunder, fiery meteors, subterraneous explosions, land-slips, ebullitions of the sea, and water-spouts, to sulphureous steams abounding in the bowels of the earth, and fermenting with minerals, or escaping into the atmosphere, where they ferment with acid vapours fitted to promote fermentation.

In explaining the structure of solid bodies, he is of opinion "that the smallest particles of matter may cohere by the strongest attractions, and compose bigger particles of weaker virtue ; and many of these may cohere and compose bigger particles whose virtue is still weaker ; and so on for divers successions, until the progression end in the biggest particles, on which the operations in chemistry, and the colours of natural bodies, depend, and which, by adhering, compose bodies of a sensible magnitude. If the body is compact, and bends or yields inward to pression, without any sliding of its parts, it is hard and elastic, returning to its figure with a force arising from the mutual attraction of its parts. If the parts slide upon one another, the body is malleable or soft. If they slip easily, and are of a fit size to be agitated by heat, and the heat is big enough to keep them in agitation, the body is fluid; and if it be apt to stick to things, it is humid; and the drops of every fluid affect a round figure, by the mutual attraction of their parts, as the globe of the earth and sea affects a round figure, by the mutual attraction of its parts by gravity."

Sir Isaac then supposes, that, as the attractive force of bodies can reach but to a small distance from them, "a repulsive virtue ought to succeed;" and he considers such a virtue as following from the reflexion and inflexions of the rays of light, the rays being repelled by bodies in both these cases without the immediate contact of the reflecting or inflecting body, and also from the emission of light, the ray, as soon as it is shaken off from a shining body by the vibrating motion of the parts of the body, getting beyond the reach of attraction, and being driven away with exceeding great velocity by the force of reflexion, the force that turns it back in reflexion being sufficient to emit it.

We have already seen that Newton at one period of his life was a believer in alchemy, and that he even devoted much time to the study and practice of its processes. The Rev. Mr. Law[1] has stated that there were found among Sir Isaac's papers large extracts out of Jacob Behmen's works, written with his own hand, and that he had learned from undoubted authority, that in a former part of his life he was led into a search of the philosopher's tincture from the same author. He afterwards stated

[1] In his *Appeal to all that doubt or disbelieve the truths of the Gospel*, 3d edit. p. 314, Mr. Law had stated that Sir Isaac Newton borrowed his doctrine of attraction from Behmen's *Teutonic Theosopher*. A correspondent having expressed a desire to know "the foundation which Mr. Law had for such an assertion," a friend of Mr. Law's replied to this application, and quoted from a letter of Mr. Law's to himself the statement which we have given in the text. The correspondent, in a subsequent communication, expresses his disbelief that Sir Isaac could have betrayed such weakness. See *Gentleman's Magazine*, 1782, vol. lii. pp. 227, 329, and 575.

in a private letter, that his vouchers are names well known, and that they have assured him that "Sir Isaac was formerly so deep in Jacob Behmen, that he, together with Dr. Newton his relative, set up furnaces, and were for several months at work in quest of the tincture." That this statement is substantially true is proved by Dr. Newton's own letter.[1] We have seen in Sir Isaac's handwriting, *The Metamorphoses of the Planets*, by John De Monte Snyders, in 62 pages 4to, and a key to the same work, and numerous pages of alchemist poetry from Norton's *Ordinal*, and Basil Valentine's *Mystery of the Microcosm.* There is also a copy of *Secrets Revealed, or an open entrance to the Shut Palace of the King*,[2] which is covered with notes in Sir Isaac's hand, in which great changes are made upon the language and meaning of the thirty-five chapters of which it consists. I have found also among Sir Isaac's papers, a beautifully written, but incomplete copy of William Yworth's *Processus Mysterii magni Philosophicus*, and also a small manuscript in his handwriting, entitled *Thesaurus Thesaurorum sive Medicina Aurea.*[3]

There is no problem of more difficult solution than that which relates to the belief in alchemy, and to the practice of its arts, by men of high character and lofty attainments. When we consider that a gas, a fluid, and a solid may consist of the very same ingredients in different proportions; that the same elements, with one or more atoms of water, form different substances; that a virulent poison may differ from the most wholesome food only in the difference of quantity of the very same ingredients; that gold and silver, and indeed all the metals, may be extracted from transparent crystals, which

[1] See page 55 of this volume.

[2] By W. C., Lond. 1669, 8vo. "Composed by a most famous Englishman, styling himself *Anonymous* or *Eurœneus Philaletha*, who, by inspiration and reading, attained to the philosopher's stone at his age of twenty-three years. Anno Domini, 1645."

[3] In addition to these works, Sir Isaac has left behind him, in his Note-books, and separate MSS., copious extracts from the writings of the alchemists of all ages, and a very large *Index Chemicus* and *Supplementum Indicis Chemici*, with minute references to the different subjects to which they relate.

scarcely differ in their appearance from a piece of common salt, or a bit of sugar-candy;—that *Aluminum*, a metal with almost all the valuable properties of gold and platinum, can be extracted from clay;—that lights of the most dazzling colours can be obtained from the combustion of colourless salts; that gas, giving the most brilliant light, resides in a lump of coal or a block of wood; that several of the gems can be crystallized from their elements; and that diamond is nothing more than charcoal,—we need not wonder that the most extravagant expectations were entertained of procuring from the basest materials the precious metals and the noblest gems. In the daily experiments of the alchemist, his aspirations, after great discoveries, must often have been encouraged by the singular phenomena which he encountered, and the startling results at which he arrived. The most ignorant compounder of simples could hardly fail to witness the almost magical transformations of chemical bodies, and every new product which he obtained must have added to the probability that the tempting doublet of gold and silver would be thrown from the dice-box with which he gambled. When any of the precious metals were actually obtained from the ores of lead and other minerals, it was not unreasonable to suppose that they had been formed during the process, and men not disposed to speculate might have thus embarked in new adventures to procure a more copious supply, without any insult being offered to sober reason, or any injury inflicted on sound morality.

Nor were the expectations of the alchemists to find a universal medicine altogether irrational and useless. The success of the Arabian physicians in the use of mercurial preparations naturally led to the belief that other medicines, still more general in their application, and more efficacious in their healing powers, might yet be brought to light, and we have no doubt that many important discoveries were the result of such overstrained expectations; but when the alchemists pretended to have obtained such a medicine, and to have conferred longevity by administering it, they did equal violence to reason and to truth.

When a mind ardent and ambitious is fascinated by some lofty pursuit where gold is the object, and fame the impulse, it is difficult to pause even after successive failures, and to make a voluntary shipwreck of the reputation which has been staked. Hope still cheers the aspirant from failure to failure, till the loss of fortune and the decay of credit disturb the serenity of his mind, and hurry him on to the last resource of baffled ingenuity and disappointed ambition. The philosopher thus becomes an impostor, and, by the pretended transmutation of the baser metals into gold, or the discovery of the philosopher's stone, or of the universal medicine, he attempts to sustain his sinking reputation, and recover the character he has lost. The communication of the great mystery is now the staple commodity with which he is to barter. It can be imparted only to a chosen few—to those among the opulent who merit it by their virtues, and can acquire it by their diligence, and the divine vengeance is threatened against its disclosure. A process thus commencing in fraud and terminating in mysticism, is conveyed to the wealthy aspirant, or to the young enthusiast, and the grand mystery passes current for a season, till some wary professor of the art denounces its publication as detrimental to society.

The alchemy of Boyle, Newton, and Locke cannot be thus characterized. The ambition neither of wealth nor of praise prompted their studies, and we may safely say that a love of truth alone, a desire to make new discoveries in chemistry, and a wish to test the extraordinary pretensions of their predecessors and their contemporaries, were the only motives by which they were actuated. In so far as Newton's inquiries were limited to the transmutation and multiplication of metals, and even to the discovery of the universal tincture, we may find some apology for his researches; but we cannot understand how a mind of such power, and so nobly occupied with the abstractions of geometry, and the study of the material world, could stoop to be even the copyist of the most contemptible alchemical poetry, and the annotator of a work, the obvious production of a fool and a knave. Such, however, was the taste of the century in which Newton lived, and, when we denounce the mental epidemics of a past age, we may find some palliation of them in those of our own times.

Lady Mary Wortley Montague informs us,[1] that "at Vienna there was a prodigious number of alchemists. The philosopher's stone," she says, "is the great object of zeal and science; and those who have more reading and capacity than the vulgar, have transported their superstition (shall I call it?) or fanaticism from religion to chemistry; and they believe in a new kind of transubstantiation, which is designed to make the laity as rich as the other kind has made the priesthood. This pestilential passion has already ruined several great houses. There is scarcely a man of opulence or fashion that has not an alchemist in his service; and even the Emperor is supposed to be no enemy to this folly in secret, though he has pretended to discourage it in public."

In these times, and even earlier, Sir Isaac Newton lived. Leibnitz, his great rival, was also an alchemist. In his early life he was secretary to the Society of Rosicrucians at Nuremberg—a secret association which practised alchemy, and which existed in several of the larger towns in Germany. Leibnitz, however, soon renounced his faith in the mystic art, and, there is reason to believe, from one of Newton's letters to Locke,[2]

[1] In a letter dated January 2, 1717, and supposed to be written to the Abbé Conti.—*Letters and Works*, vol. ii. p. 130. See p. 229 of this volume.

[2] See page 76. When Locke, as one of the executors of Boyle, was about to publish some of his works, Newton wished him to insert the second and third part of one of Boyle's recipes (the first part of which was to obtain "a mercury that would grow hot with gold"), and which Boyle had communicated to him on condition that they should be published after his death. In making this request, Newton "desired that it might not be known that they came through his hands." And he adds, "One of them seems to be a considerable experiment, and may prove of good use in medicine in analysing bodies. The other is only a knack. In dissuading you from too hasty a trial of this recipe, I have forborne

that he also had learned to have but little confidence even in the humbler department of the multiplication of metals.

to say anything against multiplication in general, because you seem persuaded of it, though there is one argument against it which I could never find an answer to, and which, if you will let me have your opinion about it, I will send you in my next." Letter to Locke, August 2, 1692.—King's *Life of Locke*, vol. i. pp. 410, 413.

Even at the beginning of the present century, some distinguished individuals thought favourably of alchemy. Professor Robison, in writing to James Watt, says, "The analysis of alkalis and alkaline earths will presently lead, I think, to the doctrine of a reciprocal convertibility of all things into all. . . . I expect to see alchymy revive, and be as universally studied as ever." Feb. 11, 1800.—Muirhead's *Origin and Progress of the Mechanical Inventions of James Watt*, vol. ii. pp. 271, 272. Lond. 1854.

CHAPTER XXVI.

Newton's first attack of ill health, and his recovery—History of his acquaintance with Dr. Pemberton, who superintends the third edition of the Principia—Their Correspondence—Improvements in the third edition—Change in the celebrated Scholium—And in the Scholium on the Motion of the Moon's Nodes—Demonstration of Machin and Pemberton—Publication of the third edition—Newton attacked with the Stone—Conduitt acts for him in the Mint—His Letter recommending Colin Maclaurin as Assistant to Gregory—His liberality on this occasion—Maclaurin's Letter to Newton—Visit of the Abbé Alari to Newton—His acquaintance with Samuel Crell—He presides at the Royal Society on the 2d March—His last illness—And Death on the 20th March 1727—His body lies in state—His Burial and Monument in Westminster Abbey—Statues and Pictures of him—His Property—His Descendants.

Although Sir Isaac had now attained to a very advanced age, he had for a long time enjoyed almost uninterrupted health. In 1722, however, when he had entered his eightieth year, he was seized with incontinence of urine, which, though at first ascribed to stone, and thought incurable, was owing merely to a weakness in the sphincter of the bladder. Dr. Mead advised him to give up the use of his carriage, neither to dine abroad, nor with large parties at home, and to limit his diet to a little butcher-meat and broth, vegetables, and fruit, of which he was always very fond. By these means he recovered slowly, though never perfectly, as the disease was always brought on by motion of any kind. In July 1722, when he was corresponding with Varignon, who died in that year, he wrote to him that he was getting slowly well, and hoped soon to enjoy his usual health;[1] but there is reason to believe that the seeds of a more painful disease, of which this was only the herald, were lurking in his constitution, and would sooner or later come to maturity.

Thus warned of his slight tenure of life, he resolved to pro-

[1] "Paulatim convalesco, et spero me salutem cito fruiturum."

ceed with the third edition of the *Principia*, for which he had been long making preparation. The premature death of Mr. Cotes had deprived him of his valuable assistance, but he had the good fortune to obtain the services of Dr. Henry Pemberton, a young and accomplished physician, who had successfully cultivated mathematical learning, and who, from various causes, was particularly qualified for the task of editing so great a work. When Pemberton was studying medicine at Leyden under Boerhaave, a gentleman lent him the *Principia*, which was then extremely scarce. Having heard that it was a work of difficult comprehension, he was surprised at his own facility in mastering its problems, and in order to pursue the subject, he devoted himself to the study of the doctrine of fluxions, and of prime and ultimate ratios, as explained in the introduction to Newton's treatise, *De Quadratura Curvarum.* Soon after this he solved the problem with which Leibnitz had challenged the English mathematicians ; and upon showing his solution to Dr. Keill, he was so pleased with the talent which it displayed, that he immediately introduced him to Sir Isaac.[1] Owing, as Dr. Wilson informs us, to "some ill offices done by a malevolent person who then had Sir Isaac's ear," the great philosopher paid no attention to the young geometer, to whom he was hereafter to owe so many obligations. Pemberton was mortified with the cold reception he had experienced ; but having the most ardent desire to become acquainted with so great a philosopher, he thought he would best accomplish his object by writing a Treatise, containing a popular account of Sir Isaac's discoveries. An unforeseen accident, however, gained for him his object by a less dilatory process. Signior Poleni, an Italian mathematician, had published in his tract *De Castellis*, an experiment which he considered as proving Leibnitz's assertion that the force of descending bodies is proportional to the square of the velocity, and not, as is commonly thought, to the simple velocity. Dr. Pemberton saw its insufficiency, and drew

[1] See this volume, p. 23, and *Macclesfield Correspondence*, vol. ii. p. 424.

up a refutation of it, which he showed to his friend Dr. Mead. The Doctor immediately communicated it to Sir Isaac, who was so well pleased with it, that he called upon Pemberton at his lodgings, and showed him a refutation of Poleni by himself, grounded on other principles.[1] In this agreeable way Pemberton secured the friendship of Newton, and they often met together to converse on mathematical and philosophical subjects. Though Sir Isaac quickly discovered the capacity of his young friend, his modesty was so great, as Wilson informs us, that he solicited Dr. Mead to prevail on Pemberton to assist him in bringing out a new edition of the *Principia.*[2]

Owing to the smallness of the impression of the second edition of that work, it seems to have been quickly sold, but having been reprinted at Amsterdam in 1713, the foreign demand for it was amply supplied. In Dr. Horsley's list of the MSS. at Hurtsbourne Park, he mentions a copy of "the second edition of the *Principia*, interlined with some written notes of Sir Isaac Newton," which is no doubt the work referred to in the

[1] Pemberton's Paper, in the form of a letter to Dr. Mead, was published in the *Philosophical Transactions* for April and May, 1722, No. 371, p. 57. Sir Isaac's refutation was added in a postscript to the letter without his name, as having been given to the author "by an excellent and learned friend of his, to whom he had been pleased to show the letter, in confirming Sir Isaac Newton's sentiment in relation to the resistance of fluids." As the subject excited much controversy, Sir Isaac's simple and intelligible view of it may be interesting to the reader. "Suppose," says he, "pieces of fine silk, or the like thin substance, extended in parallel planes, and fixed at small distances from each other. Suppose then a globe to strike perpendicularly against the outermost of the silks, and by breaking through them to lose part of its motion. If the pieces of silk be of equal strength, the same degree of force will be required to break each of them, but the time in which each piece of silk resists, will be so much shorter as the globe is swifter; and the loss of motion in the globe consequent upon its breaking through each silk, and surmounting the resistance thereof, will be proportional to the time in which the silk opposes itself to the globe's motion, insomuch that the globe, by the resistance of any one piece of silk, will lose so much of its motion as it is swifter. But, on the other hand, by how much swifter the globe moves, so many more of the silks it will break through in a given space of time; whence the number of the silks which oppose themselves to the motion of the globe in a given time being reciprocally proportional to the effect of each silk upon the globe, the resistance made to the globe by these silks, or the loss of motion the globe undergoes by them in a given time, will be always the same."—Pp. 67, 68.

[2] Dr. Wilson's *Preface* to Pemberton's *Course of Chemistry.* Lond. 1771.

memorandum which he left in the library of Christ's Church, Oxford, and which is mentioned by Professor Rigaud.[1] These notes, therefore, must have formed the new matter which was introduced into the third edition by Newton himself, and were probably copied from the volume which contains them, and transmitted to Dr. Pemberton to be inserted in the printed sheets of the second edition.[2]

The printing of the new edition seems to have commenced either in the very end of 1723, or at the beginning of 1724, and was not finished till the month of February 1726. The letters which passed between Newton and Pemberton during the progress of the work, had they been preserved, would have been interesting in many respects, and have completed the history of the *Principia*. Dr. Pemberton informs us that "he was very frequently with him, and as they lived at some distance, a great number of letters passed between them on that account."[3] The letters of Newton, however, have unfortunately been lost, but the greatest part, if not the whole, of those of Pemberton have been preserved.[4] The date of the earliest of these, which relates to a criticism on the last two lines of the sixty-third page, is February 11, 1723-4, but it was preceded by five letters without dates, so that the printing of the work must have commenced in December 1723, or in January 1724. The Preface bears the date of January 1725-6, but there is a long letter from Pemberton dated February 9, 1725-6, in which he points out the necessity of some changes in the 23d and 24th Propositions of the First Book, and pro-

[1] *Hist. Essay*, p. 106. I did not find this volume among the papers in Hurtsbourne Park, when I examined them in 1836. It appears to have been in the hands of Dr. Horsley when he edited the works of Newton.

[2] See Appendix, No. XXX., in which we have given the principal alterations and additions made in the Third Edition of the *Principia*.

[3] *View of Sir Isaac Newton's Philosophy*, Preface, p. 2.

[4] These letters are twenty-three in number, with seven sheets of Queries, containing suggestions for the improvement of the work. Only seven of the letters have dates. The Rev. Mr. Jeffrey Ekins has kindly sent me a copy of a short one in his possession, but without a date.

poses to cancel two leaves, which together with other two cancels, would require the reprinting of a whole sheet. The letters of Pemberton contain numerous suggestions for the improvement of the work, and with one or two exceptions they seem to have been implicitly adopted by Newton. He never alters a single word without permission, and when the changes which he suggests are of importance, he enters into full explanations of the grounds upon which they are proposed. I am disposed to think that Newton addressed comparatively few letters to Pemberton during the printing of the work. Their correspondence was carried on through the printing-office, and it is probable that Newton wrote his answers principally upon the proof-sheets, accepting or modifying the alterations proposed by his friend. There is only one reference in the letters to a personal interview. On another occasion, Newton leaves a new corollary with Mr. Inny's the bookseller, and in none of Pemberton's letters does he acknowledge the receipt of any letter on the subject of Newton's additions or his own suggestions. Sir Isaac was at no period of his life fond of writing letters; and least of all in his old age. He wrote scrolls of almost every letter he composed, and we are persuaded that among Pemberton's papers which have been lost, there were very few of the letters of Newton.

Among the more interesting changes made in the third edition are the changes in the celebrated Scholium on Fluxions, and in the new Scholium on the Motion of the Moon's Nodes in Prop. 33 of the Third Book. Newton, as we have already seen, has been greatly blamed by foreign writers for the omission of the paragraph about Leibnitz, which he had inserted in the two first editions of his work. Montucla[1] has ventured to insinuate that it was left out by Pemberton without Newton's consent; but Dr. Wilson, Pemberton's friend, bears witness that the new Scholium "was entirely composed by Sir Isaac,

[1] *Hist. des Mathématiques*, vol. ii. p. 338. Paris, 1758. See also *ante*, vol. i. p. 358, and APPENDIX to vol. i., No. XII.

and printed from his own handwriting." As no reference whatever is made to it in Pemberton's letters, it is probable that there was no difference of opinion about the propriety of the change, and that Pemberton saw no grounds for proposing any alteration upon the new form which had been given to the Scholium.[1]

Between the publication of the second and third edition, Mr. Machin, Professor of Astronomy in Gresham College, communicated a new demonstration of the motion of the moon's nodes. When it was sent to Pemberton for insertion, he informs Newton that he had himself invented a similar demonstration, and had mentioned it in his letter to Dr. Wilson on certain inventions of Cotes.[2] Sir Isaac, therefore, drew up, in conformity with these facts, the Scholium we have mentioned, and added to it the two propositions of Machin, as they had been first sent to him.

In February or March 1726, the third edition of the *Principia* was published, with a new preface by the author, dated January 12, 1725-26, in which he mentions the more important additions which he had made, and states that Dr. Henry Pemberton,[3]

[1] See vol. i. pp. 357-362, and APPENDIX to vol. i. No. XII. p. 426.

[2] *Epistola ad Amicum de Cotesii inventis, curvarum ratione quæ cum Circulo et Hyperbola comparationem admittunt*, pp. 6, 7, 4to. Lond. 1722. This letter is addressed *Amico Suo* J. W. *Integerrimo Dilectissimoque*, H. P. *Salutem.* See Dr. Wilson's Preface to Pemberton's *Course of Chemistry*, p. vi., for the history of this interesting volume. The Theorem of Cotes, rather prolixly demonstrated by Pemberton, was attacked with more success by Demoivre, and afterwards demonstrated directly by John Bernoulli. *Opera*, tom. iv. p. 68.

[3] Dr. Pemberton was born in London in 1694. He took the degree of medicine at Leyden, and became acquainted with Newton in the way we have already mentioned. Immediately after Sir Isaac's death in 1727, he advertised a Translation of the *Principia*, with a Comment; but the publication of Motte's Translation in 1729 prevented him from proceeding with this work. He devoted himself, however, to the completion of his "View of Sir Isaac Newton's Philosophy," part of which was submitted to Newton before his death. In Dr. Mead's letter to Conduitt, already mentioned, he says, "Dr. Pemberton has also given part of his Book to the Knight, of which he read some part immediately, and kept the papers, and seemed very well pleased. You may depend upon his having the perusal of the whole of it if he will be pleased to take the trouble." Pemberton was chosen Professor of Physic in Gresham College, and gave lectures on chemistry, which were published after his death, by his friend Dr. Wilson. He died in 1771, at the age of 77. See Preface to his *Course of Chemistry*.

"vir harum rerum peritissimus,"[1] superintended the edition.[2]

Had Sir Isaac enjoyed his usual health, he would no doubt have made greater additions to the *Principia*, but, notwithstanding the precautions which he observed, he experienced a return of his former complaint; and, in August 1724, he passed, without any pain, a stone about the size of a pea, which came away in two pieces, the one at the distance of some days from the other. After some months of tolerably good health, he was seized in January 1725, with a violent cough and inflammation of the lungs, and, in consequence of this attack, he was prevailed upon to take up his residence at Kensington,[3] where his health experienced a decided improvement. In February 1725, he was attacked in both his feet with a fit of the gout, of which he had received a slight warning a few years before, and the effect of this new complaint was to produce a beneficial change in his general health. On Sunday the 7th of March, when his head was clearer and his memory stronger than Mr. Conduitt had known it to be for some time, he entered into a long conversation on various topics in astronomy of a speculative nature, which Mr. Conduitt, who knew little of the subject, has, we think, very imperfectly reported.[4]

[1] I have found a copy of the Preface, with the date of November 1725. It is shorter than the one printed, and does not contain the well-merited compliment to Dr. Pemberton, who, as his friend Dr. Wilson tells us, valued it more than the liberal present of 200 guineas which Newton gave him. Pemberton's *Chemistry*, Preface, p. xv.

[2] Twelve fine paper copies were printed. There is one in the library of Trinity College, one in that of Queen's College, which Newton had presented to J. F. Fauquier, one in the Royal Society library, presented by Martin Folkes in the name of the President, on the 31st March 1726 (Edleston's *Correspondence*, p. lxxix.), and one in the Observatory at Oxford, which Newton had presented to Bradley. Mr. Rigaud says "that they were all originally bound with gilt leaves in red morocco, to a pattern which was much used for the Harleian Library."—*Memoirs of Bradley*, p. xi. Newton sent six copies of the work to Fontenelle, for the Academy of Sciences, for himself and for the principal mathematicians in Paris.

[3] According to Dr. Stukely, he lived at Orbell's Buildings. In Maude's *Wensleydale* he is said to have "died in lodgings in that agreeable part of Kensington called Orbell's now Pitt's Buildings."

[4] This paper was published in the Appendix to my former Life of Newton, but as Sir

Although his health was greatly improved, yet his indisposition was sufficiently severe to unfit him for the discharge of his duties at the Mint; and as his old deputy was confined with the dropsy, he was desirous, in the winter of 1725, of resigning in favour of Mr. Conduitt. Difficulties, however, seem to have been experienced in making this arrangement, but all the duties of the office were so satisfactorily performed by Mr. Conduitt, that during the last year of his life Sir Isaac hardly ever went to the Mint.

Among the last duties which he discharged with his pen, and one distinguished, too, by his usual liberality, was that of obtaining for Colin Maclaurin[1] the situation of assistant and successor to Mr. James Gregory, Professor of Mathematics in the University of Edinburgh. Mr. Maclaurin, who then filled the chair of Mathematics in Aberdeen, having applied to Sir Isaac for a testimonial of his qualifications, received the following answer, "with allowance to show it to the patrons of the University:"—"I am very glad to hear that you have a prospect of being joined to Mr. James Gregory in the Professorship of the Mathematics at Edinburgh, not only because you are my friend, but principally because of your abilities, you being acquainted as well with the new improvements of mathematics as with the former state of those sciences. I heartily wish you good success, and shall be very glad of hearing of your being elected.—I am, with all sincerity, your faithful friend and most humble servant."

To this letter Maclaurin returned the following answer:—

"Honoured Sir,—I am much obliged to you for your kind letter that Mr. Hadley transmitted to me. It has been of use to me. However, the provost or mayor of the town has thought fit to consult yourself directly on that subject, because I made

Isaac has given his opinions on the same subjects more deliberately in his letters to Bentley and elsewhere, I have not thought it advisable to reprint it.

[1] Notice of Maclaurin's Life, prefixed to his *Account of Sir Isaac Newton's Discoveries*, p. iv. Lond. 1748. I have not found a copy of this letter among Newton's papers.

some scruples to make your letter, that was addressed to me, public. I flatter myself you will, as soon as your convenience will allow, give an answer to this letter, that the want of it may not obstruct the affair.

"I have lately had a dispute with a gentleman here, who attacked your Prop. 36, lib. ii. of the *Principia*, and is supposed to be a pretty good mathematician. 'Tis about the pressure upon the circellus PQ (Prop. 36, lib. ii. Cor. 7, 8, 9, 10). He finds by a calcul of his, that the pressure upon PQ is to the cylinder on the base PQ of the height $\frac{1}{2}$ GH, as 2 AB^2 to $AB^2 + EF^2 = PQ^2$, whereas you make that proportion as 2 EF^2 to 2 $EF^2 - PQ^2$ in Cor. 10.

"I can demonstrate (and he allows it) that when CD and EF are equal, the pressure on PQ is to the cylinder on PQ of the height $\frac{1}{2}$ GH as 2 EF^2 to 2 $EF^2 - PQ^2$. But he objects, that though the proportion must be allowed in that case, yet it cannot be general, and that it ought to vary with AB, though AB does not enter into your proportion. Cor. 10, Prop. 36.

"I have answered this, and have showed, that when AB is very great, the pressure on PQ should be the weight of the whole cylinder above PQ, according to him, because the ratio of 2 AB^2 to $AB^2 + EF^2 - PQ^2$, in that case is a ratio of 2 AB^2 to AB^2, or of 2 to 1. And this I think absurd, since, by the very idea of the cataract, PQ cannot bear the whole cylinder above it.

"But I trouble you no farther. I am more and more satisfied that your book will triumph over all that oppose it; and that as it has met with resistance from the prejudices and humours of men, it will prevail the longer.—I am, with much gratitude, and the greatest respect, honoured Sir, your most obliged, most humble servant,

"COLIN MACLAURIN.

"EDINBURGH, *Oct.* 25, 1725."

In consequence of this letter, Sir Isaac returned the following answer to the Lord Provost, of which I have found two copies slightly different and more complete than the one printed in Maclaurin's Life.

"MY LORD,—I received the honour of your letter, and am glad to understand that Mr. Maclaurin is in good repute amongst you for his skill in mathematics, for I think he deserves it very well. And, to satisfy you that I do not flatter him, and also to encourage him to accept of the place of assisting Mr. Gregory, in order to succeed him, I am ready (if you please to give me leave) to contribute twenty pounds per annum towards a provision for him, till Mr. Gregory's place become void, if I live so long, and I will pay it to his order in London.[1] When your letter arrived at London I was absent from hence, which made it the later before I received it, otherwise I might have returned an answer a little sooner.—I am, my Lord, your Lordship's most humble and most obedient servant,

"ISAAC NEWTON.

"To his Lordship the
PROVOST OF EDINBURGH,
in . . . . Scotland."

It is almost unnecessary to say, that Maclaurin was appointed to the chair in November 1726; but it is interesting to notice, that Newton's recommendation of him is engraven, in two words, on the tablet erected in memory of Maclaurin, and fixed upon the south wall of the Greyfriars' Church. When a youth at College, I have often gazed upon this simple monument, and pondered over the words, to be envied by every aspirant to scientific fame,—"NEWTONO SUADENTE."[2]

[1] In the first scroll of the letter there was inserted the following passage, "For I have a kindness also for Mr. Gregory upon his brother's account, and should be glad to have a hand in helping him to a coadjutor," but it was struck out.

On the back of the two scrolls, which are written on the same page, are the following words:—"I reckon him well skilled in arithmetic, algebra, geometry, astronomy, and optics, which are the mathematical sciences proper for a university, and abundantly sufficient for a Professor."

[2] Colin Maclaurin was born in February 1698. He studied mathematics under Dr. Robert Simson at Glasgow, and was in 1717 elected Professor of Mathematics in Mari-

Notwithstanding his great age, and his imperfect health, Sir Isaac was able to attend the meeting of the Royal Society, and to receive with hospitality distinguished foreigners who were introduced to him. The Abbé Alari, the instructor of Louis XV., and the friend of Bolingbroke, spent two months in London in 1725. He paid a visit to Newton, of which the following flippant and apparently incorrect account has been given by a friend :—"He visited the University of Cambridge and the great Newton, who enjoyed, at that time, in the capital of England, the general esteem of Europe, and 50,000 livres of salary as Master of the Mint. The Abbé having gone to his house at nine o'clock in the morning, Newton began by telling him that he was eighty-three years of age. There was in his chamber the portrait of his patron, Lord Halifax, and one of the Abbé Varignon, of whose geometrical writings he had a high opinion. 'Varignon,' he said, 'and Father Sebastien, the Carmelite, are those who have understood best my system of colours.' The conversation at last turned on ancient history, with which Newton was then occupied. The Abbé, who was deeply read in Greek and Latin authors, having made himself very agreeable, was asked to dinner. The repast was detestable. Newton was stingy, and gave his guests wines of Palma and Madeira, which he had received in presents. After dinner

schal College, Aberdeen. In 1719 he became acquainted with Newton, and was elected a Fellow of the Royal Society, to whose *Transactions* he contributed two papers. He gained the prize of the Academy of Sciences for 1724, on the Percussion of Bodies. In 1742, he published his Treatise on Fluxions, which was written in answer to Berkeley's Analyst. In 1745, he took an active part in defending Edinburgh against the approach of Prince Charles; and in superintending the execution of the works which he had designed, he caught the cold, of which he died on the 14th June 1746, in the forty-eighth year of his age. Mr. Conduitt had requested him as a friend to draw up an account of Newton's discoveries for the biography of him, in which he was engaged. Maclaurin sent the MS. of it to London; but in consequence of the death of Conduitt in 1797, the MS. was returned, and it became the foundation of his admirable *Account of Sir Isaac Newton's Philosophical Discoveries,* which was published after his death. Maclaurin was a man, like Newton, of undoubted piety, and an humble Christian. He died while dictating to his amanuensis the last chapter of his work, in which he proves the wisdom, the power, the goodness, and the other attributes of the Deity.

he took the Abbé to the Royal Society, of which he was the President, and made him sit at his right hand. The business began, and Newton fell asleep. When it was over, everybody signed the register, and the Abbé among the rest.[1] Newton took him to his house, and kept him till nine o'clock in the evening."[2]

In the following year Newton received visits from Samuel Crell,[3] a distinguished German divine, who had embraced the opinions of Socinus, and was appointed minister of a Unitarian church on the frontiers of Poland. He came to England in 1726, for the purpose of printing the last of his works, which was published in that year.[4] After his return to Amsterdam, where he resided during the rest of his life, he sent to his friend Lacroze, the celebrated orientalist, in a letter dated 17th July 1727, the following account of his visits to Newton, whose death a few months before had given a great interest to everything associated with his name:—"I also conversed at different times with the illustrious Newton, who died in the month of March at the age of eighty-five. He read manuscript without spectacles, and without bringing it near his eyes. He still reasoned acutely as he was wont to do, and told me that his memory only had failed him. Gout and the stone occasionally troubled him at his very advanced age. A few weeks before his death he threw into the fire many manuscripts written in his own hand. He left, however, some to be printed, among which is one entitled *Historia Dominationis Clericorum*,

[1] The Abbé signed only the Journal Book.

[2] *Essais Historique sur Bolingbroke*, compiled by General Grimoard in *Lettres Historiques . . . de . . . Bolingbroke*, vol. i. p. 155, Paris, 1808. Mr. Edleston, from whose work we have copied the anecdote in the text, gives the following "as the simple record in the Journal Book of Alari's visit:" "Mr. Mildmay had leave to be present, as also Mr. Peter Joseph Alari, a French gentleman."—*Correspondence*, &c., p. lxxxviii.

[3] Born 1657, died 1747.

[4] *Initium Evangelii S. Johannis Apostoli ex antiquitate ecclesiastica restitutum, itidemque nova ratione illustratum*, 8vo, 1726. It was published under the name of *L. M. Artemonius*, because he had adopted the opinions of Artemon, a heretic of the third century, respecting our Saviour. The letters L M. signify *Lucas Mellerius*, the anagram of *Samuel Crellius*.

as I was assured by his physician, the celebrated Dr. Mead. He was not only deeply versed in mathematics and philosophy, but likewise in theology and ecclesiastical history. He had also written, as he himself told me, a Commentary on the Apocalypse of St. John. Whether or not he burnt it, I did not learn. He expressed a wish to read my book, and he read it when it was printing, because it seemed to contain some things that were new."[1]

Having completed the new edition of his great work, Sir Isaac seems to have abstained from all intellectual labour during the latter half of 1726, with the exception of what is indicated by two letters to the Rev. Mr. Mason,[2] and his letter to Fontenelle in June or July, accompanying the six copies of the *Principia* for the Academy of Sciences.[3] He had received much benefit from absolute rest and from the air of Kensington, but his friends found it very difficult to restrain him from going occasionally to town. In the month of August he complained of an affection of the rectum, which he thought was fistula, but Mr. Cheselden found upon examination that it was "nothing but a little relaxation of the inward coat of the gut;" and this opinion, as Dr. Mead wrote to Mr. Conduitt then in the country, "made his old friend very easy in the matter."[4]

When thus confined to the house, Sir Isaac amused himself with reading, but as Mr. Conduitt informs us, "the book which was commonly lying before him, and which he read oftenest at last, was a duodecimo Bible." "I found," he adds, "his eyes bloodshot one morning, and he complained that something swam before them. When I asked him what he thought had occasioned the disorder, he said he believed that he

[1] *Thesaurus Epistolicus Lacrozianus*, tom. i. p. 105. Edidit J. L. Uhlius, Lipsiæ, 1742-1746, three vols. 4to.

[2] One dated May 10, 1726, sending £3 for repairing the floor of Colsterworth Church, and the other dated February 4, 1727, on the assay of a piece of ore.

[3] I have found a scroll of this letter without a date, and Fontenelle's answer to it, dated July 14, 1726.

[4] This letter is dated August 11, 1726. Dr. Mead had received two letters of inquiry from Conduitt on the occasion, to which this was the answer.

had overstrained the optic nerves, for the morning or two last past he had waked before the sun was quite up, and had endeavoured to see what o'clock it was on his watch, by a very little light that came through the curtains and the shutter; upon which he left that off, and found out the hour by feeling with his hand, and his eyes soon recovered."

Thinking that he was fit for the journey, he went to London on Tuesday the 28th of February, to preside at a meeting of the Royal Society on the 2d of March, and on the following day Mr. Conduitt thought he had not seen him better for many years. Sir Isaac himself was sensible of the change, and "told his nephew smiling, that he had slept the Sunday before from eleven at night till eight in the morning without waking." These feelings, however, were fallacious. He had undergone great fatigue in going to the Society, and in paying and receiving visits, and the consequence of this was a violent attack of his former complaint. He was taken ill on Friday the 3d March, and continued so after his return to Kensington on Saturday the 4th of March. For a whole week he had no medical advice; but the moment Mr. Conduitt heard of his illness, which was on Saturday the 11th March,[1] he sent for Dr. Mead and Mr. Cheselden, who pronounced the disease to be stone in the bladder, and held out no hopes of his recovery. "The stone had probably been moved from the place where it lay quiet by the great motion and fatigue of his last journey to London." From that time he experienced violent fits of pain with very brief intermissions, and though the drops of sweat ran down his face in these severe paroxysms, he never uttered a cry or a complaint, or displayed the least marks of peevishness or impatience, but during the short intervals of relief "would smile and talk with his usual cheerfulness." On Wednesday the 15th of March he appeared to be somewhat better, and slight though groundless hopes were entertained of

[1] Mr. Conduitt has left three different accounts of his illness. Some of the facts mentioned above are found only in one of them, apparently the one first written.

his recovery. On the morning of Saturday the 18th, he read the newspapers, and carried on a pretty long conversation with Dr. Mead. His senses and his faculties were still vigorous, but at six o'clock of the same evening, he became insensible, and continued in that state during the whole of Sunday and till Monday the 20th, when he expired without pain between one and two o'clock in the morning, in the eighty-fifth year of his age,—

> . . . 'Tis done, the measure's full,
> And I resign my charge.
>
> THOMSON.[1]

His body was removed from Kensington to London, and on Tuesday the 28th March it lay in the Jerusalem Chamber, and was thence conveyed to Westminster Abbey, where it was buried near the entrance into the choir on the left hand. The Pall was supported by the Lord High Chancellor, the Dukes of Montrose and Roxburghe, and the Earls of Pembroke, Sussex, and Macclesfield, who were Fellows of the Royal Society. The Honourable Sir Michael Newton, Knight of the Bath, was chief mourner, and was followed by some other relations, and several eminent persons who were intimately acquainted with the deceased. The office was performed by the Bishop of Rochester, attended by the prebends and choir.[2]

Sensible of the high honour which they derived from their connexion with so distinguished a philosopher, the relations of Sir Isaac Newton, who inherited his personal estate,[3] agreed to devote £500 to the erection of a monument to his memory; and the Dean and Chapter of Westminster appropriated for it a place in the most conspicuous part of the Abbey, which had often been refused to the greatest of our nobility. This monu-

[1] A poem sacred to the Memory of Sir Isaac Newton.

[2] The *London Gazette*, April 4, 1727. No. 6569.

[3] These were the three children of his half-brother Smith, the three children of his half-sister Pilkington, and the two daughters of his half-sister Barton, all of whom survived Sir Isaac. *New Anecdotes of Sir Isaac Newton, by J. H., a Gentleman of his Mother's Family.* See *Annual Register*, 1776, vol. xix. p. 25 of Characters. The author of this paper was James Hutton, Esq. of Pimlico. See APPENDIX, No. XXXI.

ment was erected in 1731. On the front of a sarcophagus resting on a pedestal, are sculptured in basso relievo youths bearing in their hands the emblems of Sir Isaac's principal discoveries. One carries a prism, another a reflecting telescope, a third is weighing the sun and planets with a steelyard, a fourth is employed about a furnace, and two others are loaded with money newly coined. On the sarcophagus is placed the figure of Sir Isaac in a cumbent posture, with his elbow resting on several of his works. Two youths stand before him with a scroll, on which is drawn a remarkable diagram relative to the solar system, and above that is a converging series. Behind the sarcophagus is a pyramid, from the middle of which rises a globe in mezzo relievo, upon which several of the constellations are drawn, in order to show the path of the comet in 1681, whose period Sir Isaac had determined, and also the position of the solstitial colure mentioned by Hipparchus, and by means of which Sir Isaac had, in his Chronology, fixed the time of the Argonautic expedition. A figure of Astronomy as Queen of the Sciences sits weeping on the Globe with a sceptre in her hand, and a star surmounts the summit of the pyramid. The following epitaph is inscribed on the Monument :—

Hic situs est
Isaacus Newton, Eques Auratus,
Qui animi vi prope divina,
Planetarum Motus, Figuras,
Cometarum semitas, Oceanique Æstus,
Sua Mathesi facem preferente,
Primus demonstravit.
Radiorum Lucis dissimilitudines,
Colorumque inde nascentium proprietates.
Quas nemo antea vel suspicatus erat, pervestigavit.
Naturæ, Antiquitatis, S. Scripturæ,
Sedulus, sagax, fidus Interpres,
Dei Opt. Max. Majestatem philosophia asseruit,
Evangelii simplicitatem moribus expressit.
Sibi gratulentur Mortales, tale tantumque extitisse
HUMANI GENERIS DECUS.
Natus xxv. Decemb. MDCXLII. Obiit xx. Mar.
MDCCXXVII.

Of which the following is a literal translation :—

Here Lies
Sir Isaac Newton, Knight,
Who, by a vigour of mind almost supernatural,
First demonstrated
The motions and Figures of the Planets,
The Paths of the Comets, and the Tides of the Ocean.
He diligently investigated
The different refrangibilities of the Rays of Light,
And the properties of the Colours to which they give rise.
An Assiduous, Sagacious, and Faithful Interpreter
of Nature, Antiquity, and the Holy Scriptures,
He asserted in his Philosophy the Majesty of God,
And exhibited in his Conduct the simplicity of the Gospel.
Let Mortals rejoice
That there has existed such and so great
AN ORNAMENT OF THE HUMAN RACE.
Born 25th Dec. 1642, Died 20th March 1727.

In the beginning of 1731, a medal was struck at the Tower in honour of Sir Isaac Newton. It had on one side the head of the philosopher, with the motto, *Felix cognoscere causas*, and on the reverse a figure representing the mathematics.

On the 4th July 1755, a magnificent full-length statue of Sir Isaac Newton, in white marble, was erected in the ante-chapel of Trinity College. He is represented standing on a pedestal in a loose gown, holding a prism, and looking upwards with an expression of deep and successful thought. On the pedestal is the inscription,—

Qui genus humanum ingenio superavit.[1]

This statue, an engraving of which, taken from a photograph by the Rev. W. Kingsley, forms the frontispiece to this volume, was executed by Roubilliac, and erected at the expense of Dr.

[1] Qui genus humanum ingenio superavit, et omnes
Perstrinxit Stellas exortus ut Ethereus Sol.—*Lucretius.*

In genius who surpassed mankind as far
As does the mid-day Sun the midnight Star.—*Dryden.*

Robert Smith, the author of the *Compleat System of Optics*, and Professor of Astronomy and Experimental Philosophy at Cambridge. The statue has been thus described by a modern poet :—

> Hark where the organ full and clear,
> With loud hosannahs charms the ear ;
> Behold, a prism within his hands,
> Absorbed in thought great Newton stands ;
> Such was his brow, and looks serene,
> His serious gait and musing mien,
> When taught on eagle wings to fly,
> He traced the wonders of the sky ;
> The chambers of the sun explored,
> Where tints of thousand hues were stored.

Dr. Smith likewise bequeathed the sum of £500 for executing a painting on glass for the window at the south end of Trinity College, Cambridge. The subject represents the presentation of Sir Isaac Newton to his Majesty George III., who is seated under a canopy with a laurel chaplet in his hand, and attended by the British Minerva, apparently advising him to reward merit in the person of the great philosopher. Below the throne, the Lord Chancellor Bacon is, by an anachronism legitimate in art, proposing to register the reward about to be conferred upon Sir Isaac. The original drawing of this picture was executed by Cypriani, and cost one hundred guineas.

A colossal statue in bronze, executed by Mr. Theed, was erected in honour of Sir Isaac Newton on St. Peter's Hill, Grantham, in 1858. It represents the philosopher in the costume of the day, and in the gown of a Master of Arts, in the act of lecturing, his right hand pointing to a diagram taken from the *Principia*, and drawn upon a scroll in his left hand. The figure is twelve feet high, and weighs upwards of two tons, half of which, in the shape of old cannon, was contributed by Government. The pedestal which supports the statue is fourteen feet high, and was executed from Mr. Theed's design by

Mr. Rogers, from the marble quarries near Holyhead. The total height of the monument is twenty-six feet. The statue faces the west, and looks down upon the road along which Sir Isaac must have walked on his way to Grantham.[1]

This noble monument was inaugurated by Lord Brougham on the 21st September 1858 in an eloquent oration, which has been translated into French, and published in Paris, along with his discourse on Popular Literature, read at the meeting of the Association for the Promotion of Social Science, which was held at Liverpool in 1858.

The personal estate of Sir Isaac Newton, which was worth about £32,000, was divided among his four nephews and four nieces of the half-blood, the grandchildren of his mother by the Rev. Mr. Smith. The family estates of Woolsthorpe and Sustern went to John Newton, the heir-at-law, whose great-grandfather was Sir Isaac's uncle. This gentleman sold them in 1732 to Edmund Turnor, Esq. of Stoke Rocheford.[2] A short time before his death, Sir Isaac gave away an estate in the parish of Baydon, in Wiltshire, to the sons and daughter of a brother of Mrs. Conduitt,[3] who, in consequence of their father dying before Sir Isaac, had no share in the personal estate; and he also gave an estate of the same value, which he bought at Kensington, to Catherine, the only daughter of Mr. Conduitt, who afterwards married Mr. Wallop. This lady was afterwards Viscountess Lymington, and the estate of Kensington descended to the Earl of Portsmouth, by whom it was sold. Sir Isaac

[1] The amount of the subscription for the monument was £1630, of which £600 was rom Grantham and its vicinity.

[2] Turnor's *Collections*, &c., p. 158. See APPENDIX, No. XXXI.

[3] These were Robert and Newton Barton, and Mrs. Burr, the children of Colonel Barton. This branch of the family became extinct by the death of the Rev. John Barton. The present Charles Cutts Barton, Esq., is the heir-male of Geoffry Barton, a half-brother of Mr. Conduitt, and the great-grandson of the widow of Colonel Barton, by her marriage with Colonel Gardner, whose only daughter married the Rev. Cutts Barton, Dean of Bristol.

was succeeded as Master and Worker in the Mint by his nephew, John Conduitt, Esq.[1]

[1] Mr. Conduitt's appointment to this office was announced in the Gazette immediately after the official notification of Sir Isaac's funeral. In a MS. entitled *Memorandums touching Mr. John Conduitt,* it is stated that he went to Westminster School on the 28th June 1691,—to Westminster College in June 1701,—and, in June 1705, to Trinity College, Cambridge, where he continued till June 1707. On the 8th July he set out on his travels to Holland, Germany, &c., and returned in May 1710. Between 1710 and 1711 he went twice to Portugal, and in 1713 he visited Gibraltar, from which he returned in May 1717. On the 26th of August 1717, he was married to "Mrs. Katherine Barton" in Russell-Court Chapel, as proved by the marriage certificate now before me, written on vellum, signed by John Heylin, minister (a fellow-student of Conduitt's at Trinity, and afterwards Prebend of Westminster), and witnessed by Bernard Fletcher Clark, and Anne Powell. He sat in Parliament for Whitchurch in the Parliaments which met on March 17, 1715, October 9, 1722, and January 23, 1728. He was re-elected in 1717, after his appointment to the Mint. In 1734, when he was elected both for Southampton and Whitchurch, he had the same number of votes at Southampton as his competitor, Anthony Hanley, but as this gentleman was found not to be duly elected, Mr. Conduitt made his election for Southampton. He was born in 1688, and died at Cranbury on the 20th May 1737, in the forty-ninth year of his age. His widow, Mrs. Conduitt, erected in 1738 a handsome monument to his memory in Westminster Abbey. Mrs. Conduitt died on the 20th January 1739, in the fifty-ninth year of her age.

CHAPTER XXVII.

Permanence of Newton's Reputation—Character of his Genius—His method of Investigation similar to that used by Galileo—Error in ascribing his Discoveries to the use of the Methods recommended by Lord Bacon—The pretensions of the Baconian Philosophy examined—Sir Isaac Newton's Social, Religious, and Moral Character—His Hospitality and Mode of Life—His Generosity and Charity—His Personal Appearance—Statues and Pictures of him—Memorials and Recollections of him—His Manuscripts and Papers.

Such were the last days of Sir Isaac Newton, and such the laurels that were shed over his grave. A century of discoveries has, since his time, been added to science; but brilliant as these discoveries are, they have not obliterated the minutest of his labours, and have served only to brighten the halo which encircles his name. The achievements of genius, like the source from which they spring, are indestructible. Acts of legislation and deeds of war may confer a high celebrity, but the reputation which they bring is local and transient; and while they are hailed by the nation which they benefit, they are reprobated by the people whom they ruin or enslave. The labours of science, on the contrary, bear along with them no counterpart of evil. They are the liberal bequests of great minds to every individual of their race; and wherever they are welcomed and honoured, they become the solace of private life, and the ornament and bulwark of the commonwealth.

The importance of Sir Isaac Newton's discoveries has been sufficiently exhibited in the preceding chapters. The peculiar character of his genius, and the method which he pursued in his inquiries, can be gathered only from the study of his works, and from the history of his individual labours. Were we to judge of the qualities of his mind from the early age at which he made his principal discoveries, and from the rapidity of their succession, we should be led to ascribe to him that quickness of penetration, and that exuberance of invention, which is more characteristic of poetical than of philosophical genius. But we must recollect that Newton was placed in the most favourable circumstances for the development of his powers. The flower of his youth, and the vigour of his manhood, were entirely devoted to science. No injudicious guardian controlled his ruling passion, and no ungenial studies or professional toils interrupted the continuity of his pursuits. His discoveries were therefore the fruit of persevering and unbroken study; and he himself declared, that whatever service he had done to the public was not owing to any extraordinary sagacity, but solely to industry and patient thought.

Initiated early into the abstractions of geometry, he was deeply imbued with her cautious spirit: and if his acquisitions were not made with the rapidity of intuition, they were at least firmly secured; and the grasp which he took of his subject was proportional to the mental labour which it had exhausted. Overlooking what was trivial, and separating what was extraneous, he bore down with instinctive sagacity on the prominences of his subject, and having thus grappled with its difficulties, he never failed to entrench himself in its strongholds.

To the highest powers of invention Newton added, what so seldom accompanies them, the talent of simplifying and communicating his profoundest speculations.[1] In the economy of her distributions, nature is seldom thus lavish of her intellectual gifts. The inspired genius which creates is rarely conferred

[1] This valuable faculty characterizes all his writings, whether theological, chemical, or mathematical; but it is peculiarly displayed in his Treatise on Universal Arithmetic, and in his Optical Lectures.

along with the matured judgment which combines, and yet, without the exertion of both, the fabric of human wisdom could never have been reared. Though a ray from heaven kindled the vestal fire, yet an humble priesthood was required to keep alive the flame.

The method of investigating truth by observation and experiment, so successfully pursued in the *Principia*, has been ascribed by some modern writers of celebrity to Lord Bacon; and Sir Isaac Newton is represented as having owed all his discoveries to the application of the principles of that distinguished writer. One of the greatest admirers of Lord Bacon has gone so far as to characterize him as a man who has had no rival in the times which are past, and as likely to have none in those which are to come. In a eulogy so overstrained, we feel that the language of panegyric has passed into that of idolatry; and we are desirous of weighing the force of arguments which tend to depose Newton from the high-priesthood of nature, and to unsettle the proud destinies of Copernicus, Galileo, and Kepler.

That Bacon was a man of powerful genius, and endowed with varied and profound talent,—the most skilful logician,—the most nervous and eloquent writer of the age which he adorned, are points which have been establisned by universal suffrage. The study of ancient systems had early impressed him with the conviction that experiment and observation were the only sure guides in physical inquiries; and, ignorant though he was of the methods, the principles, and the details of the mathematical sciences, his ambition prompted him to aim at the construction of an artificial system by which the laws of nature might be investigated, and which might direct the inquiries of philosophers in every future age. The necessity of experimental research, and of advancing gradually from the study of facts to the determination of their cause, though the groundwork of Bacon's method, is a doctrine which was not only inculcated, but successfully followed by preceding philosophers. In a letter from Tycho Brahe to Kepler, this industrious astronomer urges his pupil "to lay a solid foundation for his views by actual observation, and then by ascending from these to strive to reach the causes of things;" and it was no doubt under the influence of this advice that Kepler submitted his wildest fancies to the test of observation, and was conducted to his most splendid discoveries. The reasonings of Copernicus, who preceded Bacon by more than a century, were all founded upon the most legitimate induction. Dr. Gilbert had exhibited in his Treatise on the Magnet[1] the most perfect specimen of physical research. Leonardo da Vinci had described in the clearest manner the proper method of philosophical investigation;[2] and the whole scientific career of Galileo was one continued example of the most sagacious application of observation and experiment to the discovery of general laws. The names of Paracelsus, Van Helmont, and Cardan, have been ranged in opposition to this constellation of great names; and while it is admitted that even they had thrown off the yoke of the schools, and had succeeded in experimental research, their credulity and their pretensions have been adduced as a proof that to the "bulk of philosophers" the method of induction was unknown. The fault of this argument consists in the conclusion being infinitely more general than the fact. The errors of these men were not founded on their ignorance, but on their presumption. They wanted the patience of philosophy, and not her methods. An excess of vanity, a waywardness of fancy, and an insatiable appetite for that species of passing fame which is derived from eccentricity of opinion, moulded the reasonings and disfigured the writings of these ingenious men; and it can scarcely admit of a doubt, that, had they lived in the present age, their philosophical character would have received the same impress from the peculiarity of their tempers

[1] *De Magnete*, pp. 42, 52, 169, and Preface, p. 30.

[2] See *Essai sur les Ouvrages Physico-mathématiques de Leonard de Vinci*, par J. B. Venturi. Paris, 1799, pp. 32, 33, &c. See also Carlo Amoretti's *Memorie storiche su la vita gli studi e le Opere de Leonardo da Vinci*. Milano, 1804.

and dispositions. This is an experiment, however, which cannot now be made; but the history of modern science supplies the defect, and the experience of every man furnishes a proof, that in the present age there are philosophers of elevated talents and inventive genius who are as impatient of experimental research as Paracelsus, as fanciful as Cardan, and as presumptuous as Van Helmont.

Having thus shown that the distinguished philosophers who flourished before Bacon were perfect masters both of the principles and practice of inductive research, it becomes interesting to inquire whether or not the philosophers who succeeded him acknowledged any obligation to his system, or derived the slightest advantage from his precepts. If Bacon constructed a method to which modern science owes its existence, we shall find its cultivators grateful for the gift, and offering the richest incense at the shrine of a benefactor whose generous labours conducted them to immortality. No such testimonies, however, are to be found. Nearly two hundred years have gone by, teeming with the richest fruits of human genius, and no grateful disciple has appeared to vindicate the rights of the alleged legislator of science. Even Newton, who was born and educated after the publication of the *Novum Organon*, never mentions the name of Bacon or his system; and the amiable and indefatigable Boyle treated him with the same disrespectful silence. When we are told, therefore, that Newton owed all his discoveries to the method of Bacon, nothing more can be meant than that he proceeded in that path of observation and experiment which had been so warmly recommended in the *Novum Organon*; but it ought to have been added, that the same method was practised by his predecessors; that Newton possessed no secret that was not used by Galileo and Copernicus; and that he would have enriched science with the same splendid discoveries if the name and the writings of Bacon had never been heard of.[1]

[1] "The man who first discovered that cold freezes water, and that heat turns it into

From this view of the subject we shall now proceed to examine the Baconian process itself, and consider if it possesses any merit as an artificial method of discovery, or if it is at all capable of being employed, for this purpose, even in the humblest walks of scientific inquiry.

The process of Lord Bacon, was, we believe, never tried by any philosopher but himself. As the subject of its application, he selected that of heat. With his usual erudition, he collected all the facts which science could supply—he arranged them in tables—he cross-questioned them with all the subtlety of a pleader—he combined them with all the sagacity of a judge—and he conjured them by all the magic of his exclusive processes. But, after all this display of physical logic, nature thus interrogated was still silent. The oracle which he had himself established refused to give its responses, and the ministering priest was driven with discomfiture from his shrine. This example, in short, of the application of his system will remain to future ages as a memorable instance of the absurdity of attempting to fetter discovery by any artificial rules.

Nothing even in mathematical science can be more certain than that a collection of scientific facts are of themselves incapable of leading to discovery, or to the determination of general laws, unless they contain the predominating fact or relation in which the discovery mainly resides. A vertical column of arch-stones possesses more strength than the same materials arranged in an arch without the key-stone. However nicely they are adjusted, and however nobly the arch may spring, it never can possess either equilibrium or stability. In this comparison all the facts are supposed to be necessary to the final result; but, in the inductive method, it is im-

vapour, proceeded on the same general principles and on the same method by which Newton discovered the law of gravitation, and the properties of light. His *Regulæ Philosophandi* are maxims of common sense, and are practised every day in common life; and he who philosophizes by other rules, either concerning the material system or concerning the mind, mistakes his aim."—Reid's *Inquiry into the Human Mind.* Introduction.

possible to ascertain the relative importance of any facts, or even to determine if the facts have any value at all, till the master fact which constitutes the discovery has crowned the zealous efforts of the aspiring philosopher. The mind then returns to the dark and barren waste over which it has been hovering; and by the guidance of this single torch it embraces, under the comprehensive grasp of general principles, the multifarious and insulated phenomena which had formerly neither value nor connexion. Hence it must be obvious to the most superficial thinker, that discovery consists either in the detection of some concealed relation—some deep-seated affinity which baffles ordinary research, or in the discovery of some simple fact which is connected by slender ramifications with the subject to be investigated; but which, when once detected, carries us back by its divergence to all the phenomena which it embraces and explains.

In order to give additional support to these views, it would be interesting to ascertain the general character of the process by which a mind of acknowledged power actually proceeds in the path of successful inquiry. The history of science does not furnish us with much information on this head, and if it is to be found at all, it must be gleaned from the biographies of eminent men. Whatever this process may be in its details, if it has any, there cannot be the slightest doubt that in its generalities at least it is the very reverse of the method of induction. The impatience of genius spurns the restraints of mechanical rules, and never will submit to the plodding drudgery of inductive discipline. The discovery of a new fact unfits even a patient mind for deliberate inquiry. Conscious of having added to science what had escaped the sagacity of former ages, the ambitious discoverer invests his new acquisition with an importance which does not belong to it. He imagines a thousand consequences to flow from his discovery: he forms innumerable theories to explain it, and he exhausts his fancy in trying all its possible relations to recognised difficulties and unexplained facts. The reins, however, thus freely given to his imagination, are speedily drawn up. His wildest conceptions are all subjected to the rigid test of experiment, and he has thus been hurried by the excursions of his own fancy into new and fertile paths, far removed from ordinary observation. Here the peculiar character of his own genius displays itself by the invention of methods of trying his own speculations, and he is thus often led to new discoveries far more important and general than that by which he began his inquiry. For a confirmation of these views, we may refer to the history of Kepler's Discoveries; and if we do not recognise them to the same extent in the labours of Newton, it is because he kept back his discoveries till they were nearly perfected, and therefore withheld the successive steps of his inquiries.[1]

The social character of Sir Isaac Newton was such as might have been expected from his intellectual attainments. He was modest, candid, and affable, and without any of the eccentricities of genius, suiting himself to every company, and speaking of himself and others in such a manner that he was never even suspected of vanity. "But this," says Dr. Pemberton, "I immediately discovered in him, which at once both surprised and charmed me. Neither his extreme great age, nor his universal reputation, had rendered him stiff in opinion, or in any degree elated. Of this I had occasion to have almost daily experience. The remarks I continually sent him by letters on the *Principia* were received with the utmost goodness. These were, so far from being anyways displeasing to him, that on the contrary they occasioned him to speak many kind things of

[1] The following interesting anecdote is related in Conduitt's MSS.:—"When Sir A. Fountaine was at Berlin with Leibnitz in 1701, and at supper with the Queen of Prussia, she asked Leibnitz his opinion of Sir Isaac Newton. Leibnitz said that taking mathematicians from the beginning of the world to the time when Sir Isaac lived, what he had done was much the better half; and added that he had consulted all the learned in Europe upon some difficult points without having any satisfaction, and that when he applied to Sir Isaac, he wrote him in answer by the first post, to do so and so, and then he would find it."

me to my friends, and to honour me with a public testimony of his good opinion."

The modesty of Sir Isaac Newton, in reference to his great discoveries, was not founded on any indifference to the fame which they conferred,[1] or upon any erroneous judgment of their importance to science. The whole of his life proves, that he knew his place as a philosopher, and was determined to assert and vindicate his rights. His modesty arose from the depth and extent of his knowledge, which showed him what a small portion of nature he had been able to examine, and how much remained to be explored in the same field in which he had himself laboured. In the magnitude of the comparison he recognised his own littleness; and a short time before his death he uttered this memorable sentiment: "I do not know what I may appear to the world, but to myself I seem to have been only like a boy playing on the sea-shore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me." What a lesson to the vanity and presumption of philosophers,—to those especially who have never even found the smoother pebble or the prettier shell! What a preparation for the latest inquiries, and the last views of the decaying spirit,—for those inspired doctrines which alone can throw a light over the dark ocean of undiscovered truth!

[1] The following anecdote is recorded by Conduitt, as showing Sir Isaac's indifference to fame:—"Mr. Molyneux related to us that after he and Mr. Graham and Dr. Bradley had put up a perpendicular telescope at Kew, to find out the parallax of the fixed stars, they found a certain nutation of the earth which they could not account for, and which Molyneux told me he thought destroyed entirely the Newtonian system: and therefore he was under the greatest difficulty how to break it to Sir Isaac. And when he did break it by degrees, in the softest manner, all Sir Isaac said in answer was, when he had told me his opinion, '*It may be so, there is no arguing against facts and experiments*,—so cold was he to all sense of fame at a time when, as Tillotson said, a man has formed his last understanding.'" This conversation must have taken place in 1726, when Molyneux's instrument was in use at Kew; but the nutation, though proposed at that time as an explanation of the change of declination of γ Draconis, was not discovered till 1747 by Bradley.—See Rigaud's *Life of Bradley*, p. lxii. and pp. 2, 3.

In the religious and moral character of our author, there is much to admire and to imitate. While he exhibited in his life and writings an ardent regard for the general interests of religion, he was at the same time a firm believer in Revelation. He was too deeply versed in the Scriptures, and too much imbued with their spirit, to judge harshly of other men who took different views of them from his own. He cherished the great principles of religious toleration, and never scrupled to express his abhorrence of persecution, even in its mildest form. Immorality and impiety he never permitted to pass unreproved. When Vigani told him "a loose story about a nun," he gave up his acquaintance, and when Dr. Halley[1] ventured to say anything disrespectful to religion, he invariably checked him, with the remark, "I have studied these things,—you have not."

He considered cruelty to "brute beasts" as a violation of Christian morality, and such was his tenderness for the lower creation, that he could not tolerate the sports of hunting or shooting animals. When Mr. Conduitt was one day speaking favourably of a nephew of Sir Isaac's, he urged it as an objection against him, "that he loved killing of birds."[2]

The native simplicity of Sir Isaac Newton's mind is finely portrayed in the affecting letter in which he acknowledges to Locke, that he had thought and spoken of him uncharitably; and the humility and candour in which he asks forgiveness could have emanated only from a mind as noble as it was pure.

[1] Mr. Hearne, in a memorandum dated April 4, 1726, states that a great quarrel happened between Sir Isaac Newton and Mr. Halley. We have not been able to find any traces of it. If we suppose the above date to be 1727, the rumour of a quarrel may have originated in the fact, that on the 2d March 1727, Sir Isaac had called attention to the omission on Halley's part, as Astronomer-Royal, to send to the Society a copy of his Annual Observations, as required by the late Queen's letter.—See *Memoirs of the Astronomical Society*, vol. viii. p. 188.

[2] "Whiston," says Mrs. Conduitt, "had spread it abroad that Sir Isaac abstained from eating rabbits because strangled, and from black puddings, because made of blood. This," she adds, "is not true. Sir Isaac said that meats strangled were forbidden, because that was a painful death, and the letting out the blood the easiest,—that animals should be put to as little pain as possible, and that the reason why eating blood was forbidden, was because it was thought eating of blood inclined men to be cruel.—C. C."

When Locke wrote to Sir Peter King that Newton "is a nice man to deal with, and a little too apt to raise in himself suspicions where there is no ground," he referred to an imperfection of character which we have not scrupled to notice, whether in his controversies with Hooke or with Flamsteed. It would be a sacrifice of truth, and an empty compliment to the memory of so great a man, to speak of him as exempt from the infirmities of our common nature. When Bishop Burnet has said that he valued him for something still more valuable than all his philosophy—for having the *whitest* soul he ever knew—we may well decline to search for shades on a tablet so pure. It is far from the duty of a biographer, who has been permitted to inspect the private and sacred relics of the dead, to sit in judgment on the failings they may disclose. It is enough that he deals honestly with what is known, and makes no apology for what is socially or morally wrong. Other biographers are under no such restraint. In searching even the recesses of great minds for the manifestation of a common humanity, the philosopher may throw light upon those compensatory adjustments by which great talents and high position are sometimes united with social and even moral failings. If in estimating the character of Newton, Professor De Morgan has pointed out more conspicuously than other biographers the failings to which we have referred, he has yet drawn his character with such tenderness and truth, that we accept of it as a noble tribute to the noblest of our race. "It is enough," he says, "that Newton is the greatest of philosophers, and one of the best of men;"—that "we cannot find in his character an acquired failing;"—"that all his errors are to be traced to a disposition which seems to have been born with him; and that, admitting them in their fullest extent, he remains an object of unqualified wonder, and all but unqualified respect." Nor is the tribute of the poet less just than that of the mathematician. When Pope expressed a wish for "some memoirs and character of Newton as a private man," he did "not doubt that his life and manners would make as great a discovery of virtue and goodness and rectitude of heart, as his works have done of penetration and the utmost stretch of human knowledge."[1]

After Sir Isaac took up his residence in London, he lived in a very handsome style, and kept his carriage, with an establishment of three male and three female servants. In his own house he was hospitable and kind, and on proper occasions he gave splendid entertainments, though without ostentation or vanity. In his diet he was frugal, and in all his habits temperate. When he was asked to take snuff or tobacco, he declined, remarking "that he would make no necessities to himself." His dress was always simple, but on one occasion, when he opposed the Honourable Mr. Annesley in 1705, as a candidate for the University, he is said to have put on a suit of laced clothes.

Sir Isaac does not appear to have had much taste for the fine arts. He used to say of his friend the Earl of Pembroke, "that he was a lover of *stone dolls*."[2] Notwithstanding his dislike of sculpture, he seems to have been considered a judge of pictures. He was chosen a "Commissioner for Paintings, and having a dispute with Archbishop Wake, whom he opposed, he told a story of a Bishop who said on that subject, 'that when this snow (pointing to his grey hairs) falls, there will be a great deal of dirt in churches.' After this he went to no more of their meetings."[3]

His generosity and charity had no bounds, and he used to remark, that they who gave away nothing till they died, never gave at all. Though his wealth had become considerable by a prudent economy, yet he had always a contempt for money, and he spent a considerable part of his income in relieving the poor—in assisting his relations,[4] and in encouraging ingenuity

1 Pope's letter to Conduitt. See APPENDIX, No. XXV.

2 *Conduitt's MSS.*

3 This anecdote, which may relate to the putting up of pictures in churches, I have given in the words of Mrs. Conduitt, with whose initials it is signed.

4 "He was very kind to all the Ayscoughs. To one he gave £800, to another £200, and to a third £100, and many other sums; and other engagements did he enter into

and learning.[1] He was scrupulously exact and regular in all matters of business; and though he disregarded money, allowing his rents often to remain unpaid, he had a deep sense of justice, and was very strict in demanding from his tenants at Woolsthorpe, even in very small matters, a rigorous performance of their obligations.[2] His conduct, however, was not always influenced by this principle. When he had been imposed upon in purchasing an estate at Baydon, in Wiltshire, for which he had paid double its value, and was told that "he might vacate the bargain in equity," he replied, "that he would not for the sake of two thousand pounds go into Westminster Hall to tell that he had been made a fool of."[3] The same unwillingness to have recourse to legal proceedings showed itself on another occasion. He one day missed bank bills to the amount of upwards of £3000, and he suspected that his pocket had been picked by W. Whiston, a nephew of Whiston, who had bought an estate in land of that value without any

also for them. He was the ready assistant of all who were any way related to him—to their children and grandchildren."—*Annual Register*, 1776, vol. xix. p. 25. He gave a regular allowance to his niece, Mrs. Pilkington, and on the 12th August 1725, he presented £100 to Mary Clarke to "augment her portion."

[1] He gave money to Stirling, and brought him from Venice; and in 1719 and 1720 he presented to Pound, the astronomer, one hundred guineas, in two gifts of fifty guineas each.—Rigaud's *Bradley*, p. iii., in note.

[2] In 1687-8 he had a law-suit with Mr. Storer, his tenant at Woolsthorpe, in order to compel him to scour the drains, and repair the thatch, and the walls, and palings of the swine-cot and cow-house, which he was bound by his lease to leave in good order. I have found the scroll of a long and characteristic letter addressed to a friend, "who had undertaken the office of an arbitrator." He thanks him for doing so, and expresses his hearty wish that he "may inherit the blessing promised to peace-makers."—See Appendix, No. XXXII.

There is another scroll of a short letter to "Cosin John Newton," his heir-at-law, written about May 1720, and of a similar character. "I understand," he says, "that Thomas Hubbard agreed with you to leave his farm at Lady-day next, and that I was to allow him ten pounds for his manure. But now I am told that he would become tenant to it at eleven pounds per annum. This would be departing from the bargain already made, in order to make a new one. But there being sufficient witnesses of the bargain already made, I expect that he stand to it, and I desire you to demand it of him in my name, and to send me his answer, if he refuses to sign articles pursuant to what has been already agreed upon."

[3] *Conduitt's MSS.*

visible means of paying for it. Notwithstanding the magnitude of the loss, he could not be prevailed upon to prosecute the supposed delinquent, and when Mr. Conduitt asked him how much he had lost, he only answered "too much."[1]

His liberality, indeed, was in some instances excessive. On one occasion he offered Cheselden as a fee a handful of guineas out of his coat pocket, and when he refused them, saying that a guinea or two was the most he ought to have, Sir Isaac laughing said, "Suppose I do give you more than your fee."[2] To Dr. Cheyne, who refused to take money from him, he was less indulgent. According to a statement made by Dr. Arbuthnot to Conduitt, he one day told Sir Isaac that Dr. Cheyne had written an ingenious book on mathematics, but that he had not money to print it. "Bring it to me," said Sir Isaac; and when the manuscript was brought to him, he offered Cheyne a bag of money, which he refused, and "Newton would see him no more."[3]

The habits of deep meditation which Sir Isaac Newton had acquired, though they did not show themselves in his intercourse with society, exercised their full influence over his mind when in the midst of his own family. Absorbed in thought he would often sit down on his bedside after he rose, and remain there for hours without dressing himself, occupied with some interesting investigation which had fixed his attention. Owing to the same absence of mind, he neglected to take the requisite quantity of nourishment, and it was therefore often necessary to remind him of his meals.[4]

In his personal appearance, Sir Isaac Newton was not above the middle size, and in the latter part of his life was inclined to be corpulent. According to Mr. Conduitt, "he had a very lively and piercing eye, a comely and gracious aspect, with a fine head of hair as white as silver, without any baldness, and when his peruke was off was a venerable sight." Bishop Atter-

[1] *Conduitt's MSS.* [2] *Ibid.* [3] *Ibid.*

[4] See this volume, pp. 48 and 52.

bury asserts,[1] on the other hand, that the lively and piercing eye did not belong to Sir Isaac during the last twenty years of his life. "Indeed," says he, "in the whole air of his face and make there was nothing of that penetrating sagacity which appears in his compositions. He had something rather languid in his look and manner which did not raise any great expectation in those who did not know him." This opinion of Bishop Atterbury is confirmed by an observation of Mr. Thomas Hearne,[2] who says that "Sir Isaac was a man of no very promising aspect. He was a short well-set man. He was full of thought, and spoke very little in company, so that his conversation was not agreeable. When he rode in his coach one arm would be out of his coach on one side, and the other on the other." Sir Isaac never wore spectacles, and never "lost more than one tooth to the day of his death.'

Beside the statue of Sir Isaac Newton executed by Roubilliac, there is a bust of him by the same artist in the library of Trinity College, Cambridge.[3] Several good paintings of him are extant. Two of these are in the hall of the Royal Society of London, and have, we believe, been often engraved. Another, by Vanderbank, is in the small combination-room in Trinity College, and has been engraved by Vertue. Another, by Valentine Ritts, is in the Hall of Trinity College; but the best, from which the frontispiece to Volume I. is copied, was painted by Sir Godfrey Kneller, and is in the possession of Lord Egremont at Petworth.[4] At Hurtsbourne Park there are several

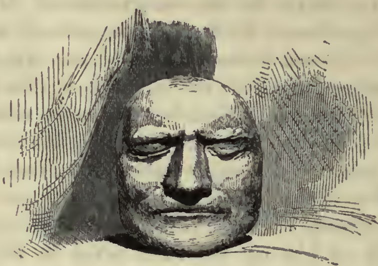
[1] *Epistolary Correspondence*, vol. i. p. 180. Sect. 77.

[2] MSS. Memoranda in the Bodleian Library.

[3] It is not true, as has been stated, that the original of this bust is in the possession of the Marquis of Lansdowne. The bust of Newton at Bowood Park is a copy of the one in the Library of Trinity, executed for his Lordship by Bailey.

[4] "I have taken," says Dr. Stukely, "several sketches from his side face, which are very like him. I being present with him and Sir Godfrey (Kneller) at painting his great picture to be sent to France, desired Sir Isaac to let Sir Godfrey paint his side face, a profile as we call it, for me. 'What!' said Sir Isaac, 'would you make a medal of me?' and refused it, though I was then in highest favour with him."—Stukely's *Letter to Conduitt*, Grantham, July 22, 1727.

portraits of Newton, the most interesting of which,[1] taken in his old age by Sir James Thornhill, is in the chapel, representing him at full length, seated on a chair, and in his own hair.[2] In Trinity College Library there is a cast from his face after death, by Roubilliac, from which the following engraving is taken, after a photograph from the cast by the Rev. Mr. Kingsley.



Every memorial of so great a man as Sir Isaac Newton has been preserved and cherished with peculiar veneration. His house at Woolsthorpe, of which we have given an engraving, has been religiously protected by Mr. Turnor of Stoke Roche-

[1] Owing to the state of the varnish, Mr. Henneman tried in vain to obtain a photograph of this picture, for the purpose of being engraved for this work.

[2] In the Library of the University of St. Andrews there is a mezzotinto portrait of Sir Isaac, with the following inscription in capital letters beneath the picture :—

SIR ISAAC NEWTON.

Drawn and scraped, MDCCLX, by James Macardel, from an original portrait painted by Enoch Seeman, now in the possession of Thomas Willis, F.R. and A.SS.

Les Italiens, ces peuples ingénieux, ont craint de penser. Les Français n'ont osé penser qu'à demie, et les Anglais, qui ont volé jusqu'au ciel, *parcequ'on ne levra point coupé les ailes*, sont devenu les précepteurs des nations. Nous leur devont tout depuis les loix primitives de la gravitation, depuis le calcul de l'infinit, et la connaissance précise de la lumière, si vainement combattues, jusqu'à la nouvelle charrue et à l'insertion de la petite vérole combattues encore.—*Ode sur la mort de Madame de Bareith, avec une lettre par* Mons. de Voltaire.

ford, the proprietor. Dr. Stukely, who visited it in Sir Isaac's lifetime on the 13th October 1721, gives the following description of it in his letter to Dr. Mead, written in 1727 :—"'Tis built of stone, as is the way of the country hereabouts, and a reasonable good one. They led me up stairs and showed me Sir Isaac's study, where I suppose he studied when in the country in his younger days, or perhaps when he visited his mother from the University. I observed the shelves were of his own making, being pieces of deal boxes which probably he sent his books and clothes down in on those occasions. There were some years ago two or three hundred books in it of his father-in-law, Mr. Smith, which Sir Isaac gave to Dr. Newton of our town."[1]

When the house was repaired in 1798, a tablet of white marble was put up by Mr. Turnor in the room where Sir Isaac was born, with the following inscription :—

"Sir Isaac Newton, son of John Newton, Lord of the Manor of Woolsthorpe, was born in this room on the 25th December 1642."

> Nature and Nature's laws lay hid in night,
> God said, "Let Newton be," and all was Light.[2]

The following lines have been written upon the house :—

> Here Newton dawned, here lofty wisdom woke,
> And to a wondering world divinely spoke.
> If Tully glowed, when Phædrus' steps he trode,
> Or fancy formed Philosophy a God :
> If sages still for Homer's birth contend,
> The Sons of Science at this dome must bend.
> All hail the shrine ! All hail the natal day,
> Cam boasts his noon,—This *Cot* his morning ray.

The celebrated apple-tree, the fall of one of the apples of

[1] Turnor's Collections, p. 176.

[2] The original of these lines, which we have seen in Pope's own handwriting, is slightly different, and inferior to those in the text :—

> Nature and all her laws lay hid in night,
> God said, "Let Newton be," and all was Light.

which is said to have turned the attention of Newton to the subject of gravity, was taken up in 1820 in a state of decay.[1]

Many interesting relics of Sir Isaac have been preserved with religious care. His reflecting telescope, as we have seen, is in the possession of the Royal Society of London; and in the Library of Trinity College, Cambridge, there is a large collection of articles which belonged to him—a terrestrial globe, a ring dial, a small brass quadrant, a mariner's compass, a parallel ruler, a pair of compasses, three locks of his silver-white hair, one of which was presented by Colonel C. Burr in 1835. The door of his book-case is in the museum of the Royal Society of Edinburgh. A descendant of Dr. Bentley possesses a watch presented to him by Newton in 1717;[2] and among the memorials given to the Royal Society by the Rev. Mr. Turnor, is a gold watch, said to have been presented by Mrs. Conduitt to Sir Isaac in January 1708.[3] A chair, said to

[1] The anecdote of the falling apple is not mentioned by Dr. Stukely, nor by Pemberton, who conversed with Newton about the origin of his discoveries, and mentions the anecdote of Newton's sitting in a garden. I find, however, a reference to an apple-tree in the following memorandum by Conduitt. "In the same year (at his mother's in Lincolnshire), when musing in a garden it came into his thoughts that the same power of gravity, which made an apple fall from the tree to the ground, was not limited to a certain distance." See vol. i. p. 24, note.

After quoting some interesting passages from Kepler on gravity, Mr. Drinkwater Bethune justly remarks, "Who, after perusing such passages in the works of an author which were in the hands of every student of astronomy, can believe that Newton waited for the fall of an apple to set him thinking for the first time on the theory which has immortalized his name? An apple may have fallen, and Newton may have seen it; but such speculations as those which it is asserted to have been the cause of originating in him, had been long familiar to the thoughts of every one in Europe pretending to the name of Natural Philosopher."—*Life of Kepler*, p. 24. See vol. i. p. 235.

[2] "This is to acquaint you," says N. Facio, "that I have agreed with Mr. Benjamin Steele, the watchmaker, at £15, for him to make the watch for Dr. Bentley. It will be with four pierced rubies and four diamonds, and I hope will be worth the money."—Letter to Newton, dated Worcester, June 15, 1717.

[3] This date is obviously an error, as Miss Barton did not become Mrs. Conduitt till 1717. Professor De Morgan, who examined it, says, "that any one who looks at the inscription will see that it is not an old watch. It is neither ornamented nor placed in a shield or other envelope, while the case is beautifully chased, and has an elaborate design representing Fame and Britannia examining the portrait of Newton."—*Notes and*

have been used by Sir Isaac, is in the possession of J. Hogarth, Esq., London. It was sent to Grantham when his statue was inaugurated, and was occupied by Lord Brougham on the platform. Mr. Hogarth has given an account of the manner in which it came into his possession,[1] but he has produced no evidence whatever to establish the genuineness of the relic.

One of the most interesting and valuable relics of Sir Isaac is a silver box, beautifully carved, which he presented to the Earl of Abercorn, the great-grandfather of Sir George Hamilton Seymour, G.C.B., who has kindly placed it in my hands. It is three and a half inches in diameter, and one and a half inch deep.[2] It bears on the lid the Hamilton arms, a crown surmounted by a tree crossed with a saw; and the bottom of it as well as the lid is carved with enigmatical numbers, forming a perpetual Julian Kalendar. When Sir George was our ambassador at Brussels in 1840, and at St. Petersburg in 1853, he submitted the box to Professor Quetelet and M. Otto Struve of Pulkova, who have given a satisfactory explanation[3] of all

Queries, No. 210, p. 430. The dial-plate is obviously new. Mr. Turnor, in whose possession I saw the watch, told me that he purchased it in the Curiosity Shop at Warwick.

[1] King's *Biographical Sketch of Sir Isaac Newton*, p. 71.

[2] In the woodcut the light parts are silver, and the dark ground is filled up with a substance which is dark in all the compartments and shields containing numbers, and reddish in the merely ornamental portions.

[3] The following is the explanation given by M. Otto Struve:—

"The engravings compose a perpetual Julian Kalendar, and one very complete for the first 38 years of the last century, but which may still be partly used at the present day and in the future.

"1. *The Lid of the Box.*

"The numbers in the 19 shields which form its periphery, give in the first lines the dates of Easter for the years from 1700 to 1738. The month of *March* is there indicated by :, the month of *April* by A. In a shield (the 12th) we find also the sign + in the middle of two numbers of the *first* line (1 + 29). The sign here indicates that the *first* number belongs to the month of *April*, and the *second* to the month of *March*. In all the other cases the two numbers of the *first* line are those of the months indicated by the signs above mentioned.

"Each shield refers to two years, which are 19 years distant from one another. The *first* shield, which relates to the years 1700 and 1719, is that which is placed above the crown (beneath the Hamilton arms), and a little to the right. In setting out from this *first* shield in a direction to the right, the numbers in the *second* line indicate the two

THE LID OF THE BOX.

THE BOTTOM OF THE BOX.

the legends except the following, which is one of those upon the bottom of the box:—

Round For Sou: & Back For Shin: 4
4: 48 5: 36 6: 24 7: 12 8
From Sun Set or From Moons Rise

years after 1700 to which Easter corresponds in the first line. Such numbers are found only in each fourth shield between which the numbers corresponding to the intermediate years ought to be supplied. In place of numbers, the second line presents to us, for these intermediate years, the initials of the days of the week which refer to the dates given in the central square of the lid. All the dates in this central square fall upon the same day of the week, indicated for each year by the initials which we read in the second and in the third line of the peripheral shields. The sign + which we find near some of the initials, indicates that the corresponding year is leap year, and that for this reason the days of the week have made leaps of two days. In the shields where there are numbers in place of initials, we must supply the days of the week with the assistance of the initials in the adjoining shields.

"The initials in the third line of the peripheral shields are only the continuation of those in the second line. The numbers in the third line are the *golden numbers*, and correspond equally to the two years indicated in the second line. The large cross ✠ which is in the *eleventh* shield for the year 1710, indicates that in this year a new lunar cycle commences. For this year the golden number is 1. As in the second line in the shields, and also in the third line where there is no mark of initials, we must supply them with the assistance of the adjacent shields, and *vice versa* for the numbers.

"With respect to the central square, we must still add that the Roman numerals indicate the month,—No. I. signifying March; II. April, and so on. The Arabic numerals are the days of the month indicated above or below, to which correspond the initials of the days of the week in the peripheral shields. It is thus that, for example, for the year 1700 all the dates of the central square are Monday, for 1710 Saturday, &c. This part of the Kalendar may find an application even at present. For this purpose we must subtract 1700 from the year in question, and divide the difference by 28. The remainder is the year of the solar cycle for which we must seek in the peripheral shields the initial of the day of the week which corresponds, for the year in question, to the dates furnished by the central square.

"2. *The Bottom of the Box.*

"In the central rectangle the small arrows attached to the numbers point to the true solar time of sunset for the beginning of each month, Old Style, where they give the numbers of hours between the true noon and the rising or setting of the sun, that is, the semi-diurnal arc of the sun for the date in question. The months are there indicated as on the lid, by Roman cyphers. The Arabic numerals 4, 5, &c., are the entire hours, the half-hours being indicated by *fleur-de-lys*, and the quarters by points, and it is possible to obtain from this table the time required to two or three minutes nearly. This table is suited nearly to the latitude of London, the computer having neglected the effect of refraction.

"The elliptical compartments, both above and below the central rectangle, contain

Professor De Morgan's explanation of this compartment will be found below.[1]

the equation of time. The letters in it are the initials of the names of the month, and the numbers indicate either the days of the month, or the quantity of the equation of time. The sign of the sun signifies that at the four dates near which it is found, the equation of time is *zero.* These four dates divide the year into four periods. For each of these periods the elliptical compartments give with their corresponding dates the maximum of the equation of time expressed in minutes and in seconds. In the two periods when this equation is greatest, we find also the dates at which the equation is $\frac{1}{4}$, $\frac{1}{2}$, or $\frac{3}{4}$ of the corresponding maximum. The sign ⁓ is a little obscure, but it may be satisfactorily explained if we suppose it to indicate that the numbers which are under it do not belong to the preceding maximum of the equation.

"The legend on the right side of the central rectangle gives the time of high water for the days of full moon, but these times differ considerably from those now observed.

"The legend on the left hand of the central rectangle is still a little enigmatic. The supposition of M. Quetelet, that the numbers in it are more precise indications of the hours of sunrise and sunset, cannot be correct, for these hours are given with more precision in the central rectangle. The constant difference of 48 minutes between each adjacent couple of numbers, makes us suppose, on the contrary, that these numbers relate to the relative diurnal motion of the sun and moon, which is confirmed also by the words in the legend.

"OTTO STRUVE.

"POULKOVA, $\frac{13}{1}$ *April* 1853."

[1] The word *Sou* may mean *Setting* or *Southing. Shin* moans *Shining* or *Rising.* Upon the more probable supposition that *Sou* means *Setting,* the legend directs us to go *Round* for *Setting,* and *Back* for *Rising,* which would give—

		FROM BOX.		FROM HOPTON'S "CONCORDANCY OF YEARES."	
		Setting.	Rising.	Setting.	Rising.
Beginning of	January,	4^h 0′	8^h 0′	4^h 0′	8^h 0′
	February,	4 48	7 12	4 45	7 13
	March,	5 36	6 24	5 41	6 19
	April,	6 24	5 36	6 42	5 18
	May,	7 12	4 48	7 35	4
	June,	8 0	4 0	8 9	3 51
	July,	8 0	4 0	8 0	4 0
	August,	7 12	4 48	7 17	4 43
	September,	6 24	5 36	6 19	5 41
	October,	5 36	6 24	5 24	6 26
	November,	4 48	7 12	4 26	7 34
	December,	4 0	8 0	3 50	8 10

The last two columns are a part of the table of the sun's rising and setting from Arthur Hopton's "Concordancy of Yeares," 1615. In the last column of this table 7^h 13′ is a misprint for 7^h 15′.

"If *sou* is to be read *southing,* it means that the southing is 4^h, 4^h 48′, &c., after rising; but this is not the most likely meaning."

The manuscripts, letters, and other papers of Newton have been preserved in different collections. His correspondence with Cotes relative to the second edition of the *Principia*, and amounting to between sixty and a hundred letters, a considerable portion of the manuscript of that work, and five letters to Dr. Keill on the Leibnitzian controversy, are preserved in the Library of Trinity College, Cambridge, and have been published by Mr. Edleston in the interesting volume to which we have so often referred. Newton's letters to Flamsteed, about thirty-four in number, are deposited in the Library of Corpus Christi College, Oxford, and those of Flamsteed are at Hurtsbourne Park. In the British Museum, and in the Library of the Royal Society of London, there are many letters of Newton and his correspondents. Several letters of Newton, and a copy of the original specimen which he drew up of the *Principia*, exist among the papers of Mr. William Jones (the father of Sir William Jones), which are preserved at Shirburn Castle, in the library of Lord Macclesfield.[1] But the great mass of Newton's papers came into the possession of the Portsmouth family, through his niece, Lady Lymington, and have been safely preserved by that noble family. An account of many of the manuscripts has been given in the preceding pages, and several of the most important letters and papers will be found in the Appendix to this work.

[1] This tract, *De Motu*, occupying nineteen pages, and consisting of four theorems and seven problems, forms No. I. of the Appendix to Professor Rigaud's *Historical Essay on the Principia*, and is the same which Newton sent to Halley in November 1684.—See Edleston's *Correspondence*, p. lv., and vol. i. pp. 298, 305.

APPENDIX.

No. I.

(Referred to in page 23.)

The Abbé Anthony Conti, or Conty, whose name appears so prominently in the Fluxionary controversy, was a noble Venetian, who obtained some distinction as a philosopher and a poet. He was born at Padua on the 22d January 1677, and, at the age of twenty-two, he retired to Venice into the Congregation of the Oratoire, where he became a priest, and remained nine years. Disgusted with the scholastic philosophy of the day, he studied Bacon and Locke, and devoted himself to the studies of mathematics, natural philosophy, and natural history, which he prosecuted at Padua. Having gone to Paris, where he was a favourite in society, he met with M. Remond de Montmort, an eminent mathematician, who accompanied him to England to observe the great solar eclipse of the 15th April 1715. He was then introduced to Newton, and was on very intimate terms with him. He took a great interest in his controversy with Leibnitz, but being acquainted also with the German philosopher, he found it difficult to take an impartial course between the two extreme opinions of the day. We shall meet with him again when we come to the consideration of Newton's Chronology, which was the ground of a serious difference with its author.[1] Conti wrote a philosophical poem entitled *Il Globo de Venere,* and four tragedies, which were published at Venice in 1739. He died at Padua on the 6th March 1749.

The following very interesting letter,[2] referred to in the text, was written to Brook Taylor, one of the Committee who drew up the report on the *Commercium Epistolicum.*

1.—Letter of the Abbe Conti to Brook Taylor.

"Monsieur,

"Je m'en vais vous expliquer en peu de mots les raisons qui m'ont engagé dans la querelle de Mons. Newton et de M. Leibnitz. Mr. Newton

[1] Conti's defence of himself, referred to in page 240, note, is published without his name in the *Bibliothèque Française* for May and June 1726. Amsterdam, pp. 182-193.
[2] From the *Life of Brook Taylor*, p. 121.

me pria d'assembler à la Société les Ambassadeurs et les autres Ministres étrangers. Il souhaitait qu'ils assistassent à la collation qu'on devoit faire des papiers originaux, qui se conservent dans les archives de la Société avec d'autres lettres de M. Leibnitz. Mr. le Baron de Kirmansegger[1] vint à la Société avec les Ministres des Princes ; et après que la collation des papiers fut faite, il dit tout haut, que cela ne suffisoit pas, que la véritable méthode pour finir la querelle, c'étoit que Mr. Newton luy-même écrivît une lettre à Mr. Leibnitz dans laquelle il luy proposât les raisons et en même tems luy demandât des réponses directes. Tous les Ministres des Princes qui étoient présents goûtèrent l'idée de Mr. Kirmansegger ; et le Roy même à qui on la proposa le soir, l'approuva, ayant dit tout cela à Mr. Newton, cinque ou six jours après il m'escrivit une lettre pour envoyer à Mr. Leibnitz à Hanover. Mr. Newton, peut-il dire que je l'ay prié de m'adresser cette lettre? Cependant la nécessité, de l'envoyer à Hanover, et de l'accompagner d'une des miennes m'engagea dans la querelle. La lettre qui fut portée à Hanover par le Baron de Discau, resta plus d'un mois à Londres. Mad^me la Comtesse de Kirmansegger la fit traduire en François par M. Coste : le Roy la lut et l'aprouva fort, en disant que les raisons étoient très simples et très claires, et qu'il étoit difficile de répondre à des faits. J'ay lu à Mons. Newton la lettre que j'escrivois à Mons. Leibnitz ; c'est Mr. de Moivre qui me l'avoit corrigé et j'en conserve encore la brouillon : Mr. de Moivre y-avoit ajouté quelque chose à l'égard de la manière équivoque dont Mr. Leibnitz avoit proposé le problême. Mr. Leibnitz fut fort irrité de la lettre que je luy avois envoyée, comme il paroit par sa réponse, et par des expressions assez fortes qu'il avoit avancées contre moy dans ses lettres à S. A. R. la Princesse de Galles. Il écrivit plusieurs lettres pour sa justification que j'ay donné à Mons. Newton à proportion qu'elles m'ont tombés daus les mains, Mr. Newton en fit une espèce de réponse qui fut imprimée avec la première lettre à la fin de l'Histoire des Fluxions ; les lettres que Mr. Leibnitz m'avoit addressées, furent aussi imprimées dans le même livre ; et *Mr. Leibnitz* [evidently a mistake for Mr. Newton] en fit non seulement ôter mon nom ; mais encore ne me fit aucune part qu'on les imprimoit. Quand Mr. de Mesaus[2] luy proposa de les imprimer de nouveau en Hollande, il luy donna son approbation, et dit même qu'il luy fourniroit quelque autre petit papier. J'ignore ce qui est arrivé d'après, parce que j'ay quitté l'Angleterre. On dit que Mr. Newton a changé de sentiment et qu'il se plaint de moy de l'avoir engagé dans la querelle avec Mr. Leibnitz ; je le prie très humblement de réfléchir à des faits qui sont incontestables ; et par lesquels il paroit assez que je n'ay eu d'autre part à la question qu'autant qu'il voulut bien m'en faire. J'ay essuyé tous les reproches des Allemans, et de Mr. Leibnitz luy-même pour soutenir ses raisons. Je les ai aussi soutenus en France où malgré tout ce qu'on a l'addresse de luy écrire en Angleterre, on n'est pas trop dans ses intérêts comme il pense. J'ai pense un jour me brouiller avec un grand mathématicien, chez une Dame, où on parloit de cette dispute ; il soutenoit que tous les argumens du Commercium Epistolicum n'étoient pas concluans ; et que Mr. Newton n'y avoit aucune part, non plus qu'aux lettres qu'on avoit imprimées par son ordre. J'aurois bien d'autres choses à dire la-dessus ; mais je suis las d'entendre parler d'une matière qui n'est pas agréable. On a voulu me

[1] Kilmansegg or Kilmansegger. [2] Des Maizeaux.

commettre avec Mr. Newton, et je ne sçay pas pourquoi ; je l'ay toujours honoré et respecté ; et je luy ay toujours dit la vérité sans aucun intérêt : mais si les plaintes continuent, je ne pourray pas me dispenser de faire imprimer la simple histoire d'un fait, qui fera voir au public que je n'ay pas prétendu me mêler dans cette querelle pour acquérir du nom. Mr., je suis

"Votre très humble et très obéissant serviteur,

"CONTY."

"A Paris, ce May 22, 1721."

The following is the only letter from Conti to Newton which I have found among the Portsmouth Manuscripts :—

2.—LETTER FROM THE ABBE CONTI TO SIR ISAAC NEWTON.

HANOVER, 10 *Decembre*, 1716.

"MONSIEUR,

"Je vous demande pardon si je n'ay pas pu vous écrire jusque à cette heure. Je suis tombé malade depuis que je suis icy, et je ne suis pas encore revenu de ma maladie. Je n'ay vu ni Le Roy,[1] ni La Cour, et je suis obligé de garder la chambre depuis vingt jours.

"M. Leibniz est mort ; et la dispute est finie. Il a laissé plusieurs lettres et plusieurs manuscrits qu'on imprimera, aussi des manuscrits d'autres sçavants, une qui est Traité de M. Des-Cartes qui n'est point paru jusque ici. Il y a des Dialogues sur les articles de la Téodicea ; une instruction au Prince Eugène sur les exercices militaires ; une instruction au Czar pour faire fleurir les arts et les sciences dans son pais ; beaucoup des remarques sur la langue universelle, et sur l'étimologie des mots. Comme je espère que le Roy me donnea la permission de voir les papiers je remarquerai s'il y a quelque chose touchant votre dispute, mais peut-etre qu'on cachera ce qui ne fait point d'honneur à la mémoire de M. Leibnitz. On a commencé de travailler sur sa vie. M. Wolfius aura le soin d'écrire tout ce qui appartient aux Mathématiques.

"M. Leibnitz a travaillé pendant toute sa vie à inventer des machines qui n'ont point réussi. Il a voulu faire une espèce de moulin à vent pour les mines, un carosse qui tire sans chevaux, un carosse qui se change, un chaire à porteur, et un charette ; jusque des Souliers à ressort. Il y a deux modelles de sa machine arithmétique, mais elle est très composée, et on en dit qu'elle n'est à la fin que la machine de Pascal multipliée.

"Vous aurez vu l'insolente dissertation, qu'on a imprimée dans les actes de Lipsic au mois de Juin. M. Bernoulli prétend à cette heure d'être l'inventeur de calcul intégral. Je suis sûr que la dissertation vous fera rire.

"Je ne sçay pas si l'ambassadeur de Venise vous a prié de proposer à la Société Royal M. le Marquis Orsi Sénateur de Bologne, et un de plus grand sçavants que nous avons en Italie. Il est célèbre en France par plusieurs livres qu'il a écrit, et il est un Seigneur qui a beaucoup de mérite et de talent. On dit qu'il a refusé autrefois d'être Cardinal.* Il s'est addressé à

[1] Conti was a great favourite of the King, who had invited him to Hanover, and with whom he dined every day.

* this would recommend Orsi to Newton

moi pour vous prier de cette grace, et je le fais volontiers, car je connois les mœurs et le sçavoir de M. le Marquis Orsi.

"Si il y aura quelque chose de nouveau touchant l'affaire de M. Leibnitz je vous en informerai avec toute l'exactitude. Il n'y a peut-être un personne plus intéressé pour votre gloire que moy. J'en ay l'obligation, et même l'inclination. Je suis avec tout la zèle, et en vous priant de faire mes compliments à Madame votre nièce,

"Monsieur,

Votre très-semble et tres obéissant serviteur,

"CONTI."

No. II.

(*Referred to in pages* 27, 42.)

M. Pierre Remond de Montmort, to whom Bernoulli addressed the letter mentioned in the text, was born in Paris on the 27th October 1678. He came to London in 1700, when he made the acquaintance of Newton. He visited Dr. Gregory at Oxford, who showed him his Commentary on the *Principia*, and who afterwards told him in the course of their correspondence, that he was preparing a new edition of the *Principia* under Sir Isaac's eye, a task which he did not live to execute. This fact is mentioned in a short letter to Sir Isaac himself,[1] in which he begs his acceptance of his newly published work *Essai d'Analyse sur les Jeux de Hazard*, and expresses his sorrow for the death of Gregory. In 1704, he purchased the estate of Montmort, close to the residence of the Duchess d'Angoulême, whose niece and god-daughter, Mademoiselle de Romicourt, he married. He corresponded with Leibnitz, the Bernoullis, and other distinguished mathematicians, both in England and on the Continent, by whom he was much esteemed, both as a geometer and a member of society. He was particularly attached to our countryman Brook Taylor, by whom several of his letters have been preserved. He paid a second visit to England in 1715, and was a great admirer, as we shall afterwards see, of the beauty and accomplishments of Miss Catherine Barton, Sir Isaac Newton's niece. He was elected in 1716 one of the *Académiciens Libres* of the Academy of Sciences. When on a visit to Paris in 1719, he was seized with small-pox, and died on the 7th October 1719, deeply lamented by the population of the three parishes which belonged to him.

The history of the very interesting letter from Bernoulli which forms this Appendix, is curious, and is given by Montmort himself in a long letter to Newton, written in bad English, and dated March 27, 1718. It contains messages from the two Bernoullis, together with an extract of Nicolas Bernoulli's paper on Trajectories,[2] and a part of the following letter which he did not know had been previously in the hands of Newton.

[1] Dated Paris, 16th February 1799.

[2] See page 30.

When Bernoulli had learned that Newton did not approve of the challenge made to the English mathematicians, he communicated "the whole story of that affair" to Montmort, and desired him to send to Newton an extract of his letter. Montmort, lest he should annoy Newton by "giving him the trouble of an answer," sent the extract to Brook Taylor, and never learned from him that it had been forwarded to its destination. The following is the extract found among Sir Isaac's papers :—

Letter from John Bernoulli to M. Remond de Montmort.

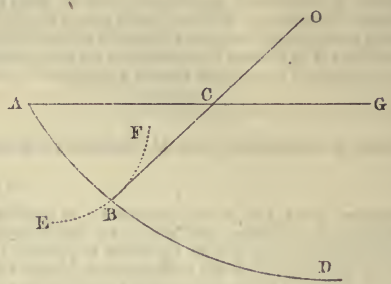
"*Avril* 8, 1717.

"Je vous proteste, Mons. que je n'ai jamais eu la pensée de me commettre avec Messieurs les Anglois, ni d'entrer en lice, quand même quelqu'un d'eux m'attaqueroit, bien loin de les défier le premier ; le temp et le repos me sont trop précieux pour les consumer en vaines disputes ; mais voici, ce qui c'est. Mons. Leibnitz m'ayant demandé si je ne pouvois pas luy fournir quelque problême pour le proposer à Messieurs les Anglois et en particulier à M. Keil pour la solution duquel seroit requise une adresse particulière dont on ne put s'aviser aisément sans la connoissance de quelques unes des méthodes que nous avions trouvées dans le temps que j'étois encore en Hollande et que M. Leibnitz ne trouvoit pas apropos d'en faire part encore au public, me priant pour cela de menager le secret afin de s'en servir un jour utilement contre ceux qui voudroient nous braver, comme il arrive aujourdhuy. Pour faire donc plaisir à Monsr. de Leibnitz, j'ay imaginé un problême qui me paroissoit avoir les qualités telles qu'il pouvoit souhaiter. Je luy en fis part avec une double solution, afin qu'il pût, s'il le jugeoit apropos, le proposer aux Anglois mais sous son nom. J'ay sujet d'être étonné de voir que M. Leibnitz m'ait produit comme auteur, et proposant de ce problême, et cela malgré moy et même à mon insçu. Vous aurez donc la bonté de désabuser Mr. Newton de l'opinion où il est à cet égard ; et de l'assurer de ma part que je n'ai jamais eu le dessein de tenter messieurs les Anglois par ces sortes de défis, et que je ne désire rien tant que de vivre en bonne amitié avec luy, et de trouver l'occasion de luy faire voir combien j'estime son rare mérite, en effect je ne parle jamais de luy qu'avec beaucoup d'éloges. Il seroit pourtant à souhaiter qu'il voulût bien prendre la peine d'inspirer à son ami Mr. Keill des sentiments de douceur et d'équité envers les étrangers, pour laisser chacun en possession de ce que luy appartient de droit, et à juste titre, car de vouloir nous exclure de toute prétention, ce seroit une injustice criante. Voicy cependant le problême dans les propres termes que je l'ay communiqué à Mons. Leibnitz, puisque vous temoignez le désirer."

Bernoulli's Problem.

"Super Recta AG tanquam Axe ex puncto A, educere infinitas curvas, qualis est ABD, ejus naturæ ut radii osculi in singulis punctis B et ubique ducti BO secentur ab axe AG in C in data ratione, ut nempe sit BO : BC : :

: *n.* Deinde construendæ sunt Trajectoriæ EBF primas curvas ABD normaliter secantes."



In his correspondence with Montmort, Bernoulli expressed the greatest anxiety to be on good terms with Newton. In a letter dated March 17, 1718, he desires to know if Montmort has sent the preceding extract to Newton, and implores him again to disabuse the English of the false opinion that he and his nephew have any design of entering into disputes with them, or diminishing the value of Newton's discoveries; and he asks him, as a special favour, to do this in reference to Mr. Newton, whose esteem and friendship are very precious to him.

Montmort, in reply to this letter, stated that these expressions in reference to Newton and the English would not be considered very compatible with the *Epistola pro eminente Mathematico,* which he had inserted in the Acts of Leipsic, and he gave him very frankly his opinion of that letter. Bernoulli replied to this letter in the following words, which Montmort sent to Newton :—

"Je ne m'en suis mêlé en aucun façon, ni de la forme que mon ami vouloit donner à la réponse, ni des expressions dont il se serviroit, et que je n'approuve pas toutes Il m'a qualifié de titres que je n'ai jamais eu la vanité d'ambitionner. Avec cela il a parlé de M. Keill d'une manier qui ne peut qu'aigrir son esprit. Cela ne me plaisoit pas. J'aurois souhaité que mon apologiste eust dit les choses simplement et nettement sans toucher sur personalités. Certe que je luy aurois recommandé avec empressement s'il m'avoit communiqué son dessein, lorsqu'il m'offrit par une lettre obligeante de vouloir défendre ma cause contre M. Keill me priant seulement de luy envoyer les preuves authentiques lesquels je ne pouvois pas luy refuser."

Fontenelle justly remarks, in his Eloge on Montmort, that though he was more connected with the English than with the Germans by personal acquaintance, yet he was perfectly neutral between the rival analysts, always speaking the truth to both parties, and in a tone which made truth acceptable. See p. 212, and APPENDIX, No. XVII.

While this correspondence was going on, Varignon had been endeavouring to effect a reconciliation between Newton and Bernoulli. See p. 227, and APPENDIX, No. XIX.

No. III.

(*Referred to in pages* 36, 40.)

In the 24th Chapter of this Volume, the reader will find some account of James Wilson, M.D., the editor of Robins' *Mathematical Tracts,* the friend of Pemberton, and the editor of his *Course of Chemistry.*

1.—LETTER FROM A. B. [JAMES WILSON, M.D.] TO SIR ISAAC NEWTON.

"LONDON, *December* 15, 1720.

"SIR,

"I saw the other day, in the hands of a certain person, several mathematical papers, which, he told me, were transcribed from your manuscripts. They chiefly related to the doctrine of series and fluxions, and seemed to be taken out of the Treatises you wrote on those subjects in the years 1666 and 1671. I was not permitted to peruse them thoroughly, but one of the papers I particularly took notice of, and it contained a deduction of your Binomial Theorem from a corollary in your Quadratures, with some improvement, as a series for the rectangle under any two dignities of two binomials. These papers, I observed, had been very incorrectly copied, so that I endeavoured all I could to dissuade the possessor of them from getting them printed, of which nevertheless he seemed very fond. I therefore thought it behoved me to acquaint you with this matter, and, as I have not the honour to be known to you, I believed the less troublesome way to do it would be that of a letter.

"And now, Sir, permit me to say, that it is the earnest desire of everybody conversant in these subjects, that you would be pleased to publish yourself what you have formerly written; for this would effectually prevent their being ever printed incorrectly and unworthy of yourself. Nor ought you to deny that pleasure to your well-wishers and admirers, in reading your noble inventions of this kind, whereof you have expressed yourself to have been so sensible at the time you first made these discoveries. Your analysis Per Æquationes Numero Terminorum Infinitas, Quadraturas, &c., give us so exquisite a delight, that when we read your letters to Mr. Oldenbourgh, and your remarks on Leibnitz's reply to your letter to the Abbot Conti, we glow, as it were, with a desire of seeing all that you wrote on these subjects in 1671, and the years preceding. I have heard, indeed, that you were prevented from publishing one Treatise, by reason it had in it the Determination of the Radius of Curvity, which Huygens published afterwards in his *Horologium.* But this objection can now be of no force, since the world has been lately informed by your letter written to Mr. Collins in 1672, Decemb. 10, that you had applied your method *ad resolvendum abstrusiora problematum genera de curvitatibus,* &c., long before the publication of Huygens's book. Nor indeed was this unknown to Apollonius in respect to the Conick Sections, as appears from his Fifth Book. It was a very great satisfaction to your countrymen and friends to observe, that by the happy discovery of Mr. Collins's papers, you had an opportunity of triumphing over such disingenuous persons, who laboured all they were able to defraud you of the honour of some of your inventions. But such is the force of prejudice on some minds, that you cannot but observe that there are in the world dishonest men, who, contrary to their own conscience and knowledge, still raise a clamour on this head. To shame, therefore, such obstinate people, to make your right to those inventions evident to all, even the least knowing in these matters, and to put an end for ever to all disputes, the best method would be to publish all that you have formerly written on this subject, whereby we should have an exact and adequate notion of fluxions and their uses, which cannot be had from what has been delivered by others. But as they produced these things abroad first, those that are learners have recourse to their writings, and consequently mention them as the authors thereof. This has happened through your own backwardness in giving to the world what you had discovered so long ago. However, there is still a way left to retrieve all; for, as their pretended methods are grounded on the notion of indivisibles, so they have given a wrong idea of what you alone had found out, and have erred egregiously, when they even attempted to apply Second, &c., Differences, as they call 'em, to mathematical figures. This, tho' they cannot but be now at length very sensible of, yet by their cavils they would dissemble their being conscious of their errors. But these would be so apparent to every eye, if you would publish all your papers, that those who had a mind to be rightly instructed in these matters, would have recourse alone to your immortal writings; so that all succeeding mathematicians would constantly mention with honour the Series, Fluxions, &c., of the great Newton, when the differentials and integrals of Leibnitz and Bernoulli shall be quite forgotten.

"I am, your most obedient and most humble servant, A. B."

"*P.S.*—I had just the liberty to transcribe the Theorem I mentioned at the beginning of my letter, and it was this:—

$$\overline{P+PQ}|^{\lambda}\times\overline{M+MN}|^{\mu}=P^{\lambda}M^{\mu}+\frac{\frac{\lambda\times Q}{\mu\times N}A}{1}+\frac{\frac{\overline{\lambda-1}\times Q}{\overline{\mu-1}\times N}B+\overline{\lambda+\mu}\times NQA}{2}+\frac{\frac{\overline{\lambda-2}\times Q}{\overline{\mu-2}\times N}C+\overline{\lambda+\mu-1}\times NQB}{3}+\frac{\frac{\overline{\lambda-3}\times Q}{\overline{\mu-3}\times N}D+\overline{\lambda+\mu-2}\times NQC}{4}\text{ etc.}$$

"Amongst the papers I likewise observed that there were some which deduced even the first principles of geometry from the fluxions of points, &c.

"I have since met with another person, who told me he had likewise a copy of your manuscripts. But he would not let me see them, or inform me how he came by them. I imagine, when you sent any of your friends your papers, the person they got to transcribe them took a double copy, which is a frequent practice, in order to make profit by it; so that they are in different hands. To prevent these things being ever published incorrectly, the only way is to let them come abroad yourself; for to declare that such papers as shall be published without your knowledge or consent, are imperfect and faulty, will not be sufficient to deter some bookseller or another from adventuring on the printing them for the hopes of gain. Nor need the publishing them be any trouble to you; for any of your friends would gladly undergo the labour of seeing them correctly printed.

"In the introduction to your Quadratures, you have given us an exact idea of fluxions, but it is too short, and does not instruct us how the superior fluxions are represented by Lines. The truth is, the trifling objections that are made by your antagonists would never have been raised, if you had given us your papers, where your fluxions are illustrated by various examples. And is not this a pity, since you have pleased to permit the publication of your illustrating the common algebra. Nothing less than this can make all foreigners and prejudiced persons acquiesce, and at length to acknowledge you to be the inventor of a method that is so admirably suited both to the investigating and demonstrating the most difficult mathematical truths."

2.—Letter from James Wilson, M.D., to Sir Isaac Newton.

"London, 21*st January* 1720-21.

"Sir,

"As some time ago I presumed to let you know that I had seen copies of several of your manuscripts, and having since been permitted to transcribe some of them, I take the liberty to send them to you, that you may compare them with the originals, to see after what manner they have been copied. They contain three problems, which I take to be the 2d, 3d, and 4th of your Treatise wrote in 1671. Here is also a paper in English containing five problems, which I guess to be part of that which you have mentioned in your remarks on Leibnitz's letter to the Abbot Conti, as dated 13th November 1665. The other papers seemed to have not been so well copied as these, so I did not write them out. They contained, I observed, several problems, as to find the curvature and areas of curves, and to compare curves together, &c. There was also a paper containing six examples, showing how to deduce the areas of curves from the tables in your Quadratures, with Constructions and Synthetick Demonstrations. It concluded with saying, that it was here judged proper to demonstrate by the means of moments, as it had an analogy to what the ancients have done on the like occasions. I have been likewise told by one that he had a copy of a manuscript of yours, entitled Geometria Analytica, which he highly prized, but this I never saw.

"When I had the honour of seeing you at Mr. Innys's shop, you was pleased to object against publishing these manuscripts; that you apprehended it would occasion disputes concerning their antiquity. The followers of Leibnitz are, it is true, an obstinate sort of people, and no proof, however clear, seems sufficient to make them lay aside their prejudices, yet on such an occasion I cannot think they should be more than ordinarily exasperated; for thereby you will not do more than by what you have said, when you published your Quadratures, and in your remarks on Leibnitz's reply to your letter to the Abbot Conti. The publishing indeed of the *Commercium Epistolicum* raised their fury, because that not only proved you to be the inventor of fluxions, but moreover made it appear that their master was a plagiary. However, notwithstanding this, the defamatory writings they spread abroad on that occasion were without a name, as if they were ashamed of them; and the person who has been charged as the author of them has since thought fit to deny it. But suppose this should raise ever so great a clamour, I cannot see that you need be concerned the least about it, for, in publishing these papers, you would not pretend to vindicate to yourself the right to these inventions from their antiquity; for that you rely on the arguments that are drawn from the papers contained in the *Commercium Epistolicum*, which the Leibnitians themselves do not pretend to say, are not of an older date than their master's letter of June 21, 1677.

"But then I think these papers ought to be published on many accounts. By that means young mathematicians will be able readily to perceive the force of the arguments contained in the *Commercium Epistolicum*, and in its admirable abridgment, before they receive the least prejudice from the cavils of your antagonists. These, I think, are now all reduced to this, that it does not appear from the *Commercium* that you were acquainted with the true characteristic and algorithm of fluxions or differences before their master. The weakness of this cavil would appear evident even to the most prejudiced, if you would publish all your papers. Again, your Book of Quadratures, which all intelligent persons must own is the perfectest piece that ever saw the light, seems not to be well understood by foreigners (and perhaps not by some at home); for otherwise a certain confident person durst not lay claim to many things contained in it, under the notion of his integral calculus. But the publishing your papers would enable all to see the beauties of that noble Treatise; and this is now absolutely necessary, since there are pretenders in the world to these inventions. Lastly, as various copies of your manuscripts, more or less imperfect, are got abroad, nothing but causing them to be printed yourself can prevent their coming out incorrect and mangled, which ought not to be the fate of such excellent things.

"I am, Sir, with the profoundest respect,

"You most obedient and most humble servant,

"JAMES WILSON.

"P.S.—I humbly desire that, when you have perused these papers, you would be pleased to seal them up, and to leave them with your servants, that I may have them again upon calling for them some time or other.

"At page 48 of the *Commercium Epistolicum*, it is said by Mr. Collins, that the doctrine of Series, &c., was the subject of your lectures at Cambridge, and that these lectures were reserved there, which, if so, they might afford convincing proofs of your right to these inventions.

"In your remarks on Leibnitz's reply to your letter to the Abbot Conti, I think you seem too readily to acknowledge that Leibnitz might have found out by himself your method of an arbitrary series; for in a Scholium of your *Principia*, you say that one of the things which you concealed under a cypher, in your letter of October 24, 1676, was 'Data Æquatione quotcunque Fluentes quantitates involvente, Fluxiones invenire, et vice versa.' Now, might not Leibnitz by that means be helped to decypher what was besides concealed in that letter? Amongst which was that very method of assuming a series, which he did not publish till some years after you had helped him to a key in the Scholium above-mentioned.

"I hope you will pardon this freedom, for it is not my purpose to go on in troubling you thus with impertinent letters.

"Sir, I am your most obedient

"And most humble servant,

"JAMES WILSON."

No. IV.

(*Referred to in page* 58.)

LETTER FROM SIR ISAAC NEWTON TO DR. THOS. BURNET.

"SIR,

"Your argument, p. 118, I acknowledge good against those who suppose only hills and mountains taken out of ye sea, and it may be good agt those who suppose all ye earth higher than ye sea taken out thence, but one who would have mountains and ye sea made by removing earth from one place to another, might suppose (if it were necessary) all the earth a quarter of a mile or half a mile lower than the top of the seas, or then the lowest valleys, or even lower than that, was thrown out of ye deep. But the opinion being to me absurd, I say no more of it. I could wish I was as well satisfied wth your argument about ye oval figure of ye earth, for it seems hard to me that a constant force applied to stretch a membrane (as you figuratively term ye atmosphere) should make it shrink, unless you suppose it at first overstretched by a tumultuary force, and so to return by way of undulation, and that the limus of ye earth hardened while it was at ye ebb. But whatever may be ye reason of the earth's figure, you desire my opinion what that figure is. I am most inclined to believe it spherical, or not much oval; and my chief reason for that opinion is ye analogy of ye planets. They all appear round so far as we can discern by telescopes, and I take ye earth to be like ye rest. If its diurnal motion would make it oval, that of Jupiter would much more make Jupiter oval, the *vis centrifuga* at his equator, caused by his diurnal motion being 20 or 30 times greater than the *vis centrifuga* at ye equator, caused by the diurnal motion of ye earth, as may be collected from the largeness of his body and swiftness of his revolutions. The sun also has a motion about his axis, and yet is round.

What may be argued from y^{e} dimensions of y^{e} earth's shaddow collected by lunar eclipses I cannot tell, nor what from y^{e} measures on y^{e} earth answering to a degree in several latitudes, not knowing how exactly those measures were made or the latitudes of places taken.

"You seem to apprehend that I would have the present face of the earth formed in y^{e} first creation. A sea I believe was then formed, as Moses expresses, but not like y^{e} sea, but with an eaven bottom without any precipices or steep descents, as I think I exprest in my letter. Of o^{r} present sea, rocks, mountains, &c., I think you have given the most plausible account. And yet if one would go about to explain it otherwise, philosophically, he might say that as saltpetre dissolved in water, though y^{e} solution be uniform, crystallizes not all over y^{e} vessel alike, but here and there in long barrs of salt; so the limus of y^{e} chaos, or some substances in it, might coagulate at first, not all over y^{e} earth alike, but here and there in veins or beds of divers sorts of stones and minerals. That in other places w^{ch} remained yet soft, the air w^{ch} in some measure subsided out of the superior regions of y^{e} chaos, together wth y^{e} earth or limus by degrees extricating itself gave liberty to the limus to shrink and subside, and leave the first coagulated places standing up like hills; which subsiding would be encreased by the draining and drying of that limus. That the veins and tracts of limus in the bowels of those mountains also drying and consequently shrinking, crackt and left many cavities, some dry, others filled with water. That after the upper crust of the earth by the heat of the sun, together with that caus'd by action of minerals had hardened and set; the earth in the lower regions still going closer together left large caverns between it, and the upper crust filled with y^{e} water, w^{ch} upon subsiding by its weight, it spread out by degrees till it had done shrinking, which caverns or subterraneal seas might be the great deep of Moses, and if you will, it may be supposed one great orb of water between y^{e} upper crust or gyrus and the lower earth, though perhaps not a very regular one. That in process of time many exhalations were gather'd in those caverns which would have expanded themselves into 40 or 50 times the room they lay in, or more, had they been at liberty. For if air in a glass may be crowded into 18 or 20 times less room than it takes at liberty, and yet not burst the glass, much more may subterranean exhalations by the vast weight of y^{e} incumbent earth be kcept crowded into a less room before they can in any place lift up and burst that crust of earth. That at length somewhere forcing a breach, they by expanding themselves, forced out vast quantities of water before they could all get out themselves, w^{ch} commotion caused tempests in y^{e} air, and thereby great falls of rain in spouts, and all together made y^{e} flood, and after the vapours were out, y^{e} waters retired into their former place. That the air w^{ch} in y^{e} beginning subsided with y^{e} earth, by degrees extricating itself, might be pent up in one or more great caverns in the lower earth under y^{e} abyss, and at y^{e} time of y^{e} flood, breaking out into y^{e} abyss, and consequently expanding itself, might also force out y^{e} waters of y^{e} abyss before it. That the upper crust or gyrus of earth might be upon the stretch before y^{e} breaking out of y^{e} abyss, and then by its weight shrinking to its natural posture, might help much to force out the waters. That the subterraneal vapors which then first break out and have ever since continued to do so, being found by experience noxious to man's health, infect the air and cause that shortness of life w^{ch} has been ever since the flood. And that several pieces of earth either at y^e flood or since falling, some perhaps into y^e great deep, others into less and shallower cavities, have caused many of those phenomena we see on y^e earth, besides the original hills and cavities.

"But you will ask how could an uniform chaos coagulate at first irregularly in heterogeneous veins or masses to cause hills. Tell me then how an uniform solution of saltpetre coagulates irregularly into long bars; or to give you another instance, if tinn (such as the pewterers bring from y^e mines in Cornwel to make pewter of) be melted and then let stand to cool till it begin to congeal, and when it begins to congeale at y^e edges, if it be inclined on one side for y^e more fluid part of y^e tin to run from those parts w^ch coagulate first, you will see a good part of y^e tin congealed in lumps which after the fluider part of y^e tin which congeals not so soon is run from between them, appear like so many hills, with as much irregularity as any hills on y^e earth do. Tell me y^e cause of this, and y^e answer will perhaps serve for the chaos.

"All this I write not to oppose you, for I think the main part of your hypothesis as probable as that I have here written, if not in some respects more probable. And tho' the pressure of y^e moon or vortex, &c., may promote y^e irregularity of y^e causes of hills, yet I did not in my former letter design to explain the generation of hills thereby, but only to insinuate how a sea might be made above ground in your own hypothesis before the flood, besides the subterranean great deep, and thereby all difficulty of explaining rivers, and the main point in w^ch some may think you and Moses disagree might be avoyded. But this sea I not [do] not suppose round the equator, but rather to be two seas in two opposite parts of it where the cause of y^e flux and reflux of o^r present sea deprest y^e soft mass of y^e earth at that time when y^e upper crust of it hardened.

"As to Moses, I do not think his description of y^e creation either philosophical or feigned, but that he described realities in a language artificially adapted to y^e sense of y^e vulgar. Thus when he speaks of two great lights, I suppose he means their apparent not real greatness. So when he tells us God placed these lights in y^e firmament, he speaks I suppose of their apparent not real place, his business being not to correct the vulgar notions in matters philosophical, but to adapt a description of the creation as handsomely as he could to y^e sense and capacity of y^e vulgar. So when he tells us of two great lights, and y^e stars made y^e 4th day, I do not think their creation from beginning to end was done the 4th day, nor in any one day of y^e creation, nor that Moses mentions their creation, as they were physicall bodies in themselves, some of them greater than this earth, and perhaps habitable worlds, but only as they were lights to this earth, so therefore though their creation could not physically [be] assigned to any one day, yet being a part of y^e sensible creation which it was Moses's design to describe, and it being his design to describe things in order according to the succession of days, allotting no more than one day to one thing, they were to be referred to some day or other, and rather to the 4th day than any other, if they [the] air then first became clear enough for them to shine thro' it, and so put on y^e appearance of lights in y^e firmament to enlighten the earth. For till then they could not properly be described under y^e notion of such lights, nor was their description under that notion to be deferred after they had that appearance, tho' it may be the creation of some of them was not yet completed. Thus far, perhaps, one might be allowed to go in y^e^ explaining y^e^ creation of y^e^ 4th day, but in y^e^ third day for Moses to describe y^e^ creation of seas when there was no such thing done neither in reality nor appearance, me thinks is something hard, and that y^e^ rather becaus if before y^e^ flood there was no water but that of rivers, that is, none but fresh water above ground, there could be no fish but such as live in fresh water, and so one half of y^e^ fift day's work will be a non entity, and God must be put upon a creation after y^e^ flood, to replenish one half of this terraqueous globe w^th^ whales, and all those other kinds of sea fish we now have.

"You ask what was that light created the first day? Of what extent was the Mosaical chaos? Was y^e^ firmament, if taken for y^e^ atmosphere so considerable a thing as to take up one day's work? and would not y^e^ description of y^e^ creation have been complete without mentioning it? To answer these things fully, would require comment upon Moses whom I dare not pretend to understand: yet to say something by way of conjecture, one may suppose that all y^e^ planets about o^r^ sun were created together, there being in no history any mention of new ones appearing or old ones ceasing. That they all, and y^e^ sun too, had at first one common chaos. That this chaos, by y^e^ spirit of God moving upon it, became separated into several parcels, each parcel for a planet. That at y^e^ same time y^e^ matter of y^e^ sun also separated from the rest, and upon y^e^ separation began to shine before it was formed into that compact and well defined body we now see it. And the preceding darkness and light now cast upon y^e^ chaos of every planet from the solar chaos, was the evening and morning, w^ch^ Moses calls y^e^ first day, even before y^e^ earth had any diurnall motion, or was formed into a globular body. That it being Moses design to describe the origination of this earth only, and to touch upon other things only so far as they related to it, he passes over the division of y^e^ general chaos into particular ones, and does not so much as describe y^e^ fountain of that light God made, that is, y^e^ chaos of y^e^ sun, but only w^th^ respect to the chaos of the earth, tells us that God made light upon the face of y^e^ deep where darkness was before. Further, one might suppose that after y^e^ chaos was separated from y^e^ rest, by y^e^ same principle w^ch^ promoted its separation (w^ch^ might be gravitation towards a centre), it shrunk closer together, and at length a great part of it condensing, subsided in y^e^ form of a muddy water or limus, to compose this terraqueous globe. The rest w^ch^ condensed not, separated into two parts, the vapors above and the air, w^ch^ being of a middle degree of gravity ascended from y^e^ one, descended from y^e^ other, and gathered into a body stagnating between both. Thus was the chaos at once separated into three regions, the globe of muddy waters below y^e^ firmament, the vapors or waters above the firmament, and y^e^ air or firmament itself. Moses had before called the chaos *the deep* and *the waters*, on y^e^ face of w^ch^ y^e^ spirit of God moved, and here he teaches the division of all those waters into two parts, with a firmament between them: w^ch^ being the main step in y^e^ generation of this earth, was in no wise to be omitted by Moses. After this general division of y^e^ chaos, Moses teaches a subdivision of one of its parts, that is, of the miry waters under y^e^ firmament into clear water, and dry land on the surface of the whole globous mass, for w^ch^ separation nothing more was requisite then that y^e^ water should be drained from y^e^ higher parts of y^e^ limus to leave them dry land, and gather together into ye lower to compose seas. And some parts might be made higher than others, not only by ye cause of ye flux and reflux, but also by ye figure of ye chaos, if it was made by division from ye chaos of other planets, for then it could not be spherical. And now while the new planted vegetables grew to be food for animals, the heavens becoming clear, for ye Sun in ye day, and Moon and starrs in ye night, to shine distinctly through them on the earth, and so put on ye form of lights in ye firmament, so that had men been now living on ye earth to view the process of ye creation, they would have judged those lights created at this time. Moses here sets down their creation as if he had then lived, and were now describing what he saw. Omit them he could not, without rendering his description of the creation imperfect in ye judgment of ye vulgar. To describe them distinctly as they were in themselves, would have made ye narration tedious and confused, amused ye vulgar, and become a philosopher more then a prophet. He mentions them, therefore, only so far as ye vulgar had a notion of them, that is, as they were phenomena in ye firmament; and describes their making only so far, and at such a time, as they were made such phenomena. Consider, therefore, whether any one who understood the process of ye creation, and designed to accommodate to ye vulgar not an ideal or poetical, but a true description of it as succinctly and theologically as Moses has done, without omitting any thing material wch ye vulgar have a notion of, or describing any being further then the vulgar have a notion of it, could mend that description wch Moses has given us. If it be said that ye expression of making and setting two great lights in ye firmament is more poetical then natural, so also are some other expressions of Moses, as when he tells us the windows or floodgates of heaven were opened (Gen. vii.), and afterwards stopped again (Gen. viii.); and yet the things signified by such figurative expressions are not ideall or moral, but true. For Moses, accommodating his words to ye gross conceptions of ye vulgar, describes things much after ye manner as one of ye vulgar would have been inclined to do had he lived and seen ye whole series of what Moses describes.

"Now for the number and length of ye six days: By what is said above, you may make the first day as long as you please, and ye second day too, if there was no diurnal motion till there was a terraqueous globe,—that is, till towards ye end of that day's work. And then if you will suppose ye earth put in motion by an eaven force applied to it, and that ye first revolution was done in one of our years, in the time of another year there would be three revolutions, of a third five, of a fourth seaven, &c., and of ye 183d year, 365 revolutions, that is, as many as there are days in our year,—and, in all this time, Adam's life would be increased but about 90 of or years, wch is no such great business. But yet I must profess I know no sufficient naturall cause of ye earth's diurnal motion. Where natural causes are at hand, God uses them as instruments in his works, but I doe not think them alone sufficient for ye creation, and therefore may be allowed to suppose that, amongst other things, God gave the earth its motion by such degrees, and at such times, as was most suitable to ye creatures. If you would have a year for each day's work, you may, by supposing day and night was made by the annual motion of the earth only, and that the earth had no diurnal motion till towards the end of ye six days. But you'l complain of long and dolefull nights; and why might not birds and fishes endure one long night as well as those and other animals endure many in Greenland; or rather why not better then the tender substances wch were growing into animals might endure successions of short days and nights, and consequently of heat and cold? For what think you would become of an egge or embryo wch should frequently grow hot and cold? Yet if you think ye night too long, its but supposing the Divine operations quicker. But be it as it will; me thinks one of the Tenn Commandmts given by God in Mount Sina, prest by divers of ye prophets, observed by or Saviour, his Apostles, and first Christians for 300 years, and with a day's alteration by all Christians to this day, should not be grounded on a fiction. At least divines will hardly be persuaded to [be]lieve so.

"As I am writing, another illustration of ye generation of hills, proposed above, comes into my mind. Milk is as uniform a liquor as the chaos was. If beer be poured into it, and ye mixture let stand till it be dry, the surface of ye curdled substance will appear as rugged and mountainous as the earth in any place. I forbear to describe other causes of mountains, as the breaking out of vapours from below before the earth was well hardened,—the settling and shrinking of ye whole globe after ye upper regions or surface began to be hard. Nor will I urge their antiquity out of Prov. viii. 25, Job xv. 7, Psalm xc. 2, but rather beg yor excuse for this tedious letter, which I have ye more reason to do, because I have not set down anything I have well considered, or will undertake to defend."

There is no signature to this letter, but the whole is distinctly written in Sir Isaac's hand, and almost without any corrections or interlineations, which is very unusual in his manuscripts.

No. V.

(*Referred to in page* 61.)

PART OF A LETTER FROM SIR ISAAC NEWTON ON FLAMSTEED'S SPECULATIONS RESPECTING THE SUN, THE ACTION OF HEATED MAGNETS, AND THE MOTION OF COMETS.

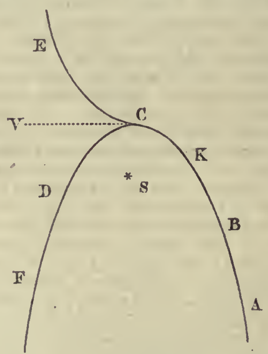
Concerning the experiment that a magnet loses its magnetism by heat, some have indeed supposed the sun to be cold, but I believe Mr. Flamsteed is not of this opinion, for they may as well affirm culinary fire to be cold. For we have no argument of its being not, but that it heats and burns things that approach it, and we have the same argument of the sun being hot. Were we ten times nearer him, no doubt we should feel him an hundred times hotter, for his light would be there an hundred times more constipated, and the experiment of the burning glass shows that his heat is answerable to the constipation of his light. So then were a body hard by the sun, his light being there about 50,000 times more constipated, his heat would be 50,000 times greater than we feel it in a hot summer day, which is vastly greater than any heat we know on earth. Now, though the inward part of the sun were an earthly gross substance, yet if the liquid shining substance which Mr. Flamsteed supposes to swim upon it, be then hot, it will heat the matter within as certainly as melted lead would heat an iron bullet immersed in it. Nor is it material whether the liquid matter on the sun be of any considerable thickness. An iron bullet would heat as fast in a quart as in an ocean of melted lead, this difference only excepted, that the bullet would cool a small quantity of lead more than a great one. If then the liquid matter swimming on the sun be but so thick as not to be cooled by the central parts (as it must be), it will certainly heat the central parts, for it imparts heat to the contiguous matter as fast as if it were thicker, and keeps of all cool environing mediums (the instrument of cooling things), from coming near the central parts to cool them. By which means the central parts must become as hot, as if the hot fluid matter surrounding it equalled the whole vortex. The whole body of the sun, therefore, must be red hot, and consequently void of magnetism, unless we suppose its magnetism of another kind from any we have, which Mr. Flamsteed seems inclinable to suppose.

As for a great magnet exercising its directive virtue more strongly than its attractive on a small one, it holds in all cases I had opportunity to observe, and till a contrary instance can be brought, I am inclined to believe it holds generally. Mr. Flamsteed puts a case of a little magnet thrown violently by a great one. In this case, certainly, the motion of the small magnet neither helps nor hinders any part of the operation of the great magnet upon it, only that it shortens the time the great magnet has to operate in. Were the great magnet thrown along by the side of the little one, that it might have time enough to work on it, no doubt it would direct it as well as attract it. For do not magnets thus operate when trajected through the air in a ship under sail, more swiftly about the centre of the earth by the diurnal motion thereof, still more swiftly about the sun by the annual motion! And were the greater of the two magnets stopped, would it operate otherwise? Surely not. It would only want time to operate in. And if for want of time it directed not, much less would it attract or repel. If an instance could be found where a projected magnet is attracted or repelled by a fixed one, but not directed, it would be material. But such an instance, I doubt, will scarce be found. But if for want of time to perform the magnetical operations in, it be neither sensibly attracted, repelled, nor directed, it is nothing to the purpose. On the other hand, I add, that a resting magnet, if it have a large sphere of activity, so that it may have time to perform its operations before the projectile magnet be out of its reach, it will direct more strongly than attract, and I give this instance of it. Let the earth represent the great resting magnet, the mariner's needle a small one: this is directed as strongly by the earth when the ship is in its swiftest motion under sail, as when it rests, so far as observations have hitherto been made, and consequently were the ship ten times swifter, would still be as much directed, and yet so little attracted by the earth as not to become sensibly heavier thereby. The case is the same with a comet continuing long within the reach of the sun's magnetism.

The instance of a bullet shot out of a cannon, and keeping the same side forward, may be a tradition of the gunners, but I do not see how it can consist with the laws of motion, and therefore dare venture to say that upon a fair trial it will not succeed, excepting sometimes by accident. The trial may be thus made upon a spell or bridge such as school-boys play with: lay a large ball, one hemisphere of which is white, the other black. Either hemisphere lying upwards, strike the edge of the bridge to make the ball rise, and if the ball receive not any circulating motion from the stroke, you will see the hemisphere which is laid upwards continue upwards as well falling as rising. If I did not know the event of the experiment by the reason of it, yet I could guess at it by what I have observed of a hand-ball tossed up.

To the foregoing objections may be added this:—If the comet be attracted in its access to the sun, and repelled in its recess, and so being continually accelerated would be swifter in its recess than in its access, contrary to what Mr. Flamsteed and others believe. For the magnetic repulse continually urging the comet to the sun, would make it go away faster and faster continually.

Another objection may be this:—Let S be the sun, ABCD the line of the



comet's motion, according to the hypothesis, ABC that part in which the comet is attracted, CDF that part in which it is repelled. When the comet comes at C, being there neither attracted nor repelled, it ought to proceed on in the line of the determination of its motion CV, and verge neither to D on the one hand, nor to E on the other; but when it is advanced a little farther, and begins to be repelled, the repulse will not make it verge from the line of its motion's direction CV towards the sun, but drive it from the sun, that is it does not make it verge from CV towards D, but towards E, and so go away in the line CE. But I should have put the point where it begins to be repelled a little sooner, as at K. If to avoid this difficulty the comet be made to pass between the sun and us, that supposition is urged by the difficulty mentioned in my former letter. But all these difficulties may be avoided by supposing the comet to be directed by the sun's magnetism as well as attracted, and consequently to have been attracted all the time of its motion, as well as in its recess from the sun, as in its access towards him, and thereby to have been as much retarded in its recess as accelerated in his access, and by this continual attraction to have been made to fetch a compass about the sun in the line ABKDF, the *vis centrifuga* at C overpowering the attraction and forcing the comet there, notwithstanding the attraction, to begin to recede from the sun."

The date of the letter, of which this was intended to form a part, is April 16, 1681.

No. VI.

(*Referred to in page* 71.)

Letter from Sir Isaac Newton to Dr. Covel.

"Sr,

"I have had an account of the solemnity of the Proclamation; and I am glad to understand it was performed wth so much decence by the wiser and more considerable part of ye university, and generosity on yor part. The next thing is a book of verses. If you do it at all, ye sooner ye better. Concerning ye new Oaths wch you are to administer, I need not give instructions to you about their legality. But because many persons of less understanding (whom it may be difficult to persuade) will scruple at them, I will add my thoughts to yours, that you may have the fuller argument for convincing them, if I can add anything to what you have not thought of; ffor, seeing these Oaths are the main thing that ye dissatisfied part of ye University scruple, I think I cannot do the University better service at present than by removing the scruples of as many as have sense enough to be convinced wth reason. The argument I lay down in the following propositions:—

"1. Fidelity and Allegiance sworn to ye King is only such a ffidelity and obedience as is due to him by ye law of ye land; ffor were that ffaith and allegiance more than what the law requires, we should swear ourselves slaves, and ye King absolute; whereas, by the law, we are ffree men, notwithstanding those Oaths.

"2. When, therefore, the obligation by the law to ffidelity and allegiance ceases, that by the Oath also ceases; ffor might allegiance be due by the oath to one person, whilst by the law it ceases to him and becomes due to another, the oath might oblige men to transgress the law and become rebells or traitors; whereas the oath is a part of the law, and therefore ought to be so interpreted as may consist wth it.

"Fidelity and Allegiance are due by ye law to King William, and not to King James. For the Statute of 25 Edw. 3, wch defined all treasons against ye King, and is ye only statute to that purpose, by the king understands not only a king *de jure* and *de facto*, but also a king *de facto*, though not *de jure*, against whom those treasons lye. Whence the Ld Chief Justice

Hales, in his Pleas of the Crown, page 12, discoursing of that statute, tells us that a *king de facto and not de jure, is a king within that Act, and that treason against him is punishable, tho' the right heir get the crown.* And that this has been the constant sense of the law, S^r^ Rob^t^. Sawyer also, upon my asking him about it, has assured me. And accordingly, by another statute in the first of Hen. 7, 'tis declared treason to be in arms against a king *de facto* (such as was Richard the Third) tho' it be in behalf of a king *de jure.* So then by y^e^ law of y^e^ land all things are treason against King William w^ch^ have been treason against former kings; and therefore the same fidelity, obedience, and allegiance w^ch^ was due to them is due to him, and by consequence may be sworn to him by y^e^ law of y^e^ land. Allegiance and protection are always mutuall; and, therefore, when K. James ceased to protect us, we ceased to owe him allegiance by y^e^ law of y^e^ land. And, when King W. began to protect us, we begun to owe allegiance to him.

"These considerations are in my opinion sufficient to remove y^e^ grand scruple about the oaths. If y^e^ dissatisfied party accuse the Convention for making y^e^ P. of Orange King, 'tis not my duty to judge those above me; and therefore I shall only say that, if they have done ill, 'Quod fieri non debuit, factū valet.' And those at Cambridge ought not to judge and censure their superiors, but to obey and honour them according to the law and the doctrine of passive obedience.

"Yesterday a bill for declaring the Convention a Parliament was read y^e^ 2^d^ time and committed. The Committee have not yet finished their amendments of it. There is no doubt but it will pass. I am in haste,

"Yo^r^ most humble servant,

"Is. Newton.

"London, *Feb.* 21, 1688-9."

No. VII.

(*Referred to in page* 77.)

Letter from John Locke to Mr. Newton.

"London, 26*th July* 1692.

"Sir,

"Finding noe better conveyance, I have sent you the 8th Chapter by Martin the carrier. It was delivered to his owne hands yesterday. I would beg y^u^, if y^u^ have soe much leisure, to read, correct, censure, and send it back by the same hand this weeke, else I fear the presse will stay. I deferred it so long, in hopes to send all together by a safe hand, missing that I have ventured but one chapter at once. As soon as this comes back, I will send the next.

"Mr. Boyle has left to Dr. Dickson, Dr. Cox, and me, the inspection of his papers. I have here inclosed sent you the transcript of two of them that came to my hand, because I knew you desired it. Of one of them I have sent you all there was;—of the other, only the first period, because it was all you seemed to have a minde to. If you desire the other periods, I will send you them too. If I meet with anything more concerning the process he communicated to you, you shall have it; and if there be anything more in relation to any of Mr. Boyle's papers, or anything else wherein I can serve you, be pleased to command,

"Sir,

"Your most affectionate and most humble servant,

"J. LOCKE.

"*First Period.*

"℞ ☿. ℔ x, cleanse it well with ℔ j of flowers of 🜍 in 24 hours. To these ℔ x, take ℥ ij of minerel soap *u* 2, shake it with the ☿, so as it may first imbody with it, and afterwards, by further agitation, be spued out by it. This worke may last 24 hours or more. To the same ☿, adde ℥ j more of the soape, and worke as before. This doe 7 times. Then before any Durca be added, the matter must be kept in agitation as before for at least 7 days, for 'twill be the better (if it be forced in) adding no soap to it. The soap being to make it throw out any feculency that may lie concealed in ☿ in the forme of a powder, whereby the ☿ will remain the purer.

"℞ ℔ j of choice ☿, and grind with it for 2 or 3 hours, or longer, if need be, ʒ j of our dry soap, till it have been $\overline{aaa}$ with it, and thrown out again in the form of an unpromising powder. Then put to it another dram of the soap, and proceed as before. Afterwards adde a 3d dram, and set aside the powder that will be thrown out as formerly, and thus impregnate the liquor with ʒ j after another, till you have incorporated with it, as far as you can doe it, by grinding a whole ounce."

No. VIII.

(*Referred to in page* 82.)

LETTER FROM DR. BENTLEY TO SIR ISAAC NEWTON.

"*Feb.* 18, $169\frac{2}{3}$.

"HON^D SIR,

"Understanding y^t the publication of my sermons might be delayed a while without any damage to y^e bookseller, I have kept them in my hands, and shall keep them a little longer. And, though there were yet several matters in them, about which I would have purchased your opinion at no small rate, nevertheless I had not presumed any further to interrupt your worthy design with questions from a stranger. But y^r unexpected and voluntary favour by the last post doth encourage me to request you, y^t you would run over this abstract and thread of my first unpublished sermon; and to acquaint me with what you find in it y^t^ is not conformable to truth and your hypothesis. My mind would be very much at ease, if I have that satisfaction, before y^e^ discourses are out of my power.

"Proved, in y^e^ 6 s^on^ 'That y^e^ present system of y^e^ world cannot have been eternal. So y^t^ matter being eternal (according to y^e^ Atheists) all was once a chaos, y^t^ is, all matter was evenly or near upon evenly diffused in the mundane spaces.'

"I proceed therefore in this 7^th^ to show, y^t^ matter in such a chaos could never naturally convene into this or a like system. To which end we must consider some systematical phænomena of y^e^ present world. And

"(1.) All bodies around our earth gravitate, even y^e^ lightest comparatively, and in their natural elements.

"(2.) Gravity or y^e^ weight of bodies is proportional to y^e^ quantity of matter, at equal distances from y^e^ center.

"(3.) Gravity is not peculiar to terrestrial bodies, but common to all y^e^ planets and y^e^ sun. Nay the whole bodies of sun and planets mutually gravitate toward one another; and in a word 'all bodies gravitate toward all. This universal gravitation or attraction is y^e^ *τὸ φαινόμενον* or matter of fact, for y^e^ demonstration of which I must referr you to . . . . . Indeed as to the cause and origin of this gravity he was pleased to determin nothing. But you will perceive in the sequel of this discourse y^t^ it is above all mechanism or power of inanimate matter, and must proceed from a higher principle and a divine energy and impression.' (I have written these words at large, y^t^ you may see if I am tender enough, how I engage your name in this matter.)

"(4.) Now if gravity be proportional to y^e^ q^ty^ of matter, there is a necessity of admitting a vacuũ.

"(5.) And to estimate w^h^ proportion y^e^ void space in our system may bear to y^e^ solid mass. Refined gold (though even y^t^ be porous, because dissoluble in ☿ and aqua regia, and y^e^ tanted non impossibility y^t^ the figures of its corpuscles should be adapted for total contact, is to common water as 19 to 1, and water to common air as 850 to 1, so y^t^ gold is to air as 16,150 to 1, so y^t^ y^e^ void space in the textur of co^n^ air is 16,150 times as big as y^e^ solid mass. And because air hath an elastick endeavour to expand itself, and y^e^ space it occupies, being reciprocally as its compression, the higher it is, 'tis y^e^ less compressed and more rarefied, and at y^e^ hight of a few miles it has some million parts of void space to one of real bodie; and at y^e^ hight of 1 terr. semid (as . . . . hath calculated), tis so very tenuious, y^t^ a sphære of our common air (already 16,150 parts nothing) expanded to y^e^ thinness of y^t^ region would more than take up y^e^ whole orb of Saturn, which is many million millions of times bigger than all y^e^ globe of y^e^ earth: and yet higher above y^t^, y^e^ rarefaction gradually increases in immensum. So y^t^ the whole concave of y^e^ firmament, except sun, planets, and atmospheres may be considered as a mere void.

"(6.) Esto hypothesis; That every fixt starr is a sun; so y^t^ the proportion of void space to matter y^t^ is found in our sun's vortex will near upon hold in y^e^ rest of y^e^ mundane space. [I know what Kepler says, Epitome Astron. p. 36, therefore quæro, if this hypothesis may pass.] Allow then y^t^ the globe of y^e^ earth is entirely solid and dense, and y^t^ all y^e^ matter of our sun, planets, atmospheres, and æther, is about 50,000 times as much as yᵉ bulk of yᵉ earth. Astronomers will bear us witness yᵗ we are liberal enough. Now the Orbis Magnus (7000 terr. diam. wide) is 343,000,000,000 times as big as the whole earth, and therefore is 6,860,000 times as big as all yᵉ matter of our system. But by the doctrine of yᵉ parallaxis, we cannot well allow less (in yᵉ Copernican hypothesis) than 100,000 diam. of yᵉ Orb Mag: for the diameter of yᵉ firmament. So yᵗ the whole concave of yᵉ firmament is (in yᵉ 3 plic. Prop.) 1,000,000,000,000,000 times as big as yᵉ sphære of yᵉ Orbis Magnus, and therefore (multiplying this by 6,860,000) it is 6,860,000,000,000,000,000,000 times as big as all yᵉ matter of our system. So yᵗ if all yᵗ matter was eavenly disperst in yᵉ concave of yᵉ firmament, every corpuscle would have a sphære of void space around it 68,600 . . . . times bigger than its own dimensions: and yᵉ diameter of yᵉ sphære would be above 19,000,000 times longer than yᵉ diameter of yᵉ corpuscle (supposing yᵉ corpuscle to be sphærical). And further, because of yᵉ equal sphæres of other corpuscles about yᵗ corpuscle, yᵉ void space about every corpuscle becomes twice as wide as it was, having a diam. compounded of yᵉ diameter of its own sphære, and yᵉ 2 semidiameters of yᵉ sphæres of yᵉ 2 next corpuscles opposite, so yᵗ every atom has a void space about it 8+68,600 . . . times as big as yᵉ atom, and would be distant 19,000,000 times its own length (if sphærical) from any other corpuscle. And by yᵉ same supposition of equal diffusion in yᵉ whole surface of yᵉ void sphære about every atom whose diam. is 38,000,000 times as long as yᵉ diam. of yᵉ atoms, there can be no more than 12 atoms placed at equal distances from yᵉ central one and from each other (like yᵉ center and angles of an icosaedron). So yᵗ lastly, every atom is not only so many million millions of times distant from any other atom, but if it should be moved and impelled (without attraction or gravitation) to yᵉ length of yᵗ distance, it is many more million millions odds to an unit, that it doth not hit and strike upon one of those 12 atoms. But yᵉ proportion of this void to matter within our firmament, may hold in all yᵉ other mundane spaces beyond it. [The measure of yᵉ Orbis M. 7000 terrest. diam. and of yᵉ firmamēt 100,000 diam. of yᵉ Orbis Magnus I take from And: Tacquet, being round numbers. If you substitute better instead of them, yᵉ calculation may be soon altered.]

"I am aware, yᵗ half of yᵉ diameter of yᵉ firm: should be allowed for yᵉ radij of yᵉ several vortices of yᵉ next fixt stars, so yᵗ the space of our sun's vortex should be diminished, as 8 to 1. But because yᵉ semʳ. of yᵉ firm. may be immensely greater thā we supposed it, we think yᵗ abatement not worth considering.

"(1.) Now the design of all this is to show, which (if yᵉ premises be granted) is evident at first sight, yᵗ in yᵉ supposition of such a chaos, no quantity of common motion (without attraction) could ever cause these stragling atoms to convene into great masses and move, as they do in our systē, a circular motion being impossible to be produced naturally, unless there be either a gravitation or want of room.

"(2.) And as for gravitation, 'tis impossible yᵗ that should either be coeternal and essential to matter, or ever acquired by it. Not essential and coeternal to matter; for then even our system would have been eternal (if gravity could form it) against our Atheists' supposition, and what we have proved in our last. For let them assign any given time, yᵗ matter convened from a chaos into our system, they must affirm y^t before y^t given time matter gravitated eternally without convening, which is absurd. [Sir, I make account, y^t your courteous suggestion by your last, y^t a chaos is inconsistent with y^e hypothesis of innate gravity, is included in this paragraph of mine.] And again, 'tis unconceivable, y^t inanimate brute matter should (without a divine impression) operate upon and affect other matter without mutual contact; as it must, if gravitation be essential and inherent in it.

"(3.) But then if gravitation cannot be essential to matter, neither could it ever be acquired by matter. This is self-evident, if gravitation be true attraction. And if it be not true attraction, matter could never convene from a chaos into a system like ours (paragraph 1.) Nay, even now, since y^e forming of our system, gravitation is inexplicable otherwise than by attraction. 'Tis not magnetism, as you have shown. 'Tis not y^e effect of vortical motion; because it is proportional to y^e Q of matter, for if the earth was hollow, there would be no less weight of bodie in y^e air (according to vortices), than if it was solid to y^e centre: there would be no less pressure toward y^e sun, if y^e whole space of y^e sun were a mere void, than if a dense bodie. Again: A vortical motion, without gravitation antecedent to it, supposeth and requirs, either an absolute full, or at least a dense texture of y^e æthereal matter, contrary to what is proved before, and what appears from y^e motions of comets: and besides, as you have shown, it contradicts y^e phænomena of y^e slower motion of planets in Aphelijs qua Perihelijs, and y^e sesquialteral proportion of y^r periodical motions to their orbs. In a word, if gravity be not attraction, it must be caused by impulse and contact; but y^t can never solve universal gravitation, in all scituations, lateral as well as descending, &c., according to y^e phænomena of your hypothesis.

"[Sir, to my conceptions, universal gravitation, according to your doctrine, is so impossible to be solved mechanically, y^t I was much surprised to see you warn me what I ascribed to you, for you pretended not to know y^e cause of it. As to innate gravity, you perceive y^t it is wholy against my purpose and argumentation. If I used y^t word, it was only for brevity's sake. But I must needs desire your judgment of w^t is here deliver'd to y^t purpose. I look't a little into Hugenius de la Pesanteur, when it newly came out, and I well remember that it cannot be reconciled to your doctrine; and Varignon's book I read, which, besides y^t it cannot explain universal gravity, is confuted by y^e most vulgar phænomena. He makes long filets of *materia subtilis* reach from y^e top of y^e earth's vortex to y^e earth. All bodies descend y^t are in y^e lower half, because y^e superior part of y^e filets are y^e longer. All ascend in y^e higher half, for y^e contrary reason. But in y^e middle of them there is a considerable space of equilibriū, indifferent both to ascent and descent, w^{ch} he calls *espace de repose,* and in y^t y^e moon moves in a circle without ascending or descending very well. Therefore, in y^e filets of y^e Sun's vortex, all y^e space between Mercury and Saturn is an *espace de repose* a small distance for y^e equilibrium; so much longer than y^e whole half of y^e filets from Mercury to y^e body of y^e sun.]

"(4.) But though we could suppose gravitation essential to matter, or rather supervene into matter while it was diffused in a chaos, yet it could never naturally constitute a system like ours.

"(1.) For if matter be finite, and seeing extension is not matter, y^e summe of y^e mundane matter must consist of seperate parts divided and disterminated by vacuum; but such parts cannot be positively infinite, any more than there can be an actually and positively infinite arithmetical summe, which is a contradiction in terms. It may be said y^t all bodies have infinite *puncta*, so y^t there are infinite summs. Indeed at y^t rate all numbers are infinite, as containing infinite fractions. Even fractions themselves are infinite. But such *puncta* are not *quanta*, so y^t the case is different *toto genere*. Can a positive summe contain infinite ones, twos, or infinite *given* fractions? Can it have infinite *quota* and *quanta* as y^e atoms we speak of are? I say, then, if matter be finite, it must be in a finite space. But, then, by universal gravity, in an even diffusion, all matter would convene in one mass, in y^e middle of y^e space, and, if never so unevenly diffused, all would convene still into one mass, though not in y^e middle of y^e mundane space, but in y^e center of y^e common gravity.

"(2.) Nay, though we suppose it once constituted, even then, even now, all would convene together in a finite system. I grant, y^t if y^e whole world was but one sun, and all y^e rest planets moving about him, they would not convene; but in several fixt starrs y^t have no motion about each other, they, with their systems of planets, would all convene in y^e common center of mundane gravity, if y^e present world was not sustained by a Divine power.

"[Sir, in a finite world, where there are *outward* fixt starrs, this seems plainly necessary; but in y^e supposition of an infinite space, let me ask your opinion. I acquiesce in your authority, y^t in matter diffused in an infinite space, 'tis as hard to keep those infinite particles fixt at an equilibrium, as poise infinite needles on their points upon an infinite speculum. Instead of particles, let me assume fixt starrs, or great fixt masses of opake matter, is it not as hard y^t infinite such masses in an infinite space should maintain an equilibrium, and not convene together; so y^t, though our system was infinite, it could not be preserved but by y^e power of God.]

"(3.) Moreover, in such a chaos, though gravity should supervene to matter, y^e planets could never acquire their transverse motions about y^e sun, &c. If they were formed in y^e same orbs they now move in, they could never begin to move circularly; y^e ætherial matter could not impress it, for y^t is too thin, and is indifferent to east or west, as appears from comets. Nor could gravity act in a horizontal line, as they move in where there is no inclination nor descent. Now, therefore, suppose the planets to be formed in some higher regions, and first descend towards the sun, wherby they would acquire their velocities; but then they would have continued their descent to y^e sun unless a Divine power gave them a transverse motion against y^t vast impetus y^t such great bodies must fall with; so y^t on all accounts there's a necessity of introducing a God.

"[As to what you cite from Blondel, I have read y^e same in Hon. Fabri's *Astronomia Physica*, and Galilæo's *System.* p. 10 and 17, who adds, y^t by the velocity of Saturn, one may compute at what distance from y^e sun it was formed, according to y^e degrees of acceleration found out by himself in the progression of odd numbers. (But he must surely have erred, not knowing w^t you have since shewn, y^t y^e velocity of descent as well as weight of bodies decreases as y^e square of y^e distance increases,) and y^t there is y^t proportion of y^e distances and velocities of all y^e planets *quam proxime*, as if they all dropt from y^e same hight. (But you seem to reject this, saying, y^t the gravitation of y^e sun must be doubled at y^e very moment they reach their orbs.) I confess I could make no use of y^e passage of Galilæo and Fabri, because I could not calculate, so y^t I said no more, but in general as above, and y^e rather because I knew that there must be some given hights, from whence each of them descending, might acquire their present velocities. But I own, y^t if I could understand y^e thing, it would not be only ornamental to y^e discourse, but a great improvement of y^e argument for a Divine power; for I think it more impossible y^t they should be all formed naturally at y^e same y^n at various distances; and 'tis y^e miracle of all miracles if they were naturally formed at such intervals of time, as all of them to arrive at their respective orbs at y^e very same moment, which is necessary, if I rightly conceive your meaning about doubling y^e sun's attraction; for if Mercury fell first, and when he reached his own orb, y^e sun's attraction was doubled. That continuing doubled, y^e descents of y^e succeeding planets would be proportionably accelerated, which would disturb y^e supposed proportion betwixt Mercuries velocity and theirs.

"Hon^d. Sir, This is y^e contents of y^e former sermon; y^e latter is an argument of a divine goodness from y^e meliority in our system, above what was necessary to be in natural causality. I hope I shall have no need to give you more trouble in y^t. But, Sir, while I am writing this, I have received a letter from my bookseller, calling away for y^e press. Let me but begg of you, by the next post, some brief hints what you approve of, and what not; for I have resolved to expect your answer, let him be never so clamorous. S^r, I heartily ask your pardon for giving you the trouble of this, which I must increase likewise by another piece of boldness in desiring your good leave to present you with my 8 poor discourses when these 2 last are made publick.

"Sir, I am your most obliged & hu. ser^t,

"R. BENTLEY.

"*For*

The Hon^d. MR. ISAAC NEWTON,
Math. Prof. and Fellow
of TRINITY COLLEGE, in
CAMBRIDG."

"*Post-paid* 5."

No. IX.

(*Referred to in page* 99.)

LETTER FROM SAMUEL PEPYS TO MR. NEWTON.

"*Dec.* 21, 1693.

"SIR,

"If to what you have done, and which I can in noe wise sufficiently acknowledge your favour in, it could bee excusable to come once more to you upon y^e^ same errand; it should bee to aske you whether B's disadvantage (in his contest with A) bee anything different under his obligation to fling 2 sixes at one throw with 12 dyes, from what it would bee were he to doe it twice with 6 dyes at a time out of one box, or at once out of 2 boxes with that number in each. I being yet (must owne) unable to satisfy my selfe touching y^e^ difference, *i.e.*, how it arises, though at y^e^ same time you have putt mee beyond all doubt of A's having y^e^ advantage in y^e^ maine of B. Nor must I conceale my being at y^e^ same losse how to comprehend, even flinging 12 dyes at one throw out of a single box (y^e^ sayd dyes being tinged ½ greene, ½ blew), they being lesse provided for turning up a six with either of these different-coloured parcells while flung together out of y^e^ same box, then were y^e^ 6 blew to bee throwne out of one box, and y^e^ 6 greene from another; in which latter case, I presume each of them severally would bee equally entitled to the producing of a six with A's 6 white ones, and by consequence of 2 when flung together.

"I am conscious enough that this is but fumbling, and that it arises only from my not knowing how to make y^e^ full use of your Table of Progressions; but pray bee favourable to my unreadinesse in keeping pace with you therein, and give me one line of farther help. I am most thankefully, deare Sir,

"Your obliged and most humble
"and faythful serv^t^,
"S. PEPYS."

No. X.

(*Referred to in page* 103.)

1.—LETTER FROM DR. JOHN MILL TO MR. NEWTON.

"MY MOST HONOURED FRIEND,

"I am heartily sensible of your many signal favours and civilities to me when last at Cambridge. I hope you have not forgot your kind promise of remarking in paper your thoughts of the varieties you have met with in the Apocalypse. Whatever I have not observed already in my book, I would willingly add in my Appendix, which is going on, and will have many things in it very considerable. My book, as long as it is with you, is in as safe hands as I can desire. If you please, you may take the first fair opportunity of conveying it hither. I think the best way will be by our Oxford carrier, if the waters be low enough. You may send for him, and put the book, carefully packed up, into his own hands. And if your servant go along with him and see it put up in his pack, it will do well; we cannot be too careful in a matter of this consequence. I have been mighty curious since I saw you last, in observing something which I have all along slighted as trivial hitherto, the points of distinction in the old Alexandrian copy. And now I find them extraordinary accurate and regular; there is but one note for all manner of distinctions indeed, and 'tis at the top of a word, as our modern Greek colon (:), but then 'tis placed with such exactness and caution everywhere, as to distinguish the notions and ideas in each clause and sentence infinitely better than we could do with all our modern apparatus of distinctions. I am so very fond of their way of distinguishing the text, that I could heartily wish, when I collated the Beza MS., I had marked all the distinctions. For a last, may I presume to beg your favour to transcribe any one single page in the Greek, and to point it exactly according to the copy, 'twill be a mighty obligation.

"My most humble service to my noble worthy friend, your master, as also to Dr. Covel. He put an Arabic charm in my hands, which I have not yet returned. The next return of the carrier he shall surely receive it, with a translation of some part of it. I hope our common friend Mr. Laughton is well. Pray give him my thanks for all civilities.

"But I doubt I trespass upon your time and studies. I wish you all imaginable health and happiness, and remain ever, with the greatest sincerity of affection,

"Worthy Sir,

"Your most obedient humble servant,

"Jo. Mill.

"St. Edmond Hall, Oxon,
Novr. 7, 1693.

"These for the truly honor'd Mr. Professor Newton,
at his lodgings in Trinity College,
"Cambridge."

"2.—Letter from Mr. Newton to Dr. John Mill.

"Sr,

"I feare you think I have kept yor book too long: But to make some amends for detaining it so long, I have sent you not only my old collations so far as they vary from yours, but also some new ones of Dr. Covil's two MSS; ffor I have collated them anew, & sent you those readings wch were either omitted in yor printed ones, or there erroneously printed. In collating these MSS., I set the readings down in the margin of yor book, & thence transcribed them into a sheet of paper, wch you will find in your Book at ye end of ye Apocalyps, together wth my old collations, & a copy of a side of Beza's MS. The collations I send you of Dr Covil's two MSS. you may rely upon; ffor I put them into Mr. Laughton's hand wth ye two MSS., & he compared them wth ye MSS. and found them right. In the other collations you will find that Stephens made several omissions & some other mistakes, in collating the Complutensian edition, tho' its probable that he collated this edition wth more diligence & accurateness than he did any of ye MSS. Where I have noted any readings of ye Alexandrin MS., I desire you would collate that MS. again wth my reading, because I never had a sight of it. I could not observe any accurateness in ye stops or commas in Beza's MS. You may rely upon the transcript of something more then a side of it, wch you will find in your Book at ye end of the Apocalyps. In your little MS. book, which I return you, tyed up together w[th] your New Testament, you will find those transcripts you desired out of MSS., except two, w[ch] were in such running hands y[t] I could not imitate them, nor did it seeme worth the while, y[e] MSS. being very new ones.—I am, in all sincerity,

"Y[r] most humble & most obedient servant,

"Is. Newton.

"Trin. Coll. Cambridge,
Jan. 29, 169$\frac{3}{4}$."

This letter is followed by one leaf headed *Spicilegia Variantium Lectionum in Apocalypsi ex MSS. Sin. et Cov.* 2. It is written on both sides, and appears to be the "sheet" referred to in line 7 of Newton's letter. After this come two leaves containing three pages of various readings in the Apocalypse, which appeared to be "my old Collations," referred to in line 2d of the letter.

No. XI.

(*Referred to in page* 119.)

TABLE OF REFRACTIONS SENT BY FLAMSTEED TO NEWTON.

I have thought it right, for the reasons mentioned in the note on page 119, to give the following table of refractions, communicated by Flamsteed to Newton, on the 11th October 1694, which has been omitted in the copy of the letter published by Mr. Baily in his Life of Flamsteed, p. 134.

⊙ Distantia visa a vertice.	× Refr. ⊙ and ♀.	⊙ Refr. simplex.	⊙ Distantia visa a vertice.	× Refr. ⊙ and ♀.	⊙ Refr. simplex.
77° 00′	2′ 00″	3′ 30″	88° 40′*	19′ 30″	
80 00	3 40	5 10	88 52	20 50	
81 00	4 00	5 30	89 00	21 30	
82 00	5 00		89 11	23 20	
83 00	6 00		89 20	24 20	
84 00	7 00		89 27	25 30	
85 00	8 00		89 30	26 30	
86 00	10 00		89 38	27 10	
87 00	12 30	14 00	89 44	28 30	
87 30	13 30		89 49	29 20	
88 00	16 00		89 51	30 00	
88 25	17 25		89 55	31 00	
88 35	18 45		90 02	32 03	

* Dist. vera 89°.

No. XII.

(*Referred to in page* 149.)

LETTER FROM MR. FLAMSTEED TO MR. NEWTON.

"THE OBSERVATORY, *Jan.* 2, 169$\frac{8}{9}$.

"SIR,

"I was in your neighbourhood on Saturday last, but thought it [not right] to disturb you with a visit when I had nothing to offer [excepting] my respects, and the usual wishes of many happy years, this. I had not troubled you now, but that on my way home I received a letter from Dr. Wallis, in which he mentions that *I have received the packet* (that is my [letter] on the parallax of the pole star), *and at the same time I received another letter from one in London,* which desired me not to PRINT ANY PARAGRAPH OF THE LETTER WHICH SPEAKS OF YOUR GIVING MR. NEWTON OBSERVATIONS OF THE MOON. *He is a friend of both of you, but he does not give his* REASONS WHY. I thought but to acquaint you with *it, and desire your advice upon it.* S^{r}, I wrote my letter to Dr. Wallis in great haste, and when I had much other business in my hands, in November last, and to silence some busy people who are always asking *why I did not print,* I took occasion to let them know, that since the year 1689, when I was first fitted for it, I have been laying in a stock of observations to rectify the places of the fixed stars; that in 1694 I rectified my solar tables, and laid a foundation for the reclassification of the fixed stars; that in 1695 I furnished you with 150 observed places of the moon, and with the places also calculated from my tables, in order to the correction and restitution of her theory: That I had tables for abridging the labour (usually employed in calculating the stars' places from my *data*) under my hands and others, to make the catalogue more useful, and I wrote my letter in English, and the good Doctor having promised me a week's work as a recompense for my pains, I sent him word that I would excuse that, if he would save me the labour of putting it into Latin. It was then but three sheets which (he accepting the condition) I sent him, and thereby gained time to copy six months' observations from my books, and furnish my country calculators with the right ascensions, &c., of the stars in the southern constellations, to calculate their longitudes and latitudes from. In a fortnight's time I received two of the three sheets from the Doctor loose in a wrapper, from Dr. Gregory, with directions to leave them, when perused (for him to return), at Mr. Hindmarsh's, a bookseller's shop, *where the nonjurats resort,* in Cornhill. The third sheet soon followed, but on perusal of them I found it was requisite to add almost another, to explain some places where I had been too short, or where the Doctor, not having thoroughly understood my meaning (by reason he had not seen my instruments, nor was acquainted with my methods), had not expressed it as I would myself. This took me up more time than I expected, which made me to send my packet by the post, lest Dr. Gregory should not convey it so soon to the Doctor as I desired. However, I gave Dr. Gregory notice that I had returned it, and he was as diligent to write to Dr. Wallis as above, for what occasion I know not. I shall give you the whole paragraph wherein I have mentioned my accommodation of you with materials, and I assure you I have not mentioned you on that account any where besides in my letter, onely [in] the book I have, where I shew that if we allow the nutation, this parallax must be greater, as much as it is. My words are these :—'Contraxeram etiam cum D° Newtono doctissimo tunc temporis in academia Cantabrigiensi Professore necessitudinem cui lunæ loca ab observationibus meis ante habitis deducta 150 dederam, cum locis simul è tabulis meis ad earum tempora supputatis tum similia in posterum prout assequerer promiseram cum elementis calculi mei in ordine ad emendationem theoriæ lunaris Horroccianæ qua in re spero eum successus consecuturum expectationi suæ pares.'

"Sr, this is the paragraph, and *all of it.* I think there is not near so much in it as I acknowledge to myself, and (I have heard from other worthy gentlemen) you have acknowledged to them, and therefore cannot think it was from any intimation of yours (tho' he says it wd be *displeasing to you if it were printed*), but out of a design to ingratiate with you that he put an arrest upon this paragraph. I think the word *Horroccianæ* may be omitted, tho' I put it in because you allow that theory as far as it goes, you found the faults of it by the differences from my observation. He was a countryman, and tho' your theory will be new in that (tho' you give us the reasons, and derive it from natural cause), yet he gave the groundplot, and it will be an honour both to you and me to do him justice.

"Sr, My observations lie the king and nation in at least 5000lb. I have spent above 1000lb. out of my own pocket in building, instruments, and hiring a servant to assist me now near 24 years. 'Tis time for me (and I am now ready for it) to let the world see I have done something that may answer this expense, and I therefore hope you will not deny me the honour of having said that I have been useful to you in your attempts to restore the theory of the moon. I might have added the observations of the comets, places given you formerly of the superior planets, and observations at the same time with the moons, but this I thought wd look like boasting, and therefore I forbore it.

"I desire you would please to let me know by a line whether Dr. Gregory ever showed you my letter, I mean Dr. Wallis his translation of it, which I think I have altered in the paragraph above from what it was, but cannot say in what words, because I returned the Doctor his copy, with my transcript of it enlarged and altered, together ; but whenever 'tis printed, you will find it agree with the copy above exactly.

"Sr, I am told Dr. Gregory is to be tutor in mathematics to the Duke of Gloucester, *which place, I was told some months ago* (when the settling of his household was first discourst of) *was designed for me.* To make a variance betwixt you and me and Dr. Wallis, and to engage you to procure him the favour of Mr. Montague, I am apt to believe he recommends himself in this business. He thinks, perhaps, it will depreciate me, and keep me from being his competitor. Let him not trouble himself. I have an interest much beyond his whenever I please to move that way, but I do not think the Duke yet fit for a mathematical tutor, or that he will be this four or five years. I hate flattery, and shall not go to court on this account till I am sent for, or have notice that I am desired. That place might, indeed, afford me the opportunity of procuring help for my assistance, or I could defray the charges out of pay; but I fear it would be as prejudicial to me otherwise, and therefore shall not move to traverse the Doctor's designs, except he force me to it by his *treacherous behaviour*.

"Sr, I beg an answer to this letter speedily, and you need tell me no more but that you have seen the paragraph before, or not seen it; that you gave such orders to Dr. Gregory or not, that I may return an answer to Dr. Wallis; and hereafter, if any such flatterers as he come to say any thing to you that may tend to make a difference betwixt us, pray tell them you will inform me, and you will forthwith be rid of them. I shall always use the same course towards you, whereby a friendship that began early may continue long and be happy to both of us, which, through God's blessing, I hope it may, at least I shall always endeavour it, being ever, Sr,

"Your most affectionate friend and humble servant,

"JOHN FLAMSTEED, M.R.

"Pray enquire what company Dr. Gregory keeps, that you may not be deceived in his character. The Scotch think to carry all before them by the Bp of Salisbury, whom I esteem (next the Bp of Wester above the rest of the clergy), but I cannot think him wise in placing his countrymen about the young Duke.

"TO MR. ISAAK NEWTON,
Warden of the Mint,
at his house in German Street, near
St. James's, London.—These present."

Owing to the great decay of the paper, the first lines of this letter are hardly legible.

No. XIII.

(*Referred to in page* 168.)

ARTICLES OF AGREEMENT BETWEEN CHURCHILL, FLAMSTEED, AND THE REFEREES.

"Articles of agreement made this day of October,[1] in the fourth year (1705) of the reign of our Sovereign Lady Anne, by the grace of God Queen of England, Scotland, France, and Ireland, Defender of the Faith, &c., between the Honourable Francis Robarts, Esq., Sir Chr. Wren, Kt., Sir Is. Newton, Kt., David Gregory, Doctor of Physic, and John Arbuthnot, Doctor of Physic, on the one part; Mr. John Flamsteed, Her Majesty's Astronomer at the Observatory in Greenwich, on the other part; and Mr. Aunsham Churchill of parish, in London, on the third part.

"Whereas His Royal Highness Prince George of Denmark, out of his great generosity and propension to encourage arts and sciences, hath been

[1] Flamsteed says that they were dated November 10.

pleased to defray the charges of printing all the Astronomical Observations of the said Mr. John Flamsteed made at the said Observatory, in a book entitled *Historia Cœlestis*, and to refer the care and management of the said impression to the said Fr. Robarts, Esq., Sr Chr. Wren, Sr Is. Newton, Dr. Gregory, and Dr. Arbuthnot; and whereas the said referees, by and with the consent of the said Mr. John Flamsteed, have treated with the said Mr. Aunsham Churchill for printing the same, it is hereby covenanted and agreed between the said parties as followeth:—

"I. That the said Aunsham Churchill shall print, or cause to be printed, four hundred copies, well corrected, and only four hundred copies of the said *Historio Cœlestis*, upon the same paper, and with the same letter with the paper and letter in the specimen hereunto annexed; and for every 400 copies of every sheet so printed off, shall receive the sum of thirty and four shillings.

"II. That for making the impression correct, the said A. Churchhill, at his own proper cost and charges, send the corrected proof of every sheet to the place appointed, or to be appointed by the said referees, to be there further corrected, compared with the original, and allowed by the said Mr. J. F. or his order, before the same be printed off.

"III. That the said Mr. J. F., or his said order, shall have access to the press at all times, and be allowed to stand by the same while the said number of 400 copies of any sheet or sheets shall be printed off, and then to break the press without delay, let, hindrance, or molestation of or from him the said A. Ch., or his printer, or printers, their servants or agents, or any of them, on any pretence whatever.

"IV. That the 400 copies of every sheet, within 14 days after the same shall be printed off, shall, at the charge of the said A. Ch., be sent to the order of the said referees, to be kept for his Royal Highness till the whole be printed off, excepting the two last copies of the sheet, or two copies last printed off, which, at the charges of the said A. Ch., shall be sent the one to the said J. Fl., or his order, the other to the order of the said trustees, to be examined and collated with the last proof, and with the original papers of the said Mr. J. F.; and that every sheet in which any error shall be found, which is not the error of the copy, be corrected, and shall be reprinted at the sole cost and charges of the said A. Ch., both for paper and printing.

"V. That the said A. Ch. shall set five sheets per week, abating only a sheet for every holiday, provided that the said A. Ch. be supplied with sufficient MS. copy, and that sufficient dispatch be made in correcting the proof-sheets.

"VI. That within two months after the said book shall be in the press, the said referees, or the major part of them, shall sign an order for the said A. Ch. to receive of the Treasurer of His R. Highness the sum of three hundred pounds, advanced in part of payment, for the paper and printing of the said book. And after the impression of the said book shall be finished, the said referees, or the major part of them, shall sign an order for the said A. Ch. to receive the remainder of the money which, after the rate of 34s. per sheet, shall then be due to him, the said A. Ch., for the paper and printing of the whole impression.

"VII. That the said A. Ch. shall not have, or claim, or endeavour to have, any right, title, or interest, either in the original copy or in the printed copies or any part thereof."

On another leaf of the same sheet, though not immediately following the preceding articles, I find the following articles relating to Flamsteed, which, like the preceding, are written in Newton's own hand, and afford ample materials for the defence of the referees.

ARTICLES OF AGREEMENT BETWEEN FLAMSTEED AND THE REFEREES.

"I. That the book shall be printed in two volumes, the *first* to consist of three parts, namely :—

"1st, The catalogue of the fixed stars.

"2d, The observations of the fixed stars, planets, &c., by the sextant, telescope, and micrometer, from the year 1675 to the year 1689 inclusively; and

"3dly, The places of the planets and comets computed from those observations, together with a general Preface.

"The *second* to consist of two parts, viz. :—

"1st, Observations made by the wall quadrant, telescope, and micrometer in and after the year 1689, until the finishing of the impression.

"2dly, The places of the planets and comets computed from them.

"II. That Mr. Flamsteed shall, with all convenient speed, prepare and deliver in to the said referees or their order, fair and correct copies of his Catalogue of the fixed stars, and of the observations to be printed in the two volumes, with fair and correct schemes in folio, of the figures of eclipses, and other telescopic phenomena, to be graved in copper plates. And that within months he shall deliver in to the said referees a fair copy of the observations to be printed in the second volume.

"III. That the said Mr. John Flamsteed shall, with all convenient despatch, compute, or cause to be computed, the places of the planets which remain to be computed, and deliver in to the said referees fair and correct copies of all their true places, computed from the observations and well corrected, to be printed in the two volumes at the end of the Observations.

"IV. That after the first volume shall be printed off, the said referees shall sign an order for the said Mr. Flamsteed to receive of the trustees of His Royal Highness, for the charges of copying the same, and correcting the press, the sum of fifty pounds, and for the charges of computing the apparent longitudes and latitudes of the planets in that volume (not exceeding hundred l. and lat.) after the rate of 1s. 6d. per place; and for computing the true longitudes and latitudes of the moon from the apparent places, not exceeding places, after the rate of 1s. 6d. per place. And after the second volume shall be printed off, they shall sign a like order for the said Mr. John Flamsteed to receive £50 more for copying and correcting the same, after the rate of 6d. (?) per place for computing the places of the other planets in longitude and latitude, not exceeding places, and after the rate of 1s. 6d. per place, for computing the true longitudes and latitudes of the moon, not exceeding places, provided the computation be performed exactly to the satisfaction of the said referees.

"V. That the said John Flamsteed shall suffer the said referees, or their order, or any of them, to collate the said fair MS. copies and schemes, and also the printed copies, with all or any of the original papers in his custody, from whence the said MS. copies and schemes were taken, and with the first minutes from whence those papers were drawn up, and for that end shall, at the request of the said referees, lend the said papers and minutes, or any part of them, to the order of the said referees, the person to whom they are lent giving a receipt for the same.

"VI. That the said Mr. John Flamsteed shall, before next Michaelmas,[1] fairly describe schemes in folio of a fit size for the book containing the figures of the eclipses and other telescopical observations proper to be described, of the same magnitude as in the MS., and shall assist a graver with his directions for graving the same in copper-plate, and examine the plates and correct their faults, so that the schemes may be exact, and the said [graver] shall roll off, or cause to be rolled off, four hundred schemes from every plate, upon four hundred sheets of the same paper with that of the book, to be bound up with the book in such a manner that they may be laid open readily and conveniently."

On the back of the folio page which contains the preceding articles, and immediately after them, I find the following paragraph, which is not numbered, but which seems to be an alternative mode of paying Flamsteed instead of the one in Art. IV.

"That the said referees, or the major part of them, shall also sign orders for the said Mr. John Flamsteed, to receive of the said Treasurer of H. R. H. the sum of two hundred and [fifty] pounds for his charges in agents, servants to calculate observations, copy papers and schemes, and correct the press, the one half thereof to be paid so soon as the first volume of the said book shall be printed off,[2] and the other half thereof to be paid as soon as the second volume of the said book shall be printed off, provided the same Mr. John Flamsteed shall well and truly observe, perform, fulfil, and keep all and singular the articles, covenants, and agreements above-mentioned, specified and declared, which on his part ought to be observed, performed, fulfilled, and kept."

Immediately after this, I find the following additional article in reference to A. Churchill.

"That the said Mr. A. Ch. shall be bound in £1000 to perform the articles on his part."

It is obvious that Flamsteed was acquainted with these articles, as he refers to Article V. in his letters to A. and F. Churchill of the 24th May and the 7th June 1706, and in his letter to Sir Christopher Wren.—See Baily's *Flamsteed*, pp. 224, 225, and 88, line 5.

No. XIV.

(*Referred to in page* 178.)

The following are the cancelled and the substituted paragraphs in Flamsteed's letter to Sir Christopher Wren, dated 19th July 1708 :—

[1] October 11, 1706.

[2] See Baily's *Flamsteed*, p. 261.

The following is the concluding paragraph in the *original* letter, but cancelled in the copy inserted by Mr. Baily in Flamsteed's Autobiography:—

"I am not only willing, but desirous, that the press should proceed to finish the first volume of Observations. I have spoke to Mr. Hodgson to take care of correcting the second proofs, and with him I shall leave the six sheets to be added; which when they are wrought off, Sir Isaac Newton has 175 sheets of the second volume in his hands, that the press may proceed with whilst I am completing the Catalogue, so there need be no stop on my account, as there never was, nor hereafter shall be, God sparing me life and health, and prospering, as I firmly believe he will, my sincere endeavours.

"I am, with all due respect, and for all your favours,
"Yr grateful and obliged humble servt.,
"JOHN FLAMSTEED, M. R.

"I think to send a copy of this letter to Mr. Roberts, and doubt not but he will imparte the contents of it to Sir Isaack Newton."

N.B.—The last three lines of the letter, from "I am, &c., to M. R.," and the postscript, are in Flamsteed's handwriting.

The following are the concluding paragraphs substituted in the pretended copy taken from the original by Flamsteed himself:[1]—

"I am as willing as you can be that the press may proceed: but to have it hurried on at this time, when I cannot possibly look after it, and only to find a printer in work who at other times has neglected it, would be a piece of folly, for which I am confident all the referees would condemn me. I must therefore entreat them that this resolve be suspended till my return out of the country; when, God sparing me life and health, I hope, with the assistance of the referees, to put the press into such a method, as it may have no stops, if any heed may [be] given to my advice.

"I beg your pardon for so long a letter: the occasion has forced me to be more troublesome than I ought to one of your age and employment. If you excuse me now I hope no further occasion will be of repeating it: and I shall ever own myself,

"Sir, your most obliged and humble servant,
"JOHN FLAMSTEED, M.R."

No. XV.

(*Referred to in pages* 169, 181.)

The following document, which I found in a state of decomposition, contains an account of the expense incurred by the Prince's referees, and

[1] See Baily's *Flamsteed.*

also that which was incurred by the Government in completing the *Historia Cœlestis,* as edited by Halley. It is in Sir Isaac Newton's handwriting, and on the back of a folio containing his observations on Bernoulli's letter of the 7th June 1713:—

"Charge.

"I received of Edward Nicolas, Esq., at one time, £250, at another, £125—total received,	£375 0 0
"Upon reckoning with the Prince's administrators, I paid back the balance of the account, the same being	25 3 0
	£349 17 0

"Discharge.

"Paid to Mr. Churchill for paper and printer,	£194 17 0
"To Mr. Flamsteed for his copy,	125 0 0
"To Mr. Machin, for correcting the copy by the minute-book and examining some calculations,[1]	30 0 0
	£349 17 0

"Some time after this Dr. Halley undertook to finish the book, and the referees of the Prince acted no further, and after the work was finished and the accounts stated, moneys were impressed to me without account to pay them off.

"Charge.

"Received,	£364 15 0

"Discharge.

"Paid to Mr. Churchill for paper and printing,	£98 11 0
"Paid for designing and graving the draughts and rolling off the plates,	116 4 7½
"Paid to Dr. Halley,	150 0 0
	£364 15 7½

"Besides £20 paid to Senr Catenaro, which I did not bring to account."[2]

See Flamsteed's Autobiography, p. 102, where he has given an impertinent account of these transactions. Flamsteed met Newton at the Ex-

[1] Flamsteed mentions this sum as given to one of Newton's servants for assisting him in the calculations.

[2] The "Figures for the frontispieces and capitals" were engraved by Catenaro, who, upon "complaining that the first agreement was too hard a bargain," received £20 additional.

chequer, when he was "passing his accounts there concerning the disbursement of the Prince's monies." He told Flamsteed of the additional £20 given to Catenaro, but he did not tell him that he paid it out of his own pocket; and Flamsteed considers it as part of the Prince's money, "thrown away by Newton only to show his liberality." The above Charge and Discharge is the account which Mr. Baily tells us he was not able to get a sight of.—Baily's *Flamsteed*, p. 102, *note.*

No. XVI.

(*Referred to in page* 182.)

LETTER FROM SIR ISAAC NEWTON TO MR. FLAMSTEED.

"SIR,

"By discoursing with Dr. Arbuthnot about your Book of Observations which is in the press, I understand that he has wrote to you by her Majesty's order, for such observations as are requisite to complete the Catalogue of the Fixed Stars,[1] and you have given an indirect and dilatory answer. You know that the Prince had appointed five gentlemen to examine what was fit to be printed at his Highness's expense, and to take care that the same should be printed. Their order was only to print what they judged proper for the Prince's honour; and you *undertook, under your hand and seal, to supply them therewith,* and thereupon your Observations were put into the press. The Observatory was founded to the intent that a complete catalogue of the fixed stars should be composed by observations to be made at Greenwich, and the duty of your place is to furnish the observations. But you have delivered an imperfect catalogue, without so much as sending the observations of the stars that are wanting, and I hear that the press now stops for want of them. You are, therefore, desired either to send the rest of your catalogue to Dr. Arbuthnot, or at least to send him the Observations which are wanting to complete it, that the press may proceed. And if instead thereof you propose any thing else, or make any excuses or unnecessary delays, it will be taken for an indirect refusal to comply with her Majesty's order. Your speedy and direct answer and compliance is expected."

This draft of a letter to Flamsteed must have been written immediately after the 23d of March 1711, the date of Flamsteed's answer to Dr. Arbuth-

[1] Flamsteed, in his petition to the Queen, December 29, 1710, distinctly states that his Catalogue of 3000 Fixed Stars was finished and ready to be transcribed. "I have made further advances," he adds, "than 'tis proper to mention here, and might have presented your Majesty *with the whole work perfected before this time,* if his Royal Highness's noble intentions had not been prevented, and my endeavours continually obstructed by those who ought, and whose duty I conceive it was, to have seconded and promoted both."—Baily's *Flamsteed*, p. 278.

not's application in the name of the Queen, on the 14th of the same month. In the letter to Arbuthnot, which Newton justly characterizes as "indirect and dilatory," Flamsteed tells him that "a great deal more help is requisite, *and must be procured* to calculate the new Tables and the planets' places therefrom, to render the work complete, worthy of the British nation, the name it bears, her Majesty's patronage, and to commend the memory of his Royal Highness to posterity;" and he proposes that he should discourse with him a few hours, and, for that purpose, come and dine with him. The Royal Observatory was founded, as Newton states, to form a complete catalogue of the fixed stars, and Flamsteed was made Astronomer-Royal, or *Astronomical Observator*, as he was then called, for this very purpose.

No. XVII.

(*Referred to in page* 212.)

LETTER FROM M. MONTMORT TO BROOK TAYLOR.

"*Avril* 12, 1716.

"Ce seroit dommage que ce bon vin fut bu par des commis de vos douanes: étant destiné pour des bouches philosophiques, et la belle bouche de Mademoiselle Barton. Je suis infiniment sensible à l'honneur qu'elle (M^lle^ Barton) me fait de se souvenir de moy. J'ai conservé l'idée du monde, la plus magnifique de son esprit, et de sa beauté. Je l'aimois avant d'avoir l'honneur de la voir, comme nièce de Mr. Newton, prevenu aussi de ce que j'avois entendu dire de ses charmes même en France. Je l'ai adorée depuis sur le temoignage de mes yeux, qui m'ont fait voir en elle, outre beaucoup de beauté, l'air le plus spirituel et le plus fin. Je crois qu'il n'y a plus de danger que vous luy fassiez ma déclaration. Si j'avois le bonheur d'estre auprès d'elle; je serais aussitot et aussi embarassé que je le fus la première fois. Le respect et la crainte de luy deplaire m'obligeroit ce [de] me taire et à luy cacher mes sentimens. Mais à 100 lieues loin et separé par la mer je crois qu'un amant peut parler sans être temeraire, et une dame d'esprit souffrir des déclarations sans qu'elle puisse se reproché[r] d'avoir trop d'indulgence. Il vint icy, il y a quelques jours, une personne de sa part. Je n'y étois pas, vous pouvez croire qu'il fût bien reçu par Mad^me^ de Montmort aussitot qu'il se fut nommé de Mad^elle^ Barton. Il ne voulust point dire ce que l'amenoit, il dit seulement qu'il reviendroit. Mad^me^ de Montmort jugea que c'est une personne qui fait icy des commissions pour des personnes de qualité d'Angleterre. Je voudrais bien que Mad^elle^ Barton voulust m'honorer du soin du luy faire les emplettes et de me faire son comissionaire. Outre le plaisir de servir une si belle personne j'aurais celui de m'acquitter envers Mr. Newton d'une partie des obligations que je luy ai."

No. XVIII.

(*Referred to in page* 215.)

EXTRACTS FROM SWIFT'S LETTERS TO STELLA, IN WHICH MRS. BARTON AND LORD HALIFAX ARE MENTIONED.

"1710, *Sept.* 28.—I dined to-day with Mrs. Barton alone at her lodgings."[1]

"1710, *Oct.* 1.—To-morrow I go with Delaval, the Portugal envoy, to dine with Lord Halifax at Hampton Court."

"1710, *Oct.* 13.—Lord Halifax is always teasing me to go down to his country house,[2] which will cost me a guinea to his servant, and twelve shillings coach hire, and he shall be hanged first. Is not this a plaguy silly story? But I am vexed at the heart, for I love the young fellow, and am resolved to stir up people to do something for him. He is a Whig, and I'll put him upon some of my cast Whigs, for I have done with them, and they have, I hope, done with this kingdom for our time."

"1710, *Oct.* 14.—What, another! I fancy this is from Mrs. Barton; she told me she would write to me, but she writes a better hand than this."

"1710, *Nov.* 28.—Lord Halifax sent to invite me to dinner, where I staid till six, and crost him in all his Whig talk, and made him often come over to me."

"1710, *Nov.* 30.—To-day I dined with Mrs. Barton alone."

"1710, *Dec.* 19.—I visited Mrs. Barton."

"1711, *Jan.* 23.—I called at Mrs. Barton's, and we went to Lady Worsley's,[3] where we were to dine by appointment."

"1711, *Jan.* 24.—As for my old friends, I never see them, except Lord Halifax, *and him very seldom*."

"1711, *March* 7.—Mrs. Barton sent this morning to invite me to dinner, and there I dined just in that genteel manner that M. D. (Stella and Dingley) used, when they would treat some better sort of body than usual."

"1711, *April* 3.—I was this morning to see Mrs. Barton. I love her better than any body here, and see her seldomer. Why really now, so it often happens in the world that when one loves a body best—psha, psha, you are so silly with your moral observations."

"1711, *April* 10.—I have been visiting Lady Worsley and Mrs. Barton to-day."

"1711, *May* 29.—Pr'ythee, don't you observe how strangely I have

[1] This is the only place where Swift speaks of Mrs. Barton's lodgings; and it is important to observe, that Newton was at that very time removing from Chelsea to St. Martin's Street, so that Mrs. Barton was probably occupying lodgings for a short time while the house was preparing for her uncle. It is quite clear also, from the extracts dated October 9, 25, and November 28, 1711, that Mrs. Barton was living at Newton's house in Leicester Fields. At this time, too, Mrs. Barton, at Swift's request, carried a message from Bolingbroke to Newton.—See this volume, p. 206, and Edleston's *Correspondence*, &c., Lett. xxi. p. 36.

[2] Had Mrs. Barton lived with Halifax, Swift, "who loved her better than any body in London," would not have been teased by the invitation.

[3] The wife of Sir Robert Worsley, Bart., and only daughter of Viscount Weymouth.

changed my company and manner of living? I never go to a coffee-house; you hear no more of Addison, Steele, Henley, Lady Lucy, Mrs. Finch, Lord Somers, Lord Halifax, &c."

"1711, *July* 6.—An ugly rainy day; I was to visit Mrs. Barton."

"1711.—*July* 18.—To-day I took leave of Mrs. Barton, who is going into the country."

"1711, *Oct.* 9.—I lodge, or shall lodge by Leicester Fields. . . . Did I tell you that my friend Mrs. Barton has a brother drowned, that went on the expedition with Jack Hill? He was a Lieutenant-Colonel, and a coxcomb; and she keeps her chamber in form, and the servants say she receives no messages."

"1711, *Oct.* 14.—I sat this evening with Mrs. Barton; it is the first day of her seeing company; but I made her merry enough, and we were three hours disputing upon Whig and Tory. She grieved for her brother only for form, and he was a sad dog."

"1711, *Oct.* 25.—I sat this evening with Mrs. Barton, who is *my near neighbour*."[1]

"1711, *Nov.* 20.—I have been so teased with Whiggish discourse by Mrs. Barton and Lady Betty Germaine,[2] never saw the like. They turn all this affair of pope-burning into ridicule, and indeed they have made too great clutter about it, if they had no real reason to apprehend some tumults."

"1711, *Nov.* 28.—I am turned out of my lodging by my landlady, but I have taken another lodging hard by in Leicester Fields."

"1711, *Dec.* 16.—I took courage to-day, and went to Court with a very cheerful countenance. It was mightily crowded; both parties coming to observe each other's faces. I avoided Lord Halifax's bow till he forced it on me, but we did not talk together."

After reading the preceding passages, it would be difficult to understand how Mrs. Barton, whom Swift esteemed and loved, could have ever resided under the roof of Lord Halifax as his mistress.

The following letter[3] endorsed by Swift, "My old friend Mrs. Barton, now Mrs. Conduitt," is the only one of hers that has been preserved:—

"GEORGE STREET, *November* 29, 1733.

"SIR,

"Mrs. Barber did not deliver your letter till after the intended wedding brought me hither. She has as much a better title to the favour of her sex than poetry can give her, as truth is better than fiction, and shall have my best assistance. But the town has been so long invited into the subscription, that most people have already refused or accepted, and Mr. Conduitt has long since done the latter. I should have guessed your holiness would rather have laid than called up the ghost of my departed friendship, which since you are brave enough to face, you will find divested of every terror,

[1] Mrs. Barton lived with Newton in Martin Street, Leicester Fields.

[2] Professor De Morgan says that Mrs. Barton's intimacy with Swift was probably through Halifax. It was more probably through Lady Betty Germaine, whom Swift had known from her childhood. Lady Betty was a daughter of the Earl of Berkeley, to whom Swift had been chaplain and private Secretary. Many of her letters to Swift are published in his *Correspondence*.

[3] Swift's *Works*, vol. xvii. p. 101. Edit. Edin. 1784.

but the remorse that you were abandoned to be an alien to your friends, your country, and yourself. Not to renew an acquaintance with one who can twenty years after remember a bare intention to serve him, would be to throw away a prize I am not now able to repurchase; therefore, when you return to England, I shall try to excel in, what I am very sorry you want, a nurse. In the mean time I am exercising that gift to preserve one who is your devoted admirer.

"Lord Harvey has written a bitter copy of verses upon Dr. Sherwin, for publishing, as 'tis said, his Lordship's epistle, which must set your brother Pope's spirits all a working. Thomson is far advanced in a poem of 2000 lines, deducing liberty from the patriarchs to the present time, which, if we may judge from the press, is now in full vigour. But I forget I am writing to one who has the power of the keys of Parnassus, and that the only merit my letter can have is brevity. Please therefore to place the profit I had in your long one to your fund of charity, which carries no interest, and to add to your prayers and good wishes now and then a line to

"Sir, your obedient humble servant,

"C. CONDUIT.

"Mrs. Barber, whom I had sent to dine with us, is in bed with the gout, and has not yet sent me her proposals."[1]

No. XIX.

(*Referred to in page* 227.)

1.—LETTER FROM VARIGNON TO NEWTON.

"Nobilissimo Doctissimoque Viro
"D. D. Newtono Equiti Aurato
"Regiæ Societatis Anglicanæ Præsidi Dignissimo
"S. P. D. Petrus Varignonius.

"Exoptatissimam mihi Effigiem tui, quâ me donare dignatus es, vir humanissime ac munificentissime, gaudenti gratissimoque animo nuper accepi. Tui spectandi percupidus capsam statim distraxi, evòlutâque telâ, in hujus effigiei vultu et fronte et oculis quasi spirans mihi visum est tuum summum atque eminens ingenium cum oris dignitate conjunctum, etiamnumque videtur. Paucis post diebus venit ad me Cl. Taylorus (quatuor abhinc vel quinque mensibus hic habitans) qui eam intuitus attente, suo usus conspicillo, tibi simillimam esse pro certo mihi affirmavit; quod admodum me delectavit ac delectat. Porro sculptam alteram tui imaginem,

[1] Mrs. Barber was a great friend and favourite of Swift. She was the author of a volume of poems, which were dedicated to the Earl of Orrery, and the proposals here referred to, were probably proposals to publish her poems by subscription.—See Swift's *Works*, vol. xvii. p. 77, and vol. xviii. p. 55.

jam inde a decem circiter annis habebam ex dono amici Angli (Oxoniensis nomine Arnold) qui cum me sæpius de te magnalia loquentem audisset, reversus Londinum, illinc eam ad me misit, pergratam mihi fore existimans: recte quidem. Sed cum Sculpta tui similitudinem ex vero non effingat æque ac picta, hanc nihilominus semper exoptavi, quâ nunc mihi datur videre tandem illustrissimum ac doctissimum eum virum quem amplius triginta annos summâ veneratione colebam ob ingentia ejus merita presertim in Mathesim quam promovit et auxit immensum, cujusque legibus astrictam primus demonstravit esse Naturam. Quantas autem pro tanto dono (quod antea pecuniæ summâ quâvis emissem si aliunde quam a perhonorifica mihi tuæ liberalitatis magnificentiâ obtinere potuissem) gratias agere tibi debeam, optime intelligo et intime sentio. Sed tantas ut eas expedire verbis nequeam; nec etiam eas quas habeo tibi maximas pro eo quod me monuit Cl. Moivræus te non dedignari mei quoque imaginem quam nudiusquartus idcirco misi D^o^ Ayres (capellano D. D. Equitis Sutton, excellentissimi legati vestri apud nos) in longiore capsula volutatam, quam pridie mihi officiosissime promiserat se missurum fore Londinum ad D^um^ Preverau (apud D. D. Craggs Sanctioris consilii Anglicani commentariensem) ut eam tibi reddat, quam benigne accipias rogo. Vale, mihique tuorum in me Beneficiorum æternum memori favere perge.

"Dabam Parisiis, Die 28th Novemb. 1720, N. S.

"P.S.—Post Scriptam hanc Epistolam D. Nicole ex Angliâ recens me invisit ac monuit, dum apud te pranderet, aut cœnaret, propinasse te toti generatim academiæ nostræ Parisiensi, speciatimque Cl. Fontenelle, ac etiam mihi; pro quo honore novas habeo tibi gratias et ago maximas. Contemplatus etiam D^us^ Nicole pictam effigiem tui, de eâ censuit penitus idem ac D. Taylorus nimirum eam tibi persimilem esse; quod meum de eâ obtentâ gaudium auxit."

2.—Letter from Newton to Varignon.

"Viro celeberrimo D^no^ Abbati Varignon Regio Mathesis Professori et Academiæ Scientiarum Socio apud Parisienses
Is. Newtonus S. P. D.

"Clarissime D^ne^,

"Accepi Historiam et Commentaria ex Archivis Academiæ Scientiarum pro anno 1719, pro quibus gratias tibi reddo quammaximas. Accepi etiam schedam primam Libri de Coloribus elegantem sane et specie nobilem. Et ne D^nus^ Montalanus expensa moleste habeat dabo illi libras viginti sterlingas, et expensa compingendi libros insuper solvam. Gratias tibbi reddo quamplurimas quod insinuasti libros plures amicis donandos esse, scilicet Cardinali Polignac, et filio Cancellarii, et Bibliothecæ Academiæ. Vellem et alios donandos esse filio et nepoti D. Joannis Bernoullii, et alios Abbati de Comitibus,[1] et P. Sebastian, et D. Remond. Sed et gratias tibi maxi-

[1] The Abbé Conti. Newton must have forgotten or forgiven the offence which he had taken at the Abbé, for having "assisted Leibnitz in engaging him in new disputes."

mas reddo quod onus in te suscipere digneris conferendi correctiones Dni Coste et Dni Moyvre inter se, et quod optimum videbitur eligendi; ut et emendandi quæcunque alia occurrerint. Metuebam utique ne correctiones Dni Coste, inter plurima tua negotia, molestiam nimiam tibi crearent. Sed cum hocce onus in te suscipere non dedigneris, eo magis me tibi obligasti. Schema tuum libris singulis præfigendum probo, sed nondum a Pictore delineatum est. Pictorem mox adibo.

"In sententia mathematici Judicis quam D. Leibnitius D. Joanni Bernoullio ascripsit, publice accusor plagii. Et epistola quam D. Bernoullius ad me misit, et qua se talem sententiam scripsisse negavit, videbatur ad me missa ut remedium contra injuriam illam publicam: et eo nomine licentiam mihi datam esse putabam diluendi injuriam illam auctoritate De Bernoullii, præsertim cum is me non prohibuerit. Attamen Epistolam illam non nisi privatim communicavi, et Keilio nullam dedi licentiam aliquid evulgandi ex eadem, et multo minus scribendi contra Bernoullium ob ea quæ in Epistola illa mihi amice scripserat. Et hac de causa Keilium quasi liti studentem vehementer objurgavi: sed ille jam mortuus est.[1]

"Conqueritur D. Bernoullius quod ipsum vocavi *hominem novum*, et *mathematicum fictum*, et *Equitem erraticum.* Sed contra Bernoullium nondum cœpi scribere. Hæc omnia dixi scribendo contra Leibnitium, et ejus argumenta repellendo.

"1. Dixerat utique D. Leibnitius *Keilium esse hominem novum et rerum anteactarum parum peritum cognitorem*, id est, hominem qui floruit post tempora Commercii quod Leibnitius habuit cum Oldenburgio: et idem objeci Leibnitio Bernoullium judicem constituenti, cui utique commercium illud antiquum annis plus triginta post mortem Oldenburgii ignotum fuerat.

"2. Cum D. Leibnitius sententiam Judicis mathematici Bernoullio ascriberet, vocavi judicem illum *mathematicum* vel *fictum mathematicum*, id est, mathematicum qui vere author esset sententiæ illius, vel fingebatur esse author. Nam cum Bernoullius ab authore sententiæ illius citabatur tanquam ab authore aversus, dubitabam utrum ille author esset, necne. Et Bernoullius ipse literis ad me datis affirmavit se non fuisse authorem.

"3. D. Leibnitius in Epistola sua prima ad Abbatem de Comitibus quæstionem de primo methodi differentialis inventore deseruit, et ad disputationes novas confugit de gravitate universali et qualitatibus occultis et miraculis et vacuo et atomis et spatio et tempore et perfectione mundi: Et sub finem Epistolæ Problema Bernoullii ex Actis Eruditorum desumptum proposuit mathematicis Anglis: Et initio proximæ suæ ad abbatem Epistolæ contulit hanc novam controversiam cum *duello*, scribens se nolle in arenam descendere contra milites meos emissarios, sed cum ipse appare-

See pp. 239, 240, and APPENDIX No. I. p. 347. The conduct of the Abbé in reference to his Chronology appears to have revived the former feelings of Newton.

[1] John Keill was born in Edinburgh in 1671, and studied mathematics there under David Gregory, whom he accompanied to Oxford in 1694, having obtained one of the Scotch Exhibitions in Balliol College. He acquired a high reputation at Oxford as a teacher of the Newtonian philosophy, with apparatus provided by himself. His *Introductio ad Veram Physicam* appeared in 1701, and his *Introductio ad Veram Astronomiam* in 1708. He was appointed Savilian Professor of Astronomy at Oxford in 1710, and in 1711 he entered the lists against Leibnitz and Bernoulli, as the able and staunch champion of Newton, as will be seen in Chapters XIV. and XV. of this work. He died in 1721, in the 50th year of his age.

rem, se lubenter mihi satisfactionem daturum. Et ad hæc omnia alludens non contra Bernoullium sed contra Leibnitium scripsi in observationibus meis in hanc ejus Epistolam, ubi dixi quod *Epistolæ et chartæ antiquæ* (ex mente Leibnitii scilicet) *jam abjiciendæ sunt, et Quæstio* (de primo methodi inventore) *deducenda est ad rixam circa Philosophiam et circa res alias: et magnus ille Mathematicus quem D. Leibnitius judicem sine nomine constituit, jam velum detrahere debet* (secundum Leibnitium scilicet) *et a partibus Leibnitii stare in hac rixa, et chartam provocatoriam ad mathematicos in Anglia per Leibnitium mittere quasi duellum, vel potius bellum, inter milites meos emissarios* (uti loquitur) *et exercitum discipulorum in quibus se felicem jactat; methodus esset magis idonea ad Quæstionem de primo inventore dirimendam quam examinatio veterum et authenticorum scriptorum, et scientiæ mathematicæ imposterum factis nobilibus equitum erraticorum vice argumentorum ac Demonstrationum implendæ essent.*

"Hoc totum contra Leibnitium scripsi, et non contra Bernoullium. Leibnitius Bernoullium constituit judicem. Leibnitius eundem ex judice constituit advocatum. Leibnitius Commercium Epistolicum fugit quasi a judice suo condemnatum. Leibnitius vice Quæstionis de primo Inventore disputationes novas de Quæstionibus Philosophicis proposuit, et Problema tanquam a Bernoullio misit a Mathematicis Anglis solvendum. Leibnitius fuit eques ille erraticus qui vice argumentorum ex veteribus et authenticis scriptis desumendorum, introduxit alias disputationes, quas ipse contulit cum duello. Ad hoc duellum ille me provocavit methodi infinitesimalis gratia. Hæc methodus erat virgo illa pulchra pro qua eques noster pugnabat. Quæstionem de primo methodi hujus inventore per victoriam in hoc duello dirimere sperabat, et Virginem lucrari non examinatis veteribus et authenticis scriptis in Commercio Epistolico editis, per quæ Quæstio illa dirimi debuisset. Problemata mathematica proponi possunt exercitii gratia, sed non ad dirimendas lites alterius generis: et solus Leibnitius eadem in hunc finem proposuit.

"Hæc tibi scripsi non ut in lucem edantur, sed ut scias me nondum cum Bernoullio lites habuisse. Contra illum nondum scripsi, neque in animo habeo ut scribam: nam lites semper fugi.

"Ds. Moivreus mihi dixit D. Bernoullium picturam meam optare: sed ille nondum agnovit publice me methodum fluxionum et momentorum habuisse anno 1672, uti conceditur in Elogio D. Leibnitii in Historia Academiæ vestræ edito. Ille nondum agnovit me in Propositione prima Libri de Quadraturis, anno 1693 a Wallisio edita, et anno 1686 in Lem. 2 Lib. 2. Princip. synthetice demonstrata, Regulam veram differendi differentialia dedisse, et Regulam illam anno 1672 habuisse, per quam utique curvaturas curvarum tunc determinabam. Ille nondum agnovit me anno 1669, quando scripsi Analysin per series, methodum habuisse quadrandi curvilineas accurate, si fieri possit, quemadmodum in Epistola mea 24 Octob. 1676, ad Oldenburgium data, et in Propositione quinta Libri de Quadraturis, exponitur; et Tabulas Curvilinearum quæ cum Conicis Sectionibus comparari possunt per ea tempora a me compositos fuisse. Si ea concesserit, quæ lites prorsus amovebunt, picturam meam haud facile negabo. Vale.

"DABAM LONDINI,
26 Sept. 1721. St. Vet."

Varignon, in replying to this letter on the 9th December 1721, N.S., acknowledges having received it by the hands of M. Arlaud,[1] "qui gratissime mihi de te narravit, et cum quo ad multam usque noctem honorificentissime de te sum collocutus." Then follows the paragraph relating to Bernoulli, which we have already given in page 228, and the letter concludes with some details respecting the frontispiece and diagrams for the French edition of his *Optics*, then publishing under the superintendence of Varignon.

No. XX.

(*Referred to in pages* 18, 231.)

1.—Letter from John Bernoulli to Newton.

"Viro Illustrissimo atque Incomparabili Isaaco Newtono S.P.D. Johannes Bernoulli.

"Opticam tuam Angl. à te mihi dono datam nuper accepi missu Celeb. Varignoni, à quo etiam exemplar Lat., sicut intelligo, accepturus sum. Pro utroque hoc egregio munere non minus quam pro aliis jam sæpius mihi acceptis tanquam totidem tuæ erga me benevolentiæ signis nunc demum debitas persolvo gratias, quas, quod fateor, dudum persolvere debuissem. Noli, quæso, officii hujus neglecti causam imputare animo ingrato et beneficiorum immemori, à quo semper quam maxime abhorrui; noli etiam credere, me ideo minus ingentia tua merita coluisse. Quin potius, si quid fidei verba mea merentur, id tantum ex silentio meo colligas velim, quod te divini ingenii virum, cui parem non habet ætas nostra, ego præ summa veneratione compellare non audebam; certe ne nunc quidem auderem, nisi nuper, quod animum addidit, intellexissem, juxta stupendas ingenii dotes etiam comitatis et affabilitatis virtutem usque adeo esse tibi connatam, ut ab inferioris conditionis hominibus, qualem me lubens profiteor, litteras accipere plane non detrectes. Cæterum quanti æstimaverim tuam amicitiam, qua, uti percepi ex litteris virorum clariss. Monmortii et Moivrei, me antehac dignatus es, eosdem hos viros antestor, ac præsertim quidem Moivreum, qui ea de re luculentissimum testimonium coram perhibere poterit. Sed nescio quî factum, ut post accensam facem feralis illius belli, quod maximo scientiæ Mathematicæ probro ante aliquot annos exortum inter quosdam utriusque nationis Britannicæ et Germanicæ Geometras, ego nec Britannus nec Germanus sed Helvetius, qui à partium studio alienissimus sum, et quidvis potius facerem, quam aliorum litibus me sponte immiscere, gratia tamen tua, ut fama fert, exciderim. Quod si ita esset, quamvis contrarium sperem, non possem non credere, hocce infortunium fuisse mihi conflatum à supplantatione quorundam sycophantarum, qui ex rabida quadam aviditate sibi suisque popularibus ædificandi

[1] M. Arlaud, an eminent Swiss painter, who resided in Paris, and improved some of the diagrams for Coste's French translation of Newton's *Optics*, which appeared in 1722.—See Edleston's *Correspondence*, &c., p. 88.

monumenta ex ruderibus destructæ aliorum existimationis et famæ, nos omnes non-Anglos insontes cum sontibus, nî statim per omnia applaudere velimus, acerbissimis contumeliis proscindunt. Itaque non dubito quin tibi, vir maxime, de me quoque multa falsa et conficta fuerint narrata, quæ gratiam, qua apud te flagravi, si non delere, saltem imminuere potuerunt. Sed non est ut multis me excusem: provoco ad scripta mea quæ extant; docebunt quam singulari cum laude de te tuisque inventis, quavis data occasione, locutus fuerim. Ecquis aliter posset, qui magnitudinem meritorum tuorum considerat? Quam mirabundus autem etiamnum illa deprædicem atque extollam quovis loco et tempore, privatim æque ac publice, in litteris, in sermonibus, in orationibus, in prælectionibus, illos loqui sinam qui me legunt, qui me audiunt. Sane si quid sapio, gratior erit posteritati commemoratio meritissimæ tuæ laudis à nobis instituta, utpote ex sincero animo et calamo profecta, quam nonnullorum ex vestratibus immodicus ardor (non dico te laudandi, nam satis laudari non potes, sed) tibi omnia, etiam ea quæ ipse non desideras, arrogandi, et exteris relinquendi nihil. Fallunt haud dubie, qui me tibi detulerunt tanquam auctorem quarundam ex schedis istis volantibus, in quibus forsan non satis honorifica tui fit mentio. Sed obsecro te, vir inclyte, atque per omnia humanitatis sacra obtestor, ut tibi certo persuadeas, quicquid hoc modo sine nomine in lucem prodierit, id mihi falso imputari. Non enim mihi est in more positum, talia protrudere anonyma quæ pro meis agnoscere nec vellem nec auderem. At vero non sine dolore audivi, te in quibusdam Epistolis libro (quem non vidi) cl. Raphsoni annexis ita de me loqui, ut inde concludi possit, quod me suspiceris auctorem nescio cujus scripti sine nomine publicati, et quod suspicio ista tibi subnata sit ex litteris quibusdam Leibnitii, qui me Auctorem esse affirmaverit. Quale fuerit illud scriptum jam non inquiro, interim certum te volo, à me non esse profectum, si præsertim tibi, quem tanti facio, non usquequaque esset decorum; absit autem ut credam Leibnitium, virum sane optimum, me nominando fucum vobis facere voluisse; credibile namque potius est ipsum vel sua vel aliorum conjectura fuisse deceptum; qua in re etsi data opera me offendere noluerit, non tamen omni culpa vacabat, quod tam temere et imprudenter aliquid perscripserit, cujus nullam habebat notitiam; fecisset utique melius, si antea ex me ipso quid de re esset rescivisset. Sed festinabat vir bonus, existimans forsan, causam suam aliquid inde roboris accepturam, parum sollicitus, utrum mihi incommoda necne futura esset conjecturalis illa relatio. Sed tandem absolvo, hoc unum maxime in votis habens, ut, nullo relicto dubio, tibi liquidissime constet animi in te mei integritas atque candor conjunctus cum perpetua tui admiratione atque veneratione, ut constet quoque me grata et memori mente usque et usque recolere quæ in me contulisti favoris et amoris signa. Non enim sum nescius, quantum tibi debeam non solum pro splendidis Librorum tuorum muneribus, quibus me subinde mactasti, sed et pro honorifica mei in vestram Societatem Reg. Scient. receptione, quippe quam ex tua commendatione mihi contigisse omnino perspectum habeo. Quod superest, Vale, Vir Illustrissime, atque mihi immortalium tuorum meritorum cultori studiosissimo fave. Dabam Basileæ, a. d. iii. Non. Quintil.[1] CIↃIↃCCXIX.

[1] July 5th.

2.—Letter from John Bernoulli to Newton.

"Prænobili ac Toto Orbe Celeberrimo Viro D. Isaaco Newtono S. P. D. Joh. Bernoulli.

"Litteræ Tuæ insigni voluptate me affecerunt, Vir Illustrissime. Ex iis intellexi Te, neglectis litibus mathematicis, eadem me prosequi benevolentia et amicitia, qua me olim dignatus fueras. Facis certe prout decet virum candidum et generosum, qui non facile patitur sibi eos designari, quos amore suo dignos judicat. Qualis sit epistola illa, de qua dicis quod sit 7 Junii 1713 data ad D. Leibnitium, mihi non constat. Non memini ad illum eo die me scripsisse, non tamen omnino negaverim, quandoquidem non omnium epistolarum à me scriptarum apographa retinui. Quodsi fortassis inter innumeras quas ipsi exaraveram una reperiretur, quæ dictum diem et annum præ se ferret, pro certo asseverare ausim, nihil in ea contineri quod probitatis nomen tuum ullo modo convellat, neque me unquam ipsi veniam dedisse, ut quasdam ex Epistolis meis in publicum ederet, et talem imprimis quæ tibi, etsi contra spem et voluntatem meam, non arrideret. Quocirca denuo te rogo, vir illustr., velis tibi persuasum habere, mentem mihi nunquam fuisse aliter de te loqui quam de viro summo, nedum existimationem tuam vel probitatem sugillare. Absit ut dicam, te famam apud exteras Gentes captasse; spero tamen, te non respuere elogia à nobis ultro oblata, utpote sincera et te digna, atque adeo magis acceptanda quam quæ ex immoderato partium studio offeruntur. Quod tibi jam seni (cui incolumitatem per novum quem propediem auspicabimur annum et per multos secuturos ex animo apprecor) non liceat studiis mathematicis incumbere, acerbe dolebit Orbis eruditus, quem hucusque ditasti tot stupendis inventis. Ego quidem nondum senex, ad senium tamen vergo, aliisque distringor negotiis, ut nec mihi amplius fas sit rei mathematicæ tam sedulam operam dare, uti solebam. Quod memoras, vir Amplissime, de libro Raphsoni, eum scil. iterum impressum esse cum nonnullis Leibnitii epistolis, in quibus affirmet, me Auctorem esse prædictæ epistolæ (quæ quid contineat probitati tuæ injurium hariolari non possum), hoc certe liti sopiendæ non conducit; ipsum vero librum Raphsoni fortasse nunquam videbo, quia ejusmodi libri ex Hollandia huc raro deferuntur. Hoc interim considerari à vestratibus vellem, si per testimonia certandum esset, melius id fieri adducendo alias epistolas quam a Leibnitio scriptas, quippe qui in propria causa non haberi potest pro idoneo teste. Sunt mihi epistolæ virorum quorundam doctorum ex nationibus nullam in hac lite nationali partem habentibus, quas si publici juris facerem, nescio an illi ex vestratibus, qui tanto cum fervore ad injurias usque mecum expostulant, magnam inde gloriandi causam acquirerent. Habeo inter alia documenta authentica apographum à D. Montmortio nuper defuncto mathematico, ut nosti, dum viveret perdocto atque nulli parti addicto, utpote Gallo; habeo, inquam, apographum ab eo mihi transmissum alicujus epistolæ, quam ipse ad cl. Taylorum scripserat 18 Decemb. 1718, et quæ vel sola magnam litis partem dirimeret, sed non ex voto Taylori cæterorumque ejus sequacium.[1] Ab istis autem evulgandis libenter abstinebo, modo vestri desinant, quod pacis causa optarem, nostram lacessere patientiam. Lubens credo quod

[1] This letter will be found in p. 398, sect. 3.

ais de aucto Corollario 1, Prop. xiii. Lib. 1. Operis tui incomparabilis Princip. Phil., hoc nempe factum esse antequam hæ lites cœperunt, neque dubitavi unquam, tibi esse demonstrationem propositionis inversæ quam nude asserueras in prima Operis Editione; aliquid dicebam tantum contra formam illius asserti, atque optabam, ut quis analysin daret qua inversæ veritatem inveniret à priori, ac non supposita directa jam cognita. Hoc vero, quod te non invito dixerim, à me primo præstitum esse puto, quantum saltem hactenus mihi constat. Unum superest, quod pace tua monendum habeo. Retulit mihi nuperrime Amicus quispiam ex Anglia redux, me esse ejectum ex numero Sodalium Illustr. vestræ Societ. Reg., id quod collegerit ex eo quod nomen meum non repererit in Catalogo Londini viso Sociorum (in ampla scheda annuatim imprimi solita) pro anno 1718. Et quominus dubitarem monstravit mihi librum aliquem Anglicum impressum an. 1718, cui titulus *Magnæ Britanniæ Notitia*, ubi in parte postrema pag. 144 videre est Catalogum Membrorum exterorum Societatis Regiæ, atque in illo nomen Agnati mei, sed meo nomine, quod miror, prorsus exulante. Liceat ergo ex te quærere, utrum ex decreto Illustr. Societatis fuerim expunctus, et quid peccaverim vel quonam delicto ejus indignationem in me concitaverim, an vero Secretarius (qui nî fallor tum temporis fuit Taylorus) propria auctoritate me proscripserit. Quid? ideone locum in illustri hoc corpore mihi non ambienti tam honorifice obtulissetis, ut postea tanto turpius ex eo me ejiceretis? Hoc equidem ob insignem vestram æquitatem suspicari vix possum. Quare enixe te rogo, vir Nobilissime, ut quid ea de re sit me quantocyus facias certiorem. Vale, ac mihi studiosissimo tui porro fave. Dabam Basileæ, a d. xxi. Decemb. CIƆIƆCCXIX.

3.—LETTER FROM JOHN BERNOULLI TO NEWTON.

"Illustrissimo atque Nobilissimo Viro Isaaco Newtono S. P. D. Joh. Bernoulli.

"Ad te iterum venio, Vir Inclyte, ut iteratas persolvam gratias pro novo munere quo me beasti, nec me tantum, sed et filium meum atque Agnatum. Accepi nimirum tria inter nos tres distribuenda Exemplaria nitidissime compacta Optices tuæ Parisiis nuper editæ, quæ Cl. Varignonius, paulo ante obitum suum, cunctis qui sinceritatem cum eruditione conjunctam amant vehementer lugendum, mihi nomine tuo transmiserat. Etsi nesciam quid sit quo hanc tuam erga me meosque munificentiam demeruerim aut postea demereri possim; id saltem persuasum tibi habeas, vir maxime, neminem esse qui immortalia tua inventa ex vero rerum pretio pluris æstimet et simul sincerius quam ego. Hoc cumprimis quod de Lumine et Coloribus systema pro ingenii tui sagacitate felicissime eruisti me summum habet admiratorem; inventum sane quovis ære perennius et à posteritate magis quam nunc fit suspiciendum. Sunt enim qui illud partim ex invidia partim ex imperitia obtrectare non verentur, quin et cum nihil habent quod pretium ejus imminuat, audent inventionis laudem tibi surripere eamque sibi arrogare. En exemplum in quodam Hartsoekero, homine inepto et in Geometria prorsus hospite, qui in opusculo aliquo in lucem protruso perfricta fronte sustinet novam tuam Colorum theoriam eorumque diversam refrangibilitatem sibi dudum notam variisque experimentis perspectam fuisse, antequam quicquam ea de re inventum à te aut evulgatum fuisset, quod apud me summam excitavit indignationem, sicut et hoc quod reliquas tuas rerum Physicarum explicationes utut ingeniosissimas, præsertim quæ ad systema planetarium spectant, ubi omnia cum phænomenis tam mirifice consentiunt, admodum salse et scoptice traducit, quamvis de rebus istis non aliter argumentetur quam cæcus de colore, nec mirum, siquidem homo sit ἀγεωμέτρητος et omnis humanitatis expers, nemini parcens, imo summorum virorum atque de re mathematica ac philosophica optime meritorum famam arrodere non dubitans. Ita ut mirer, neminem ex vestratibus adesse, qui tuam, Vir Illustrissime, existimationem vindicet contra rudem et barbarum hominem. Ad me quod attinet, fateor me ab illo tractari multo acerbius; nihil enim injuriarum est, nihil aculeorum quod in me non sparserit, idque non aliam ob causam quam quod aliquis ex meis discipulis phosphorum meum mercurialem ab illius morsibus defenderit. Licet indignus sit homo cui ego respondeam, unum tamen est quod me magnopere urit; scilicet, ut me omnium risui exponat, impudentissime comminiscitur, me mihimet ipsi tribuisse titulum *excellentis Mathematici*, et, ut calumniæ crimen à se amoliatur, te, Vir Illustrissime, ejus Auctorem facit, dum locum citat ex tomo 2. Collectaneorum D[r]. Desmaiseaux (*Recueil de diverses pièces*), p. 125, l. 32, ubi loqueris de epistola illa 7 Junij 1713, quam Leibnitius à me scriptam esse contenderat, et in qua prout erat impressa in scheda illa volante 29 Julij 1713, elogium illud, sed quod parenthesi includebatur, mihi erat adscriptum. Hinc malitiose colligit calumniator, quasi insinuare volueris, me eò arrogantiæ processisse ut hunc mihi titulum sumserim, cum tamen te voluisse contrarium dicere luculentissime pateat ex verbis quæ locum citatum immediate sequuntur, quibus nimirum fateris in eadem illa epistola per Leibnitium altera vice edita in Novis Litterariis citationem parenthesi inclusam esse omissam; unde sponte fluit Auctorem epistolæ non fuisse Auctorem parentheseos, sed hanc fuisse insertam ab eo qui schedam volantem 29 Julij edidit; possum itaque haberi pro Auctore Epistolæ, et tamen non haberi pro Scriptore elogii parenthetici. Interim quicquid sit, calumnia Hartsoekeri in te magis quam in me redundat, eam enim ex verbis tuis maligne detortis elicere conatur. Quid igitur faciendum statuas, ut innocentia in tutum collocetur apud eos qui Collectanea Desmaisavii non viderunt, libentissime equidem ex te ipso intelligerem, si qua responsione me dignari volueris. Quod superest, te, Vir Nobiliss., rogatum volo nomine Celeb. nostri Scheuchzeri, vestræ Societatis Regiæ Socii, ut Filio ipsius, qui nunc Londini agit, te accendendi alloquendique copiam indulgeas; id namque in maxima laude sibi ponet, quod viderit summum Philosophorum et Mathematicorum Principem. Vale, et me Nominis tui Cultorem perpetuum amare perge. Dabam Basileæ, a. d. vi. Feb[r]. CIƆIƆCCXXIII.

No. XXI.

(*Referred to in page* 233.)

Brook Taylor, LL.D., the son of John Taylor of Bifrons House in Kent, was born on the 18th August 1685, and died on the 29th December 1731.

In 1701 he was admitted into St. John's College, Cambridge, where he entered upon the study of mathematics and natural philosophy. In 1708 he wrote his treatise "On the Centre of Oscillation." In 1712 he was elected a fellow of the Royal Society, and in the same year he presented to that body his paper "On the Ascent of Water between two Glass Planes." His most important works—his *Methodus Incrementorum*, and his treatise *On the Principles of Linear Perspective*, were published in 1715. In the following year he paid a visit to Count Remond de Montmort and the Abbé Conti in Paris, with the first of whom he maintained a friendly correspondence. He was chosen Secretary to the Royal Society on the 13th January 1714, an office which he resigned on the 21st October 1718. In the following letters he appears as one of the champions of Newton in the fluxionary controversy.

1.—LETTER FROM BROOK TAYLOR TO SIR ISAAC NEWTON.

"SIR,

"The great loss to our family of my good dear mother, has made it necessary for me to make haste home, and I find the circumstances of our family will not suffer me to be in town before the rising of the Royal Society; wherefore I am under the necessity to beg the favour of you, Sir, to excuse me for not attending you in Crane Court, and that you will be pleased to get M. Desaguliers, or some other person, to do the Secretary's business at the meetings of the Society; and I hope I shall another time have an opportunity of making the Society some amends for my present absence.

"Upon my coming to London on Tuesday night, I found a letter from Mr. Montmort, dated the 31st March, N.S., wherein he gives me the following account of what passed at the French Academy relating to D. Keill's paper, which it seems they don't care to print.

"'Le plus grand nombre s'est opposé à faire imprimer le morceau de Mr. Keill dans les mémoires de l'Académie par la raison que Mr. Keill est étranger à l'Académie, et que cela est contre les Statuts. Je pris la parole, représentai 1° que le morceau est excellent; 2° que M. Newton est attaqué dans les mémoires par M. Nas Bernoulli qui non plus que Mr. Keill n'est pas Membre de L'Académie; 3° que s'il étoit jamais permis de faire exception à une règle générale, c'estoit en faveur d'un aussi grand homme que M. Newton. Je compte qu'il sera imprimé s'il est avoué et reconnu de Mr. Newton ou de Mr. Halley au nom de la Société Royale.'[1]

"These are Mr. Montmort's own words, which I thought it my duty to communicate to you, not knowing what sort of an account Mr. Fontenelle may have given in his letter to Dr. Halley.[2] Mr. Montmort, in all his letters to me, seems to take a particular pleasure in expressing the great respect he has for you, Sir; and in one of his last he tells me he has sent to me a hamper of champagne wine, and begs your acceptance of 50 bottles of it.[3] I can send it from hence either by land carriage or by water, if you

[1] This letter is published in the *Contemplatio Philosophica*, pp. 84-88.

[2] See Edleston's *Correspondence*, &c., p. 187.

[3] This is the wine mentioned in p. 385, as intended for Miss Barton.

will be pleased to let me know whither I shall direct it. I will send it as soon as it comes to my hand. Pray, Sir, do me the favour to make my most humble service acceptable to Mrs. Barton.—I am,

"Sir,

"Your most faithful and most obedient servant,

"BROOK TAYLOR.

"BIFRONS, near CANTERBURY,
22*d April* 1716.

"To Sir Is. NEWTON."

2.—EXTRACT OF A LETTER FROM M. MONTMORT TO BROOK TAYLOR, dated *Jan.* 22, 1717.[1]

"Pour moi je soutient icy et je l'ai toujours soutenu hautement que M. Newton a été maître du Calcul différentiel et intégral avant tout autre géomètre, et que dès l'année 1677 il sçavoit tout ce que les travaux de M. Leibnitz et M. Bernoulli ont découvert depuis."

3.—LETTER FROM M. MONTMORT TO BROOK TAYLOR.

"MONTMORT, *ce* 18 *Decr.* 1718.

"Je suis très persuadé, Monsieur, que vous n'avez point eu dessein de vous faire honneur de ce qui n'étoit point à vous, et de vous l'approprier ; outre que vous avez l'esprit et le cœur trop élevé pour être capable d'une telle petitesse, vous êtes trop riche de votre propre fond pour avoir besoin du bien d'autruy. Je crois que quand vous avez donné au public vostre excellent livre *Methodus Incrementorum*, vous étiez peu instruit de l'histoire des nouvelles découvertes. Je croirois même que vous ne l'estes pas assez à présent pour un homme destiné comme vous à jouer un grand rolle parmy les Sçavants de ce siècle. Les connaissances historiques inutiles à la vérité pour la perfection de l'esprit sont absolument nécessaires à un autheur qui faute de les avoir court risque de porter des jugemens injustes, de bâtir sur le fond d'autruy contre son intention, de mal apprécier le mérite des autheurs, et enfin de se tromper dans de faits dont un lecteur sévère suppose qu'on est instruit parcequ'on devroit l'être. En voici quelques uns dont il est apropos que vous ayez connaissance.

"Mr. Huygens est inventeur de la théorie de centres d'oscillations, et de percussion. M. Jacques Bernoulli l'a rendue plus claire, plus facile, et plus parfaite. Voyez les Mémoires de l'Académie en 1703 et 1704. M. J. Bernoulli ayant cru qu'on y pouvoit ajouter quelque chose, a donné en 1714 dans ces Mémoires un beau morceau sur cette matière. Je crois qu'il a donné un second dans les Actes de Leipsic. Je ne sçai quand, car je ne les ai pas ici. Il est vrai que Mrs Bernoulli ny M. Leibnitz n'ont point donné dans les journaux de Leipsic les analyses de la chaînette, de la courbure d'une voile enflée par le vent, et de celle que prend un linge pressé par le poid d'un fluide qu'il contient ; mais il me semble que les solutions qu'ils ont donnés de ces problêmes sont très justes. J'ai parmy

[1] This extract is published at the end of Keill's letter to Bernoulli.

mes vieux papiers des démonstrations de tout ce que M. Jac. Bernoulli a avancé en 1691 p. 288 de l'identité qu'il y a entre la chaînette et la courbe de la voile, et aussi entre la courbe du linge et l'élastique. Vous trouverez dans la nouvelle théorie de la manœuvre des vaisseaux publié en 1714 les analyses des courbes velaria, catenaria, lintea. Je n'adjouterai point que ces analyses couvrent depuis plus de 25 ans entre les mains de plusieurs géomètres de toutes nations ; à qui M. Jean Bernoulli a communiqué les leçons manuscriptes qu'il avoit fait, étant à Paris pour M. Le M. de l'Hôpital. Toutes ces analyses à l'exception de celle de la courbe élastique s'y trouvent. Je les ai vu dans un manuscript de ces leçons que le P. Reyneau tira in 1692 d'un ami de M. Bernoulli. Le fait est constant, et j'en suis temoin avec peut-être plus de cent personnes, mais je n'admets que les monuments publics telles qu'est l'impression.

"Il y avoit quelque chose a redire à ce que M. Jac. Bernoulli avoit donné en 1694 touchant la courbure des ressorts. Il a perfectionné cette matière dans les mémoires de l'Académie de l'année 1705. Je me souviens dans ce moment que l'analyse des chaînettes se trouve dans la solution que M. Jac. Bernoulli a donné en 1701 de son problême des Isopérimètres. Il est vray Mr. que la solution que Mr. Jean Bernoulli a donné en 1706 dans les mémoires de l'Académie du prob. des Isopérimètres n'est pas exempte de faute. Il a eu le bonheur de s'en appercevoir le 1er, et avant que d'estre relevé par d'autres. Vous en verrez une nouvelle et très belle solution dans les Actes de Leipsic au mois de Janvier de cette année. Sa méthode est fondé sur la considération de trois élémens contigus de la courbe au lieu qu'il n'en considéroit que deux dans celle qui a paru en 1706. Elle n'est presque point différente dans le fond de celle de Monsr. Herman qui ne me plait pas moins. Elles sont toutes deux entées sur celle de feu M. Bernoulli. Il la regarde comme son chef-d'œuvre : c'est un morceau d'une grande force, et qui me paroit surpasser en difficulté toutes les productions de ce jour. Je sçai bon gré au pauvre defaut d'avoir tenu ferme à soupçonner et dire qu'il y avoit faut et paralogisme dans l'analyse de son frère, et de n'avoir pas lâché ces 50 ecus qui n'étoient pas bien gagnés.

"Je ne sçai si vous sçavez que M. De la Hire en 1702 dans les Mémoires de l'Académie, et M. Herman dans les Journaux de Leipsic un peu de temps après, ont entrepris de déterminer la courbe que décrit un rayon de lumière passant dans notre Atmosphère. Je crois qu'il y a faute dans M. de la Hire. Je ne me souviens pas de ce qui m'a paru il y a quelques années de la solution de M. Herman ; vous en jugerez et de ce qu'ils disent sur la densité de l'Atmosphère.

"J'ay été fort surpris de trouver ce qui suit dans votre lettre. 'As to the owning of any one as inventor or improver of the method, besides Sir Isaac Newton, I knew of none. I saw nothing anywhere that seemed to me an improvement upon what Sir Isaac had published. I was sensible that several had applied the method with good success, and understood pretty much of it ; but I always took Sir Isaac Newton not only for the inventor, but also for the greatest master of it.' Je pense comme vous Mr. sur le mérite de Mr. Newton. Je parle toujours comme d'un homme au dessus des autres, et qu'on ne peut trop admirer. Mais je ne puis m'empêcher de combattre l'opinion où vous estes que le Public a reçu de Mr. Newton, et non de M. Leibnitz et Bernoulli les nouveaux calculs, et l'art de les faire servir à toutes les recherches qu'on peut faire en Géométrie. C'est une erreur de fait. Il vaut mieux que moi qui n'ay là dessus aucune prévention, ni rien qui me porte à en avoir, qui fait profession d'estre votre amis, et qui le suis plus sans comparaison que des Géomètres Allemands que je n'ai jamais vu ; il vaut mieux, dis je, que je vous fasse remarquer la fausseté qu'un adversaire à qui vous donneriez avantage sur vous et qui vous reprocheroit avec apparence de vérité que votre zèle pour la gloire de vostre nation vous rend partiel et vous fait oublier toutes les règles de l'équité. Je n'examinerai point ici les droits de Mrs. Newton et Leibnitz à la première invention du calcul différentiel et intégral. Je vous rapporterai quand vous voudriez le détail des réflexions qu'un long et sérieux examen m'a fourni, et j'espère que vous n'en serez pas mécontent. Je veux seulement vous faire remarquer qu'il est insoutenable de dire que Mrs. Leibnitz et Bernoulli ne sont pas les vrays et presque uniques promoteurs de ces calculs. Voici mon raisonnement, jugez en. Ce sont eux et eux seuls qui nous ont appris les règles de différentier et d'intégrer, la manière de trouver par ces calculs les tangentes des courbes, leur pointes d'inflexion et de rebroussement, leurs plus grandes, et leurs plus petites ordonnées, les développés les caustiques par réflexion, et par réfraction, les quadratures des courbes, les centres de gravité, ceux d'oscillation, et de percussion, les problêmes de la méthode inverse des tangentes, tels que celuycy par ex. qui donne tant d'admiration à M. Huygens en 1693 *trouver la courbe dont la tangente est à la partie intercepté de l'axe en raison donné.* Ce sont eux qui les premiers ont exprimé des courbes méchaniques par les équations différentielles, à en abaisser les dimensions, et à les construire par les logarithmes, ou par des rectifications des courbes quand cela est possible ; et qui enfin par de belles et nombreuses applications de ces calculs aux problêmes les plus difficiles de la Méchanique tels que sont ceux de la chaînette, de la voile, l'élastique, de la plus viste déscente, de la paracentrique, nous ont mis et nos neveux dans la voye des plus profondes découvertes. Ce sont là des faits sans réplique. Il suffit pour s'en convaincre d'ouvrir les journaux de Leipsic. Vous y verrez les preuves de ce que j'avance. Personne hors M. le M. de l'Hospital qu'on peut joindre en partie à ces Messieurs quoiqu'il ait été disciple de M. Jean Bernoulli, n'a paru avec eux sur la scène jusqu'en 1700 ou environs. Je compte pour rien ce que M. Carré en France et M. Moivre en Angleterre, de même M. Craige donnèrent dans ce temps ou peu auparavant ; tout cela n'étoit rien en comparaison de ce qu'on nous avoit donné dans les Actes de Leipsic. Il est vray Mr. que les Principes Math. de. M. Newton ont paru en 1686 [1687] ; ce sçavant ouvrage peut donner lieu de croire que M. Newton sçavoit dès-lors de ces calculs tout ce qu'on sçait aujourdhuy, M. Bernoulli même. Je ne veux pas disconvenir, et c'est une question à part. Mais il est sûr au moins que ce livre n'apprend rien de ces calculs, si ce n'est le lemme, 2d page 250, 1ère édit., mais vous sçavez qu'il ne contient que la 1ère et plus simple règle de prendre les différences, ce que M. Leibnitz avoit fait avec plus d'étendue en 1684. Je dois adjouter que dans le 2de volume de M. Wallis imprimé en 1693 on trouve plus au long les règles de ces calculs, mays quoyque ce morceau soit propre à nous donner une grande idée de ce qu'en sçavoit alors Mr. Newton il n'en apprend pas plus que l'on en trouvoit dans les journaux de Leipsic. On trouve en 1697 une solution de Mr. Newton du problême de la plus viste déscente, mais comme il n'y a point d'analyse, et qu'on ne sçait point la route qu'il a suivie, cela ne touche point à ma proposition qui est que depuis 1684, 1ère date publique de la naissance du calcul différentiel et intégral, jusqu'en 1700 ou environ, où je suppose qu'il avoit acquis presque toute la perfection qu'il a aujourdhuy, personne n'a contribué à le perfectionner que Mrs. Leibnitz et Bernoulli, à moins qu'on n'y veuille joindre pour quelque part M. le M. l'Hospital à qui ils avoient de bonne heure révélé leur secrets. Qui apparemment en seroient encore pour tous les Géomètres d'aujourdhuy s'ils avoient voulu les tenir cachés à l'imitation de Mr. Newton, qui à mon avis a du avoir la clef de ceux là ou des pareils dès le temps qu'il a donné son fameux ouvrage, *Ph. Nat. Ppia Math.* On ne peut rien le plus beau ni de meilleur en son genre que le traité de Mr. Newton *De Quadratura Curvarum*, mais il est venu bien tard. La date de l'impression de cet ouvrage est fâcheuse, non pour Mr. Newton, qui a acquis tant de gloire que l'homme le plus ambitieux n'en pourroit désirer davantage, mais pour quelques Anglois qui semblent porter envie à ceux qui ont découvert et publié les 1ers ces nouvelles méthodes qui ont portés si long la Géométrie."

No. XXII.

(*Referred to in page* 235.)

LETTER FROM JAMES STIRLING TO SIR ISAAC NEWTON.

"SIR,

"I had the honour of your letter about five weeks after the date. As your generosity is infinitely above my merite, so I reackon myself ever bound to serve you to the utmost; and, indeed, a present from a person of such worth is more valued by me than ten times the value from another. I humbly ask pardon for not returning my grateful acknowledgements before now. I wrote to Mr. Desaguliers to make my excuse, while, in the meantime, I intended to send a supplement to the papers I sent; but now I'm willing they be printed as they are, being at present taken up with my own affair here, wherewith I won't presume to trouble you, having sent Mr. Desaguliers a full account thereof.

"I beg leave to let you know, that Mr. Nicholas Bernoulli proposed to me to enquire into the curve which defines the resistances of a pendulum, when the resistance is proportional to the velocity. I enquired into some of the most easy cases, and found that the pendulum, in the lowest point, had no velocity, and consequently could perform but one half oscillation, and then rest. Bernoulli had found that before, as also one Count Ricato, which I understood after I communicated to Bernoulli what occurred to me. Then he asked me how in that hypothesis of resistance a pendulum could be said to oscillate, since it only fell to the lowest point of the cycloid, and then rested. So I conjecture that his uncle sets him on to see what he can pick out of your writings, that may any ways be cavilled against, for he has also been very busy in enquiring into some other parts of the Principles.

"I humbly beg pardon for this trouble, and pray God to prolong your daies, wishing that an opportunity should offer that I could demonstrate my gratefullness for the obligations you have been pleased to honour me with.

"I am, with the greatest respect, Sir,
"Your most humble and most obedient servt,
JAMES STIRLING.

"VENICE, 17 *August* 1719, N. St.

"P.S.—Mr. Nicholas Bernoulli, as he hath been accused by Dr. Keill of an ill-will towards you, wrote you a letter some time ago to clear himself. But having in return desired me to assure you, that what was printed in the *Acta Paris.* relating to your 10 Prop. lib. 2, was wrote before he had been in England, sent to his friends as his private opinion of the matter, and afterwards published without so much as his knowledge. He is willing to make a full vindication of himself as to that affair whenever you'll please to desire it. He has laid the whole matter open to me; and if things are as he informs me, Dr. Keill has been somewhat harsh in his case. For my part, I can witness that I never hear him mention your name without respect and honour. When he shewed me the *Acta Eruditorum*, where his uncle has lately wrote against Dr. Keill, he shewed me that the theorems there about quadratures are all corollarys from your Quadratures; and whereas Mr. John Bernoulli had said there, that it did not appear by your construction of the curve, Prop. 4, lib. 2, that the said construction could be reduced to logarithms, he presently shewed me Coroll. 2 of the said Proposition, where you shew how it is reduced to logarithms, and he said he wondered at his uncle's oversight. I find more modesty in him as to your affairs than could be expected from a young man, nephew to one who is now become head of Mr. Leibnitz's party; and, among the many conferences I've had with him, I declare never to have heard a disrespectful word from him of any of our country but Dr. Keill."

No. XXIII.

(*Referred to in page* 235.)

LETTER FROM FONTENELLE TO SIR ISAAC NEWTON.

"MONSIEUR,

"Je suis chargé par L'Académie Royale des Sciences d'avoir l'honneur de vous remercier de la nouvelle Edition que vous avez envoyée de vos Principes des Mathématiques de la Philosophie Naturelle. Il y a déjà plusieurs années que cet excellent ouvrage est admiré dans toute l'Europe, et principalement en France, où l'on sait bien connaître le mérite étranger.

Mais présentement, Monsieur, que vous avez une place dans notre Académie, nous prétendons, en quelque façon que vous n'êtes plus étranger pour nous, et nos Savants qui ont quelque droit de vous appeler leur Confrère prennent une part plus particulière à votre gloire. On peut sans témérité vous prédire qu'elle sera immortelle par les deux Livres que vous avez publiés, où il brille de toutes parts un si heureux génie de découvertes, et où ceux-même qui savent le plus trouvent tant à apprendre.

"L'Académie vous prie Monsieur de lui faire quelque fois part de vos nouvelles productions ainsi que font Messrs. Leibnitz, Bernoulli, et les autres Savants étrangers qu'elle a adoptés. Il n'est pas surprenant qu'elle cherche à se faire honneur de ce qu'elle vous possède.—Je suis,

"Monsieur,

"Votre très humble et très obéissant serviteur,

"FONTENELLE,

"Sec. Perp. de l'Ac. Roy. des Sc.

"à Paris, ce 4 Fév. 1714."

No. XXIV.

(*Referred to in page* 235.)

LETTER FROM DR. DERHAM TO SIR ISAAC NEWTON.

"UPMINSTER, ♀ *Feb.* 20, $171\frac{2}{3}$.

"MUCH HON^{D} S^{R},

"As I was perusing the Commerc. Epist. w^{ch} y^{e} R. S. honoured me with, it came into my mind, that in some of Mr. Collins's l^{rs} to Mr. Towneley of Lanc. (now in my hands), there was something relating to that subject; and looking over Mr. Towneley's papers, I found a long l^{r} of Mr. Collins's, giving a sort of historical account of the matter, That in Sept. . . . Mercator published his *Logar.*, one of w^{ch} he sent to Dr. Wallis, &c. . . . another to Mr. Barrow, who thereupon sent him up some papers of Mr. Newton (now his successor), by w^{ch}, and some other communications, &c., it appears y^{e} s^{d} method was invented some years before by Mr. Newton, and generally applied. . . . Then follows an account of your method, and of Mr. Gregorie's performance in y^{t} kind, with what Mr. Gregory had written to him about it in Feb. 1671, and Jan. 1672, &c.

"There is a great deal more, too long to speak of; but if you think the papers may be of use to you, at your request I will bring them w^{n} I next come to London, to be looked over or transcribed; but I am engaged not to part with any of them out of my power. I have also divers of Mr. Sluse's l^{rs}, and other papers of his, from Rome and Leige, to Mr. Towneley, but they being in French, I cannot as yet give any account whether there be any thing relating to your matter in them. Not meeting you when I was last in town, I shall take this occasion to acquaint you y^{t} I have tried Mr. Huygens's glass of 122 feet at ♄, ♀, ☾, and ♂, and some fixt ✱; and I hope shortly to have a view of ♃ also. I believe it by far the best long glass I ever looked through, representing these celestials very clear and well. But I can hardly think Mr. Huygens could see tollerably through it with the eye-glass accompanying it, wch is but 6 inches focus; I, therefore, make use of eye-glasses of a larger focus. I am not yet so well accommodated for strict observations with this glass as to tell you any thing of ♄ Sat., &c., for I am forced to raise a long ladder and send my man up with the glass. Neither have I a good eye-glass to my mind, only some spectacle-glasses. But would you, or some other of my friends that have interest enough, procure me a small Prebend, to enable me to be at charges without injuring my wife and children, I promise you I would stick at no charge to get an apparatus for this noble glass, to make it as serviceable to the R. S. as in me lies; and to accomplish some other matters also for their service. Be pleased to excuse my presumption thus upon your friendship and favour, wch I desire may be no otherwise troublesome to *you* then if any thing happens in your way, and you have no other friend capable of it, you would, for the service of the R. S. as well as myself, think of me, and at the same time pardon,

"Most Hond Sr,

"Your affectionately humble servant,

"Wm. Derham.

"If you have any commands, direct to Upminster, near Rumford, Essex, by the general post."

In another letter, which possesses no interest, dated May 11, 1714, he requests Newton to fulfil his promise of giving him his "castigations" for a third impression of his *Physico-Theology.*

No. XXV.

(*Referred to in pages* 242, 334.)

LETTER FROM POPE TO MR. CONDUITT.

"Sr,

"I make use of the liberty you gave me, of a free criticisme, in the inclosed; without any formalityes, or asking an excuse from you in my turn. I think nothing can be more proper than the first part of your dedication, which relates to the author of the work: Whatever thoughts flow from *that,* or take rise from *that,* render your compliment to the Queen (in my opinion) the more graceful as well as the more just (and proper for you as a relation, and intrusted with so valuable a depositum.) As to what depends not on *that,* I would only wish you avoided as much as possible the common topicks of dedications and addresses: Your real subject (I mean both Sir Isaac Newton and Her Majesty), will shine of themselves; and a shortness, a dignity, and plainness will become them. For instance I cannot but think, y[t] after y[u] have said, that *S[r] Isaac carried arts and sciences in a few years farther than all others had in whole ages;* it flattens if not contradicts it, to add afterw[ds] y[t] *in the present reign they may be advanced to a much greater height.* I w[d] omit that paragraph w[ch] I have marked between two crosses ×. It takes very much from the praise of S[r] I. N., and I fear unjustly, to imagine that any Prince's reign can *make* Newtons, however it might *incourage* or *admire* them.

"I mean in general only that I w[d] shorten those parts w[ch] are mere panegyric, independent on the occasion the book and author give you: the character of sincerity w[ch] y[u] so rightly touch upon in the King, I w[d] keep exactly as it is, and anything in short that is characteristical. I prefer (since your commands are, that I sh[d] chuse what I like) the column on the right hand: only in one place I think what you say of the Queen's encouragement of arts, is almost a repetition of the same thing elsewhere. I have marked it by inclosing y[t] passage with a line and two crosses × ×. The rest I believe may stand.

"Upon the whole I really approve it; and you ought to pardon my freedom, since you caused it. If I am ever so much in the wrong, it will be at least an instance of my good intention. I am ashamed to be so particular in things of so little importance as my objections, which are indeed so very slight. But the apprehension that you might soon want the papers, and the consciousness that I could not be serviceable enough to you to excuse a longer delay, made me write this, rather than wait for an opportunity of talking with you. Methinks y[u] should end the dedication with returning once more to Sir Isaac Newton. What little I've added, is only a hint to that effect. I am sincerely of opinion that your dedication is very just, and decent, and well judged. I c[d] wish it were inlarged with some Memoirs and Character of him, as a private man. I doubt not his life and manners w[d] make a great Discovery of Virtue and Goodness and Rectitude of heart, as his works have done of Penetration and y[e] utmost Stretch of human knowledge.—I am, Sir, your most obedient humble servant, "A. POPE.

"TWICKENHAM,
"*Nov[r].* 10[th], 1727."

The following are the additions referred to in the letter, and written upon a separate leaf:—

"Y[r] Majesty does not think these instructions and entertaining pursuits below your exalted station; and yourself a proof that the abstruser parts of them are not beyond y[e] reach of y[r] sex," &c.

"Formed by such models?

"That *liberty* and *knowledge* (as this glorious prospect gives us reason to hope) may be equally and jointly perpetuated; and that the bright *example* set in this reign by the Royal patrons of both, may be transmitted with the sceptre, to those of the same great line: to y[e] end that this age may be as illustrious, and this nation as distinguished, for every other felicity and glory; as it is, and ever must be, for having been honoured with such a man as Sir Isaac Newton, is the most sincere prayer of,

"Madam, may it please y[r] Majesty, &c."

No. XXVI.

(*Referred to in page* 271.)

In 1831, soon after the publication of my Life of Sir Isaac Newton, I received the following letter from Dr. Burgess, Bishop of Salisbury, who had distinguished himself by several learned and able works in defence of the doctrine of the Trinity.

"SALISBURY, *Nov.* 30, 1831.

"SIR,

"I beg your acceptance of the enclosed pages, which were occasioned by *the perusal of your very interesting Life of Sir Isaac Newton, which I read with great pleasure, till I came to the statement* of the contents of Sir Isaac's Dissertation on 1 John v. 7, and 1 Tim. iii. 16. I thought the restatement of his opinions on these subjects injurious to his memory, as he had expressly and anxiously suppressed them. I was desirous of counteracting the present abuse of Sir Isaac's authority by Socinians and Unitarians, but I was unwilling to deliver these pages to the public, without communicating them to yourself.—I am, SIR,

"With very sincere respect,

"Your obedient Servant,

"T. SARUM."

The pages thus enclosed by the Bishop, bore the title of *Appendix on Sir Isaac Newton's suppression of his Dissertation on* 1 *John* v. 7, *and* 1 *Tim.* iii. 18. It consisted of ten printed pages, and contained the following criticisms on my work.

"The name of Sir Isaac Newton has been lately employed by Socinians and Unitarians, in opposition to the doctrine of the Trinity, on the authority of a Tract, which he anxiously and deliberately suppressed. Dr. Brewster, in his recent publication of the Life of Sir Isaac Newton, *has, it is much to be regretted, done the same injustice to the memory of Sir Isaac* by his restatement and revival of the general contents of the suppressed Dissertation on the controverted verse of St. John, and by *omitting to notice Sir Isaac's suppression of the Tract.* The preceding REMARKS *on the general Tenour of the New Testament* had hardly left the press, when I first met with Dr. Brewster's Life of Sir Isaac Newton in the 24th volume of the *Family Library.* The popularity of the work, of which Dr. Brewster's volume is a very interesting portion, has induced me to add this Appendix to my Remarks, in order to counteract, as far as may be, *the injury done to the name of Sir Isaac Newton, and its influence on public opinion.*"

"The revival and restatement of these abortive criticisms are *injurious to the memory of the writer,* because *it omits to notice* that the Tract which contains them was deliberately and anxiously suppressed."—Pages 81, 82.

As soon as I received the preceding letter, and its enclosure, I informed the Bishop of the great mistake which he had committed, in charging me with having injured the memory of Sir Isaac Newton, by omitting to notice that he had suppressed his Tract. I directed his attention to page 274 of the work, in which I had not only mentioned the fact, but had even printed at full length Newton's own letter to Locke suppressing his Dissertation. To this letter I received the following answer :—

"PALACE, SALISBURY, *Dec.* 10, 1831.

"SIR,

". . . I am still more sorry that I should have overlooked, or rather not have seen, at the time I printed my Appendix, the account in your work to which your letter directs me, and which I have since read, of the suppression of the Dissertation. The pages of your work (281-284) containing the statement of Sir Isaac Newton's opinions and paraphrase, *were shown to me by a friend*, and as they contained no allusion to the suppression of the Dissertation, I was led to suppose that you had altogether omitted to notice it. When I reprint my Appendix, I shall certainly correct my oversight.—I am, SIR,

"Your obedient Servant,

"T. SARUM."

I am unwilling to characterize the incompatibility of the statements contained in these two letters, but having overlooked the offence at the time, I may now express my surprise, that after having committed such a gross error, Dr. Burgess never thought of correcting it, either by reprinting the few pages of his Appendix, or by inserting a fly leaf in his volume.

The charge of having injured Sir Isaac Newton, and of having produced, by "the revival and restatement of his abortive criticisms," an *influence on public opinion* which it was necessary for Dr. Burgess to counteract, is too ridiculous to require refutation. The Bishop himself, in his "Tracts on the Divinity of Christ,"[1] has quoted from this very Dissertation, in order to show that it is not Antitrinitarian, and yet he denounces others for following his example. He has referred also, times without number, to the subject of Newton's Arian tendencies, and thus compelled his readers to peruse the very Dissertation of which he is afraid. Such an attempt to stifle the truths of history is of very rare occurrence. Dr. Horsley, the great champion of the Trinity, did not scruple to give importance to Newton's Dissertation, by publishing it along with the Principia ; and I should have betrayed the trust committed to me, had I not given an account of the theological writings of a man, who was described by one Bishop as "knowing more of the Scriptures than them all," and by another as having "the whitest soul" he ever knew.

No. XXVII.

(*Referred to in page* 276.)

IRENICUM : OR ECCLESIASTICAL POLITY TENDING TO PEACE.

THESIS 1.

"The cities of the Israel before the Babylonian captivity were governed by elders, who sat in the gate of the city, and put the laws of Moses in

[1] He even says that Newton wrote against 1 John v. 7, *as other orthodox* persons have done. Page xxi. *Tracts*, &c. Lond. 1820.

execution, and had a place of worship in or near the gate, and sometimes a high place for sacrificing upon a neighbouring hill.—See Deut. xix. 12, and xxi. 19, 20, 21, and xxii. 18, 19, and xxv. 7, 8, and Ruth iv. 2, and Josh. xx. 4, and Psal. vii. 4-8. And in this sense it is said that the gates of hell, that is the magistrates in the gates of idolatrous cities, shall not prevail against the true Church of Christ.

THESIS 2.

"The government of the Jewish Church, being dissolved by the Babylonian captivity, was restored by the commission of Artaxerxes Longimanus, king of Persia, to Ezra, authorizing him to set magistrates and judges to judge the people who knew the laws of God, and to teach them who knew them not, and to execute judgment upon those who would not do the Law of God and the law of the king, whether it were unto death or to banishment, or to confiscation of goods, or to imprisonment.

"For the forming of this government being left to the discretion of Ezra, it may be presumed that he would pursue the ancient form of Jewish government so far as it was practicable.—See Ezra x. 14.

THESIS 3.

"The government then set up by Ezra was by courts of judicature, composed of elders; the highest court being the Sanhedrim, composed of 70 elders, originally instituted by Moses; and the second court being composed of 23 elders in the outward gate of the temple; and the other courts sitting in the synagogues of the cities, and being composed of the elders of the city, not more in number than 23, not fewer than three.—See Matt. x. 17, and xxiii. 34; Luke xii. 11, and xxi. 12.

THESIS 4.

"The government set up by Ezra continued till the days of Christ, and was then extended over all the Roman Empire; and the Jews, by the permission or connivance of the Romans, erected synagogues wherever they were sufficiently numerous to do it; and the elders of cities were called rulers of their synagogues.—See Acts xv. 21, Matt. x. 17, and xxiii. 34, and Luke xii. 11, and xxi. 12.

THESIS 5.

"The same government continued among the converted Jews of the circumcision in the regions of Phœnicia, Syria, &c., till the end of the fourth century, or longer, and the chief ruler of the synagogue was called by them the Prince of the Synagogue.

THESIS 6.

"The same government was propagated from the Jews to the converted Gentiles, the name of synagogues being changed to that of churches, and the name of Chief Rulers and Princes of the Synagogues unto that of Presidents and Bishops, the Bishop being the President of the Council of Elders, called in the Greek Presbyters, and the Presbyters in the Council being at length called Prebendaries, from the allowances made to them out of the revenues of the Church for their attendance. But the name of churches was of a larger extent, being given also to single assemblies in private houses, and other places not attended with a Board of Elders, and collectively to the churches of a kingdom or nation, or in the whole world.

THESIS 7.

"It is therefore the duty of bishops and presbyters to govern the people according to the laws of God and the laws of the king, and in their councils to punish offenders according to those laws, and to teach those who do not know the laws of God; but not to make new laws in the name of either God or the king.

THESIS 8.

"The Church is constituted, and her extent and bounds of communion are defined by the laws of God, and these laws are unchangeable.

THESIS 9.

"The laws of the king extend only to things that are left indifferent and undetermined by the laws of God, and particularly to the revenues and tranquillity of the Church, to her courts of justice, and to decency and order in her worship; and all laws about things left indifferent by the laws of God ought to be referred to the civil government.

THESIS 10.

"The king is supreme head and governor of the Church in all things indifferent, and can nominate new bishops and presbyters to succeed in vacant places, and deprive and depone them whenever they may deserve it.

THESIS 11.

"The being of the Church doth not depend upon an uninterrupted succession of bishops and presbyters for governing her; for this succession was interrupted in the time of the Babylonian captivity until Ezra, by the commission of Artaxerxes, appointed new governors. And therefore if it should be again interrupted, the Christian people, by the authority or leave of the king, may restore it. The Christian church was also in being before there was a Christian synagogue.

THESIS 12.

"All persons baptized are members of Christ's body called the Church, even those who are not yet admitted into the communion of the synagogue of any city. For all members circumcised were members of the Church of the Jews in the time of the Babylonian captivity before Ezra restored their polity. And in the days of Ahab, when there remained only 7000 in Israel who had not bowed the knee to Baal, these were the true Church of God, though without an external form of government; and the worshippers of Baal under their external form of government were a church of idolaters, such a church, as in scripture, is called the synagogue of Satan, who say they are Jews and are not, a false church with regard to the God whom they worshipped. And the three thousand baptized by Peter were a Christian church, though they had not yet a bishop, or presbyter, or synagogue, or form of government.

THESIS 13.

"By imposition of hands men are admitted into the communion of the synagogue of a city, and by excommunication they are deprived of that communion, and return into the state they were in by baptism alone, before they were received into communion by imposition of hands, except the sin for which they were excommunicated; and by new imposition of hands they may be received into communion again without new baptism, and therefore by excommunication they do not lose the privilege or benefit of baptism.

THESIS 14.

"Men are not to be excommunicated without breaking one or more of the articles upon which they are admitted into communion. For this would be to alter the bounds of communion settled by the laws of God in the beginning of the Gospel.

THESIS 15.

"To impose any article of communion not imposed from the beginning is a crime of the same nature with that of those Christians of the circumcision who endeavoured to impose circumcision, and the observation of the law upon the converted Gentiles. For the law was good if a man could keep it, but we were to be saved not by the works of the law, but by faith in Jesus Christ; and to impose those works as articles of communion, was to make them necessary to salvation, and thereby to make void the faith in Jesus Christ. And there is the same reason against imposing any other article of communion which was not imposed from the beginning. All such impositions are teaching another gospel.

THESIS 16.

"To refuse communion with any church or synagogue merely upon account of the laws of the king in matters indifferent, unless these laws are imposed not merely as laws of the civil government, but as articles of religion and communion, is disobedience to the king, and schism in relation to the Church.

THESIS 17.

"To distinguish churches from one another by any difference in customs or ceremonies, or in other laws than the laws of God, is improper, and tends to superstitions. And if the distinction occasions a breach of communion, the person insisting upon it as a matter of religion is guilty of the schism. For the distinction being taken from things which are only of human authority and external to religion, ought not to be considered as a part of religion, nor to enter into the definition of a Church.

THESIS 18.

"The fundamentals or first principles of religion are the articles of communion taught from the beginning of the Gospel in catechising men in order to baptism and admission into communion; namely, that the catechumen is to repent and forsake covetousness, ambition, and all inordinate desires of the things of this world, the flesh, and false gods called the devil, and to be baptized in the name of one God, the Father, Almighty, Maker of Heaven and Earth, and of one Lord Jesus Christ, the Son of God, and of the Holy Ghost.—See Heb. v. 12, 13, 14, and vi. 1, 2, 3.

THESIS 19.

"After baptism we are to live according to the laws of God and the king, and to grow in grace and in the knowledge of our Lord Jesus Christ, by practising what they promised before baptism, and studying the Scriptures, and teaching one another in meekness and charity, without imposing their private opinions, or falling out about them.

THESIS 20.

"The commission to teach and baptize was given to the Apostles as the disciples of Christ, and to their disciples, and the disciples of their disciples, to the end of the world, there being no bishops or presbyters or church government yet instituted among the Christians. But after the institution of governments, the governors appointed men to catechise and baptize, except in cases of necessity, where the original right returned. For Tertullian has told us that in his days the rule was, In casu necessitatis quilibet laicus tingit."

' These *Theses* were called *Positions* in the original MS., but the term *Thesis* was afterwards susbtituted. We have placed them according to their number, and not as in the manuscript.

No. XXVIII.

(*Referred to in page* 279.)

QUÆRIES REGARDING THE WORD ὁμοούσιος.

QUÆRE 1. Whether Christ sent his apostles to preach metaphysics to the unlearned common people, and to their wives and children?

QUÆRE 2. Whether the word ὁμοούσιος ever was in any creed before the Nicene; or any creed was produced by any one bishop at the Council of Nice for authorizing the use of that word?

QUÆRE 3. Whether the introducing the use of that word is not contrary to the Apostles' rule of holding fast the form of sound words?

QUÆRE 4. Whether the use of that word was not pressed upon the Council of Nice against the inclination of the major part of the Council?

QUÆRE 5. Whether it was not pressed upon them by the Emperor Constantine the Great, a catechumen not yet baptized, and no member of the Council?

QUÆRE 6. Whether it was not agreed by the Council that that word should, when applied to the Word of God, signify nothing more than that Christ was the express image of the Father? and whether many of the bishops, in pursuance of that interpretation of the word allowed by the Council, did not, in their subscriptions, by way of caution, add τουτ' ἔστιν ὁμοούσιος.

QUÆRE 7. Whether Hosius (or whoever translated that Creed into Latin) did not impose upon the Western Churches by translating ὁμοούσιος by the words *unius substantiæ,* instead of *consubstantialis*? and whether by that translation the Latin Churches were not drawn into an opinion that the Father and Son had one common substance, called by the Greeks *Hypostasis,* and whether they did not thereby give occasion to the Eastern Churches to cry out, presently after the Council of Sardica, that the Western Churches were become Sabellian?

QUÆRE 8. Whether the Greeks, in opposition to this notion and language, did not use the language of three Hypostases, and whether in those days the word Hypostasis did not signify a substance?

QUÆRE 9. Whether the Latins did not at that time accuse all those of Arianism who used the language of three Hypostases, and thereby charge Arianism upon the Council of Nice, without knowing the true meaning of the Nicene Creed?

QUÆRE 10. Whether the Latins were not convinced, in the Council of Ariminum, that the Council of Nice, by the word ὁμοούσιος, understood nothing more than that the Son was the express image of the Father—the acts of the Council of Nice were not produced for convincing them. And whether, upon producing the acts of that Council for proving this, the Macedonians, and some others, did not accuse the bishops of hypocrisy, who, in subscribing these acts, had interpreted them by the word ὁμοιούσιος in their subscriptions?

QUÆRE 11. Whether Athanasius, Hilary, and in general the Greeks and Latins, did not, from the time of the reign of Julian the Apostate, acknowledge the Father, Son, and Holy Ghost to be three substances, and continue to do so till the schoolmen changed the signification of the word hypostasis, and brought in the notion of three persons in one single substance?

QUÆRE 12. Whether the opinion of the equality of the three substances was not first set on foot in the reign of Julian the Apostate, by Athanasius, Hilary, &c.?

QUÆRE 13. Whether the worship of the Holy Ghost was not first set on foot presently after the Council of Sardica?

QUÆRE 14. Whether the Council of Sardica was not the first Council which declared for the doctrine of the Consubstantial Trinity? and whether the Council did not affirm that there was but one hypostasis of the Father, Son, and Holy Ghost?

QUÆRE 15. Whether the Bishop of Rome, five years after the death of Constantine the Great, A. C. 341, did not receive appeals from the Greek Councils, and thereby begin to usurp the universal bishopric?

QUÆRE 16. Whether the Bishop of Rome, in absolving the appellants from excommunication, and communicating with them, did not excommunicate himself, and begin a quarrel with the Greek Church?

QUÆRE 17. Whether the Bishop of Rome, in summoning all the bishops of the Greek Church to appear at the next Council of Rome, A. C. 342, did not challenge dominion over them, and begin to make war upon them for obtaining it?

QUÆRE 18. Whether that Council of Rome, in receiving the appellants into communion, did not excommunicate themselves, and support the Bishop of Rome in claiming appeals from all the world?

QUÆRE 19. Whether the Council of Sardica, in receiving the appellants into communion, and decreeing appeals from all the churches to the Bishop of Rome, did not excommunicate themselves, and become guilty of the schism which followed thereupon, and set up Popery in all the West?

QUÆRE 20. Whether the Emperor Constantius did not, by calling the Council of Millain and Aquileia, A. C. 365, abolish Popery? and whether Hilary, Lucifer, . . . were not banished for adhering to the authority of the Pope to receive appeals from the Greek Councils?

QUÆRE 21. Whether the Emperor Gratian, A. C. 379, did not, by his edict, restore the universal bishopric of Rome over all the West? and whether this authority of the Bishop of Rome hath not continued ever since?

QUÆRE 22. Whether Hosius, St. Athanasius, St. Hilary, St. Ambrose, St. Hierome, St. Austin, were not Papists?[1]

No. XXIX.

(Referred to in page 289.)

DE METALLO AD CONFICIENDUM SPECULUM COMPONENDO ET FUNDENDO.

"℞ cupri partes 11 vel 12, stanni optimi partes 4, arsenici albi partem 1. Cupro liquefacto injiciatur arsenicum, et cum baculo ligneo bene commisceantur agitando. Dein Stannum etiam in frusta divisum injiciatur, et massa iterum agitetur celeriter, atque omnibus sic bene commistis in formam absque mora infundantur.

"Nota 1. Quod Stannum celerrime liquefiat, et massam deinde per fumos suos porosam reddat si diutius stet super ignem ut intentius incalescat.

[1] The Quæries after Number 14 are not numbered in the original.

"2. Massa itaque si iterum fundenda est non debet plusquam ad liquefactionem incalescere.

"3. Potest cuprum antequam miscetur cum stanno, purgari liquefaciendo et injiciendo in 12 uncias cupri liquefacti, primo unciam unam arsenici ac duas tresve uncias antimonii crudi, deinde tres vel quatuor uncias salis nitri per vices, donec totus sal deflagraverit. Tunc massa frigefacta abjiciatur scoria salinosa, et iterum fundatur metallum injicianturque novi salis nitri duæ tresve unciæ per vices, donec totus deflagraverit, ut prius; eritque satis depuratum si sal supernatans post refrigerium albus exierit; sin secus, tertio liquefaciendum est cum sale. Sed interea summe cavendum est a fumis arsenici.

"Ex cupro sic purgato componi potest metallum cum arsenico et stanno ut supra; sed compositio habebitur fortius reflectens et (quantum conjicio) magis resistens ærugini si omisso arsenico injiciatur ad duodecim partes cupri liquefacti, primo una pars Zineti seu Margaritæ albæ et una pars reguli antimonii per se sine ♂te facti, deinde quatuor partes stanni ut supra. Signum optimæ compositionis est ut metallum instar vitri læve appareat ubi frangitur.

"5. Si metallum in formam infusum, inter refrigerandum in fragmenta sponte dissiliat, id arguit nimium esse stanni pro quantitate cupri. Quare tunc aliquantulum novi cupri, puta duodecima pars totius massæ per se pendenda est huic fuso injicienda reliqua massa diffracta.

"5. Metallum sic formatum debet esse satis crassum ne inter terendum et poliendum vel minime flecti queat. Majori enim ἀκριβείᾳ debent metalla ad usus opticos formari quam vitra. Quæ ego pro tubo septem fere digitorum fudi, erant quadrantem digiti circiter crassa, duosque digitos lata. Primò quidem formabam tenuiora, et minus lata, sed ex illis nihil perfectum construere potui."

No. XXX.

(*In reference to pages* 306, 307.)

ALTERATIONS AND ADDITIONS MADE IN THE THIRD EDITION OF THE PRINCIPIA.

After perusing Pemberton's letters to Newton, I drew up a list of the additions made to the *Principia* in the third edition, and of the more important alterations upon the second, in so far as they could be gathered from the letters. It occurred to me, however, that some of his distinguished successors in Cambridge must have had occasion to notice these additions and alterations, and I accordingly applied to J. C. Adams, Esq., of Pembroke College, who has obligingly favoured me with the following interesting communication:—

"I have made no regular comparison of the several editions of the Principia, except with reference to some special points. Some of the differences, however, which I have noticed between the 2d and 3d editions, I will mention below.

"The proof that Newton was acquainted with the true principles of calculating the motion of the moon's apogee, and that he had actually determined that motion to a considerable degree of approximation, is supplied by a scholium which follows prop. 35 of the 3d book *in the first edition.* In the subsequent editions this scholium is greatly enlarged, but the evidence on the point above mentioned is unfortunately omitted.

"In the correspondence between Newton and Cotes, published by Mr. Edleston, I can find no allusion to the old scholium.[1]

"Many have supposed that Newton intended to find the motion of the moon's apogee in the 2d corollary to prop. 45 of his 1st book, p. 141, but this is a complete mistake. In the 1st and 2d editions no reference whatever is made to the moon in this corollary; and in the 3d edition is added the remark, 'Apsis lunæ est duplo velocior circiter,' for the express purpose of pointing out that the corollary is not applicable to the case of the moon. In fact, the disturbing force, the effect of which is found in this corollary, is only *one part* of the sun's disturbing force on the moon, and to the other part the method of the corollary is plainly inapplicable.

"To the 2d edition of the Principia, at page 419 (451 of 3d), there is added a very elegant proposition by Machin and Pemberton, respecting the motion of the moon's node. It may be thus stated:—The mean rate of motion of the sun from the moon's node is a mean proportional between the rates of motion with which the sun separates from the node when in syzygy and quadrature respectively. I may mention that when stated in this form, the proposition is equally applicable to the motion of the moon's apogee. (See p. 553, bottom.)

"I will now mention some of the other changes which I have noticed in the 3d edition.

"Lemma 11, Cor. 4, p. 31, 2d edit. last line, 'quamque alias sesquialteram dicunt,' is omitted in 3d edit.

"Prop. 4, Cor. 1, p. 38, two lines, from 'centripetæ sunt' to 'in ratione,' are omitted.

"Cor. 2, p. 39, one and a half line, from 'centripetæ sunt' to 'in ratione,' is omitted.

"Prop. 8, p. 44, line 14, 'Circulo' is changed into 'Semicirculo;' and in the scholium, p. 45, 'Et simili argumento corpus movebitur,' is changed into 'Et argumento haud multum dissimili corpus invenietur movere.'

"Prop. 10, Cor. 2, p. 47, for 'ad axes alteros,' is substituted 'ad idem punctum axis communis.'

"At the end of corollary 1, prop. 13, p. 59, 53,[2] is added, 'Eademque velocitate.'[3]

"Prop. 14, Cor. p. 54, after 'QT × SP,' is added, 'quæ dato tempore describitur.'

"Prop. 17, page 57, line 8, 'Sit istud L,' is changed into 'Sit L coni sectionis latus rectum.'

"At the end of prop. 17, p. 64, 57, is added the sentence, 'Nam si corpus,' &c.[4]

[1] There is a slight allusion to it in the *Correspondence,* &c., p. 109.

[2] The *first* number for the page is the number in the 3d edition, and the *second* number is that in the 2d edition.

[3] Suggested by Pemberton. [4] Id.

"Scholium to prop. 21, p. 65, at the beginning the sentence ending with 'potest' is added.

"Near the end of the scholium to prop. 31, p. 112, 104, *Ward's* name is omitted.

"At the end of corollary 19 to prop. 66, p. 182, 167, is added, 'Nisi quatenus motus fluendi,' &c.

"At end of corollary 20 to same prop., p. 183, 168, are added 15 lines, 'Nisi quod loca maximarum,' &c.

"In book ii. p. 246, 226, the Leibnitz scholium is replaced by another.

"To prop. 13, p. 269, 250, a short scholium is added.

"To prop. 14, p. 274, 252, a scholium is added.

"At end of scholium to prop. 22, p. 292, 270, the sentence is added, 'Cæterum per experimenta constat,' &c.

"At the end of corollary 3, prop. 29, p. 303, 280, the clause, 'Quæ et generalior sit,' &c., is omitted.

"Page 292, 2d edit., 'Denique cum receptissima Philosophorum ætatis hujus,' is changed into 'Denique cum nonnullorum,' &c.

"Page 314, Lemma 5, the second paragraph is added, 'Hæc ita,' &c.

"Page 316, Cor. 1, 'duplicata' is omitted in the 3d line.

"Page 317, prop. 39, a scholium of 7 lines is added.

"Page 325, Exp. 13, 'Mense Junio 1710,' is added.

"In book iii., at the end of Regula iii. p. 389, 358, is added, 'Attamen gravitatem corporibus essentialem esse minime affirmo,' &c. Also Regula iv. is added.

"Phenom. i., p. 390, 359, Pound's observations of the elongations of Jupiter's satellites are given. The account of phen. ii. is enlarged. To phen. iii. is added, 'Hos enim luce a sole,' &c.[1]

"Phen. iv., the periodic times of the planets are added.

"Prop. 4, p. 397, 364, Huygens's determination of the force of gravity by means of the pendulum, is cited at greater length, &c. A scholium is added.

"Prop. 5, p. 399, 365, a short scholium is added.

"Prop. 8, corollary 1, p. 406, 370, the masses of the planets are changed.

"Prop. 10, p. 407, 373, 'Ostendimus utique in scholio,' &c., is added.

"Prop. 17, p. 411, 377, a short paragraph is added on the rotation of Jupiter and of the Sun. Also reference is made to Mercator and Cassini.

"Prop. 19, p. 413, 378. Some changes are made in the account of measurements of degrees; and in the 3d edit., Pound's measures of the polar and equatorial diameters of Jupiter are given.

"Prop. 20, p. 418. A paragraph in p. 384 of 2d edit., 'Hæc ita se habent,' &c., is omitted; as is also another in p. 387, on the figure of the earth, derived from the measures of Picart and Cassini.

"Prop. 24, p. 424, 390, 'Vis solis vel lunæ,' &c., is added.[2]

"Scholium to prop. 35, of 3d edit. The paragraph 'Si computatio accuratior desideretur,' &c., in p. 425 of 2d edit., is omitted.

"Two corollaries are added to prop. 37, p. 471 of 3d edit.

"The fig. to Lemma 10, p. 490, 431 of 3d edit., is simpler than in the former editions.

"In p. 497 of 3d edit., the places of stars compared with the comet of 1680, are given according to Pound.

[1] Suggested by Pemberton. [2] Id.

"Prop. 42, p. 523, of 3d edit., Bradley's observations of the comet of 1723 are given. A paragraph at the end of this prop. at p. 481 of edit. 2, attributing the acceleration of the moon's motion to an increase of the mass of the earth, due to the condensation of vapours from the interplanetary spaces, is omitted in the 3d edit.

"Many of the above changes and additions are very trifling, but I thought I might as well mention what I had noted."

From the letters of Pemberton, I have collected the following additions and alterations, which are not mentioned by Mr. Adams. The pages are numbered as in the second edition.

Page 9, line 14, *vas* placed after *postquam* at Pemberton's request.
,, 17, after *Lemmate* 23, *ejusque corollario* is added. Pemb.[1]
,, 19, line 10, after *retardantur*, a whole page nearly is added on the fall of heavy bodies; and for *hujus ætatis*, in line 14, is substituted *ætatis superioris*. Newton had made it *ætatis novissimæ*, but Pemberton says that *superioris* is Ciceronian.
,, 20, line 16, after *corpus A*, we read (*ut ita dicam*) *in chordam arcus T A quæ velocitatem ejus exhibet, ut habeatur*, &c. Line 10 from bottom, *quiescens* added after *corpus B*. Pemb.
,, 42, Cor. 3 is considerably changed at Pemberton's desire.
,, 46, line 4, *aliæ* inserted after *diametri*. Pemb.
,, 51, line 7, *opposita* substituted for *conjugata*. Pemb.
,, 51, 52, the diagrams greatly simplified. Pemb.
,, 59, 60, Pemberton suggests a change on Prop. 17, which is not adopted.
,, 64, line 16, after *Positione* three new lines are added in place of the last seven lines of *Cas.* 1. *Cas.* 2 is also changed. Pemb.
,, 79, a paragraph of six lines is added to Prop. 24, by Newton; but Pemberton proposes to have the leaf cancelled, and demonstrates, at some length, the truth of the paragraph which he wishes to substitute. See this Volume, p. 306.
,, 87, lines 14 to 18 slightly changed. Pemb.
,, 147, lines 13-15 altered by Pemberton.
,, 249, Cor., two lines, 'Si centro C, &c.' are added at the beginning.
,, 299, line 14, a slight alteration by Pemberton.
,, 300, par. 2. See p. 155, *note*.
,, 303, line 16 from bottom, *finge* is substituted for *concipe*.
,, 305, line 14, after *debet*, the paragraph is greatly enlarged.
,, 321, the second paragraph of Exp. 3 is greatly altered by Pemberton.
,, 326, after the table, there is inserted an account, occupying two pages, of Desaguliers' experiments in 1719. In the fine paper copy of the third edition, the word *ambientis* following *concavæ*, in the third line of the Additions, is struck out. See Horsley, Newtoni *Opera*, tom. ii. p. 427, *note*.
,, 333, line 21, at *expandent*, Pemberton adds an explanatory note.

[1] The word *Pemb.* indicates that the alteration was made at the suggestion of Pemberton.

Page 364, line 4 from bottom, after *revolvantur,* is inserted *manente lege gravitatis.*

,, 367, line 5, after *proxime,* the words *uti calculis quibusdam initis,* are changed into *uti calculo quodam inito.*

,, 376, Pemberton proposes to leave out the last sentence of the page *Et hi motus,* &c., but it was not done.

,, 376, Prop. 14, Pemberton proposes to alter Cor. 1, but it was not done.

,, 377, the whole of Prop. 17 is new, and greatly enlarged.

,, 378, Prop. 19 is greatly changed. A paragraph of five lines, and placed at the beginning of the Proposition about Norwood's measurement of a degree, is new. The first paragraph in the 2d edit. is greatly altered.

,, 379, the first paragraph is altered.

,, 381, the two last paragraphs are greatly changed, and Pound's measures of Jupiter's oblateness are given in a table followed by two new paragraphs.

,, 384, paragraph first, and the three last lines of paragraph second, omitted at Pemberton's suggestion.

,, 386, 387, more than a page is omitted, and a large paragraph "*Virga ferrea,*" &c., added. Pemb.

,, 389, line 3, *a prioribus Astronomis non observatæ* substituted for *nondum observatæ.*

,, 389, line 32-34, Pemberton suggests "a brief hint at the principle whence the precept contained in this line was deduced," but it is not given.[1]

,, 390, line 19, an additional reference is made to the new Cor. 20, Prop. 66, Lib. 1. Pemb.

,, 415, at the end of this page is added a scholium with Machin's two Propositions on the motion of the moon's nodes. Pemberton suggested the reference to what had been done in his *Epistola ad Amicum de Cotesii Inventis,* pp. 6 and 7.

,, 427, line 2, "25472 *ut supra in Prop.* 19," is substituted for "85820;" and in line 8, "85472," for "85820."

,, 430, 431, the latter half of Cor. 7 is greatly altered.

,, 433, the diagram and letters are altered. Pemb.

,, 464, line 23, *fere* is omitted. Pemb.

,, 467, line 7, Pemberton says that this is inconsistent with *Optics,* Qu. 11, but no change is made.

,, 469, line 23, and 471, lines 27, 28, objected to by Pemberton, but not changed.

,, 472, Pemberton criticises the explanation of the ascent of vapour from comets' tails, and proposes to substitute *Sectionibus Conicis* for *Ellipticis,* but no change is made.

,, 474, line 17, *Lusitania* for *Portugallia.* Pemb.

[1] If Newton had complied with Pemberton's suggestion, all the difficulties connected with the motion of the moon's apogee would have been avoided. The paragraph to which Pemberton's suggestion relates, viz., "Diminui tamen debet motus Augis sic inventus in ratione 5 ad 9 vel 1 ad 2 circiter, ob causam quam hic exponere non vacat," clearly implies that Newton knew the reason.

Page 481, Before the paragraph beginning line 7-19, *Vapores,* &c., mentioned by Mr. Adams, is inserted an account, occupying a page, of the great star in Cassiopeia, seen in November 1572, by Cornelius Gemma. The paragraph beginning with *Vapores,* and ending with *Migrare,* immediately follows it; but the last paragraph, beginning with *Decrescente,* is omitted.

In place of omitting, as he has done, the long paragraph, "Si computatio," as in the Scholium, to Prop. 35, lib. iii. pp. 415 and 463 of third edition, Sir Isaac drew it up in a different form, which I find written as follows on the back of one of Pemberton's letters, without a date:[1]—"Ut radius ad sinum distantiæ Lunæ a Sole ita angulus quidam Q ad Variationem secundam si Lunæ lumen augetur, addendam si diminuitur. Sic habebitur . . . . longitudo Lunæ Angulus vero Q ex observationibus determinandus est. Et interea pro eodem usurpari potest angulus 1′ 45″ donec accuratius determinetur." "It does not appear," says Mr. Adams, to whom I sent the paragraph, "why it was not inserted, as it describes what is now called the Parallactic inequality, and the co-efficient given is not far from the truth. Owing to the want of the paragraph, the process of finding the moon's longitude terminates very abruptly."

No. XXXI.

(*Referred to in pages* 317, 321.)

OBSERVATIONS ON THE FAMILY OF SIR ISAAC NEWTON.

In the year 1705, Sir Isaac Newton gave into the Herald's Office an elaborate pedigree, stating, upon oath, *that he had reason to believe* that John Newton of Westby, in the county of Lincoln, was his great-grandfather's father, and that this was the same John Newton who was buried in Basingthorpe Church on the 22d December 1563. This John Newton had four sons, John, Thomas, Richard, and William Newton of Gunnerly, the last of whom was great-grandfather to Sir John Newton, Bart. of Hather. Sir Isaac considered himself as descended from the eldest of these, *he having, by tradition from his kindred ever since he can remember, reckoned himself next of kin (among the Newtons) to Sir John Newton's family.*

The pedigree, founded upon these and other considerations, was accompanied by a certificate from Sir John Newton of Thorpe, Bart., who states that he had heard his father speak of Sir Isaac Newton *as of his relation and kinsman;* and that *he himself believed that Sir Isaac was descended*

[1] In this letter Pemberton calls Newton's attention to lines 24, 25, 26, 27 of page 341, and asks him to compare them with the second paragraph of the Scholium to Prop. 34, Book ii. p. 300; "for," he says, "if what is inserted in these lines before us be universally true, without any restriction, how can what is delivered in that paragraph be of any use in the forming of ships?"

from John Newton, son to John Newton of Westby, but knoweth not in what particular manner.

The pedigree of Sir Isaac, as entered at the Herald's Office, does not seem to have been satisfactory either to himself or to his successors, as it could not be traced with certainty beyond his grandfather; and it will be seen from the following interesting correspondence, that, upon making farther researches, he had found some reason to believe that he was of Scotch extraction.

EXTRACT OF A LETTER FROM THE REV. DR. REID OF GLASGOW TO DR. GREGORY OF EDINBURGH, DATED MARCH 14, 1784.

"I send you on the other page an anecdote respecting Sir Isaac Newton, which I do not remember whether I ever happened to mention to you in conversation. If his descent be not clearly ascertained (as I think it is not in the books I have seen), might it not be worth while to inquire if evidence can be found to confirm the account which he is said to have given of himself? Sheriff Cross was very zealous about it when death put a stop to his inquiries.

"When I lived in Old Aberdeen above twenty years ago, I happened to be conversing over a pipe of tobacco with a gentleman of that country, who had been lately at Edinburgh. He told me that he had been often in company with Mr. Hepburn of Keith, with whom I had the honour of some acquaintance. He said that, speaking of Sir Isaac Newton, Mr. Hepburn mentioned an anecdote, which he had from Mr. James Gregory,[1] professor of Mathematics at Edinburgh, which was to this purpose:—

"Mr. Gregory being at London for some time after he resigned the mathematical chair, was often with Sir Isaac Newton.[2] One day Sir Isaac said to him, 'Gregory, I believe you don't know that I am connected with Scotland?' 'Pray how, Sir Isaac?' said Gregory. Sir Isaac said he was told that his grandfather was a gentleman of East Lothian; that he came to London with King James at his accession to the crown of England, and there spent his fortune, as many more did at that time, by which his son (Sir Isaac's father) was reduced to mean circumstances. To this Gregory bluntly replied, 'Newton a gentleman of East Lothian? I never heard of a gentleman of East Lothian of that name.' Upon this Sir Isaac said, 'that being very young when his father died, he had it only by tradition, and it might be a mistake,' and immediately turned the conversation to another subject.

"I confess I suspected that the gentleman who was my author had given some colouring to this story, and therefore I never mentioned it for a good many years.

"After I removed to Glasgow, I came to be very intimately acquainted with Mr. Cross, then Sheriff of Lanark, and one day at his own house mentioned this story, without naming my author, of whom I expressed some diffidence.

"The Sheriff immediately took it up as a matter worth being inquired into. He said he was well acquainted with Mr. Hepburn of Keith (who

[1] The nephew of the celebrated James Gregory, the Inventor of the Reflecting Telescope.

[2] This must have been after October 1725.—See pp. 310, 312.

was then alive), and that he would write him to know whether he ever heard Mr. Gregory say that he had such a conversation with Sir Isaac Newton. He said he knew that Mr. Keith, the ambassador, was also intimate with Mr. Gregory, and that he would write him to the same purpose.

"Some time after, Mr. Cross told me that he had answers from both the gentlemen above mentioned, and that both remembered to have heard Mr. Gregory mention the conversation between him and Sir Isaac Newton, to the purpose above narrated, and at the same time acknowledged that they had made no farther inquiry about the matter.

"Mr. Cross, however, continued the inquiry, and a short time before his death, told me that all he had learned was, that there is, or was lately, a baronet's family of the name of Newton in West Lothian or Mid-Lothian (I have forgot which): That there is a tradition in that family, that Sir Isaac Newton wrote a letter to the old knight that then was (I think Sir John Newton of Newton was his name), desiring to know what children, and particularly what sons he had, their age, and what professions they intended: That the old baronet never deigned to return an answer to this letter, which his family was sorry for, as they thought Sir Isaac might have intended to do something for them."

Several years after this letter was written, a Mr. Barron, a relation of Sir Isaac Newton, seems to have been making inquiries respecting the family of his ancestor, and in consequence of this the late Professor Robison applied to Dr. Reid, to obtain from him a more particular account of the remarkable conversation between Sir Isaac and Mr. James Gregory, referred to in the preceding letter. In answer to this request, Dr. Reid wrote the following letter, for which I was indebted to the late Sir John Robison, Sec. R.S.E., who found it among his father's manuscripts.

LETTER FROM DR. REID TO PROFESSOR ROBISON RESPECTING THE FAMILY OF SIR ISAAC NEWTON.

"DEAR SIR,

"I am very glad to learn by yours of April 4, that a Mr. Barron, a near relation of Sir Isaac Newton, is anxious to inquire into the descent of that great man, as the family cannot trace it farther, with any certainty, than his grandfather. I therefore, as you desire, send you a precise account of all I know; and am glad to have this opportunity, before I die, of putting this information in hands that will make the proper use of it, if it shall be found of any use.

"Several years before I left Aberdeen (which I did in 1764), Mr. Douglas of Feckel, the father of Sylvester Douglas, now a barrister at London, told me, that, having been lately at Edinburgh, he was often in company with Mr. Hepburn of Keith, a gentleman of whom I had some acquaintance, by his lodging a night in my house, at New Machar, when he was in the rebel army in 1745. That Mr. Hepburn told him, that he had heard Mr. James Gregory, professor of mathematics, Edinburgh, say, that, being one day in familiar conversation with Sir Isaac Newton at London, Sir Isaac said, 'Gregory, I believe you don't know that I am a Scotchman?' 'Pray, how is that?' said Gregory. Sir Isaac said he was informed that his grandfather (or great-grandfather) was a gentleman of East (or West) Lothian: that he went to London with King James I. at his accession to the crown of England: and that he attended the Court in expectation, as many others did, until he spent his fortune, by which means his family was reduced to low circumstances. At the time this was told me, Mr. Gregory was dead, otherwise I should have had his own testimony, for he was my mother's brother. I likewise thought at that time, that it had been certainly known that Sir Isaac had been descended from an old English family, as I think is said in his *Eloge* before the Academy of Sciences at Paris, and therefore I never mentioned what I had heard for many years, believing that there must be some mistake in it.

"Some years after I came to Glasgow, I mentioned (I believe for the first time) what I had heard to have been said by Mr. Hepburn, to Mr. Cross, late sheriff of this county, whom you will remember. Mr. Cross was moved by this account, and immediately said:—'I know Mr. Hepburn very well, and I know he was intimate with Mr. Gregory; I shall write him this same night, to know whether he heard Mr. Gregory say so or not.' After some reflection, he added, 'I know that Mr. Keith, the ambassador, was also an intimate acquaintance of Mr. Gregory, and as he is at present in Edinburgh, I shall likewise write to him this night.

"The next time I waited on Mr. Cross, he told me that he had wrote both to Mr. Hepburn and Mr. Keith, and had an answer from both, and that both of them testified that they had several times heard Mr. James Gregory say, that Sir Isaac Newton told him what is above expressed, but that neither they nor Mr. Gregory, as far as they knew, ever made any farther inquiry into the matter. This appeared very strange both to Mr. Cross and me, and he said he would reproach them for their indifference, and would make inquiry as soon as he was able.

"He lived but a short time after this; and in the last conversation I had with him upon the subject, he said, that all he had yet learned was, that there was a Sir John Newton of Newton, in one of the counties of Lothian (but I have forgot which), some of whose children were yet alive; that they reported that their father, Sir John, had a letter from Sir Isaac Newton, desiring to know the state of his family, what children he had, particularly what sons, and in what way they were. The old knight never returned an answer to this letter, thinking probably that Sir Isaac was some upstart, who wanted to claim a relation to his worshipful house. This omission the children regretted, conceiving that Sir Isaac might have had a view of doing something for their benefit.

"After this I mentioned occasionally in conversation what I knew, hoping that these facts might lead to some more certain discovery, but I found more coldness about the matter than I thought it deserved. I wrote an account of it to Dr. Gregory, your colleague, that he might impart it to any member of the Antiquarian Society, who he judged might have the curiosity to trace the matter farther.

"In the year 1787, my colleague, Mr. Patrick Wilson, professor of astronomy, having been in London, told me on his return that he had met accidentally with a James Hutton, Esq. of Pimlico, Westminster, a near relation of Sir Isaac Newton, to whom he mentioned what he had heard from me with respect to Sir Isaac's descent, and that I wished much to know something more decisive on that subject. Mr. Hutton said, if I pleased to write to him he would give me all the information he could give. I wrote him accordingly, and had a very polite answer, dated at Bath, 25th December 1787, which is now before me. He says, 'I shall be glad, when I return to London, if I can find in some old notes of my mother, any thing that may fix the certainty of Sir Isaac's descent. *If he spoke so to Mr. James Gregory, it is most certain he spoke truth.* But Sir Isaac's grandfather, not his great-grandfather, must be the person who came from Scotland with King James I. If I find any thing to the purpose, I will take care it shall reach you.'

"In consequence of this letter I expected another from Mr. Hutton when he should return to London, but have never had any. Mr. Wilson told me he was a very old man, and whether he be dead or alive I know not.

"This is all I know of the matter, and, for the facts above-mentioned, I pledge my veracity. I am much obliged to you, dear Sir, for the kind expressions of your affection and esteem, which, I assure you, are mutual on my part, and I sincerely sympathize with you on your afflicting state of health, which makes you consider yourself as out of the world, and despair of seeing me any more.

"I have been long out of the world by deafness and extreme old age. I hope, however, if we should not meet again in this world, that we shall meet and renew our acquaintance in another. In the meantime, I am, with great esteem, dear Sir, yours affectionately,

"THO. REID.

"GLASGOW COLLEGE, 12*th April* 1792."

This curious letter I published in the *Edinburgh Philosophical Journal* for October 1820. It excited the particular attention of the late George Chalmers, who sent me an elaborate letter upon the subject; but as I was at that time in the expectation of obtaining some important information through other channels, the letter was not published. This hope, however, has been disappointed. A careful search was made at my request through the charter-chest of the Newtons of Newton in East Lothian, by Mr. Richard Hay Newton, the representative of that family, but no document whatever has been found that can throw the least light upon the matter. It deserves to be remarked, however, that Sir Richard Newton, the alleged correspondent of Sir Isaac, appears to have destroyed his correspondence; for though the charter-chest contains the letters of his predecessors for some generations, yet there is not a single epistolary document either of his own or of his lady's.

Hitherto the evidence of Sir Isaac's Scottish descent has been derived chiefly from his conversation with Mr. James Gregory; but I am enabled to corroborate this evidence by the following information, derived, as will be seen, from the family of the Newtons of Newton. Among various memoranda in the handwriting of Professor Robison, who proposed to write the life of Sir Isaac, are the following:—

"1st, Lord Henderland informed me in a letter, dated March 1794, that he had heard from his infancy that Sir Isaac considered himself as descended from the family of Newton of Newton. This he heard from his uncle Richard Newton of Newton (who was third son of Lord William Hay of Newhall.)" "He said that Sir Isaac wrote to Scotland to learn whether any descendants of that family remained, and this (it was thought) with the view to leave some of his fortune to the family possessing the estate with the title of baronet. Mr. Newton not having this honour, and being a shy man, did not encourage the correspondence, because he did not consider *himself* as of kin to Sir Isaac," &c.

"2d, Information communicated to me by Hay Newton, Esq. of that Ilk, 18th August 1800."

"The late Sir Richard Newton of Newton, Bart., chief of that name, having no male children, settled the estate and barony of Newton in East Lothian county upon his relation, Richard Hay Newton, Esq., son of Lord William Hay."[1]—"It cannot be discovered how long the family of Newton have been in possession of the barony, there being no tradition concerning that circumstance further than that they came originally from England at a very distant period, and settled on these lands."—"The celebrated Sir Isaac Newton was a distant relation of the family, and corresponded with the last baronet, the above-mentioned Sir Richard Newton."

In writing to James Watt on the 3d May 1797, Professor Robison says,—"I believe I told you that I had been on the hunt to find documents of Sir Isaac Newton's Scotch extraction, and that he himself firmly believed that his grandfather was a younger son of Sir —— Newton of that Ilk, in East Lothian, and wrote to the last man of the family requesting information whether some of the younger sons did not attend James VI. when he succeeded to the Crown of England? I am still in hopes of finding that letter."[2]

The preceding documents furnish the most complete evidence that the conversation respecting Sir Isaac Newton's family took place between him and Mr. Gregory; and the testimony of Lord Henderland proves that his own uncle, Richard Newton of Newton, the immediate successor of Sir Richard Newton, with whom Sir Isaac corresponded, was perfectly confident that such a correspondence took place.

All these circumstances prove that Sir Isaac Newton could not trace his pedigree with any certainty beyond his grandfather, and that there were two different traditions in his family, one which referred his descent to John Newton of Westby, and the other to a gentleman of East Lothian who accompanied King James VI. to England. In the first of these traditions he seems to have placed most confidence in 1705, when he drew out his traditionary pedigree; but as the conversation with Professor James Gregory respecting his Scotch extraction took place *twenty years* afterwards, namely, between 1725 and 1727, it is probable that he had discovered the incorrectness of his first opinions, or at least was disposed to attach more importance to the other tradition respecting his descent from a Scotch family.

In the letter addressed to me by George Chalmers, I find the following observations respecting the immediate relations of Sir Isaac:—"The Newtons of Woolsthorpe," says he, "who were merely yeomen farmers, were not by any means opulent. The son of Sir Isaac's father's brother was a carpenter called John. He was afterwards appointed gamekeeper to Sir

[1] This entail was executed in 1724, a year or two before Sir Richard's death.

[2] *Origin and Progress of the Mechanical Inventions of* JAMES WATT. By James Patrick Muirhead, Esq., A.M. Vol. ii p. 252. Lond. 1854.

Isaac, as lord of the manor, and died at the age of sixty in 1725. This John had a son John, who was Sir Isaac's second cousin, and who became possessed of the whole land estates at and near Woolsthorpe, which belonged to the great Newton, as his heir-at-law. John became a worthless and dissolute person, who very soon wasted this ancient patrimony, and, falling down with a tobacco-pipe in his mouth when he was drunk, it broke in his throat, and put an end to his life at the age of thirty years, in 1737."[1]

The following account of Sir Isaac's heir-at-law is given by the Rev. Mr. Mason, Rector of Colsterworth, in a letter to Mr. Conduitt, dated March 23, 1727, three days after Newton's death :—"This morning I received from you the melancholy news of that truly great and good gentleman's death, Sir Is. N. I have, according to your desire, made Sir Isaac's heir and representative, who is the bearer of this, acquainted with it, but, God knows, a poor representative of so great a man ; but this is a case that often happens. There are two families of the Newtons in this parish, both descended from the second and third brothers of Sir Isaac's father. The second brother was Robert Newton, from whom the bearer of this, John Newton, is descended. The third was Richard, from whom descends Robert Newton, now living in this parish, so that, without dispute, John Newton, the bearer, is heir to the estate not devised by will."

No. XXXII.

(*Referred to in page* 335.)

LETTER FROM SIR ISAAC NEWTON TO A FRIEND.

"Sr,

"Before I received yors I had an account from Mr. Parish of ye arbitration, and thereupon wrote to Mr. Parkins to know how ye indentures run, and to Mr. Storer, to know distinctly what it is that his son Oliver deposes. I had a speedy answer from Mr. Parkins, whereby I understand that Mr. Storer is bound to leave all things in a tenantable repair, by a clause which you do not mention; but from Mr. Storer I have not yet received an answer, and therefore cannot write to you what I designed for putting an end to these differences.

"When I met Mr. Storer and his sons at Wolstrope, yt is at Lady-day last, I was satisfied with the removal of ye wheat hoval and with ye thatch of ye houses in view, as I went up ye yard to ye house. I do not say yt there was no faults, for I am short-sighted, and did not (yt I remember) go close to ye barn, not being then minded to call Mr. Storer to a strickt account for repairs. Thence we went into ye orchard, and I was pleased with ye repairs of ye slated house, but told Mr. Storer's sons yt he was an

[1] See this volume, page 335, *note* 2.

ill husband with y^e^ drain below, and he promised it should be scoured. Then turning to Robin's house I pointed to two very faulty places in the thatch, and Mr. Storer's son confessed it rained in, and promised it should be mended. Thence I went into y^e^ dwelling-house to receive Mr. Storer's rent, and when he was going to pay it he told me y^t^ his son found boards for y^e^ gutters of y^e^ Lucome windows w^h^ I was to pay for, but y^e^ bill was lost, and so desired y^t^ I would allow 30^s^ for these boards. After some words, I put it to him whether he could honestly affirm y^t^ y^e^ boards were worth so much. He answered he could not, but he hoped I would not stand with him for a small matter. To w^ch^ I presently answered y^t^ I would not stand with him, and so remitted 30^s^ of his rent on account of y^e^ bill w^ch^ he said was lost. About a fortnight after coming to Colsterworth, I was three or four times at Wolstrope, and one of those times going into y^e^ garden I found y^e^ walls ruinous, and in going through y^e^ pales between y^e^ garden and y^e^ house, I observed y^t^ they and y^e^ great gates were much out of order. At y^t^ time also y^e^ pales were wanting to y^e^ swine-coat and some of y^e^ long pales pluck^t^ off from y^e^ cow-house. At y^t^ time I heard also y^t^ they had carried away y^e^ fence from y^e^ new quick in y^e^ clay-field, and made money of it. Mr. Storer represents y^t^ y^e^ hedge was decayed and grown useless before; but this is to excuse one fault with another, for Mr. Storer was to keep it in repair, I paying for y^e^ wood. After I understood these things, I was called out of y^e^ country before I could speak with Mr. Storer, and afterwards, in hay time, I had notice y^t^ y^e^ Linghouse was ruinous, for want of repair, and that Mr. Storer's son refused to repair it. Soon after a friend viewed the tenements, and sent me an acc^t^ of those things out of repair wh^h^ I had observed, and some other things also wh^h^ I had not noted. And at that time, or some time after, I understood that Mr. Storer's son refused absolutely to do any repairs, and had treated Will. Cottam with ill language about it. Whereupon, considering that they had not repaired Robin's house, and left divers other things out of repair, and that Mr. Storer's son, living w^t^ his father, and being his father's agent, c^d^ not persist in a refusal of repairs, with^t^ his father's knowledge and encouragement, I resolved to call the father to a general acc^t^ for repairs, wh^h^ c^d^ not be done but by suit, and because the son was concerned in the aforesaid hedge, I resolved to sue them both, and this the rather because his son had disparaged the living at Lady-day in my hearing, I being of opinion that he did it as well behind my back as before my face, to hinder me of tenants who might put me upon calling them to account for repairs. This was the occasion of the suit which I tell you, that you may understand I was not rash in beginning it, as Mr. Storer endeavours to persuade his friends.

"I hear 'tis represented I sh^d^ be well pleased w^t^ repairs at Lady-day, and allow Mr. Storer 30^s^ on that acc^t^, and say that things were better in repair than when Mr. Burch left them. But I have told you that the 30^s^ was in discharge of a bill, and respected only the slating of the house, wh^h^ was done at my charges, and if I was pleased with what I had repaired, what is that to Mr. Storer? Because I eased [him] of repairs of the side of the house, there is the more reason that he sh^d^ leave other things in good repair. He was indeed at the charge of carriages, but that was a bargain, and I have, on the other hand, allowed him 30^s^ for boards, wh^h^ perhaps were not worth half the money. And if I was kind to him in that, he is very disingenuous to turn it to my disadvantage. For this is to snap me by the fingers for giving him bread.

"Whether I said that things were left better in repair by Mr. Storer than by Mr. Burch I do not remember, and if it be understood generally, it's manifestly false. For I c[d] not say so of Robin's house, because I complained of its being out of repair, nor of the garden walls, because I had not then viewed them, nor of the gates and pales, because I did not see any repairs of late done to them, nor c[d] I say so of the repairs of anything for wh[h] I now sue. But of the slated house, and, if you please, of all the houses taken one with another, I might, and do now say, that they were better in repair when Mr. Storer left than when he entered. But then I add, that this is nothing to Mr. Storer's purpose, for 'tis my charge of 11[lb] 10[s] in slating, which makes amends for all the rest. And if I have repaired the main building substantially, that must not excuse Mr. Storer from repairing what belongs to his own share. So you see that what Mr. Storer alleges himself amounts to nothing. In short, as I did not begin this suit without just occasion, so now I have begun it I do not intend to end it without satisfaction. If Mr. Storer will send me a satisfactory answer to my last, I'll endeavour to make a final end in my next, but if he goes on to misrepresent things, I'll solicit Mr. Parish to give you another meeting. I thank you for undertaking the office of an arbitrator, and that you may inherit the blessing promised to peace-makers, is the hearty wish of—

"CAMBRIDGE, *Jan.* 11*th*, $8\frac{7}{8}$."

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EDINBURGH: T. CONSTABLE,
PRINTER TO THE QUEEN, AND TO THE UNIVERSITY.