wall to have any relevance to centrifugal force and produced one of the great suggestive sayings in the history of physics: "Newton's experiment with the rotating vessel of water simply informs us, that the relative rotation of the water with respect to the sides of the vessel produces *no* noticeable centrifugal forces .... No one is competent to say how the experiment would turn out if the sides of the vessel increased in thickness and mass till they were ultimately several leagues thick." Given the effect of this remark – and the whole absolute–relative debate that Descartes initiated – on Einstein, it may not be too fanciful to suppose that if the Inquisition had condemned Galileo a few months later, and Descartes had published *Le Monde*, Newton might never have thought of the bucket nor Einstein of general relativity!

Let me conclude with a remark about Bishop Berkeley, who in *De Motu* (1721) comments that in empty space motion of two globes around a common center cannot be conceived by the imagination, but that if we "suppose that the sky of the fixed stars is created; suddenly from the conception of the approach of the globes to different parts of that sky the motion will be conceived." For this remark, Berkeley is often credited with having been a true precursor of Mach. Note, however, Berkeley's phrase 'fixed stars.' The stars were still *very fixed* in his mind, as we see from his earlier *Principles of Human Knowledge* (1710, §114):

> Philosophers who have a greater extent of thought, and juster notions of the system of things, discover even the earth itself to be moved. In order therefore to fix their notions, they seem to conceive the corporeal world as finite, and the utmost unmoved walls or shell thereof to be the place, whereby they estimate true motions. If we sound our own conceptions, I believe we may find all the absolute motion we can frame an idea of, to be at bottom no other than relative motion thus defined.

Thus, Berkeley looked *backward* to Kepler and Copernicus just as much as he looked *forward* to Mach. He never confronted the real problem of both Newton and Mach – the definition of determinate velocities if "the heavens began to move and the stars swarmed in confusion" (cf. p. 222).

But the exhortation to "sound our own conceptions" cannot be bettered at the start of our journey to the distant goal of quantum gravity – and perhaps even more remote consensus on Mach's Principle. The references are to my *The Discovery of Dynamics*, cited on p. 5.

Mach's Principle before Einstein

John D. Norton

1. Introduction

The doctrine of the relativity of motion is attractive for its simplicity. According to it, the assertion that a body moves can mean nothing more than that it moves with respect to other bodies. Acceleration has long proved to be the stumbling block for the doctrine, for, in the case of acceleration, the simplest of observations seem to contradict the doctrine. When a test body rotates, for example, it is acted upon by centrifugal forces. The presence of these centrifugal forces seems to be completely independent of whether the test body rotates with respect to bodies immediately surrounding. Thus Newton observed in his famous bucket experiment that these centrifugal forces induced a concavity in the surface of a rotating body of water and did so independently of whether the water rotated with respect to the bucket containing the water. Therefore, using these inertial forces as a marker to indicate whether the body is accelerating, it seems possible to know that a body is accelerating without any concern for whether it accelerates with respect to the other bodies around it. This outcome contradicts the doctrine of the relativity of motion as applied to acceleration.

For about a century now, the most popular escape from this unwelcome refutation has been the following simple idea. Relativists point out that experiments such as Newton's reveal only that inertial forces are not noticeably related to motion with respect to *nearby* bodies. That, however, does not rule out the possibility that inertial forces are caused by acceleration with respect to more distant bodies. If this were the case, then inertial forces would not reveal an absolute acceleration but merely an acceleration relative to these distant masses. The core idea is that the inertial forces acting on an accelerating body arise from an interaction between that body and other bodies. The idea is not so much a proposal of a definite, new physical law; rather it is the prescription

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that such a law should be found. The law recommended is only loosely circumscribed. It must be such that more distant masses play the decisive role in fixing the inertial forces on a given body, for example.

The proposal's most prominent sponsor was Albert Einstein. In the early years of his work on general relativity, he believed that his theory implemented the proposal, although he completely lost this belief in his later years. Nonetheless, the future of the proposal was guaranteed by the vigorous support of an Einstein who rapidly rose to celebrity status both inside and outside the scientific community. Einstein did not claim the proposal as his own invention. From the earliest moments, he attributed it to Ernst Mach and in 1918 gave a field theoretic formulation of the proposal its now standard name of 'Mach's Principle.' (Einstein 1918).[2]

The story of the role of the principle in Einstein's work, his enchantment with it, and his subsequent disenchantment, has been frequently told because of its enormous importance in the historical development of relativity theory and relativistic cosmology. My purpose in this paper is to explore another side of Mach's Principle, its earliest years prior to its adoption by Einstein, which so profoundly redirected and ruled its future. I will ask: What role did the principle play in Mach's own system? How was it received by Mach's contemporaries? In answering these questions, we shall find a story that is a little different from the one we might expect. With Mach now universally acclaimed as the patron of a growing literature on Mach's Principle and Machian theories, one expects to find in Mach's writings a penetrating voice of prescient clarity that easily transcends the generations that separate us from him. Instead we shall find:

- Mach's own writings that pertain to the principle were vague and ambiguous, bordering on the contradictory. The principle is never clearly stated, but at best obliquely suggested, and it remains unclear whether Mach endorsed the suggestion or condemned it as unscientific.
- It was Mach's disciples and his contemporary and later readers who extracted an unequivocal proposal from his writings. Several even claimed the idea independently of Mach.
- Mach's Principle proved to be an idea that fascinated Einstein so much that he sought to build his general theory of relativity around it. However he was in a minority in his fascination.
- Prior to the advent of general relativity, the principle was a fringe idea, often opposed by those who would become Einstein's most ardent supporters. The philosophical community was largely uninterested in the proposal. As an empirical proposal, it had no foundations because of the failure of every experimental test actually tried. As a product of philosophical analysis, it smacked of *a priori* physics.

In Sec. 2 of this paper, I will pose the question of precisely what it is that Mach proposed concerning the origin of inertia. In Sec. 3, I will argue that cases can be mounted for each of two plausible answers. In Sec. 4, I will offer a reconciliation. In Secs. 5 and 6, I will assess the broader reaction to the proposal, considering both the favorable and unfavorable responses.

Although use of the term 'Mach's Principle' is anachronistic in much of the time period under consideration, I will use the term here for lack of anything better. Over the years it has come to label a proliferation of different ideas. Here I will understand it to refer to the proposal that the inertia of a body is caused entirely by an interaction with other bodies.

2. What Mach Actually Said

In his first published reference to the principle he attributed to Mach, Einstein (1912, p. 39) formulated it as "...the entire inertia of a point mass is the effect of the presence of all other masses, deriving from a kind of interaction with the latter." A footnote appended to this sentence announced its origin:

> This is exactly the point of view which E. Mach urged in his acute investigations on the subject. (E. Mach, The Development of the Principles of Dynamics. Second Chapter. Newton's Views of Time, Space and Motion.)

The attribution is deliberate and unequivocal. Einstein, who is notorious for the infrequency of citation in his writings, is carefully naming a section of the second chapter of Mach's celebrated *The Science of Mechanics: A Critical and Historical Account of Its Development* (Mach, 1960).

Readers who turn to the relevant section of *The Science of Mechanics*, a critique of Newton's notions of absolute time, space, and motion, will find many assertions reminiscent of the principle Einstein enunciated. But nowhere will they find it stated without distracting qualification or ambiguous hesitation. Indeed if the relevant section of Mach's text was intended to state clearly and advocate forcefully the principle Einstein enunciated, then it has failed. Rather, readers of the relevant section find Mach clearly devoting his expository energies to an attack on Newton's conceptions. The assault is based on two of Mach's favorite themes, which are enunciated clearly and repeatedly. These two themes, rather than some forerunner of Mach's Principle, are what readers find as the principal content of this section of *The Science of Mechanics*. The following remarks from this section are typical:

> No one is competent to predicate things about absolute space and absolute motion; they are pure things of thought, pure mental constructs, that cannot be produced in experience. All our principles of mechanics are, as we have shown in detail, experimental knowledge concerning the relative positions and motions of bodies. Even in the provinces in which they are now recognized as valid, they could not, and were not, admitted without previously being subject to experimental tests. No one is warranted in extending these principles beyond the boundaries of experience. In fact, such an extension is meaningless, as no one possesses the requisite knowledge to make use of it. (Mach 1960, pp. 280) ...
>
> When we say that a body K alters its direction and velocity solely through the influence of another body K', we have asserted a conception that it is impossible to come at unless other bodies A, B, C ... are present with reference to which the motion of the body K has been estimated. In reality, therefore, we are simply cognizant of a relation of the body K to A, B, C ... If now we suddenly neglect A, B, C ... and attempt to speak of the deportment of the body K in absolute space, we implicate ourselves in a twofold error. In the first place, we cannot know how K would act in the absence of A, B, C ...; and in the second place, every means would be wanting of forming a judgment of the behavior of K and of putting to the test what we had predicated – which latter therefore would be bereft of all scientific significance. (Mach 1960, p. 281)

These passages recapitulate the two themes. First is the notion that physical science is or ought to aspire simply to provide economical descriptions of experience. Thus elsewhere Mach (1882) had pronounced "Physics is experience, arranged in economical order" (p. 197), and "The goal which it [physical science] has set itself is the *simplest* and *most economical* abstract expression of facts" (p. 207). The second theme is that Newton's absolute space, time, and motion are idle metaphysical excesses that are superfluous to this goal of economical description. Again elsewhere Mach (1872, 1911) had made the point very clearly. All our statements containing the terms 'space' and 'time' are really only statements of the relation of phenomena to phenomena, and the terms could be struck out without affecting the content of the statements. Mach (1872, 1911, pp. 60–61) even gave a prescription for how this striking out might be effected:[3]

> We can eliminate time from every law of nature by putting in its place a phenomenon dependent on the earth's angle of rotation. The same holds of space. We know positions in space by the affections of our retina, of our optical or other measuring apparatus. And our x, y, z in the equations of physics are, indeed, nothing else than convenient names for these affections. Spatial determinations are, therefore, again determinations of phenomena by means of phenomena.

These two themes comprise Mach's attack on Newton's conception. In his *The Science of Mechanics*, Mach now goes to some pains to emphasize the error that one may fall into if one forgets Mach's lesson and takes Newton's absolute space and time too seriously. Talk of motion of a body K in space is really only an abbreviated description of the change of relations between K and other bodies A, B, C .... If we forget that these abbreviated descriptions do depend essentially on these other bodies and try to anticipate the motion of K 'in absolute space,' that is, if these other bodies were not present, then we will illegitimately extend our science beyond its proper domain. The domain of science is experience. We have no experience of the motion of a body in a space devoid of other bodies. Our extension would cease to be science.[4]

These two themes would be the ones that every modern reader would find pursued by Mach with vigor and clarity in his critique of Newton, were it not for the modern obsession of recovering Mach's Principle from Mach's critique. As a result of this obsession, the modern reading of Mach focuses on passages that are certainly highly suggestive, but, in the last analysis, vague and ambiguous. Typical of them is the most quoted of all passages of Mach's critique (1960, p. 284), which I have broken up into three sentences, labeled s_1, s_2, and s_3, for discussion:

> [s_1] Newton's experiment with the rotating vessel of water simply informs us, that the relative rotation of the water with respect to the sides of the vessel produces *no* noticeable centrifugal forces, but that such forces *are* produced by its relative rotation with respect to the mass of the earth and the other celestial bodies.
>
> [s_2] No one is competent to say how the experiment would turn out if the sides of the vessel increased in thickness and mass till they were ultimately several leagues thick.
>
> [s_3] The one experiment only lies before us, and our business is, to bring it into accord with the other facts known to us, and not with the arbitrary fictions of our imagination.

The ambiguity of this famous passage lies in the admissibility of two readings that contradict one another:

First is the reading that returns what we now call Mach's Principle. Sentence s_1 reminds us that, in our search for causes for the centrifugal forces within the bucket, we have overlooked one possibility, the rotation of the water with respect to other bodies. We cannot rule out such a cause, as long as it is a cause that only acts when very large masses are involved. Thus s_2 agrees with Newton that rotation with respect to the walls of the bucket induce no noticeable centrifugal forces. But according to the new physical mechanism conjectured, this would not be so if the walls were substantially increased in mass and size. Sentence s_3 closes by observing that we would never have been tempted with an explanation in terms of absolute space – the "arbitrary fictions of our imagination" – had we recalled that the real business of science is economical description of experience. In this case, the experience is of Newton's experiment and of the other bodies that surround it.

The second reading recalls the two themes of Mach's critique. Since the goal of physical science is economical description of experience, s_1 reminds us of what we should really infer from Newton's experiment. We should conclude merely that there is a correlation between two experiences, the presence of centrifugal forces and rotation with respect to the stars. There is no place for a metaphysical absolute space in such descriptions. Sentence s_2 is a tease to shake the dogmatic belief of a Newtonian. It points out that the Newtonian has inferred far more than what is actually warranted by Newton's experiment. The experiment does not give us enough information to rule out the possibility of an alternative physical theory in which the centrifugal forces are caused by rotation with respect to other bodies. Sentence s_3, however, reaffirms resoundingly that such speculation lies well beyond the compass of science as economical description of experience. This speculation requires us to think of cases in which we do not and cannot have experience: for example, the walls of the bucket enlarged to a thickness of several leagues – "an arbitrary fiction[s] of the imagination" if ever there was one. Therefore Mach will not entertain such speculation.

Thus we have two readings of Mach's famous analysis of Newton's bucket experiment:

- The first escapes Newton's conclusion by proposing a new physical mechanism for the generation of inertial forces that will later be associated with the label 'Mach's Principle.'
- The second effects the escape essentially by insisting that Newton be restricted to describing the experiment only in terms of what is experienced and pointedly condemns as unscientific the proposal of Mach's Principle.

Our task now is to decide which if either is the correct reading.[5] Our resources are Mach's other writings as well as the interpretations of his contemporaries. Unfortunately we shall see that quite strong cases can be mounted for *both* readings. My accusation of the broader ambiguity of Mach's analysis rests on this unhappy fact. I now proceed to develop the case for each reading of Mach's analysis.

3. Mach Escapes Absolute Space by Urging …

3.1. … Mere Redescription. It is clear that a major component of Mach's analysis involved the simple recommendation to redescribe motion in space as experiences that do not invoke the term 'space.' Thus he wrote (1960, pp. 285–86; Mach's emphasis): "When…we say that a body preserves unchanged its direction and velocity *in space*, our assertion is nothing more or less than an abbreviated reference to *the entire universe*."

How are we to decide if in addition to this project of simple redescription Mach is also proposing a new physical mechanism? I shall assume that a proposal for a new physical mechanism must make claims about counterfactual or hypothetical systems, that is, claims about systems which are known not to exist or are not known to exist. Certainly such a proposal cannot approach the proposal of Mach's Principle unless it is prepared to license inferences about such cases as the rotation of a hypothetical bucket with walls several leagues thick or perhaps about the inertial forces induced between two bodies in an otherwise (counterfactually) empty universe.[6]

Under this criterion there would seem to be no possibility that Mach could be proposing a new physical mechanism. For the claim he repeats most in the entire analysis is that we have no business in science speculating about such systems that are beyond our experience. Merely in the passages already quoted above, Mach has made the point *three* times. And it appears elsewhere in his analysis. For example (1960, p. 285):

> The comportment of terrestrial bodies with respect to the earth is reducible to the comportment of the earth with respect to the remote heavenly bodies. If we were to assert that we knew more of moving objects than this their last-mentioned, experimentally given comportment with respect to celestial bodies, we should render ourselves culpable of a falsity.

Or again Mach considers a proposal by C. Neumann, who imagines that a rotating celestial body will still be deformed into oblateness by centrifugal forces even if the other heavenly bodies were absent. Mach (1960, pp. 340–41) insists that this latter assumption is meaningless and objects that one is simply not allowed to assume away these other masses as unimportant when experimenting in thought.[7] But if Mach refuses to allow any consideration of such hypothetical or counterfactual systems, then it is hard to see how he could be proposing a principle that even vaguely resembles the later Mach's Principle. On the contrary he must condemn any such principle as unscientific.

In places Mach does seem to urge a reformulation of the principles of mechanics. He allows for example[8]: "The principles of mechanics can, presumably, be so conceived, that even for relative rotations centrifugal forces arise."

Is Mach suggesting a reconception of mechanics in which the principles are materially changed and a new physical mechanism introduced? Or is the reconception merely a restatement of the same laws in such a way that the idle metaphysical conceptions of space and time are no longer mentioned? We may answer by looking at what Mach proceeds to do. On the pages following, what Mach actually does corresponds to the latter alternative of simple redescription. He seeks ways of restating the law of inertia so that it does not use the term 'space.' This project of redescription proves quite simple for one case (p. 286)

> Bodies very remote from each other, moving with constant direction and velocity with respect to other distant fixed bodies, change their mutual distances proportionately to the time. We may also say, all very remote bodies – all mutual or other forces neglected – alter their mutual distances proportionately to those distances.

Mach then shows (pp. 286–287) how this type of formulation of the law of inertia can be couched in the language of mathematical formulae. The usual form of the principle requires that the acceleration of a body remote from other masses be constant. That is, if the body has absolute spatial coordinates (x, y, z) and the time is t, then

$$\frac{dx^2}{dt^2} = \frac{dy^2}{dt^2} = \frac{dz^2}{dt^2} = 0.$$

Mach's goal is to rewrite the law without the absolute spatial coordinates (x, y, z). To achieve this, he considers the distances $r, r', r'', \ldots$ to the other distant masses $m, m', m'', \ldots$ from the test mass. In place of the spatial coordinates, Mach uses the mass weighted sum of these distances $(\Sigma mr/\Sigma m)$ so that the principle becomes

$$\frac{d^2}{dt^2}\left[\frac{\Sigma mr}{\Sigma m}\right] = 0. \qquad (1)$$

The project is clearly just one of redescription of existing laws and not the proposal of a new mechanism.[9] Indeed Mach soon makes it very clear that his new expression for the principle of inertia is not intended to be applied to cases remote from experience (p. 289):

> It is impossible to say whether the new expression would still represent the true condition of things if the stars were to perform rapid movements among one another. The general experience cannot be constructed from the particular case given us. We must, on the contrary, *wait* until such an experience presents itself.

Thus it is possible to present a collection of Mach quotations that drives towards the conclusion that Mach is not advancing what we now know as Mach's Principle, but condemning it. Is this an example of selective and biased quotation? Apparently not in the view of several of Mach's contemporary readers. C. D. Broad (1916) reviewed the supplement that contained a compendium of Mach's additions to the third English language edition of *The Science of Mechanics*. He reported that he now understood more clearly Mach's ambiguous discussion surrounding Newton's bucket experiment. What he understood in that discussion was not a proposal for a new physical mechanism but merely Mach's strictures about redescription:

> There is also a far clearer statement than before of Mach's much quoted remark (in connection with Newton's bucket) that "the universe is not given to us twice, but only once." It is now clear that Mach's meaning is that the Ptolemaic and the Copernican view are simply different ways of describing precisely the same set of facts, and that therefore there is no real difference between the bucket standing still with the fixed stars rotating and the bucket rotating with the fixed stars standing still. This is clearly a necessary result of the relative view, and it is one that is often overlooked.

Broad's remarks were those of a sympathetic reviewer. Far more significant was the evaluation of Paul Carus. Carus was born in Germany in 1852, received a doctorate from the University of Tübingen in 1876, and emigrated to America. There he began working for the Open Court

Publishing Company, editing its journals *Open Court* and the *Monist*. In particular, Carus became the medium, welcomed heartily by Mach, through which Mach's writings were made available in English to the American audience. Carus found a natural empathy with Mach's views[10] and was able to engage Mach in a huge correspondence spanning almost three decades, one of Mach's largest correspondences.[11] What induced Carus to publish on precisely the question that concerns us was a talk given by Philipp Frank in 1909, "Is There Absolute Motion?," published the following year as Frank (1909). Frank clearly attributed to Mach the proposal of a new physical mechanism to explain inertial forces of the type of Mach's Principle. Carus's discussion (1913, pp. 23–40) contains extensive quotation from Frank's lecture and provides the foundation for his denunciation of the suggestion that Mach was proposing a new physical mechanism:

> Another point where we feel justified in doubting Dr. Frank's exposition is the statement that Mach hypothetically assumes a new law of nature as to the efficacy of masses, besides the law of gravitation. The passage in Mach's writings to which Dr. Frank refers[12] does not (in my opinion) suggest the idea of an additional law of nature according to which the distant fixed stars should exercise a mysterious influence on the Foucault pendulum. We will later on let Mach speak for himself. In our opinion it seems that it would be sufficient to ascribe the rotation of the pendulum to its inertia while the earth revolves round itself, and this takes place in the space in which the earth has its motion, viz., the space of the Milky Way system. The pendulum remains in the plane of oscillation in which it started while the earth turns around underneath. ... There seems to me no need of inventing a new force besides gravitation. The law of inertia seems to explain the Foucault pendulum experiment satisfactorily.

Carus supports his reading of Mach with his own selection of Mach quotes, similar to those discussed here, pointing out that Mach's endeavors are devoted to elimination of the terms 'space' and 'time.'

Carus's published argument is based on widely available published writing of Mach. However, because Carus also enjoyed the privileged view of an extensive correspondence with Mach, it is tempting to conjecture that Carus is also silently drawing on this correspondence or even on discussions with Mach during one of Carus's visits to Mach in 1893 or 1907. Whether such correspondence is still extant will have to be decided by a search of the relevant archives. However, the prospect that any such correspondence existed in 1913 seems slight. If it did exist, Carus would almost certainly have published it to buttress his case. As

editor of the *Monist*, Carus had clearly been eager to publish a letter by Mach (Carus 1906a) on an earlier article by Carus (1906) on Mach's philosophy. He published it with obvious delight, embedding the letter in the pomp of an introduction and afterword by Carus, and retaining its original German "lest it lose many of the fine points in an English translation." (Carus (1913), however, showed no restraint in presenting extensive passages of Frank (1909) in English translation!)

Finally, whatever their differences over whether Mach did propose a new hypothetical law, both agreed that such a proposal is an anomaly in Mach's broader systematic proclamations in which such hypothesis is abhorred. Thus Frank notes (1909, p. 17; trans. Carus 1913, p. 32): "But Mach in this case stands in the opposite camp as in most other cases where his repugnance to all hypothesis has made him a pioneer in the phenomenological direction." And Carus (1913, p. 32) himself, speaking of Frank's broader reading of Mach, writes provocatively "Strange that Mach, with his reluctance to introduce anything hypothetical except what is absolutely indispensable, should range on the side of the theorists...."

3.2. ... a New Physical Mechanism. Or did Mach intend to recommend more than mere description? Did he intend to propose a new physical mechanism for the origin of inertial forces? Once again a case can be made for this possibility and it too rests on quotations from Mach's writings and on his interactions with colleagues and others. However, if the case for this possibility were to rest only on the first part, Mach's writings for publication, then the case would be considerably weaker than the corresponding case for his advocacy of mere redescription. For none of the writings unambiguously *endorses* a proposal for a new mechanism. Worse, it is not clear which of his writings even talks about such a proposal.

In Mach's critique of Newton's conceptions in *The Science of Mechanics* are several much cited remarks that could be taken as suggesting a new physical mechanism. However, precisely because they are rhetorical flourishes, they admit of many interpretations and do not provide a firm foundation for the case. He exclaims (p. 279): "Try to fix Newton's bucket and rotate the heaven of fixed stars and then prove the absence of centrifugal forces."

But could this not simply mean that Mach takes the case of bucket rotating/stars resting to be exactly the same as the case of bucket resting/stars rotating? Then to try to prove the absence of centrifugal forces, as Mach challenges, is obviously futile since the two cases are really just the one case described differently. Indeed the sentences immediately preceding the exclamation are devoted to arguing that the two cases are really one. Again, there is Mach's famous observation on Newton's bucket experiment (p. 284): "No one is competent to say how the experiment would turn out if the sides of the vessel increased in thickness and mass till they were ultimately several leagues thick."

As we saw above, a consistent continuation in Mach's voice would be "And I [Mach] certainly would not dare to speculate on such an unscientific thing!" – this being a plausible reading of what Mach actually says in the next sentence: "The one experiment only lies before us, and our business is, to bring it into accord with the other facts known to us, and not with the arbitrary fictions of our imagination."

Again, Mach concludes the paragraph preceding with an apparently unequivocal recommendation for a new physical mechanism for inertial forces: "The principles of mechanics can, presumably, be so conceived, that even for relative rotations centrifugal forces arise."[8]

However, the appearance is deceptive, for, as we saw above, this reconception might just be referring to a simple redescription such as leads up to Mach's equation (1) above.

More promising are his later remarks that Barbour (1989, p. 692) identifies as "Mach's clearest statement of the ideal of a *seamless dynamics*" such as would arise were he proposing a new mechanism for inertia. Mach writes (p. 296, Mach's emphasis)

> The natural investigator must feel the need of further insight – of knowledge of the *immediate* connections, say, of the masses of the universe. There will hover before him as an ideal an insight into the principles of the whole matter, from which accelerated and inertial motions result in the *same* way. The progress from Kepler's discovery to Newton's law of gravitation, and the impetus given by this to the finding of a physical understanding of the attraction in the manner in which electrical actions at a distance have been treated, may here serve as a model. We must even give rein to the thought that the masses which we see, and by which we by chance orientate ourselves, are perhaps not those which are really decisive. On this account we must not underestimate even experimental ideas like those of Friedländer [(1896)] and Föppl [(1904, 1904a)], even if we do not yet see any immediate result from them.

Once again I do not see that we can rule out the possibility that these remarks refer to Mach's project of redescription. The understanding of (1) is of immediate connection of the masses since the superfluous mediation of 'space' has been eliminated. And was not the progress from Kepler to Newton (in Machian terms) the discovery of a system of laws that yielded a far more economical summary of not just Kepler's astronomical discoveries but much else besides? It is also very possible, however, that Mach is referring to a new physical mechanism for the origin of inertia. For, as we shall see below, Friedlaender (1896) and Föppl (1904a) both clearly consider such a novel mechanism and conduct experiments to detect it.

If this passage does refer to such a novel mechanism, it still provides no evidence that the proposal of such a mechanism *originated* with Mach or that Mach *endorsed* it. The passage in question was added to the seventh German edition of 1912,[13] presumably in response to Friedlaender (1896) and Föppl (1904, 1904a). Since these works already propose a new physical mechanism for inertia, one can hardly say that the proposal originated with Mach's remarks of 1912. Even Mach's vague suggestion of the use of the theory of electricity as a model had been anticipated and in more precise form. Friedlaender (1896, p. 17) had raised the possibility of applying Weber's law of electrodynamics to gravitation in this context, as does Höfler (1900, p. 126), as we shall see below. Worse, Mach's language suggests that whatever he is introducing is novel and goes beyond what was already said in earlier editions. That is, *after* the Machian ideal of purification from meaninglessness has been achieved, there is a new goal, some "further insight," a speculative "ideal." In one sentence, we are invited "even [to] give rein to the thought [*sogar dem Gedanken Raum geben*] that the masses which we see, and by which we by chance orientate ourselves, are perhaps not those which are really decisive." This invitation would hardly be necessary if we had already made space for that thought in the earlier text of the earlier editions. The thought for which we are to make space might even be a distinctly non-Machian one. If the thought is that the decisive bodies are ones we cannot see, then it contradicts Mach's repeated and forceful pronouncements on the primacy of the observable. If the theoretical and experimental work of the Friedlaenders and Föppl is a part of such non-Machian speculation, then Mach can hardly be giving them unreserved endorsement. Indeed the passage quoted above closes with what seems to be a gentle rebuke: "Although the investigator gropes with joy after what he can immediately reach, a glance from time to time into the depths of what is uninvestigated cannot hurt him."

One could read this as a very kind way for Mach to point out to the Friedlaenders and Föppl that he finds their work to have strayed well beyond science, the domain of economical descriptions of experience, into the murky depths of unscientific speculation.[14]

Remarks published by Mach in 1872 [quoted here from Mach

(1911)] support most strongly his advocacy of a new physical mechanism for the origin of inertia – although they are still subject to the same ambiguities. In the appendix, Mach stresses that in referring motions in the law of inertia to space we should never lose sight of the fact that this is really only an abbreviated reference to other bodies. He then begins to discuss how the motion of these other reference bodies might affect the law of inertia, arriving at the following puzzle (p. 78):

> But what would become of the law of inertia if the whole of the heavens began to move and the stars swarmed in confusion? How would we apply it then? How would it have to be expressed then?

It seems clear enough that Mach's puzzle refers to the problem of stating – redescribing – the law of inertia in a form similar to (1), in the awkward case in which the heavenly bodies adopted chaotic motion. How can Mach be sure that an expression in terms of a simple mass weighted sum of distances ($\Sigma mr/\Sigma m$) will be adequate? This seems to be the same problem that Mach discusses in *The Science of Mechanics* (1960, p. 289) (see above). Mach then proceeded to another example, a free body acted upon by an instantaneous couple so that it rotates. He continues (p. 79)

> Here the body makes very strange motions with respect to the celestial bodies. Now do we think that these bodies, without which one cannot describe the motion imagined, are without influence on this motion? Does not that to which one must appeal explicitly or implicitly when one wishes to describe a phenomenon belong to the most essential conditions, to the causal nexus of the phenomenon? The distant heavenly bodies have, in our example, no influence on the acceleration, but they have on the velocity.

The ambiguity of these remarks resides in the unexplained terms 'influence' and 'causal nexus.' What do they mean? What sort of influence is suggested?[15]

Mach then makes the remarks that most strongly suggest that he is seeking a new physical mechanism. He seems to be conjecturing the form of the law that governs it:

> Now, what share has every mass in the determination of direction and velocity in the law of inertia? No definite answer can be given to this by our experiences. We only know that the share of the nearest masses vanishes in comparison with that of the farthest. We could, then, be able completely to make out the facts known to us if, for example, we were to make the simple supposition that all bodies act in the way of determination proportionately to their masses and independently of the distance, or proportionately to the distance, and so on.

This talk of 'share' and 'masses' acting in proportion to their mass and distance might well be a conjecture of some new physical mechanism. However, it can also be read as a part of Mach's project of redescription. As we have seen, Mach gives such a redescription of the law of inertia in terms of the mass weighted sum of distances ($\Sigma mr/\Sigma m$) or its second time derivative $d^2/dt^2(\Sigma mr/\Sigma m)$. The 'share' of each mass m, m', m'' ... in the reformulated law would simply be the magnitude of the term each mass contributes to these sums. The functional dependence of these contributions are then exactly of the type Mach mentions. In the first sum, for example, each mass contributes a term proportional to its mass and to its distance from the test body. And the nearest masses certainly contribute vanishingly small terms in comparison with the remaining masses.

In my reading, one thing makes it clear that Mach intends in this passage to propose only a redescription of the law of inertia and not a new physical mechanism. That is the sentence immediately following the passage quoted above, which closes the paragraph and Mach's discussion: "Another expression would be: In so far as bodies are so distant from one another that they contribute no noticeable acceleration to one another, all distances vary proportionately to one another."

This expression is clearly offered as a variant or, possibly, a special form of the general laws discussed. Yet it is just a redescription of the inertial motion of a collection of noninteracting bodies that avoids mention of space.[16] There is no hint of some new physical mechanism that would enforce the proportional variation of distances.

This discussion is the best evidence in Mach's published writing for his advocacy of a new physical mechanism for the origin of inertial forces. But it does not make a good case. Even in the collective judgments of Mach's sympathetic contemporaries, its intent is unclear. As we have seen, Frank (1909) found it to advocate a new mechanism; Carus (1913) did not. My judgment is also that it is ambiguous, but I think its most natural reading is as a proposal for simple redescription. In my view, this same judgment must also hold of Mach's published corpus on Newton's bucket experiment and the law of inertia. The only unequivocal proposal Mach makes is for a simple redescription of the experiment and the law in a formulation that does not use the term 'space.' It remains unclear whether Mach intended to propose and endorse a new physical mechanism for the origin of inertial forces.

However it is dubious that this verdict represents Mach's real intentions. What speaks loudly against this verdict is that the majority of Mach's contemporaries and confidants understood him to be proposing a new physical mechanism. On this point Carus is in a clear minority. Indeed the view that Mach proposed a new mechanism is a commonplace of the literature from around 1900 and on to the year of Mach's death in 1916. It is mentioned[17] by Friedlaender (1896, p. 9), Höfler (1900, pp. 122-26), Föppl (1904a, p. 383), Frank (1909), Cassirer (1910, p. 177),[18] Petzoldt (1912, p. 1057), Schlick (1915, p. 166), and, of course, Einstein, whose repeated attributions, commencing with Einstein (1912), brought the viewpoint to the broadest audience. If this view were an outright misreading of Mach, then Mach had ample opportunity to correct it. But this correction never came.[19] He even mentioned the work of the Friedlaenders (1896) and Föppl (1904a) in later editions of his *The Science of Mechanics* (1960, p. 296). Surely that is the point at which Mach would issue a correction if both works were misrepresenting his position. Or are the somewhat indirect remarks quoted above ("...a glance ... into the depths of what is uninvestigated...") intended as a gentle rebuke?

It would seem that any corrections that Mach may have issued would have been so gentle as to escape later reporting, or, at least, any reporting of which I am aware. In particular, in a letter of June 25, 1913, Einstein reported to Mach that Einstein's new theory had yielded a new physical mechanism for the origin of inertia and Einstein attributed that idea directly to Mach[20]: "...*inertia* has its origins in a kind of *interaction* of bodies, quite in the sense of your reflections on Newton's bucket experiment."

Yet Einstein's later writings contain no trace of hesitation in continuing this attribution to Mach. Similarly, Frank (1957, p. 153) continues the attribution. Again, in a letter of January 11, 1910 (Blackmore and Hentschel 1985, pp. 66-67) to Mach, Föppl mentions his "treatment of the question of relative motion" - presumably Föppl (1904a). He commented with relief that Mach "at least had no fundamental misgiving [*Bedenken*] to raise against [it]." We might well wonder what Mach did say to evoke such a response!

Fortunately, within Mach's surviving correspondence there is a record of how Mach responded to such attributions. In a letter of September 3, 1904, which contained an enthusiastic response to the fifth German edition of Mach's *The Science of Mechanics*, Petzoldt put to Mach a series of questions and proposals about Mach's ideas on the law of inertia. In particular, he attributed to Mach the idea that the thickened walls of Newton's bucket could induce centrifugal forces and expressed his own doubts on this notion (Blackmore and Hentschel 1985, p. 36, Petzoldt's emphasis):

> I still cannot reconcile myself to your observation (p. 247) on the possible variation of the experiment through the thickening of the bucket walls. With this you still make the appearance of centrifugal forces dependent on the magnitude of the surrounding bodies instead of the (relative) rotations of the bodies. The centrifugal forces are still aroused only through relative rotation against the *locations* of the masses of the earth and the other heavenly bodies. I am inclined, however, very much to the belief, which you also admit, that the heavenly bodies here play only a chance role like the axial rotation of the earth for the determination of temporal processes, and hope for future experiences on the deeper relations of things, without shutting my eyes to your doubt over whether such experiences will ever be accessible to us as people.

Mach's response in a letter of September 18, 1904, is lengthy and unfortunately never actually mentions the walls of Newton's bucket. He does make clear that he dislikes Petzoldt's idea that the *locations* of the masses may be the decisive thing. He objects (Blackmore and Hentschel 1985, p. 39, Mach's emphasis): "A bare, efficacious *location* has been observed by no one."

However, he does clearly leave the impression that Mach's own view of inertia is that it is a matter to be decided by experiment. After explaining that his original thoughts on inertia were formulated before the ascendancy of Faraday's conception of local action and of a medium or material intervening between bodies ("aether, space or whatever it is called"), he continued: (p. 38, Mach's emphasis)

> As long as one attends to bodies alone, one conceives naturally of gravitational processes and inertial motions as determined by them alone or, correspondingly, through other masses. If one now also is not to expect a positive result from the Friedländer fly-wheel experiment,[21] since the mass and velocity of the wheel is too small, then a greatly refined Foucault experiment could still show that a pendulum or gyroscope orients itself not *only* according to the fixed heavenly stars, but also in part is influenced by the earth, which is, after all, a powerful flywheel. However should such an experiment definitely come out negative, that would also be a great gain in insight. ... If I conceive of gravitation as carried through a medium, then I can conceive of the state of this medium still as only determined by the masses of bodies, for the reaction accelerations depend on the masses of the bodies. But if *one* body that is very distant and unaccelerated with respect

> to the others is in motion, then its motion can only be described with reference to the latter. The idea that this motion is *determined* by the latter bodies cannot be dismissed without further ado. In any case, the orientation of the motion through the distant bodies can be a merely *apparent* one. Perhaps the motion is a concern only of the moving body and the medium *alone*. Perhaps each body conducts itself in space like Dirichlet's bodies in a frictionless fluid.[22]

The letter closes with a very brief sketch of an experiment designed to detect the Friedlaenders' effect arising from the earth's motion.

With a response such as this, it is no surprise that Petzoldt (1912, p. 1057) should proceed to attribute to Mach the conjecture that the relative rotation of masses induces centrifugal forces, the same effect sought experimentally by the Friedlaenders and Föppl. However the only definite point that Mach has made is to rule out Petzoldt's proposal with his disparagement of a "bare, efficacious location." His answer strongly suggests that he expects or would welcome a positive outcome of a Friedlaender style experiment. But he has still not positively asserted that he believes that a thickened bucket in Newton's experiment would induce inertial forces – his original passage in *The Science of Mechanics* insists that no one is competent to assert this! And for all our pursuit of Mach's writings, we still do not have a clear statement from Mach that *he* conjectures that the origin of inertia lies solely in an interaction of bodies through some new physical mechanism.

4. A Reconciliation?

This is the puzzle that Mach's writings on inertia pose for us. We must reconcile two facts. Mach's publications contain only a clear advocacy of the view that one ought to *redescribe* Newton's bucket experiment and the law of inertia in such a way that the term 'space' does not arise. If there is a suggestion of a new physical mechanism to explain the origin of inertial forces, then its discussion is vague, and the proposal of a new mechanism might even be condemned as unscientific. On the other hand, Mach must have been aware that the proposal of exactly such a new causal mechanism was routinely attributed to him, but, in spite of ample opportunity, there is no evidence that he ever moved to correct this misattribution – if it did in fact need correcting. In brute form, we are left wondering whether Mach did intend to propose a new mechanism but was simply incompetent in expressing his intention. Or, if he did not intend a new mechanism, we must ask why Mach allowed such widespread misinterpretation of his work.

I can offer two reconciliations, although neither is attractive. The first is that Mach was unwilling to see the need for a new physical mechanism in his system. That is, he was an adherent of the relativist doctrine with respect to motion, which leads to the need for a new mechanism to account for inertial forces. Mach, however, was simply unwilling to embrace this consequence, so willingly embraced by other relativists, and simply tried to avoid committing himself. There is some evidence for this view. It stems from Hugo Dingler, who had been sanctified by an extremely favorable mention from Mach in the penultimate paragraph of his preface to the last edition of *The Science of Mechanics*. He reported in Dingler (1921, p. 157) that Mach's[23]

> ...only salvation [from the problem of centrifugal forces] was to bring the centrifugal appearances into relation with the fixed stars, and, in fact, Mach also accepts this in the last (7th) edition of his *Mechanics* (I cannot really decide how much this was already the case in earlier editions); he was forced to it, even though this also obviously contradicted his sensibilities.

To the last sentence, Dingler appended the crucial footnote

> I thank Herr Dr. Ludwig Mach for the fr[ien]dl[y] communication that this consequence was always "especially tormenting" [*besonders quälend*] for his father, that he knew for a long time of the monstrous conclusions deducible from it, yet did not draw them, but rejected them.

Thus Mach's behavior could be explained by a horror and unwillingness to accept what his system had produced. In this account, his aversion would be so profound that he would be unable to address the horrific consequence squarely in both his writings and in his private correspondence and discussions.

There are two difficulties with this view. First, contrary to Dingler's suggestion, Mach's system offered a perfectly good reason for rejecting a new physical mechanism: It transcended the economical description of experience that was the proper domain of science. With perfect consistency and in clear conscience, Mach could denounce this new mechanism as unscientific, if he disliked it so much, and there would be no need to be tormented. Second, by 1921, Dingler had become an outspoken critic of relativity theory and, as a disciple of Mach, may well have been overeager to seek reasons to remove Mach's support from relativity theory.[24]

A second more plausible reconciliation is the one I favor. It depends on Mach's somewhat idiosyncratic notion of the true nature of causation. In Sec. 3, when seeking to judge whether Mach's proposals advanced beyond mere redescription to a new physical mechanism, I used the criterion that such a mechanism must make claims about counterfactual or hypothetical systems, for that was clearly required if Mach's proposals were to approach what later became Mach's Principle. However Mach's view of physical science as merely economical description of experience rules out exactly such considerations. A causal connection for Mach is merely a functional dependence extracted from experience. He makes this very clear in (Mach 1911, p. 61; Mach's emphasis) when he writes

> The present tendency of physics is to represent every phenomenon as a function of other phenomena and of certain spatial and temporal positions. If, now, we imagine the spatial and temporal positions replaced in the above manner [by phenomena], in the equations in question, we obtain simply *every phenomenon as function of other phenomena.*
>
> *Thus the law of causality is sufficiently characterized by saying that it is the presupposition of the mutual dependence of phenomena.* Certain idle questions, for example, whether the cause precedes or is simultaneous with the effect, then vanish by themselves.
>
> The law of causality is identical with the supposition that between the natural phenomena α, β, γ, δ, ..., ω certain equations subsist.

One cannot overemphasize how different this view is from the common view of causation. Newton's inverse square law of gravity is commonly understood to legislate that the sun causes an acceleration of the earth that varies directly with the inverse square of the distance that separates them. And this is assumed to hold whether the two masses are the sun and earth of our actual universe or a sun and earth in some hypothetical universe devoid of all other matter. As Mach's frequent protestations above show, he does not allow, in general, this assuming away of the other masses of the universe. Now it is not clear whether Mach would want this prohibition to apply in this case. If it does apply, however, then the relevant causal law simply becomes the assertion of the functional relationship between the sun–earth distance and the acceleration of the earth towards the sun that happens to obtain *in our universe alone*.

If we now apply precisely this same thinking to Newton's bucket experiment, we arrive almost verbatim at many of Mach's pronouncements on the experiment and the law of inertia. And we do so without Mach ever proposing the type of new physical mechanism soon to be suggested under the banner of Mach's Principle. If we seek the fundamental causal relation revealed by Newton's bucket experiment, we must recover the functional relation of the actual phenomena – and that is merely

> ... that the relative rotation of the water with respect to the sides of the vessel produces *no* noticeable centrifugal forces, but that such forces *are* produced by its relative rotation with respect to the mass of the earth and the other celestial bodies.

It now follows immediately that, using Mach's definition, the centrifugal forces in the bucket and the mass of the earth and other celestial bodies stand in a causal relation. Speaking loosely, in a way that risks 'idle questions,' we might identify these masses as the cause of the forces. Also, to identify the role that each of the masses play in the functional relation is just to identify their causal role. Mach might well describe this as their 'influence,' a term with obvious causal connotations. Or he might well ask: "What share has every mass in the determination of direction and velocity in the law of inertia?" And if the relevant functional relation is linear in mass, he might well describe the body as 'acting' in proportion to its mass. Further, a result such as (1) appears to non-Machian readers merely to describe a functional relation and nothing more. But to Mach, the very fact that it describes a functional relation between phenomena of our world makes it the statement of a causal relation. Finally, Mach can offer a functional relation such as (1) as the fundamental causal relation pertaining to inertia, that is, the law of inertia, without needing to suggest that this same relation would obtain were the motions of the masses of the universe to be very different. For the functional relation need only obtain for our actual experiences to qualify as a causal relation.[25]

There is an unappealing aspect of this resolution. The resolution rests on the assumption that what Mach meant by causation is very different from what the same term meant for the many proponents of what came to be known as Mach's Principle. Thus, when Einstein wrote to Mach that "*inertia* has its origins in a kind of *interaction* of bodies, quite in the sense of your reflections on Newton's bucket experiment," Einstein's notion of causal interaction extended well beyond the simple functional relations of phenomena. It included relations on hypothetical and counterfactual systems of precisely the type denounced by Mach. What remains unexplained is how Mach could repeatedly allow such misattributions to pass without objection or correction by him.

5. Early Sponsors of Mach's Principle

Whatever may have been Mach's attitude to the principle that came to bear his name, his writing proved to be a continuing inspiration to the advocates of the principle and it prospered under their guidance. Prior to Einstein, the sponsors of the principle formed a scattered group, largely on the fringe of the physics community. Typically, the members of this group thought that the existence of the new physical mechanism was an issue to be settled by experiment.[26] They devoted their energies to devising and executing such experiments – and to the writing of labored but generally inconsequential treatises.

Mach ensured remembrance of two such experiments, those of the Friedlaenders and Föppl, by citing them in his *The Science of Mechanics* (1960, p. 296). The work of the Friedlaenders is described in the short, two-part monograph, Friedlaender (1896). The first part, written by Immanuel Friedlaender, describes how Immanuel's pursuit of the relativity of motion and the problem of centrifugal forces lead him to what we would now call Mach's Principle (p. 14):

> Without knowing that this had already been done by Mach, I have doubted the completeness of these foundations of mechanics for many years now. In particular I have come to the conviction that the appearance of centrifugal forces ought to be explicable also through regular mechanical knowledge [*Erkenntnis*] from the relative motions alone of the systems concerned, without resorting to absolute motion.

In just a few words, Immanuel is able to state clearly the call for a new physical mechanism which would supplement the existing laws of mechanics and explain centrifugal forces in terms of relative motions alone. Yet, ironically, he gives priority for this idea to Mach, even though I have been unable to find a similarly clear formulation of the idea in Mach's writings. Immanuel then proceeded to describe his efforts to detect this mechanism experimentally. He expected that the spinning of a fly wheel would produce forces directed away from its axis through this mechanism, just as the rotation of the heavens about the earth produces centrifugal forces. He proposed to detect these forces with a torsion balance, "the most sensitive of all physical instruments" (p. 15). However, when he sought to carry out these experiments in a rolling mill in Peine in November 1894, this necessary but extreme sensitivity of the

balance proved to be his undoing. His results were inconclusive since he was unable to control disturbing influences. He lamented (p. 16): "A sensitive torsion balance is, however, a tricky instrument and a rolling mill certainly not the most comfortable or most favorable location for precision measurement."

Upon the failure of these experiments, he turned to his brother, Dr. Benedict Friedlaender, who only then informed him of Mach's work. Jointly they developed their ideas, upon which Benedict reported in the second part of the monograph. Immanuel concluded by stating his expectation that the correct formulation of the law of inertia ought to lead to "a unified law" which combined both gravitation and inertia as an action of masses. The idea that this new mechanism be integrated with the law of gravitation is not usually attributed to Mach, but is considered Einstein's innovation. Of course, in the Friedlaenders' hands it was merely speculation, but at least we see that the unification Einstein effected was not so completely unanticipated.

Föppl (1904) described his attempt to perform an improved version of the Foucault pendulum experiment. The purpose of the experiment was to reveal the precise disposition of an inertial system, correcting for the acceleration of the laboratory on the surface of the earth. He explained that "Foucault's pendulum experiment is afflicted with such sources of error that its accuracy leaves much to be desired even with careful execution" (p. 5). Föppl described how his experiment employed a carefully suspended gyroscope. Its precessional motion would reveal the disposition of an inertial frame of reference. Föppl hoped his experiment might decide whether (p. 5): "... the terrestrial phenomena of motion is itself influenced by the rotation of the earth in such as way that, for [these motions], the rotation of the earth does not coincide with that [rotation] with respect to the fixed star heaven."

In other words, Föppl is interested in comparing two reference systems. The first is the reference system of the fixed stars. The second is the inertial reference system in the neighborhood of the earth's surface revealed by the motions of bodies, such as the pendulum of Foucault's experiment. These systems are routinely assumed to coincide. Föppl conjectures that they may not because of "a possible, special influence of the rotation of the earth" (p. 5). In the event, Föppl reported that he could detect no deviation from coincidence within the accuracy of his experiment.

The report of this experiment was communicated to the Munich Academy on February 6. It was not until a further communication of November 5 (Föppl 1904a) that we find what led Föppl to conjecture such a special influence. His inspiration was the work of Mach on the relativity of motion. According to Mach, Föppl reported, an inertial system "obtains its orientation from the masses of the system of the universe in some kind of law governed manner." (p. 383). Föppl later (p. 386) considered the bodies of the universe divided into a large and a small group. An inertial system is determined by the combined group. Therefore, if the larger group is used to define a rest system of reference, the inertial frame will execute some motion in it, such as a rotation. This rotation would appear as Coriolis forces in the rest system of the larger group; they would not be regarded as merely artifacts of calculation but as "physically existing forces exerted by the smaller group on each test point." Föppl then explained that these were the considerations that led to the experiment described in his earlier communication. If one takes the fixed stars as the larger group of bodies and the earth as the smaller, then these forces would be the "special influence of the earth" sought.

If Mach ensured remembrance of the work of the Friedlaenders and Föppl, then Einstein similarly ensured remembrance of the work of Hofmann. In (Einstein 1913, §9), he discussed what he called the "hypothesis of the relativity of inertia," the hypothesis that inertial resistance is merely resistance to acceleration with respect to other bodies. As to the origin of the idea, Einstein wrote

> It is well known that E. Mach, in his history of mechanics, first advanced this point of view with all sharpness and clarity, so that here I can simply refer to his exposition. I refer also to the ingenious pamphlet of the Viennese mathematician W. Hofmann, in which the same point of view is advanced independently.

The work referred to is (Hofmann 1904).[27] The forty three page pamphlet is a wordy and labored defense of the relativity of motion. It seeks to escape the inference from centrifugal forces to absolute acceleration by urging that these forces arise from an interaction with the remaining masses of the universe. Unlike Föppl and the Friedlaenders, Hofmann (pp. 28–30) conjectured a new mechanical law that would lead to this interaction and perhaps this is what attracted the description of 'ingenious' from Einstein. Hofmann considered the standard result of traditional mechanics that the kinetic energy (*die lebendige Kraft*) of a body of mass m moving at velocity v is $mv^2/2$. He found this result unsatisfactory since, in the case of two masses m and M in relative motion, the kinetic energy of m with respect to M is not the same as the kinetic energy of M with respect to m. Therefore Hofmann proposed a new, symmetric law for the kinetic energy L of two bodies of mass m and M in relative motion with speed v and at a distance r

$$L = kMmf(r)v^2, \tag{2}$$

where k is a constant and f some function to be determined. For consistency with known results in mechanics, Hofmann indicated that the kinetic energy of a mass of actual experiment derives contributions from all the masses of the universe according to (2), so that (2), upon integration over all these masses, must yield the familiar $mv^2/2$.

Hofmann's law contains a mechanism in which inertial resistance is resistance to acceleration with respect to other bodies; for, in the case of two masses m and M, an attempt to change the relative velocity v will change the kinetic energy and thus require a force. In the case of a body in relative rotation with respect to the bodies of the rest of the universe, one would expect this same mechanism to yield centrifugal forces.

Hofmann did not develop the technical details and formal consequences of his supposition (2) in any systematic or extensive manner. This task was carried out by Reissner (1914, 1915). Reissner gave the usual attribution to Mach. Curiously, however, he made no mention of Hofmann, even though Hofmann's law (2) is the fundamental supposition upon which Reissner's theory is built. Perhaps we should allow for the possibility that Reissner independently arrived at the same supposition. In any case, the years 1914 and 1915 were not the time to construct a theory embodying the relativity of inertia, for such a theory would have no chance of competing with Einstein's general theory of relativity, whose brilliance came to outshine all competitors. By 1916, Reissner (1916) had turned his attentions to work on the latter theory, developing his celebrated solution of Einstein's field equations.

There is a small puzzle associated with the pamphlet. Einstein attributes its positing of the relativity of inertia as independent of Mach. Certainly the pamphlet itself makes no claim either way; no works by other authors are mentioned, and Mach is never named. However there is sufficient similarity between parts of Mach's and Hofmann's analysis to raise suspicion of an unacknowledged debt by Hofmann to Mach. Hofmann, for example, couches part of his discussion in terms of Newton's bucket experiment. He even proposes consideration of what would happen if the water-filled bucket were surrounded by a very heavy ring which is set into as rapid a motion as possible (p. 32) – close indeed to Mach's suggestion of the thickening of the walls of the bucket. Perhaps Einstein's attribution of independence from Mach derives from the failure of the text of Hofmann (1904) to cite Mach. However, Einstein may also have the claim directly from a meeting with Hofmann, which might have happened during Einstein's visit to Vienna for the 85th *Naturforscherversammlung* in September 1913 - (Einstein 1913) is the text of a lecture he delivered at that meeting. Again, Einstein describes Hofmann as a Viennese mathematician. That information could not be gleaned from the pamphlet alone, which simply described Hofmann as a professor and gave no affiliation.

The work of the Friedlaenders, Föppl, and Hofmann enables us to start to assemble an image of the group working around 1900 on what is to become Mach's Principle. First, the group members are on the fringes of the physics community. Only Föppl has any status in this community.[28] And they are an isolated and fragmented group. None of these authors cites any of the others. Indeed, the work of the Friedlaenders and of Hofmann were published in such obscure vehicles that we are now probably only aware of them because they happened to be cited by Mach and Einstein. In any case they are difficult works to procure. Thus we might well conjecture that the works discussed so far are but a random sample of other similar works which may be unknown because of their obscure vehicles of publication or a failure to publish at all.

This conjecture is confirmed by Höfler's (1900, pp. 122–26) report. He described experiments of which he was aware and which were designed to test the relativity of motion. Höfler knew of the Friedlaenders' experiment and described Mach's remark about the thickening of the walls of Newton's bucket as a thought experiment. In addition, he described an experiment due to Johannesson (1896). The experiment, only incompletely described by Höfler, involved rotation in connection with an oil droplet or sphere. Johannesson's results did not correspond at all with Johannesson's expectations. The design of the experiment seems flawed and Höfler devoted a page-long footnote to conjectures on where the deficiencies of the experiment may have been. He made clear that no positive result came from the experiment. Höfler also described another proposal for an experiment by Herr Dr. Karl Neisser.[29] The proposal involved examining the behavior of a gyroscope in air and in atmospheres of reduced pressure. Neisser, a relativist about motion, somehow managed to infer from this doctrine that the behavior of a gyroscope must at least in part be dependent on the relative rotation of the wheel against the air. Therefore he expected that a spinning gyroscope would lose its stability if enclosed in a chamber from which the air is pumped and that it would fall down like a gyroscope that is not

spinning. Höfler added a remark to the proofs of his volume that Neisser had informed him that he had been able to perform the experiment, but the expected effect had not occurred. Höfler's report confirms that there was more interest around 1900 in what became Mach's Principle. But it would also seem that these further investigations were not as competently executed.

6. Early Critics of Mach's Principle

When Einstein incorporated Mach's Principle into the foundations of his general theory of relativity, he drew it in from these fringes into a new mainstream. In fact, Einstein's work defined what the new mainstream was to be in the physics of space, time, and gravitation and also, as it happened, a new mainstream in philosophy of space and time. Thus the principle enjoyed an enviable prominence. Einstein incorporated the principle or its precursors into most of his accounts of general relativity in the 1910s and 1920s. And, in his hands, the principle acted as midwife at the birth of modern relativistic cosmology. Einstein's efforts to ensure the place of the principle in his theory in 1917 led to his modification of his gravitational field equations and the introduction of the Einstein universe - not to mention the Einstein–de Sitter controversy.[30] The principle also rapidly entered into a popular and semi-popular literature on relativity, written for a wider, popular audience eager to come to grips with Einstein's great revelations. [See, for example, Born (1924, Ch. VII).] Finally the principle came to enjoy the sponsorship of leading philosophers and became a paradigm of the fruitfulness of the interplay of physics and philosophy. Prominent among these sponsors was Hans Reichenbach, leading figure in the logical empiricist movement, whose works in philosophy of space and time would dominate the discipline for several generations. [See (Reichenbach 1928, Sec. 34).]

6.1. Among the Physicists. The rapidity of the principle's rise and its lasting prominence tend to obscure the fact that it ascended only over a considerable if somewhat quiet opposition that persisted throughout this period as a tenacious skepticism towards the principle. That opposition can be located clearly in two areas: among physicists both before and after the advent of general relativity, and among philosophers, both of the neo-Kantian old guard and of the new generation that spawned logical positivism.

Prior to Einstein's championing of the principle, it is difficult to find broad measures of the overall feeling of the physics community concerning it. Little was said in opposition to it. But it was not a strong position which could expect or demand response from its critics, since, as we have seen, support for the principle lay scattered and disorganized in the fringes of the community. Of course, this fact itself indicates a broader lack of support. However, we have two fairly clear expressions of opposition. Toward the end of the first decade of this century, Ernst Mach and Max Planck engaged in a fairly bitter, polemical exchange (Planck 1909, 1910; Mach 1910). At issue was the reality of atoms, defended resolutely by Planck against Mach's skepticism, and the viability of Mach's notions of economy of thought in science and the elimination of metaphysics. As Planck's assault become more bitter, he decided to mention another area of disagreement with Mach, the relativity of motion, even though this was not the focus of their dispute. He wrote (Planck 1910; taken from Blackmore's translation 1992, p. 145 with minor corrections)

> Where Mach attempts to move forward by relying on his theory of knowledge quite often he runs into error.
>
> Here belongs Mach's strenuously fought for but physically entirely useless thought that the relativity of all translational movements also corresponds to a relativity of all rotary movement, that therefore, one cannot decide at all in principle whether the fixed stars rotate around the earth at rest or the Earth rotates around the fixed stars. The equally general and simple principle that in Nature the angular velocity of an infinitely distant body circling a finite, rotating axis cannot possibly possess finite value is therefore for Mach either false or not applicable. According to Mach's mechanics, one is just as bad as the other.
>
> The conceptual errors about physical matters which this unallowable transfer of the principle of the relativity of rotary movements from kinematics into mechanics has already caused, if they were depicted more closely at this point, would lead us too far astray. It therefore naturally follows that Mach's theory cannot possibly account for the immense progress which is intimately associated with the introduction of the Copernican theory – a circumstance which should suffice by itself to put Mach's theory of knowledge into considerable doubt.

The target of Planck's skeptical ridicule is the relativity of all motion. Since this relativity is the motivation for what soon becomes known as Mach's Principle, Planck's scorn would presumably extend to that principle. It might well be the "conceptual errors about physical matters" to which Planck alludes. Frank (1957, p. 153) did report Planck's remark as aimed directly at this principle.

It is tempting to dismiss Planck's intemperate remarks as a petulant outburst. Even if it was, there is no reason to dismiss its basic sentiments as insincere. Whatever its origin, opposition from Max Planck was very serious. Perhaps it reflected a broader consensus. If not, Planck was sufficiently influential that his views could foster such a consensus. Worse, while we now principally remember Planck for his contribution to quantum theory, he was also one of the earliest well-placed supporters of Einstein's special theory of relativity. He energetically threw in his lot and his prestige with Einstein's theory at a time when the theory's author was still a little-known patent clerk with a proclivity for incorporating bizarre philosophy into his physics.[31] Clearly Planck's opposition to a full relativity of motion did not derive from any ill-considered antipathy to the general idea of the relativity of motion.

Philipp Frank was both physicist and philosopher. As physicist, like Planck, he was one of the early group that took up active research in special relativity. With Hermann Rothe, he first discovered one of the most frequently rediscovered results in special relativity – that the group property and requirement of linearity already powerfully constrain the possible transformation laws between inertial coordinate systems: The only viable options remaining are the Galilei transformation or the Lorentz transformations, with c^2 an undetermined factor (Frank and Rothe 1911). This publication, which was not Frank's first on special relativity, introduced the term 'Galilei transformation.' Frank also had very favorable relations with Einstein: Einstein recommended Frank as Einstein's own successor at the German University in Prague and Frank later published a biography of Einstein (Frank 1947). Thus we might well expect that Frank would have been sympathetic to the view that played such a prominent role in Einstein's thinking. Yet the final outcome of Frank's 1909 lecture, discussed above, is a decision *against* the Machian view, which, in Frank's hands, contains Mach's Principle. Frank (1909) attributed to Mach the view that inertia arises through "a formal, new law of nature about the action of masses" (p. 17). This view allows Mach to retain his relativist position and to answer affirmatively to the question of whether the future behavior of a system of bodies is determined solely by their relative motions and not any absolute motion of the entire system. Frank prefers a view intermediate between the relativism of Mach and antirelativism or absolutism. He considers the absolute motion of mechanics merely a special case of relative motion, that is, it is motion relative to 'fundamental bodies' or 'inertial bodies,' such as the fixed heavenly stars. This somewhat tortured, hybrid position enables him to claim establishment of his conclusion, stated in emphasized text (p. 18): "Physical phenomena do not depend only on the relative motion of bodies; this however still does not admit the possibility of the concept of an absolute motion in the philosophical sense."

Whatever may have been the broader feeling about Mach's Principle in the physics community in this early period, one would expect that, after its endorsement by Einstein, the principle would enjoy the broader support of the physics community, at least through the late 1910s and 1920s, the period of the euphoria over Einstein's discovery of general relativity. Of course, it is widely known that at least one member of the astrophysical community dissented. Willem de Sitter was clearly an enthusiastic supporter of Einstein's general theory of relativity. For example, in 1916 and 1917, when relations between the English and German physics communities were stretched by the bitterness of the Great War, de Sitter took upon himself the task of informing his English colleagues of Einstein's new theory by means of a series of communications to the Royal Astronomical Society. At the same time, however, he found himself disputing sharply Einstein's view that his general theory of relativity satisfied the relativity of inertia or what came to be called Mach's Principle. (See Kerszberg 1989 for a recent account of the controversy.)

There is some evidence that a majority in the physics community at this time did not agree with Einstein's view that Mach's Principle, in some suitable form, was one of the fundamental postulates of general relativity. (Einstein (1918) had listed Mach's Principle along with the principle of [general] relativity and of equivalence as the fundamental postulates of general relativity, when he published a carefully worded defense of his view of the foundations of the theory.) This is an outcome of a survey of expositions of general relativity which I recently completed (see Norton 1993, especially Sec. 4.2). Emphasis on Mach's Principle as a fundamental postulate of general relativity tended to be concentrated in popular and semi-popular expositions. Otherwise, most typically for serious textbook expositions, the principle found no place in the accounts of the foundations of the theory, with Einstein's own expositions comprising the major exception. Or the principle may appear later in discussion devoted to the cosmological problem, as in (Pauli 1921). It is difficult to know what to read into this treatment – or lack of treatment – of the principle. It certainly does not rule out the possibility that many of these authors regarded the principle as an uncontroversial consequence of the theory that they simply did not

choose to discuss.

Laue (1921), at least, makes clear that his omission of Mach's Principle was based on reservations concerning the place of the principle in the theory. The goal of Einstein's (1917) famous cosmological paper was to eliminate the need to posit Minkowskian boundary conditions for the metric tensor in general relativity, for Einstein held that such boundary conditions violated the Machian requirement that the inertia of a body be fully determined by other masses alone. His ingenious solution was to abolish spatial infinity by means of the Einstein universe, which became an admissible solution of this field equations after the introduction of the cosmological term. Laue (1921, p. 180) discussed Einstein's proposal in the context of Laue's treatment of Minkowskian boundary conditions:

> According to the fundamental idea of the general theory of relativity, the inertia of a single body should vanish if it is at a sufficient distance from all other masses. For inertia can only be a relational concept, which can be applied only to two or more bodies. ... With the boundary conditions mentioned, however, the inertia [of a single body] continues to exist. Such considerations have led Einstein to the hypothesis of a space which runs back on itself like the surface of a sphere.[32] To us the whole question seems clarified too little physically for us to want to go into the matter. In the following we understand 'infinity' to be regions inside our fixed star system for which the mentioned boundary conditions hold, but which are sufficiently far distant from the bodies of the gravitational field under consideration.

6.2. Among the Philosophers. When it comes to the philosophical community in the period prior to the mid 1910s, it is more difficult to assess the broader view towards what will become Mach's Principle. The principle seems not to have been a major focus of philosophical debate and, for this reason, not to have many supporters or detractors. In 1912, Joseph Petzoldt wrote an article on special relativity and its epistemological connection to relativistic positivism. Because of Petzoldt's close connection and sympathy with Mach and his positivist views and because they had corresponded on precisely this question, we might expect the principle to figure in his article. It is mentioned only briefly in a footnote (p. 1057), and Petzoldt takes no position on it, beyond merely suggesting that further experiments like those of the Friedlaenders and Föppl may settle the question. Perhaps his correspondence with Mach in 1904 had not assuaged the doubts he initially expressed to Mach as quoted above in Sec. 3.2. Frank (1909), in mapping out 'relativist' and 'antirelativist' positions, wrote of the work of Höfler (1900) and more recently Hamel (1909a) as opposing Mach, characterizing their disagreement as a controversy (p. 12) and Höfler as writing a "polemic against Mach's thesis."

However, a reading of the sources Frank cites does not give one the impression of a polemical dispute over the specific question of whether inertia arises from some interaction of accelerated bodies mediated by a new physical mechanism. Hamel [(1909a), and the closely related (1909)] was devoted to developing Hamel's own axiomatic development of mechanics, with the discussion of Mach's views in preliminary surveys of the alternatives. Hamel does not directly address the question of a new physical mechanism for inertia. The closest is a critique of Mach's strictures against absolute space (for example Hamel 1909a, pp. 363–64). Höfler does rehearse lengthy debates over the relativist and absolutist positions. Yet his specific attitude to the possibility of a such a new physical mechanism is very sober and undogmatic. He seems fully prepared to let actual experiment decide. For this reason, presumably, he gave the careful review (discussed above) of experiments designed to detect the mechanism. He then stated his view (or rather buried it in grammar of bewildering complexity!) (pp. 125–26):

> From my point of view I must admit in any case that, in so far as it is allowed at all, or even is ones duty, *before* an experiment, to form ideas over what can reasonably emerge from it, I expect nothing from all such experiments that could become somehow in the future a direct experimental proof for the relativity of rotational motion. I hold this negative expectation not without expressed experiential, even in part experimental foundations. Rather I believe that, f[or] i[nstance], according to the total experiences of mechanics so far, in an axially symmetric* system, such as would be a bucket with miles thick walls in rotation about its geometric axis, no force couples would arise on the water mass inside and therefore according to these mechanical experience so far, it cannot be set into rotation. More precisely: It cannot be set into rotation any more than [a water mass] in a bucket with walls of ordinary thickness, of which we know of course (or at least for the present believe to know), that only the innermost layer is acted upon by friction.

He continued to quote Hertz and Mach to stress the dependence of the question on experiment and the possibility of new experiments overturning the outcomes of old experiments, concluding: "But do I have to give up our current law of inertia, the foundations of our whole present mechanics, for such a 'possibility?'"

It is difficult to fault the good sense of this unadventurous assessment. Let experiment decide, Höfler says. But he notes his skepticism about a positive outcome, since the mechanism sought would have to be quite unlike anything encountered so far in mechanics. The footnote to the word 'symmetric' sought to drive this last point home. Yet, ironically, in the attempt to dismiss them, the footnote ended up anticipating a Machian class of mechanical theories modeled after electrodynamical laws!

> One must at least say that a *geometrically* axially symmetric system is not also *phoronomically* [kinematically] *and dynamically* axially symmetric, even only because it rotates about its axis. But in this case force effects would be ascribed to mass particles propagating in different directions, f[or] i[nstance], antiparallel, and [those effects] should be functions of the direction (and speed?); and also this assumption (an analogy to Weber's electrodynamic hypothesis) is certainly at least suggested by nothing in the current experiences of mechanics and would hardly allow explanation of the current experiences, upon which, after all, the thesis of the relativity of motion depends.

Höfler's work lies in the neo-Kantian mainstream. It is actually an afterword to an edition of Kant's *Metaphysische Anfangsgründe der Naturwissenschaft*, and the two are bound as one volume. Thus it would seem that the neo-Kantians, a dominant force in German language philosophy at this time, had no objection of Kantian principle to the possibility of inertia arising from some new physical mechanism. But that did not guarantee assent from the neo-Kantians. The leading neo-Kantian, Ernst Cassirer (1910, pp. 176–77) attributed to Mach the notion that the fixed stars are "one of the *causative factors* on which the law of inertia is dependent." He felt the view untenable since it amounted to robbing the law of inertia of its status as a law:

> If the truth of the law of inertia depended on the fixed stars as these definite individuals, then it would be logically unintelligible that we could ever think of dropping this connection and going over to another system of reference. The principle of inertia would in this case not be so much a universal principle of the phenomena of motion in general, as rather an assertion concerning definite properties and 'reactions' of a given empirical system of objects; – and how could we expect that the physical properties found in a concrete individual thing could be separated from their real 'subject' and transferred to another? ... [the meaning of principle in this view] corresponds in no way to the meaning and function it has actually fulfilled

in scientific mechanics from the beginning.

While we would not expect unqualified support from neo-Kantians for ideas attributed to Ernst Mach, we would expect such ideas to receive a more sympathetic hearing from members of the Vienna Circle, a discussion group which met in Vienna in the 1920s and out of which the logical positivist movement sprang. Ernst Mach was the spiritual inspiration for the group - Frank (1949, p. 79) called Mach "the real master of the Vienna Circle." Frank himself was one of the longest standing members of the Circle; his early discussion meetings with the mathematician Hans Hahn and economist Otto Neurath starting in 1907 had laid the foundations for the group of the 1920s. Yet as we have seen, Frank (1909) did not endorse the proposal for a new physical mechanism for inertia that he read in Mach's works. This opposition was no longer voiced in Frank's later writings, however. (See, for example, Frank 1947, Ch. 2, §8; 1957, p. 153.)[33]

In 1922, Moritz Schlick was appointed to Ernst Mach's old chair at the University of Vienna, and it was around Schlick that the Vienna Circle organized itself. Thus it is somewhat surprising to discover that the principal burden of Schlick (1915) was to drive a wedge between Mach's analysis of inertia and the treatment given by Einstein in the context of his general theory of relativity.[34] Einstein's approach is praised and Mach's is condemned. Schlick states Mach's escape from Newton's argument in his bucket experiment as follows (p. 166): "Experience does not show us that centrifugal forces do not also arise if the entire fixed heavenly stars were to rotate around it."

Against Mach's view, Schlick levels two objections. The first is aimed at Mach's often stated view that there is no distinction between the cases of the bucket rotating and stars at rest and the case of the bucket at rest and stars rotating, so that (Schlick quoted Mach as saying)

> The experiment [of testing whether rotating stars induce centrifugal forces] cannot be carried out, the idea is completely meaningless, since the two cases do not sensibly differ from one another. I hold the two cases to be the same case and the Newtonian distinction an illusion.

Schlick responds that Mach's proposal is not at all beyond test. He refers to Einstein's work on the relativity of rotation that has led to experimentally testable conclusions. Presumably Schlick means that if rotation relative to the distant stars induces inertial forces, then one would also expect rotation relative to other bodies to induce forces, such

as Einstein (1912, 1913) found in his developing general theory of relativity. For example, a rotating shell of matter induces Coriolis forces within it. Schlick's second objection is (p. 166): "...the assertion that the two cases do not differ sensibly, a *petitio principii*, is evoked by ignoring the difference between kinematic and dynamic ways of consideration."

That is, he objects that one can define motion purely kinematically if one wishes; but that does not ensure that all the physical facts associated with motion are reducible to kinematics. Newton's theory supposes otherwise. It posits the possibility of kinematically identical systems which differ dynamically - for example a rotating and non-rotating body. And the difference between the two is a fact of sense experience (p. 168): "We can also ascertain the absolute rotation of a body, according to the Newtonian view, through muscular sensation, for we will find with its help that centripetal forces are needed for the body to keep its shape and to hold together its parts."

Mach's analysis ironically had turned into an exercise in *a priori* physics (p. 167): "It is curious to observe how sometimes exactly the attempt always to stick with just sensible experience leads to clever, *a priori* postulates, since one forgets that experiences can only be isolated from one another in abstraction."

Schlick proceeded to compare Mach's view with that of Einstein in his general theory of relativity. He asked if Einstein's theory amounted to "a great triumph of Mach's philosophy, since it had asserted the relativity of all motion as necessary." Schlick felt it did not represent such a triumph for three reasons:

> The first reason, which is already completely decisive, is one we have already presented, in that we have showed the arguments that led Mach to his conclusion are completely untenable. If, nevertheless, it turns out to be correct, it would result more from an accidental coincidence than a proper verification. With Mach the conclusion arises as a necessity of thought; with Einstein it is posited as a fundamental assumption of a theory and the decision of how far it may be considered valid is finally still left to experience.

The second objection referred to the lack of general covariance of the then current version of general relativity and to Einstein's belief that a generally covariant theory would be physically uninteresting. Thus Einstein's theory contradicted Mach's view, which required the equivalence of all reference systems. This objection could not stand for long, since, in November 1915, Einstein advanced the final generally covariant version of his theory and retracted his objections to general covariance. (See Norton 1984 for an account of this episode.) The third objection repeated parts of the first: Mach had just had a clever idea; but Einstein had built a theory on it. Schlick however was anxious – if not over anxious – to deprive Mach even of much credit for having a clever idea. He called the idea "very obvious" and explained in a footnote (p. 171):

> In order to show just how obvious the idea is, I might perhaps mention that I had already thought of it as a 6th form boy [Primaner] and in conversations stubbornly defended the assertion following from it that the cause of inertia must be assumed to be an interaction of masses. I was delighted, but not at all surprised, to come across the idea again shortly, when I got to know Mach's *Mechanics*.

It is difficult to overlook the unpleasant, dismissive tone of Schlick's remarks. It is somewhat reminiscent of Planck's tone, as is Schlick's general argument. For Planck was clearly happy to endorse a relativity of inertial motion, which formed the foundation of Einstein's special theory of relativity. He was unable to find kind words for Mach's proposal that this relativity be extended to all motion. Thus we may wonder if it is mere coincidence that Schlick studied physics under Max Planck at the University of Berlin, taking his doctorate in 1904. Is there some kind of unhealthy conspiracy against Mach plotted by the students of Mach's opponents? If one wants to, one can always find fragments of evidence for conspiracies. Laue, too, was an assistant of Planck in Berlin, and Frank was a student of Mach's arch rival, Boltzmann! However, I think there is no weight of evidence for such a conspiracy theory. The opposition of Frank and Laue is mild and mildly stated. It is more compatible with seriously considered disagreement. Schlick, however, was more intemperate. He was not prepared to concede anything to Mach. He closed his paper by noting that particular relativistic assertions made by positivists such as Mach were more likely to be refuted than confirmed by advances in physical science. Moreover, the investigations of Mach or other positivists on the concept of time did not pave the way for Einstein's special theory of relativity. "No one anticipated, f[or] i[nstance], the relativization of simultaneity."[35]

7. Conclusion

Mach presents us with a perplexing puzzle in his analysis of Newton's bucket experiment and the law of inertia. On the one hand, in his publications, the only unequivocal proposal is that we eliminate the odious notion of space by redescribing the relevant experiment and law in a way that does not use the term 'space.' If there is a suggestion of a new physical mechanism that would reach from the distant stars to cause the inertial forces in Newton's bucket, then the proposal is made vaguely and we are left to wonder whether Mach endorses it or condemns it as unscientific. On the other hand, if Mach did not wish to propose a new physical mechanism for the origin of inertia, then, in the course of the final two decades of his life, he passed over numerous opportunities to correct many who publicly attributed such a proposal to him.

I favor the view that Mach's published pronouncements cease to be ambiguous when we recognize that Mach held an extremely restrictive view of causation. Specifically, Mach held a causal relation to be nothing more than a functional relation between actual phenomena and prohibited speculation on hypothetical or counterfactual systems as unscientific. All we are allowed to infer from Newton's bucket experiment is that centrifugal forces arise when there is relative rotation between the water in the bucket and the other bodies of the universe. That alone is the causal relation. We have no license to infer to an absolute motion or even what would happen if (counterfactually) the walls of the bucket were made several leagues thick. This reading exonerates Mach of equivocation, ambiguity and inconsistency in his publications. However, it requires that the proposal of a new physical mechanism, as commonly attributed to Mach, is incorrect, and it leaves unexplained why he failed to correct this frequent misattribution in the final decades of his life.

If Mach did not propose such a mechanism, then at least the proposal was widely attributed to him in the 1890s and 1900s. It was then the focus of work of a scattered and disconnected group of investigators, largely on the fringes of the physics community. August Föppl was perhaps the only member of this group with any standing in the physics community. There is some indication of the proposal arising independently of Mach. Immanuel Friedlaender claimed his own version of the proposal came prior to knowing of Mach's work. Einstein attributed independent introduction of the proposal to Hofmann. Because of their lack of cohesion and because they tended to publish in obscure vehicles, it is likely that the full extent of their work is not now appreciated. The known work tends towards actual experimental test of the mechanism (unlike Mach) and labored but rather inconsequential treatises.

It is difficult to gauge the broader view of the proposal for a physical mechanism to explain inertia, prior to its sponsorship by Einstein. The difficulty is that the proponents of the view were largely on the fringes of the physics community and could not expect or demand a response from the mainstream. At least Max Planck, in 1910, spoke out strongly against Mach's insistence on the relativity of motion, while at the same time energetically supporting the relativity of inertial motion in Einstein's special theory of relativity. In 1909, Philipp Frank also weighed the possibility of a new physical mechanism to explain inertia and decided against it. After Einstein's sponsorship of what became Mach's Principle, the notion was widely celebrated by both physicist and philosopher. It seemed to provide a paradigm of fruitful interaction between the two disciplines. However, in the physics community its celebration tended to be focused in the popular and semi-popular expositions of general relativity. In general, as a review of expositions of general relativity from the 1910s and 1920s shows, the broader physics community did not wish to present Mach's Principle as one of the fundamental postulates of general relativity.

Prior to Einstein's sponsorship, the philosophy community devoted little attention to the proposal. If it was noticed at all, it accrued a mention in passing in the more traditional debates over absolute and relative motion. Criticism offered was sober and, in my view, largely justified. For example, if Mach's proposal was to be construed narrowly as urging the replacing of the Newtonian law of inertia by the observation that free bodies move uniformly with respect to the fixed stars, then Cassirer objected that this was a retrograde step for science, for it replaced a general law by an extremely restrictive description of one case. If Mach's proposal was for a new law, then Höfler felt that its merit was to be settled by experiment. But all experiments so far had yielded no positive results. This was hardly an encouraging foundation for overturning the Newtonian principle of inertia, then one of the most successful of scientific laws. In a similar vein, Schlick complained that Mach was engaged in *a priori* physics, an ironical twist given Mach's emphasis on the supremacy of experience. One could, Schlick noted, define motion purely kinematically. But this by no means guaranteed that the complete physics of moving bodies ought to be determined solely by kinematic relations. Newton had certainly supposed otherwise – and the dynamic effects to which he appealed, inertial forces, were matters of direct experience. The centrifugal forces that distinguish rotation are directly sensed by the muscles. Thus Schlick was at pains to distinguish Mach's view, which legislated *a priori* and seemed uninterested in experimental test, from Einstein's view, in which one constructed a definite theory with definite predictions that could be subject to experimental test.

If there is a moral in the early history of Mach's Principle, it lies exactly in Schlick's last point. As long as the relativity of all motion is posited dogmatically and Mach's Principle derived from it as *a priori* physics, then it is moribund. Its promise lies in the realm of empirical science, in the attempt to draw the doctrine of relativity and Mach's Principle into a physical theory that can be subject to experimental test, where one allows that experience may speak against it.[36] It was Einstein's recognition of this point that enabled him to breathe life into Mach's Principle.

NOTES

[1]I am grateful to John Earman, Peter Galison, and Ulrich Maier for assistance in procuring sources for this paper and to Julian Barbour for helpful discussion.

[2]While Einstein is usually credited with christening the principle, Schlick (1915) had already used the term ["das Machsche Prinzip" (p. 170) and "...des Machsches Postulats" (p. 171)]. It is a little unclear precisely to what Schlick's terms referred. They most likely referred to Mach's general proposal for a relativity of all motion, from which, Schlick noted (p. 171), it follows that "the cause of inertia must be assumed to be an interaction of masses."

[3]In his synopsis of his critique of Newton's ideas, Mach (1960, pp. 303–304) gives another example of how the terms 'space' and 'time' can be eliminated in this case from the fundamental propositions of Newton's mechanics.

[4]Mach (1960, pp. 272–73) gives a similar analysis of time. When we say that some process takes time, this is simply an abbreviated way of saying that the process has a dependence on another thing such as the changing position of the earth as it rotates. If we forget that this is all we may mean, we can fall into the error of thinking of time as an independent entity. In fact time has "neither a practical nor a scientific value" and "It is an idle metaphysical conception."

[5]In recent literature, it has been urged that Mach did not intend to propose a new physical law and merely intended a redescription of Newton's theory that preserved its true empirical content. See, in particular, Strauss (1968). Wahsner and von Borzeszkowski (1988, pp. 602–603) also found Mach's goal merely to be redescription of existing Newtonian theory.

[6]This criterion may contradict Mach's own highly restrictive

pronouncements on causality which do not seem to admit such hypothetical or counterfactual claims. However if we rule out the possibility that Mach did allow such claims in his analysis - possibly in contradiction with his own general view of causality – then it seems to me that we have settled the question in advance of whether Mach actually proposed what we now know as Mach's Principle. (Added in proof) Julian Barbour (this volume, p. 218) has identified a remark found only in early editions of Mach's, *Mechanics*, as employing counterfactuals in the way I require. The remark is suggestive but still contains no positive proposal for a new mechanism. Rather it casts doubt on whether a particle in the set up described would move according to Newtonian prescriptions if the fixed stars were absent or not unvarying. The remark makes no positive claim about how the particle would move in this counterfactual circumstance. It does not even deny outright that the motion will not be Newtonian. It is merely "very questionable." These sentiments fit well with Mach's repeated exhortation that we have no business proclaiming what would happen in situations beyond our experience, such as if there were no fixed stars. All such attempts are dubious.

[7]As almost everywhere, Mach's precise point remains clouded by ambiguity. We cannot assume away these bodies, he says, since we cannot assume that "the universe is without influence on the phenomenon here in question." Is Mach assuming there is some influence? If so, he does not say so. What does Mach mean by 'influence' in any case?

[8]The text is a slightly corrected version of the standard English translation (1960, p. 284) "The principles of mechanics can, indeed, be so conceived, that even for relative rotations centrifugal forces arise." I am grateful to Herbert Pfister for pointing out an error in this standard translation of the *wohl* of Mach's original "Die mechanischen Grundsätze können also wohl so gefasst werden, dass auch für Relativdrehungen Zentrifugalkräfte sich ergeben."

[9]Mach continues in a similar vein, using mass weighted sums of distances, to treat the motion of two bodies which do exert a force upon each other.

[10]Carus (1906, p. 332) calls Mach a "kindred spirit" and is "proud to count him among my dearest personal friends," although "there are no doubt differences between Mach's views and mine."

[11]For more details see (Holton 1992, pp. 30–33) and (Thiele 1971).

[12]He refers to a passage from Mach (1911) which will be discussed below.

[13]See Mach (1915, p. 44).

[14]These passages do not exhaust the relevant passages from *The Science of Mechanics*, although I have found none that provide brighter illumination. I leave to readers the task of deciding what Mach intended when he asked of the bodies A, B, C, ... "... whether the part they play is fundamental or collateral" and that "it will be found expedient provisionally to regard all motions as determined by these bodies" (p. 283).

[15]I cannot resist observing that if this consideration is intended to show a Newtonian that the distant masses engage in some causal interaction with the body in question, then it is an extremely odd argument. Under the Newtonian viewpoint, the reason that distant celestial bodies are so valuable for describing the motion of the rotating body is precisely that there are *no* causal interactions between them and the body.

[16](Added in proof) Julian Barbour (this volume, p. 216) doubts this claim. He points out that distances between inertially moving bodies do not in general vary in direct proportion to one another. In support he cites Mach's own equation for the distance between two inertially moving bodies [Barbour's equation (1)]. I do not find the situation so unequivocal. Since a is constant and $|dr/dt| \leq a$, the equation does give the stated linear dependence in the limit in which r becomes sufficiently large. Mach's words are all too few, but he is considering bodies separated by great distances. Are these distances great enough to bring us towards this limit? (If not, so that the bodies are close but just not interacting, how can Mach escape this equation, whose derivation requires little more than simple geometry?)

[17]Many of these authors were sufficiently close to Mach to meet or enter into correspondence with him, including Petzoldt, Frank, Föppl, and Einstein.

[18]Cassirer is sufficiently unsure of the attribution to indicate that he infers it from Mach's writing by introducing it as "Mach himself must, according to his whole assumption, regard the fixed stars ... as one of the *causal factors*" and felt the need to support the attribution with a lengthy quotation from Mach (1911).

[19]Mach's celebrated July 1913 renunciation of the role of 'forerunner of relativity' in the preface to his *Optics* (1921, pp. vii–viii) is far too vague to be such a correction, since it clearly refers to Einstein's relativity theory in general. Wolters (1987) also urges that this famous renunciation was forged by Ernst Mach's son Ludwig.

[20]Blackmore and Hentschel 1985, p. 121. Otto Neurath also wrote in about 1915 along similar but vaguer lines to Mach (Blackmore and Hentschel 1985, pp. 150–152), although one might no longer reasonably expect a response from an ill Mach who would die in 1916.

[21]The experiment sought inertial dragging effects in the vicinity of a spinning fly-wheel.

[22]Mach here cites a passage in his *The Science of Mechanics* (1960, p. 283, Mach's emphasis) where he reports the result that "...a rigid body experiences resistance in a frictionless fluid only when its velocity *changes*." He conjectures about the possibility of this result as a "primitive fact," introduced prior to the notion of inertia, were our world filled with some hypothetical, frictionless medium, which would be an alternative to "the forlorn idea of absolute space."

[23]Dingler here raises the possibility that Mach's position on this matter may have altered considerably through the years 1883–1912 of the various editions of *The Science of Mechanics*. I have been unable to check this possibility thoroughly. However the task of comparison has been eased considerably by a remarkable and unusual volume (Mach 1915). This volume contains, in

English translation, a compendium of the extensive additions and alterations made in preparation of the 7th German edition of the work. It is interesting to speculate why such a compendium, useless without the earlier volume, should be published at all, rather than simply publishing a complete, updated text. In any case, in examining the volume, I could see no evidence of a significant shift in Mach's viewpoint with respect to the matters at issue here.

[24]Recall also that, on Wolters's (1987) account, Ludwig Mach was hardly a reliable source for his father's views pertaining to relativity theory, since Wolters accuses Ludwig of forging his father's famous renunciation of his role as 'forerunner of relativity theory.'

[25]It is helpful to compare the analysis of Newton's bucket experiment under Mach's view of causation and under a view that leads to some version of Mach's Principle. In both, we arrive at the result that the centrifugal forces in the bucket arise from the rotation of the water relative to the distant masses *A*, *B*, *C*, ... Mach requires that we halt analysis at this point. The other view makes the assumption, decried by Mach, that this one relation can be decomposed into parts. It regards the interaction between the water and the masses *A*, *B*, *C*, ... as the compounding of many smaller interactions between the water and mass *A*, between the water and mass *B*, ... These smaller interactions are understood to obtain in the circumstance in which we have a universe devoid of all matter excepting the water and mass *A*, etc.

[26]This emphasis is quite different from Mach's. He seems less interested in experimental tests. His *The Science of Mechanics* only mentions the possibility of real experimental test in later editions in response to the experiments of the Friedlaenders and Föppl and does so in an equivocal way. He did however propose an experiment to Petzoldt in their correspondence of 1904, as we have seen.

[27]I am grateful to the editors of *The Collected Papers of Albert Einstein* (Draft of 1992) for determining that this was the work referred to by Einstein.

[28]For example, he was the author of (Föppl 1894), one of the most important German language introductions to Maxwell's electrodynamics, and first of the famous series. Many German physicists learned vector analysis from its self-contained exposition of vector analysis.

[29]Neisser is identified as one of the 'Teilnehmer des Kant-Maxwell-Collegs' and the only reference given is to a conference in 1893 on the question "Is absolute motion, if not discernible, at least conceivable?" at the philosophical society of the University of Vienna.

[30]For discussion of the role of the relativity of inertia and Mach's Principle in Einstein's accounts of the foundations of general relativity and of Einstein's later disenchantment with the principle, see (Norton 1993, Sec. 3).

[31]Planck had entered into an encouraging correspondence with Einstein by 1906. He had given a colloquium on the theory in Berlin in the fall of 1905 and encouraged work on the theory, supervising von Mosengeil's doctoral thesis on the theory. Planck also is believed to be the one that approved Einstein's 1905 special relativity paper for publication in *Annalen der Physik* (Miller 1981, p. 2). His immensely important paper (Planck 1908) on relativistic dynamics is credited by Pais (1982, p. 150) as the first paper on relativity authored by someone other than Einstein. See (Stachel 1989, pp. 266–67). Planck's support for Einstein did not wane. He was instrumental in engineering Einstein's move to Planck's own Berlin in 1914.

[32]Laue's footnote merely cites Einstein (1917).

[33]The latter discussion does, however, recapitulate Planck's (1910) objections, but proceeds to allow that Einstein's work eventually vindicated Mach's view.

[34]The same viewpoint is advanced far more briefly and in far more muted voice in (Schlick 1920, pp. 37–40).

[35]Perhaps Schlick might have agreed with Abraham's (1914, p. 520) gibe that Einstein's new theory scarcely fitted Mach's requirement of economy, for it replaces the then standard single gravitational potential with the complication of ten potentials, the components of the metric tensor.

[36]I. Friedlaender's and Föppl's experiments fell short of this goal. While the experiments could in principle reveal a positive effect, a null outcome could not provide a decisive refutation. Since they had no definite theory that fixed the magnitude of the effect, they could not rule out the possibility that a small positive effect lay hidden behind the random error that shrouds all null results.

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Discussion

Nordtvedt: Did you consider all the experiments a failure in the sense that they saw nothing, or did they have problems? Null experiments are good experiments even when they see no effects, and perhaps you were being a bit hard on the experimentalists, particularly Föppl.
Norton: I did not mean to say that the experiments were bungled. Rather what I meant was that the results had to be inconclusive since the experimenters had no idea of the magnitude of the effect sought. Therefore a null result could not eliminate the possibility of a positive effect smaller than their experimental error. Of course the experiments were not uninformative, since they did place an upper bound on the size the effect could have.
Ehlers: I'd like to have your reaction to the following: If one takes the redescription interpretation, then it seems to me that although Mach hinted at possibilities of redescription, he would not have been able to reconstruct the whole body of Newton's theory. Newton was, I think, much more of a mathematical physicist than Mach. Mach was perhaps more an intuitive and empirically oriented physicist. The Newtonian system needs a basis for concepts such as velocities, accelerations, and so on, some framework, relatively to which these concepts are well defined. Even people like Euler struggled with the question: How can you give a meaning to the concept of velocity if you don't have some space to which you refer it? It requires a considerable amount of abstraction to consider velocity as meaningful without absolute space, namely, only have a certain class of inertial frames. So my question is: Could a redescription be given which does not lose an essential part of

the Newtonian system as a quantitative mathematical theory?

The second remark is a comment only. I think if one, as a physicist, compares these two interpretations, namely, the redescription interpretation and the interpretation that one would like to have a new mechanics, then if one cannot decide, as a historian, which of the two interpretations is truer to the text, then I think it matters that physicists are interested in history, not so much because they want to know what has been said by such and such a person but which useful suggestions are contained in earlier works. The second view, namely, looking for a new mechanics, is fruitful and interesting for bringing physics further, whereas the redescription point of view, in that sense, is not of interest. Therefore I feel, even if one cannot decide, that for a physicist the other point of view is more fruitful and interesting.

Norton: Briefly, on the second point, as a historian, I'm fairly constrained by what happens [laughter], at least, I try to be. As a physicist you try to be constrained by the world. If the two can coincide, and I can find useful things happening, all the better, but I have to stick with what was there.

On the first point, I think you can redescribe everything that Newton had in his science without talk of absolute space and time. It's simply a matter of doing what Mach prescribed. You work through Newton's texts replacing every metaphysical claim by a statement of the observational content of the claim. Whether the resulting description will be economical is the real question. And this, I think, is what has always troubled Mach's system. There was a tension between the need for the descriptions to be restricted to observation and for them to be economical. We see this clearly in the case of Mach's skepticism over atoms. We like them since they do provide a very economical systematization of many physical phenomena. But the price of the economy is talk of entities that transcend observation. So it is with spacetime structures; they are unobserved, but, as you point out, they do enable just the systematization we want. In the end, I think this problem was a major part of the transition from the simple positivism of Mach to the logical positivism of those who followed Mach. It was the realization that one cannot be so narrow and restrict all talk to experience. You also need theoretical terms. Then follows the long debate over what to do with these theoretical terms. Are spacetime structures real entities or merely convenient aids to prediction?

Rindler: Did you say Föppl and Friedlaender had no idea of the magnitude of the effect they were looking for? Why didn't they have an idea? In those days there were a number of people who had played with Maxwellian theories of gravitation. Dennis Sciama later pointed out that the Maxwellian type of gravitational theory has various Machian features. My question is, surely somebody before Dennis Sciama must have thought of that. Why isn't it that people used some kind of a Maxwellian estimate for the magnitude of the Machian effects they were looking for before they did those experiments? Of course, this would have totally discouraged them from even trying.

Norton: You're referring, I take it, to the literature in gravitation theory towards the end of the nineteenth century. They were trying to start modeling extra terms for Newton's theory on the basis of electrodynamics. I believe that a Weber-like law was one of them; there are many different variants. However, I did not find any cases of experimentalists using such laws to estimate the magnitude of the effect sought. As you point out, that is odd.

Renn: I think the answer to the question as to why the scientists who were looking around the turn of the century for Machian effects did not come up with precise ideas on the magnitude of these effects can be found in the split of two conceptual traditions, that of mechanics and that of electrodynamics, which I discuss at some length in my contribution [see p. 5]. Without much exaggeration one can say that those interested in electrodynamic theories of gravitation did not link this interest with a critique of mechanics along the lines of Mach and vice versa. A short footnote in the paper of the Friedlaender brothers, referring to a Weberian theory of gravitation, and the work of Einstein are exceptional in establishing the link between these two traditions.

Editorial Note (J.B.B.): The reader may be puzzled by the limited discussion recorded above of the issue raised by Norton of whether Mach truly intended a physically new theory of inertia or merely a redescription of Newtonian theory in relational terms. In fact, there was a fairly extended discussion at Tübingen around the passage by Mach reproduced in its entirety on p. 110 (beginning line 5) and discussed by Norton on pp. 16–17. However, examination of the discussion transcript showed that quite a large proportion of the comments, which were made without benefit of the complete exact text for examination, were either irrelevant or misleading, though Kuchař did make the important point that, irrespective of the physical significance Mach may have read into Eq. (1) on p. 17, the equation itself is mathematically incomplete, since it is a single scalar equation and therefore insufficient to describe either absolute or relative motion (cf. my comments on p. 217). Since the issue of whether Mach merely intended a redescription is discussed in some length in my own contribution (pp. 215–218) and Notes 1 and 2 on p. 230) and both Norton (in his Notes 6 and 16) and von Borzeszkowski and Wahsner (pp. 65–66) have responded to my comments, there seems little point in reproducing here the Tübingen discussion.

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Julian B. Barbour Herbert Pfister
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