Arie Arnon
Jimmy Weinblatt
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Perspectives on Keynesian Economics

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Foreword

The impressive conference on John Maynard Keynes and the collective book that it yielded were financed by a generous grant from The Thomas Guggenheim Foundation. We are deeply grateful to the foundation and in particular to Professor Thomas Guggenheim for the support and for his active participation in the conference.

I would like to take this opportunity to give you a few of my own reflections on John Maynard Keynes.

Since Keynes the Economist and his ideas will be discussed within this volume, I will not venture down the lane of "Keynesian Economics". Nevertheless, I would like to say a few words about Keynes the man, the philosopher, the leader – his impact on human society and perhaps on the course of history – and if there is a lesson we may learn from his achievements.

I would begin by saying that Keynes and Keynesianism, as perceived in the simplest way, remind me of a famous quote from a philosopher and thinker named Antonio Gramsci. He might be paraphrased as advocating that we should switch from the pessimism of reasoning to the optimism of action.

This certainly reflects Keynes's attitude toward economic policy, as he was an optimist and great believer in action. Keynes was the economist who challenged the famous definition of economics as "the dismal science", and transformed it, as Reich (1999) noted, into "a revolutionary engine of social progress".

John Maynard Keynes was cited as amongst Time Magazine's "100 Scientists and Thinkers of Twentieth Century", together with Einstein, Freud, Fleming (who discovered penicillin), Salk (who developed the polio vaccine), the philosopher Wittgenstein, the Wright brothers and others (Reich 1999). In my personal view, Keynes was high on this list.

Keynes was charismatic in presenting his ideas – the fact that he managed to recruit most of the greatest economic minds of the twentieth century as his disciples is testimony to the power of his personality. Among his most notable disciples are John Hicks, Joan Robinson, Richard Kahn, Roy Harrod, James Meade, Alvin Hansen, Paul Samuelson, James Tobin, Robert Solow, and among the "younger generation", Joseph Stiglitz and Paul Krugman – and this is a very partial list. It was noted in an influential print media that Keynes, who died in 1946, " . . . was the twentieth century's most influential economist. His ghost animated governmental

policies for 25 years after World War II". It was further said that "Keynes is enjoying an intellectual revival" (R.J. Samuelson).

Since the beginning of the 1980s, the world witnessed the fall of Keynesianism. It was accompanied by the flourishing of global market activity, especially in financial markets, and by the globalization of ownership and production. The shares of government expenditures decreased, inequalities grew, and strict policies to balance government budgets were implemented. This latter was reflected in U.S. policies, the Maastricht Rules, the recipes of the World Bank and the IMF, and in the formal economic policies of many other countries.

This is accompanied with the fact that in the 1960s economic growth in Western economies averaged 5% per year, and in the 1980s only 2.7%, and Europe had a rate of unemployment of 3% in the 1960s and 11% in the 1980s. This was not attributed to the change of the ruling political economic philosophy that switched from Keynesianism to Monetarism, a change that included a highly focused aim at reducing government involvement in the economy, a rigid striving for balanced budgets, and an indifference to social justice and income distribution. These differences in the rates of unemployment are typically explained as due to a rise of the "natural rate of unemployment" (a Friedmanist concept), which is allegedly due to over-taxation and over-regulation of governments, especially in the labor markets.

However, from 2007 or 2008, due to the financial crisis, we have been witnessing a revival of Keynesianism. People realized that there are dangerous flaws in human nature – possibly in the "Homo Economicus" – that lead to potential severe failures of free markets. One has only to note the abundance of words recently written and verbally expressed on human greed. Some of the most prominent economists have revived Keynesian formulas advocating massive government intervention. These include, among others, Robert Schiller, Paul Krugman, and Joseph Stiglitz. I apologize for not giving a comprehensive list at this time.

Economists and policy makers seem to realize that many of Keynes' ideas that fell into oblivion, both in internal and international economic policies, are both relevant and probably the only efficient recipe to avoid a world catastrophe, a prolongation of a state of deflation, and the potential emergence of extremely undesirable regimes. This resulted in massive government expenditures in the U.S., Europe, and other countries, and significant budget deficits and the re-emergence of forgotten Keynesian ideas such as the need to increase international coordination of fiscal and monetary stimulus through the IMF and the World Bank. These were ideas advocated and expressed by Keynes in Bretton Woods in 1944, and reduced by the American delegation there headed by Harry Dexter White.

Another of Keynes' ideas from Bretton Woods, the institution of an international reserve currency that he called the "Bancor", was raised recently by the Chairman of the Chinese Central Bank. He, more than anybody else, understands the problems and risks of the extensive accumulation of national currencies, namely the dollar, the euro, and the yen in the foreign exchange reserves of countries, thus advocates the switch to an international reserve currency as a substitute.

John Maynard Keynes was a rebel – he diverged from orthodox classical economics to suggest a new paradigm that had as its goal the creation of a better

world. In this sense, he should be a role model to all of us, to all economists at large, and in particular to those in academia who produce and disseminate ideas and concepts that are later used by practioners and policy-makers.

Keynes began his career during World War I as a traditional neoclassical economist, having been a student of the noted Alfred Marshall. Somewhere along the line, he started to deviate from the fundamental notions and implications of classical economics in the realm of laissez faire, and adopted a different approach. In his "Tract on Monetary Reform" (1923), he articulated a conservative monetary policy, even though it expressed views on the need to implement government policies to achieve stability. The belief in the role of government as a stabilizing agent grew during the 1920s and 1930s, and peaked with the publication of Keynes' "The General Theory of Employment, Interest and Money" in 1936.

One of the major lessons that should be learned is that there is a "herd effect" in human thought, which is particularly strong in academia, and in most scientific fields, whereby scholars tend to be prisoners of mainstream concepts and fashions. It requires much courage, strength, and persuasion to deviate from the existing consesus and introduce new and unorthodox ideas. This herd syndrome exists in most of the scientific fields, and also in economics. The potential damage of dogmatically applying ruling concepts – regardless of the circumstances and of their social effects – is in the close link between the prevalent economic thought with policy-making. This might lead societies to undesirable outcomes and conditions.

Keynes (1936, Preface) gave a superb example of originality and independence of thought from which human society, as a whole, has benefitted. He was quite specific in expressing his view on this issue: " . . . the difficulty lies not in the new ideas, but in escaping from the old ones".

Keynes was criticized and accused in a number of fields. He was accused by the Right of being a Communist and Marxist. He was accused by the extreme Left of being a brutal capitalist and totalitarian, an admirer of the Nazis and anti-semitic. I believe that all these allegations were wrong and unjustified. Keynes, who wrote the following words (1933, 21–22), was, in my view, not a Nazi supporter or an anti-semite:

> ". . . To our generation, Einstein has become a double symbol – a symbol of the mind travelling in the cold regions of space, and a symbol of the brave and generous outcast, pure heart and cheerful spirit. . . so it is not an accident that the Nazi lads vent a particular fury against him. He does truly stand for what they most dislike, the opposite of the blond beast – intellectualist, supernationalist, pacifist, inky, plump . . . How should they know the glory of the free-ranging intellect and soft objective sympathy to whom money and violence, drink and blood and pomp, mean absolutely nothing?"

In "The End of Laissez-Faire" (1926, Ch. III) Keynes sarcastically commented on Marxian Socialism: ". . . how a doctrine so illogical and dull can have exercised so powerful and enduring an influence over the minds of men and through them, the events of history . . .". Moreover, In "Essays in Persuasion" (1930, 329), he wrote:

> " . . . The love of money as a possession – as distinguished from the love of money as a means of the enjoyments of life – will be recognised for what it is, a somewhat disgusting

> morbidity, one of those semi-criminal, semi-pathological propensities which one hands over with a shudder to the specialists in mental disease . . ."

These two citations are certainly evidence that Keynes was neither a Marxist nor a brutal Capitalist. Indeed, in "The End to Laissez Faire" (1926, Ch. 5), he also wrote: "for my part, I think that capitalism wisely managed can probably be made more efficient for attaining economic ends than any alternative system in sight". Keynesian ideas evolved into what became the concept of the "mixed economy", and later, at the end of the twentieth and the beginning of the twenty-first centuries, the idea of "public-private enterprise".

John Maynard Keynes was a brilliant and charismatic man, with a legendary charm, supreme intellectual abilities, and a sharp – and at times, painful – discourse. He was adored by his followers as a prophet, and totally rejected by his opponents. Some people claim that he was one of the three most important economists in human history, the greatest and most influential economist in the twentieth century, and possibly in the beginning of the twenty-first century. Others – from both Left and Right – see in him the source of economic troubles, inefficiency, and injustice. Nevertheless, all, opponents and supporters, admire his greatness as a man and as an intellectual.

Bertrand Russell named Keynes one of the most intelligent men he had ever known and said: "Every time I argued with Keynes, I felt that I took my life in my hands, and I seldom emerged without feeling something of a fool" (cited in Hoggard 2008). His great opponent Friedrich von Hayek wrote after his death: "He was the one really great man I ever knew and for whom I had unbounded admiration. The world will be a much poorer place without him" (cited in Skidelsky 2003).

Although a conference in the history of economic thought usually deals with the thought of economists who are already dead, in view of recent events and policies, I believe that the news of the death – and here I mean the intellectual death – of Keynes and Keynesianism was premature.

Beer-Sheva, Israel J. Weinblatt

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Introduction

The years 2009–2011 collectively mark nine decades since Keynes's *Economic Consequences of the Peace* (1919), "four score and seven years" since the appearance of his *Tract* (1923), 80 years since the *Treatise* (1930) and the "platinum anniversary" of the publication of his *General Theory* (1936), tomes which attracted attention and debate then, with the *General Theory* still generating much interest and controversy amongst economists and the public today; that is in a period of emergence from what has been, in retrospect, perhaps the most serious economic shock – financial, nominal and real – since the depression years and their immediate aftermath, 1929–1939.

The present volume is the outcome of a symposium organized by the Thomas Guggenheim Program in the History of Economic Thought held at Ben Gurion University, 14–15 July 2009. This meeting brought together leading economists and historians of economics, and focused upon the relevance of Keynesian economics from the perspective of the current crisis and in historical perspective. The volume is divided into two parts. The first part applies historical and methodological perspectives to the development of Keynesian economics, and assesses lessons that may be drawn from them for the present crisis. The second part of the volume focuses upon the development of models and policy issues characterizing and emanating from Keynesian economics, their relevance for economic analysis today, and the application of the Keynesian framework to the analysis of the current crisis and possible future crises.

Samuel Hollander's paper opens the first part of the volume. He starts by addressing anomalous positions taken by Smith, Marx and Engels, and Keynes: Smith's advocacy of government intervention in the credit market; the appeal of Marx and Engels to the competitive pricing mechanism in certain cases; and

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Editor's Note:
The references to publications in this introduction appear in the respective chapters of the volume.

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what he called the "conundrum" of "Keynes distorted representation of classical macroeconomics despite his keen awareness of the true classical position", that is to say, "Keynes's misrepresentation", in Hollander's view, of Ricardian macroeconomic policy. In the conclusion to his paper, when focusing on the anomaly in Keynes's approach to Ricardo, Hollander asks: "Is that perhaps an alternative suggestion?", to explain what he takes as Keynes "deliberate distortion" regarding the Ricardian position, and his support of the Malthusian position.

In his comments on Hollander's paper, David Laidler shows that Ricardo also overlooked certain issues. For example, as Laidler points out, Ricardo elided certain approaches from his *Principles* (1817), thereby creating a dissonance between that volume and, as Laidler put it, other "down to earth policy commentaries" of Ricardo. Indeed, the "cognitive dissonance" of Keynes on the one hand, and Ricardo on the other, may have played a role both in the conundrum raised by Hollander, and the dissonance pointed out by Laidler.

In her paper, Maria Christina Marcuzzo attempts to draw *lessons* for the recent economic crisis from the philosophical *weltanschuung* underlying Keynes's approach in the *Economic Consequences of the Peace* (1919) – that is to say, his emphasis on *reasonableness* rather than *rationality*. She provides a number of historical examples of what can be called the *rational–reasonable divide*, starting with the Franco-British split at Versailles, through the Anglo-American rift over re-payment of war debt, and up to the decision by the US government *not* to bail-out Lehman Brothers. Marcuzzo further maintains that any "return to Keynes" should not only be based upon demand management policies, so as to prevent world-wide deflation and recession, or the implementation of reforms in the international financial system, but, as she suggests, also on an across-the-board utilization of Keynes's idea of *reasonableness* as against what she sees as the *misplaced* notions of individual and market rationality.

In his comment on the paper, Moshe Justman agrees with the *rational–reasonable* (i.e. Franco-British) divide at Versailles, but questions whether this characterized war-debt repayment. Indeed, he asserts that despite Keynes asking the Americans to be "reasonable" regarding this, the US position, while possibly "unfeeling", was not actually "unreasonable". As for Lehman Brothers, he concludes that while in retrospect, it may have been the *wrong* decision, in light of the uncertainty at the time, this decision could *not* be considered "unreasonable".

Michel De Vroey's paper asserts that those claiming the *General Theory* to flow from Marshallian headwaters actually diminish the theoretical contribution Keynes *thought* he was making. According to him, Marshall had *no* theory of unemployment per-se. Moreover, he shows that this *theoretical* gap was not filled by others, such as by Hicks (1932) and Pigou (1933). In the former case, the result was a "misunderstanding" of Marshall's labor market adjustment mechanism, and a convoluted treatment in the latter. Thus, if one wants to "reinforce the validity of Keynes's argument" as put forth in 1936, "trying to anchor Keynes's theory" in "Marshallian theory" is counterproductive. In his opinion, Keynes did not remove the rigid wage assumption, as he himself, among others, had thought. He praises Keynes, however, for starting "the quest" for a solution to the problem, and

concludes that Keynes was not "wrong"; he was simply unable to construct a rigorous model of his "intuitions".

In his comment on De Vroey's paper, David Colander first distinguishes between Marshallian "models" and "method". Following from this, in his view, while Keynes utilized the Marshallian "method", he did not utilize Marshallian "models" in his "attempt to develop a theory of unemployment". Rather, Keynes's approach was based upon what Colander calls "a complexity vision". He asserts that in taking this path, Keynes moved from the *ceterus paribus* assumption underlying Marshall's partial equilibrium micro-modeling method to a "macro-modeling method" based upon "interdependencies" that could bring on "serious coordination failures". In his view, Keynes "used the Marshallian method, which sees models as aids to intuition". He concludes by saying that while Keynes "did not develop an acceptable scientific theory" of how his "complex system would operate", he still provided "a vision" for those, in Colander's words, "willing to see" it.

In his paper, Mauro Boianovsky shows that Patinkin's *dynamic* interpretation of the disequilibrium nature of involuntary unemployment should be considered as his contribution to Keynesian analysis and its extension. In order to understand Patinkin's views, *all* his work, *both* published and unpublished, must be considered, not only the editions of *Money, Interest and Prices* (1956, 1965, 1989). He points, for example, to a 1957 draft typescript – that became the basis for his later rejoinder to the negative review by Hicks in *Economic Journal*. In this, Patinkin distinguished between *monetary* and *employment* theory as set out Keynes, and asserted that the latter was "the fundamental contribution of Keynesian economics". With regard to Keyne's notion of "effective demand" and employment, Boianovsky maintains that Patinkin made a "plea for a dynamic interpretation" of the theory of employment, that is to say, a "disequilibrium perspective". Taking the story up to the 1989 edition of *Money, Interest and Prices*, he says that rather than counterpoint Keynesian and Monetarist positions, Patinkin attacked key tenets of the "New Classical Macroeconomics", such as rational expectations. He concludes by maintaining that Patinkin did not belong to the "Neo-Classical Synthesis"; rather, as Patinkin himself put it, he was "more Keynesian than Keynes", albeit, a *disequilibrium* "Keynesian economist".

Michel DeVroey asserts that, in his view Boianovsky did not actually answer the question implied by the title of his paper. Following from this, he provides "an outline of the paper" that could have, according to him, been written. He starts by "questioning the Keynesian character" of Patinkin's writings, identifying two reasons for this: advocacy of the real-balance effect, and reading Keynes "through Walrasian rather than Marshallian lenses". According to him, Patinkin exhibited a form of "cognitive dissonance" regarding the slow adjustment process towards equilibrium in conjunction with the Walrasian approach he advocated; the latter taking place, according to DeVroey, in "logical time". He presents his *own* definition of what constitutes a "Keynesian" approach, delineating "two meanings": the first, based on "market failures" dealt with via demand policies; the second, emanating from a "precise conceptual apparatus", in his view, the IS-LM model.

He maintains that Patinkin *was* "Keynesian" with regard to the first, but as he "followed the Walarsian line", *was not*, on the second meaning. He claims that Patinkin was "not alone" in taking this "mixed position", and was actually a "mirror image" to the stance regarding "Keynes's conceptual apparatus", with which, in his view, Friedman "had no qualms"; and this, as against Friedman's "anti-Keynesian" policy stance. He concludes by saying that "modern neo-Keynesian authors" have also been affected by this "cognitive dissonance", albeit not attempting to "attach" their "reasoning to Keynes's".

Amos Witztum addresses a conundrum in his paper, relating to the Keynes–Robbins interaction over "the years of high theory". Witztum is struck by the dissonance of Keynes *not* attempting to *place* his 1936 volume, which was destined – as he put it in a letter to Shaw – to "revolutionise" economic theory, into the framework set out by Robbins, in 1932, and again in 1935, that, in essence, has demarcated the boundaries of "the subject matter of economics" up to the present day. He goes on to show the complex relationship between Keynes's ideas and those of his classical predecessors; this in the context of Keynes's somewhat enigmatic statement, made at the end of his 1936 volume, that once full employment is attained, the classical approach "comes to its own". In his view, given that the 1936 system set out by Keynes was based upon the delineation of "good" and "bad" equilibrium, Witztum assets that this ethical "non-neutrality", among other characteristics, did not seem to enable Keynes's approach to fit into the Robbins taxonomy of what constituted economic "theory" at the time. Despite this, however, Witztum concludes that the "implicit denial" of its place in the science of economics, that characterized Robbins's treatment of Keynes work, *from 1923 till the end of the 1930s*, "was unjustified".

In his comment, Boianovsky takes issue with Witztum's treatment of Robbins's views of Keynes. As in his own paper in this volume – on Patinkin as a Keynesian economist – he advocates utilizing the *overall* body of work of an author, in this case, Robbins, in order to assess the Robbins–Keynes nexus. He maintains that after 1934, and from 1947 onwards, especially in his 1971 *Autobiography*, Robbins was positive on Keynesian employment and demand management policies. Boianovsky *does* recognize the fundamental difference between Keynes's 1936 definition of economics "not as pure science, but as an applied, moral science", as against that of Robbins (1932), which was that economics was a "science". According to Boianovsky, however, Witztum's conclusion that Robbins "implitly denied Keynes's place in the science of economics" – *based solely on Robbins's 1932 approach* – "remains unproven".

In his paper, Russell Boyer deals with some outstanding issues in the history international macroeconomics of "Keynesian" and "Johnsonian" vintage, and disputes the conventional accounts of Harry Johnson's movement from Keynesianism to Monetarism during his tenure at the University of Chicago. In the context of the paper, Boyer attempts to show how Johnson explicitly, and his then Chicago colleague, Robert Mundell, were influenced by Milton Friedman, and *especially* by Friedman's 1953 essay on flexible exchange rates. Boyer brings together, from

a wide variety of sources, what he sees as compelling evidence of the Johnson–Mundell–Friedman nexus, and deals with the evolution of Johnson's view of Keynesianism. He then proceeds to outline, in detail, the transition in Johnson's thought in the context of the origins of the monetary approach to the balance of payments [MABOP]; going on to deal with Mundell's development of the notion of OCA, both, according to Boyer, based upon the *direct influence of the complete version of Friedman's 1953 essay, and not its better known, abridged form, in which,* according to Boyer *both of these ideas were elided*. Boyer also counterpoints the origins of the "Keynesian" view of the MABOP to the "Johnsonian" version, both as set out by Polak, as against Polak's assessment of Mundell's contribution to it. Boyer concludes by saying that, in effect, an intellectual "equilibrium", albeit *unstable*, characterized the Johnson–Mundell–Friedman connection while all three were at Chicago, and afterwards.

In his comment, Dror Goldberg comes to the defense of both Johnson and Mundell, arguing that, at most, they were "negligent and sloppy with regard to citation". He goes on to examine specific issues raised by Boyer, and asserts that Johnson's editing and abridgement of Friedman's 1953 essay, in his opinion, was "extremely reasonable". He continues on to maintain that the very title of Johnson's own paper on flexible exchange rates was a "great honor" that offset, in his view, Johnson's earlier attacks on Friedman. He goes on to justify his defense of Mundell by referring to the preface in *International Economics* where Mundell mentions Friedman, among many others, as having influenced him, seeing this as sufficient evidence of Friedman's impact on Mundell's ideas, in lieu of "exact references". Goldberg concludes by asserting that, according to Johnson, in his 1971 AER paper "The Keynesian Revolution and the Monetarist Counterrevolution", Friedman himself was lax in citation practice; with his own "quantity theory" approach having, in Johnson's view, at least, according to Goldberg having "merely reformulated Keynes's liquidity preference" approach.

In his paper, Warren Young attempts to systematically place the IS-LM approach in the framework of the Keynesian revolution. He focuses upon a number of issues relating to the relationship between the model and the revolution that emanated from Keynes's 1936 tome. In this context, he brings the reader back to an overlooked paper by Hicks, published in French in 1945, in which Hicks distanced himself from what he said in his own 1937 paper, that is to say that the *General Theory* is "the economic theory of depression". He goes on to discuss the equational–diagrammatic system summarizing what Hicks took, *in 1945*, to be representative of "Keynesian ideas", which included sequence analysis. IS-LM is nowhere to be found in Hicks's 1945 paper, except for a reference to his 1937 paper. Young then describes the diagrammatic approach of Klein (1947) and maintains that it is *identical* to that of Hicks's 1945 approach to Keynes. Following from this, he poses the question: why did the *Hicks–Klein* diagram not receive any attention by the economics profession, whereas the *Hicks–Hansen* diagram (IS-LM) became what Dornbusch and Fischer later called "the core" of modern macroeconomics. He then deals with the "sets of Keynesian ideas" and models characterizing the

ongoing Keynesian revolution. He goes on to illustrate the extensions and metamorphosis of the IS-LM approach via its plasticity and dynamization, going on to assess its canonization and dissemination.

In her comment on the paper, June Flander's outlines her objections to Hicks's 1937 approach, and highlights the importance of his 1945 paper, linking its emphasis on the "liquidity preference function" to Klein's later treatment of the 1945 "construct". She goes on to deal with the "caveats" Hicks specified regarding his 1937 SI-LL model, and criticizes the "disregard" of them by the economics profession, resulting in the widespread application of what was, in her view, a "pedagogical device of possibly useful and frequently dangerous value". She concludes by saying that the longstanding effect of Hicks's macroeconomic insights emanate from *Value and Capital* (1939), rather than his "little apparatus" of 1937 vintage.

David Colander surveys developments in macroeconomics from the 1930s onwards, and maintains that Keynes himself kept to "the classical methodology", albeit rejecting the "particular model" utilized by "Classical economists". He shows how, in the 1950s and 1960s, "grand synthesis" macroeconomic models emerged, based upon the amalgamation of general equilibrium, Keynesian theory and policy, and advances in econometrics. This *displaced* the emergence of a possible "complexity revolution" based upon non-linear dynamics. In his view, these models had "scientific" aspirations which were simply not met. With their *failure*, in the 1970s, what occurred was the *displacement* of the Keynesian research program by the New Classical Macroeconomics, which *was* "scientific". This brought about the exclusion, from graduate training, of "Keynes's method"; which in his view, consisted of the application of "pragmatic common sense. . .intuition and judgment based on institutional knowledge and experience". There was a "bifurcation", between undergraduate and graduate training; the former still utilizing models of IS-LM vintage, with the latter based on the DSGE approach; New Classical and New Keynesian alike. Colander argues that this not only has taken "Keynes's method" out of "academic macroeconomics", but has also gutted it from what he terms "central bank macroeconomics", especially in their research departments. Finally, he advocates different training for those destined for academia as against those who desire to go into "policy economics", e.g. central banks.

In his comments, Witztum maintains that Colander is more frustrated with the limiting nature of DSGE models than with "the betrayal" of Keynes's method. He takes issue with Colander's "disjunction between theory and policy", and points to a conundrum emanating from Colander's assessment of DSGE as "a much better scientific model" than "the IS-LM model". He goes on to say that "unlike Colander", he believes "that the real issue" is to deal *not* with "the question" of "how to separate policy from theory", and separate "the policy people from the theorists. . .in both teaching and research", but "how to do theory". He then states that "economic phenomena are social phenomena", and maintains that Smith and JS Mill had tied their analysis to "other aspects of social life", and "offered a genuinely complex simplified structure" that did not necessitate the "harsh" demarcation of theory and policy suggested by Colander. He goes even further by

saying that *both* Colander's interpretation of Keynes's "method" and DSGE models "are guilty of the same thing", that is, transferring the "economic problem" from the realm of "social" phenomena to that of the "as hoc" issues of determination and control of economic aggregates. Simply put, according to Witztum, "the DSGE models are true to Keynes's tradition". He concludes by saying "that there is still" much that can be ascertained "if we follow the more classical tradition" of economics as "a social theory" and not a "replicative process", as Colander would have it. In his view, the "state" of DSGE need not be the "reason to separate theory from policy". Rather, its "state" should be the "reason to expect better economic theory".

In his paper, Arie Arnon deals with the Wicksell–Keynes connection. Following a brief survey of developments up to Wicksell, he then discusses the main ideas outlined in Wicksell's 1898 volume, *Interest and Prices* and the second volume of his 1906 *Lectures* respectively. In dealing with the impact of Wicksell's volumes on Keynes's *Treatise* (1930) and *General Theory* (1936), Arnon makes the important point that "Keynes, when he came to know Wicksell's work, helped to make it accessible" to an Anglophone readership. Moreover, he notes that "the depth and timing of Keynes's acquaintance of Wicksell's theory is important", in light of the fact that between the *Treatise* and *General Theory*, "Keynes changed his own theoretical position". Arnon goes on to try to answer the question as to how Wicksell's approach came to Keynes's attention; this, most likely, in his view, via reading Hayek's works. He then turns to Patinkin's treatment of the Wicksell–Keynes connection, questioning Patinkin's view on the degree to which Wicksell was a "quantity theorist". He continues on to show the direct impact of Wicksell's volumes on Keynes's 1930 and 1936 tomes, and maintains that by 1936, Keynes "admits" his intellectual debt "to Wicksell", albeit by then having shifted his "focus" to the determination of employment rather than to "the price level". Moreover, given that by 1936 Keynes had "lost" his "trust" in "monetary policy", Arnon maintains that while "Wicksell . . .was instrumental to the *Treatise*", in his view, "by the late 1930s", Keynes's enthusiasm for Wicksell had "disappeared".

In his comment on Arnon's paper, Warren Young deals with two issues. The first relates to the perception of Wicksell as an advocate of *activist* monetary policy. Young asks if Wicksell understood, and applied, an *early* version of *strong* rational expectations, and cites Wicksell himself regarding the possible impact if the outcome of "deliberate policy. . .can be anticipated and foreseen". He then deals with the issue raised by Arnon regarding Patinkin's view of Wicksell in the context of quantity theory, and Patinkin's disagreement with Swedish economists regarding this. Young provides an alternate possibility – that of the *canonization* of Wicksell's work by some Swedish economists, as an explanation of divergent positions held by Patinkin and them as to whether Wicksell was a quantity theorist or not. Finally, Young shows that Marget may have influenced Patinkin's views on Wicksell.

Robert Dimand's paper examines Keynes's prognostications regarding the international banking system in the context of his August 1931 essay examining the impact of a "collapse in money values" on the banks, first published in *Essays in*

Persuasion (1931). Ideas developed by Keynes in his essay, seldom cited, except in the financial fragility literature, are as relevant in 2009, according to Dimand, as they were in 1931, albeit with asset prices, rather than the general price level undergoing "collapse". Dimand outlines the evolution of Keynes's approach to debt deflation between "The Great Slump of 1930" and his June 1931 Harris Lectures, in which he emphasized "the significance" of nominal " debt contracts". He then concisely deals with the Keynes–Sprague controversy, and the crisis that catalyzed Keynes's views as expressed in the essay, and compares this to the situation between 2007 and 2009, noting that while "massive injections" into the financial system have limited the fall in prices, *nominal* "values of real assets" *have* fallen. Dimand then places Keynes's 1931 ideas on debts and deflation in the *General Theory* context, showing how they re-surfaced "as a key part" of Chapter 19. Bringing the story up to the crisis of 2007–2009, he tries to explain why policy responses in the US, UK and EU differed, attributing this to differences in "collective memories" of the crises of the 1920s and 1930s, and to the lessons of Keynes's 1931 essay.

In her comments, Elise Brezis focuses on the shocks and dynamics affecting the economic system in the Great Depression and the recent crisis, and their impact on the nominal-real variable relationship. She first delineates a "disequilibrium dynamics" approach as the basis for analysis of the "real effects" of "the collapse" in nominal values, and identifies Keynes's "animal spirits" with the "herding" characterizing the recent crisis. In her comparison of the Great Depression and recent crisis, she notes that it was "the herding effects" that "led to the beginning of the disequilibrium dynamics" that characterized the latter event. According to her, there are, however, a number of factors that differentiate between the two events: the fiscal and monetary policies adopted, and the existence of flexible exchange rates as against the gold standard. While the recent crisis may have been, ostensibly, overcome, she warns that the "problem of income inequality between the financial sector and the rest of the economy", does not bode well for the future. Brezis concludes that if a Paretian "circulation of elites" does not occur naturally to rectify this inequality, that is, if a "restructuration" of wealth is blocked by what she sees as the power of the "new financial intermediation sector", then a "longer and more latent crisis" *could* emerge, in her view, in the future.

In his paper, Alex Cukierman, in concise and lucid terms, first surveys the developments in modern macroeconomics up to the DSGE framework, and maintains that the "still unfolding global financial crisis…will challenge and modify the construction and use" of such models. He asserts that "an important limitation" on them is that they do not well reflect the incentives, constraints and behavior characterizing "financial intermediaries". He goes on to compare institutions, policy responses and their performance in the Great Depression and current crisis, and then assesses the lessons learnt from the former, and if they were applied during the latter event. Cukierman then turns to the current crisis itself to evaluate the nature of lessons that can be taken from the current crisis itself. He discusses the problems of financial "runs", the role of derivatives, the "too big to fail" issue, the problem of incomplete risk assessment, and reinterprets the liquidity

trap approach to the crisis. He then compares the New Keynesian and Real Business Cycle frameworks, and proposes that what has occurred is, in essence, a "*new neoclassical synthesis*". In his final section, he reflects upon the *limitations* of this, as they still do not include the *quality* of supervision and regulation, and concluded that researchers should now focus on incorporating these characteristics of an economy and the link between them, and "the likelihood of booms and busts".

In his comments, Avia Spivak outlines both agreement and disagreement with Cukierman's paper. In his view, Cukierman is correct in categorizing the recent crisis as less severe than the Great Depression due to (1) the expanded roles of governments and central banks; (2) improvements in international coordination regarding capital markets, trade flows, and exchange rates; and (3) lessons learned from errors made by central banks in the Great Depression regarding policy activism. Spivak points to Curkierman's insight regarding bank runs by depositors, as against runs "by banks on other banks or financial institutions", and the outcome of this, which affected the global financial system. With regard to disagreement, Spivak notes the absence in Cukierman's paper of "the political and intellectual background" to the growth of "shadow banking". He also questions Cukierman's "presentation of macroeconomics as going through a process of seemingly harmonic evolution centered" on the New Keynesian approach, which Spivak – citing Krugman – criticizes as lacking an "explicit financial system"; something which Cukierman also acknowledges as "missing", and whose development is "essential". He then illustrates the problem of the New Keynesian, as against the Keynesian approach, in analyzing the recent crisis in the US case. Spivak concludes that while Cukierman is correct in attributing "success" in preventing a new Great Depression to "lessons learned" from that of the 1930s and "changes in the institutional framework", this success had has little to do with "academic development in the macro area", as exhibited, *in his view*, by the shortcomings of *both* New Classical and New Keynesian approaches in foreseeing and analyzing the recent crisis.

In the final paper of the volume, David Laidler examines the connection between macro-economic crises and ideas, and counterpoints the contributions of Keynes and Lucas to modern macroeconomics in the context of "real world economic phenomena". In his view, Lucas has both misunderstood the evolution of modern economics and presented a flawed account of Keynes's role in it. He asserts that Lucas's criticism of Keynes's *General Theory* as an "unhelpful detour in the discipline's otherwise orderly history", resulting from the "circumstances of the Great Depression", is misplaced. He goes on to implicitly maintain that while Keynes *was* able to deal with "the economics of depression", the approaches advocated by Lucas, on the one hand, and Woodford, on the other, *cannot* "deal with some of the critical. . .features of any real world economy". Moreover, according to him, the inability of their respective approaches to deal with "the recent crisis" has, in turn, "created a crisis in macro-economics". Finally, he focuses on what he sees as the importance of "animal spirits" and conventions, albeit of Keynes (1936) and *not* Akerloff-Shiller (2009) vintage. In this context, he observes that "given available analytic techniques in the late 1930s", treating expectations as

"exogenous" may actually have been "a progressive step". He points out that the difference between Keynes and Akerloff-Shiller on "animal spirits", is that Keynes used the term to refer only to investor's "spirit of enterprise", while Akerloff-Shiller use it to "characterize any deviation" of agents "from the rational maximizing norm of neo-classical economics". He then deals with Keynes (1936) on money and co-ordination, and goes on to focus on the current relevance and contemporary significance of his work, linking it to "coordination failures that generate fluctuations in real variables". Laidler concludes by linking the possibility of cyclical recurrence of "theoretical ideas" and calls "for a reconsideration" of the "insights" of Keynes "into the mechanics of inter-temporal coordination in a monetary economy and their proneness to occasional failure"; also recommending this "reconsideration" to Lucas and those who follow his approach.

In his comments, Warren Young raises the question of whether Lucas and the protagonists of the New Classical and equilibrium business cycle approaches should actually be the address for Laidler's criticism. He points to the New Keynesian treatment of coordination failure as an equally valid target for Laidler's critique, and cites examples of the New Keynesian approach progressing *away* from Laidler's view of the significance of Keynes's *General Theory* treatment of coordination failure. He then turns to Laidler's discussion of Lucas's treatment of Keynes's *General Theory*, and compares it to that of Hicks. Finally, Young points to recent developments in the augmented real business cycle approach as possibly covering Laidler's criterion of thinking about "financial crises and the depression".

Laidler's response to Young provides a fitting summing up and conclusion to the volume. In it, he acknowledges positive and negative aspects of the New Keynesian approach, referring to his earlier paper (2007) when he dealt in detail with its "deficiencies". He goes on to reiterate what he sees as the central message of his paper in the present volume. As he puts it, "money and finance" should not be considered as "frictions" which can alter "the behavior" of a "well coordinated" economic system, and thus "need to be introduced as elaborations" of a "basic model". Rather, according to Laidler, "they [money and finance] are the socio-economic institutions through which coordination is", or occasionally, "is not. . .achieved". He continues on to say that they should be considered as an *integral* part "of the analytic story"; something which, in his view, Keynes showed how to do "in one coherent way". And, according to him, this is the reason why Keynes's work "is still", in his view, "theoretically interesting and important". Laidler then turns to the real business cycle approach and provides an interesting reference to Robertson's (1926) possible contribution – in the form of "appropriate fluctuations" – to its central tenet of "productivity shocks". He refers to his own work (2003) in which he elaborated on Robertson's account. Laidler sees the changing "technology of the financial system" as bringing about the shock to the "market" interest rate. He doubts whether the *present* state of the real business cycle approach can accommodate Robertsonian-style "inappropriate fluctuations" emanating from the financial sector, but concludes that since *both* the real business cycle and New Keynesian approaches are still "works in progress", they may, *in time*, be able to assist in addressing "our post-2007 problems".

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Beer-Sheva, Israel — Arie Arnon
Beer-Sheva, Israel — Jimmy Weinblatt
Ramat Gan, Israel — Warren Young

Part I
History, Methodology, and their Current Relevance

Making the Most of Anomaly in the History of Economic Thought: Smith, Marx-Engels, and Keynes

Samuel Hollander

Abstract A discrepancy between what we expect an author to say and what he actually does say should be an inducement to examine and, if necessary, adjust or even abandon the initial presumption. Unfortunately, all too often apparent anomalies are simply discarded as error or as unrepresentative, leaving the initial interpretation unaffected. The fruitfulness of according serious attention to apparent anomaly is illustrated by reference to Smith's case for government intervention in the credit market (his support of the Usury Laws), and the Marx-Engels appeal to the orthodox pricing mechanism in evaluating contemporary reformist schemes. My third case concerns Keynes's misrepresentation of Ricardian macro-economic policy, more specifically, his unwillingness to make use of his demonstrable awareness of Ricardo's actual position to support his argument against the Return to Gold at par in 1925. Here again we face an anomalous situation, though of a different sort to the first two. The object of the exercise is the same, namely to achieve a better comprehension of the author in question by resolving the anomaly, but in this instance – unlike the first two – I have not yet arrived at a resolution.

1. To which "classical" text do we send our students to convey the operation of the freely-operating competitive pricing mechanism in the effective allocation of scarce resources? My guess is that 99 out of 100 instructors start with the *Wealth of Nations*, the 99% including J.M. Keynes. For Keynes himself warned in 1946 against hasty recourse to import and exchange-rate controls which his own writings, he deeply regretted, had encouraged. Government intervention was required "not to defeat but to implement the wisdom of Adam Smith"; "I find myself moved, not for the first time, to remind contemporary economists that the classical teaching embodied some permanent truths of great significance. . .. There are in these matters deep undercurrents at work, natural forces one can call them, or even the invisible

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hand, which are operating towards equilibrium . . .” (Keynes 1980 [1946]: 44–45). And in *The General Theory* itself, Keynes had already written that “if our central controls succeed in establishing an aggregate volume of output corresponding to full employment as nearly as is practicable, the classical theory [of resource allocation] comes into its own again from this point onward” (Keynes 1936: 378).

What though are we to make of the notorious fact that Adam Smith justified a legal maximum to the interest rate – the usury laws which imposed a maximum of 5% on private loans – as a means of preventing “prodigals and projectors” from cornering the supply of loans (Smith 1976 [1776]: 357)? Jeremy Bentham at the time objected to Smith’s justification of a measure “to direct individuals in their private concerns” (Bentham 1952 [1787]: 128), and which (he feared) created obstacles in the way of innovative investment (168–169), and neglected evasion of the law (147). John Stuart Mill pointed to the aggravation of commercial crises by the exclusion of emergency borrowing by otherwise prosperous firms (Mill 1963–1991, 3: 924–925). Modern writers have expressed their utter bewilderment. Lord Robbins, for example, found Smith’s position inexplicable; his support for a legal maximum must have occurred “in an incautious moment” (Robbins 1968: 86). George Stigler described his position as an “aberration” (Stigler 1982: 122), and as “a strange argument” which “seems to assume that lenders would pay no attention to the probability of being repaid, but only to the promised interest rate. Surely it was inconsistent with Smith’s basic theory of sensible economic behavior; here the lenders are being foolishly shortsighted” (Stigler 1988: 208–209). Jacob Viner took Smith’s position seriously, as “involv[ing] an admission . . . that the majority of investors could not be relied upon to invest their funds prudently and safely, and that government regulation was a good corrective for individual stupidity” (Viner 1958 [1927]: 242). But here precisely lies the anomaly that begs for a solution: *Would the real Adam Smith please stand up*.

At this point I wish to express my bemusement that commentators of the highest caliber could rest content with expressions of regret regarding Smith’s “incaution,” “strangeness,” “inconsistency,” “mistakes,” and make little effort to understand what *appears* to be an anomalous position. For it is precisely the presence of anomalies such as this that provide a challenge better to understand the writer in question. In this I follow J.S. Mill’s injunction: “we cannot . . . too carefully endeavour to verify our theory” – read *interpretation* – “by comparing, in the particular cases to which we have access, the results which it would have led us to predict, with the most trustworthy accounts we can obtain of those which have been actually realized. The discrepancy between our anticipations [what we expect Adam Smith to say] and the actual fact [what Adam Smith actually does say] is often the only circumstance which would have drawn our attention to some important disturbing cause which we had overlooked” (Mill 1963–1991, 4: 332). Stigler’s perspective, to the contrary, calls for the discarding of unexplained residuals, in this case the excision of the usury passage from *The Wealth of Nations*: “Most mistakes are not of great consequence: Smith would not have had to make any changes in the rest of his big book if he had dropped the paragraph on usury laws. The reluctance to acknowledge mistakes is perhaps part of the committed

missionary zeal of scientific explorers" (Stigler 1988: 210). The case illustrated for Stigler an "attribute of able economists that ... they seldom admit or correct a mistake."[1]

I shall illustrate the potential fruitfulness of Mill's injunction applied to interpretation by reference to this and two other case studies. The first two are mirror images of each other – on the one hand, Smith's support for government intervention in the credit market; and on the other, the surprising appeal by Marx and Engels to the orthodox competitive pricing mechanism in approaching contemporary reformist, including socialist, schemes of various sorts. My third case is of a different order and relates to Keynes's distorted representation of classical macroeconomics despite his keen awareness of the true classical position. I shall appeal for advice from the Keynes specialists gathered here for possible solutions to this last conundrum.

2. I turn to the first case.[2] Smith commended the historical sequence of statutory regulations lowering the legal maximum to the ruling 5%, which are said "to have been made with great propriety" in that they followed downward movements in, while always remaining marginally higher than, "the rate at which people of good credit usually borrowed" (Smith 1976 [1776]: 106). Again: "In a country, such as Great Britain where money is lent to government at 3%, and to private people upon good security at 4 and 4.5%, the present legal rate, 5% is, perhaps, as proper as any"; for the legal rate "though it ought to be somewhat above, ought not to be much above the lowest rate" (357). The justification for a maximum which somewhat exceeds the range of rates appropriate for private borrowers with high credit ratings (our "prime rates") is made out carefully, and it should have been apparent from the outset that something was at play other than simple *error* or a passing fancy.

Firstly, a legal maximum set too low would be unsustainable: "If this legal rate should be fixed below the lowest market rate, the effects of this fixation must be nearly the same as those of a total prohibition of interest," and proscribing interest altogether "instead of preventing, has been found from experience to increase the evil of usury; the debtor being obliged to pay, not only for the use of the money, but for the risk which his creditor runs by accepting a compensation for that use. He is obliged, if we may say so, to insure his creditor from the penalties of usury" (356). Again: "When the law prohibits interest altogether, it does not prevent it. Many people must borrow, and nobody will lend without a consideration for the use of their money as is suitable, not only to what can be made by the use of it, but to the difficulty and danger of evading the law" (112). When loans at interest are legal but

[1]Stigler allows that Smith's failure to respond to Bentham in his final edition of 1789 might have been no more than a matter of "lethargy" (Stigler 1988: 209). Rumors did in fact circulate that Smith was converted by Bentham's arguments. Viner properly reserved judgment: "From the information available all that can be safely inferred is that Smith bore Bentham no ill-will for his criticism and possibly did not deny that it had some force" (Viner 1965: 19). Considering the great care with which Smith made out his case, it would greatly surprise me should it ever be established that he abandoned his support for the usury laws.

[2]For a full account of the episode, see Hollander 1999, upon which the present account is based.

a limit is set below prime the result is similar, since "[t]he creditor will not lend his money for less than the use of it is worth, and the debtor must pay him for the risk which he [the creditor] runs by accepting the full value of that use" (356–357). The limiting case in this regard is a maximum "fixed precisely at the lowest market price"; this too is unsustainable since "it ruins with honest people, who respect the laws of their country, the credit of all those who cannot give the very best security, and obliges them to have recourse to exorbitant usurers" (357).

Smith thus intended to preclude only transactions involving borrowers of relatively poor credit rating who would be prepared to pay interest much exceeding 5%. The market for "safe" loans would operate free of control, a feature that would justify Keynes's designation of Smith as "extremely moderate in his attitude to the usury laws . . . he defended [their] moderate application" (Keynes 1936: 352).

Secondly, where interest rates greatly exceeding the "lowest market rate" are legally available, Smith attributed to lenders a *bias* favoring high-risk loans rather than a roughly equal distribution of preference across the spectrum of opportunities; he, in brief, denied that – faced with such opportunities – lenders would impose control over purely speculative ventures, since lenders are *prejudiced* in favor of riskiness, the majority preferring to engage in high-risk lending should the opportunity present itself: "If the legal rate of interest in Great Britain, for example, was fixed so high as eight or ten per cent, the greater part of the money which was to be lent, would be lent to prodigals and projectors, who alone would be willing to give this high interest. Sober people, who will give for the use of money no more than a part of what they are likely to make by the use of it, would not venture into the competition. A great part of the capital of the country would thus be kept out of the hands which were most likely to make a profitable and advantageous use of it, and thrown into those which were most likely to waste and destroy it" (357). The bias is, however, reversed at a legal maximum rate of interest "fixed but a very little above the lowest market rate," for then "sober people are universally preferred, as borrowers, to prodigals and projectors. The person who lends money gets nearly as much interest from the former as he dares to take from the latter, and his money is much safer in the hands of the one set of people, than in those of the other." Smith presumes here that only prodigals are prepared to offer high illegal rates, and that lenders are loath to accept such inducements, while small inducements are insufficient to compensate the risk of non-repayment. "A great part of the capital of the country," he concludes, "is thus thrown into the hands in which it is most likely to be employed with advantage."

The lack of concern with the emergence of a black credit market at an appropriate legal maximum, contrasts sharply with the assumed inevitability of such an outcome in the event of a total prohibition of loans at interest. The essence of the matter is the argument that the 5% maximum is effective, in the sense of sustainable, since lenders engage in an effective rationing process whereby "prodigals and projectors" are excluded in favor of "sober" borrowers, thereby containing excess-demand pressures.

The contrast between the behavior of lenders who at the 5% maximum seek out "sober" borrowers and the overwhelming bias towards high-risk loans should the

legal maximum be set too high (or, by extension, where high interest rates are freely available), requires explanation. Smith's portrayal can, I suggest, be accounted for in terms of his general preoccupation with irresponsibility engendered by the promise of *excessive* returns. Thus: "The high rate of profit seems everywhere to destroy that parsimony which in other circumstances is natural to the character of the merchant. When profits are high, that sober virtue seems to be superfluous, and expensive luxury to suit better the affluence of his situation" (612). Furthermore, "[w]hen the profits of trade happen to be greater than ordinary, over-trading becomes a general error both among great and small traders" (438). Where profits are high, agricultural employers are prone to adopt inefficient methods: "The planting of sugar and tobacco, can afford the expense of [inefficient] slave cultivation" (388). Regarding "the situation" of the great landowner in pre-commercial society, he writes that "it naturally disposes him to attend rather to ornament which pleases his fancy, than to profit for which he has so little occasion" (385). Smith points to typically careless consumption behavior when items absorb only a small fraction of the budget (74). And most generally, "in public, as well as in private expenses, great wealth may, perhaps, frequently be admitted as an apology for great folly" (523, added in 1784 edition). A corresponding carelessness is ascribed to the labor market in cases where individuals work part time so that their salary constitutes a fraction of total earnings (131).

We may summarize the case thus: Preferences with respect to commodities, bonds and labor are not independent of budget and price configurations. In our specific case, Smith rejected universally-applicable behavioral assumptions – whether the assumption of "sensible economic behavior" (as Stigler put it) or the assumption that most investors could not be "relied upon to invest their funds prudently or safely" (in Viner's terms). All depends on the range of opportunities open to investors, the usury laws constraining the range of available opportunities within which lenders *could* be relied upon to offer finance prudently. In particular, to allow a high interest is unacceptable to Smith because it unleashes avarice to the social disadvantage – very much an Aristotelian preoccupation (see Hollander 1999: 528–529; also Paginelli 2008). But though relatively tight markets tend to engender careful calculation, there are limits. For (as we have seen) legal interest rates set too low entail excess demand pressures on the part of "sober" borrowers that cannot be contained. In any event, Bentham's complaint that Smith neglected the potential emergence of black credit markets does not hold water, for he explains why potential excess demand at the appropriately set maximum is contained, in contrast with the illegal trades that would emerge at an inappropriately low maximum.

This is the main story emerging in the context of the usury laws. Adam Smith's justification of the contemporary usury laws was no "aberration" or "exception," but reflected his concern with unjustifiable risk-tolerance, such that in a wholly free credit market lenders' prejudice towards high-risk projects would predominate to the social detriment. But the matter is rather more complex. Smith's apparent confidence in the effectiveness of rationing imposed by lenders at the appropriate rate must be qualified in the light of concerns raised by local banking experience. The Scottish banks were, it seemed, unable to calculate objectively

the risk-worthiness of their clients, and accordingly engaged in the finance of inadvisable projects at the legal maximum. Not only that, but their incompetence was such that they themselves borrowed at effective rates far exceeding 5%, allowance made for compounding and costly commissions of different sorts; the practice of "drawing and re-drawing [bills of exchange]" might entail effective rates as high as 13–14% (Smith 1976 [1776]: 308). We are dealing with a failure of self-interest: "Had every particular banking company always understood its own particular interest" – and avoided inappropriate discounting of long-term investment projects – "the circulation never could have been overstocked with paper money" with dire consequences for the banks (302). The banks were not even aware of the nature of their loans, the "bold projectors" having disguised their operations: "It was a capital which those projectors had very artfully contrived to draw from those banks, not only without their knowledge or deliberate consent, but for some time, perhaps, without their having the most distant suspicion that they had really advanced it" (311). The hoped for control by lenders had thus not materialized, at least as far as concerned some bank accommodation.

3. I turn to my second case of "anomaly" in economic doctrine, and the potential fruitfulness of taking it seriously. I venture that few instructors would refer their students to Karl Marx or his co-worker Friedrich Engels for lessons in the operation of the workings of the invisible hand. After all, did they not focus on the anarchical character of capitalist production due in part to producers' ignorance of markets? For example: "No one knows how much of his particular article is coming on the market, nor how much of it will be wanted. No one knows whether his individual product will meet an actual demand, whether he will be able to make good his costs of production or even to sell his commodity at all" (Engels, *Anti-Dühring* (1878); *Marx-Engels Collected Works* 25: 259). The solution to the enormous waste of resources under capitalism, with an eye to cyclical instability in particular, lay precisely in "the socialized appropriation of the means of production" (266), or in "systematic definite organization" (270). And what of their attention to monopoly as characteristic of advanced capitalism – centralization, cartelization, trustification and the like?

Unlikely as it may seem, excellent lessons in the pricing system are to be discerned in the Marx-Engels texts, which have not always been properly addressed, if addressed at all, in the commentaries. Marx's technical account of the Transformation – spelled out already in the late 1850s, and again in 1861–1863 (and not only in *Capital 3*) – turns strategically on the orthodox pricing mechanism, and is directed against labor theorists, such as Karl Rodbertus (1851), who "seems to think that competition brings about a normal profit or average profit or general rate of profit by reducing the commodities to their *real value* . . ." (MECW 31: 260).[3]

[3]The *Theories of Surplus Value* provide especially impressive accounts of the "tendency" towards a uniform profit rate in terms of the demand–supply mechanism involving not only redistributions of resources, with corresponding output changes, but also – in fact to a greater extent – the allocation of *net* investment between industries. They immediately bring to mind the corresponding analyses by Walras (1954 [1874]: 225, 276, 305, 308) and by Marshall (1920: 592–593, 411–412, 418–419, 533). The process involves requisite knowledge of going rates which

But applications are my main interest today, and to these I turn. It is orthodox price theory, sometimes with a modern "Austrian" flavor, that provides the framework for much Marxian applied economics.

In *Poverty of Philosophy* Marx (1847) rejected, on price-theoretic grounds and in terms of the dynamics of growth, proposals for State confiscation of rent by James Mill, Cherbuliez and Hilditch (MECW 6: 203). Land-valuation indexes ("cadastres") – on which any confiscation would have to be based – were subject to continuous disturbance and, therefore, impractical: "[R]ent could not be the invariable index of the degree of fertility of the land, since every moment the modern application of chemistry is changing the nature of the soil and geological knowledge is … beginning to revolutionize all the old estimates of relative fertility"; moreover, demand patterns are subject to change: "fertility is not so natural a quality as might be thought [but] is closely bound up with the social relations of the time. A piece of land may be very fertile for corn growing, and yet the market price may induce the cultivators to turn it into an artificial pastureland and thus render it infertile" (203–204).[4]

A striking instance of Marx's respect for the market is provided by his objections to Napoleon III's plan to regulate French bread prices (13 December 1858; MECW 16: 110–114). His objection turns on the array of further interventions that would be required to enforce the controls. He draws on the experience of Paris which had instituted them locally, and where "the experiment proved a complete failure, the price of bread rising above the official maximum during the bad seasons, from 1855 to 1857 …" (111). His forecast regarding the extension to France as a whole in the case of "good years," and the maintenance of a price *floor*, emphasizes the unthought-of consequences of the proposed "artificial demand to be created through the means of 3 months' reserve." For "[i]mmense buildings for public granaries will become necessary over the whole of France; and what a fresh field they will open for jobs and plunder. An unexpected turn is also given to the trade in breadstuffs. What profits to be pocketed by the Crédit Mobilier and the other gambling companions of his Imperial Majesty! At all events, we may be sure that the Imperial Socialist will prove no more successful in raising the price of bread than he has been in attempts to reduce it" (114).

allow comparisons to be made by entrepreneurs of relative profitability; and the length of time particularly "high" returns must rule before responses to them occur (1861–1863; MECW *32*: 460). Mobility problems peculiar to supply and demand conditions are elaborated: "the speed of the equalization process … depends on the particular organic composition of the different capitals (more fixed or circulating capital, for example) and on the particular nature of their commodities, that is, whether their nature as use values facilitates rapid withdrawal from the market and the diminution or increase of supply, in accordance with the level of the market prices" (460–461). Also noteworthy is the role accorded *credit* in the adjustment of values to cost price, Marx citing the famous accounts by Ricardo of the role of bankers and others in the discount business (MECW *31*: 434).

[4]The objection has much in common with that of Adam Smith against "a land-tax assessed according to a general survey and valuation" (Smith 1976 [1776]: 836).

Repeatedly in his *Poverty of Philosophy* Marx insisted on the demand component: "The exchange value of a product depends upon its abundance or its scarcity, but always in relation to demand" (Marx 1847; MECW 6: 115); Proudhon "has simply forgotten about *demand*, and that a thing can be scarce and abundant only insofar as it is in demand"; Proudhon's "abundance seems to be something spontaneous. He completely forgets that there are people who produce [a product], and that it is in their interest never to lose sight of demand" (116). All this was republished by Engels in the German edition of 1885. And in the Preface to that edition, Engels's own appreciation of the allocative function of markets emerges strikingly. For he there attacks Rodbertus's labor-money scheme precisely because of its neglect of the competitive allocation mechanism.[5] The passage is one of the most revealing in the entire Marxian literature:

> To desire, in a society of producers who exchange their commodities, to establish the determination of value by labour time, by forbidding competition to establish this determination of value through pressure on prices in the only way in which it can be established, is therefore merely to prove that, at least in this sphere, one has adopted the usual utopian disdain of economic laws. . .. [C]ompetition, by bringing into operation the law of value of commodity production in a society of producers who exchange their commodities, precisely thereby brings about the only organization and arrangement of social production which is possible in the circumstances. *Only through the undervaluation or overvaluation of products is it forcibly brought home to the individual commodity producers what society requires or does not require and in what amounts*. But it is precisely this sole regulator that the Utopia advocated by Rodbertus among others wishes to abolish (MECW 26: 287; emphasis added).

Engels's appreciation of the market mechanism may be further illustrated from his important, though relatively neglected, "The Housing Question." He there approached the working-class housing transaction as a "quite ordinary commodity transaction" subject to "the economic laws which govern the sale of commodities in general . . ." (1872; MECW 23: 320; also 375). More specifically, on-going adoption of labor-displacing "machinery" and the pattern of regular trade cycles depressed average wages and employment and thus workers' purchasing power, rendering the private provision of rental accommodation for labor unprofitable relative to alternative higher-class categories. By contrast, "bourgeois socialist" writers had no other explanation to offer for the housing shortage than to represent it as "the result of the wickedness of man," as "original sin, so to speak" (341). In brief, Engels insisted on an objective price-analytic diagnosis to explain the housing shortage, in contrast with the vacuity of subjective appeals for increased provision based on "justice" and "right."

[5]Rodbertus's scheme entailed an advance by the state of labor tickets to capitalists, who allocate them to workers employed in privately-organized establishments according to labor contribution; in return the state receives consumer goods equivalent in value; and the tickets are dispensed by the recipients freely in the purchase of consumer goods made available at state outlets, *at legally required labor-based prices*, the labor tickets thus flowing back to the starting point. For elaboration of the scheme and Engels's interpretation and criticisms thereof, see Hollander (forthcoming).

Engels's critique of protectionist policies is equally revealing. His "Protection and Free Trade" makes it very clear indeed that the Infant Industry case, which he had justified on standard developmental grounds in America, must be temporary (no more than say 25 years), and that protection was "by now becoming a nuisance" (1888; MECW 26: 526). His objections to protection are in line with those of Marx against price control. "Protection is at best an endless screw, and you never know when you have done with it. By protecting one industry, you directly or indirectly hurt all others, and have therefore to protect them too. By so doing you again damage the industry that you first protected, and have to compensate it; but this compensation reacts, as before, on all other trades, and entitles them to redress, and so on *in infinitum*" (526–527). Beyond this, technical progress was so rapid and revolutionary "that what may have been yesterday a fairly balanced protective tariff is no longer so today," engendering inter-industrial and political conflicts to assure the necessary modifications (527). But "the worst of protection" is that "when you once have got it you cannot easily get rid of it. Difficult as is the process of adjustment to an equitable tariff, the return to Free Trade is immensely more difficult" (528). For "[t]he legislature, by adopting the protective plan, has created vast interests, for which it is responsible. And not every one of these interests – the various branches of industry – is equally ready, at a given moment, to face open competition. Some will be lagging behind, while others have no longer need of protective nursing. This difference of position will give rise to the usual lobby-plotting. . ." (535).

Engels's objection to contemporary recommendations for government intervention in the credit market – as by Proudhon who sought to reduce the interest rate to 1% in the first interest and finally to zero – contrasts strikingly with the Smithian position. The presumption that the interest rate can be effectively regulated by legislation Engels rejected on grounds of the operation of competitive market forces which establish the return on loanable funds subject to adjustment reflecting concern with penalties on illegal transactions: "The rate of interest will continue to be governed by the economic laws to which it is subject today, all decrees notwithstanding. Persons possessing credit [i.e. credit-worthiness] will continue to borrow money at 2, 3, 4 and more percent, according to circumstances, just as before, and the only difference will be that rentiers will be very careful to advance money only to persons with whom no litigation is to be expected. Moreover, this great plan to deprive capital of its 'productivity' is as old as . . . the *usury laws* . . . which aim at nothing else but limiting the rate of interest, and which have since been abolished everywhere because in practice they were continually broken or circumvented, and the state was compelled to admit its impotence against the laws of social production" ("The Housing Question" 1872; MECW 23: 332–333, 388).

Here is a paradoxical situation if ever there was one. Marx and Engels – who so often focus on the market as destabilizing and who opted for Communism – yet read like the arch-conservatives von Hayek and von Mises in their approach to intervention in the market system (see further Hollander 2004, 2008: 401–406). And, compounding the apparent confusion, it is Engels who, unlike Smith, condemns the usury laws on price-theoretic grounds.

Now it is true that in the context of the trade cycle (particularly the crisis), Engels's focus is on the chaotic character of markets, whereas the basic Marxian model and the applications discussed above assume the market process to be equilibrating in the orthodox fashion. Yet this contrast must not be pushed too far, if we keep in mind a further dimension to his objection to Rodbertus. Rodbertus's preclusion of a "competitive" process, Engels observed, undermined his aim to solve the problem of crises, in fact worsened the problem. For however defective the signaling mechanism provided by competitive prices might be, Rodbertus's alternative was far worse – particularly in an international environment – for individual producers would then be operating "completely blindfolded": "As soon as the production of commodities has assumed world market dimensions, the evening-out between the individual producers who produce for private account and the market for which they produce, which in respect of quantity and quality of demand is more or less unknown to them, is established by means of a storm on the world market, by a commercial crisis. If now competition is to be forbidden to make the individual producers aware, by a rise or fall in prices, how the world market stands, then they are completely blindfolded. . .. [T]he producers can no longer learn anything about the state of the market for which they are producing . . ." (MECW 26: 288).

We must attend now to the apparent anomaly that has emerged. The late T.W. Hutchison expressed amazement that Engels could have objected to Rodbertus for neglecting the rationing and information-yielding function of prices, without raising the same objection against the Communist organization that he championed, feeling – for Hutchison went further – no "intellectual or moral obligation to give some thought to the kind of economic organization which would, or could, follow" the demise of capitalism (Hutchison 1981: 14). Now rather than merely express amazement I treat the record as an *apparent* anomaly that invites investigation of the type of ideal system Engels had in mind for the future, for he gave considerable thought to the matter.

Rodbertus's scheme, as Engels understood it, retained significant features of a market system, yet rejected the competitive pricing mechanism. A similar complaint is addressed, in *Anti-Dühring*, against Dühring – that he wished to retain elements of the market system (including a circulating money medium) yet precluded its effective operation (1878; MECW 25: 275). This to Engels was an unacceptable half-way house. Like Marx, he perceived of an ideal system excluding markets – indeed excluding money – one involving centralized decisions on investment, output, pay and the absence of free consumer choice (see Hollander 2004, 2008). In short, he envisaged a rather primitive "war economy" – in the sense of the term elaborated by Lord Robbins 1976: 144 – entailing the production of goods selected by the planners, and allocated according to workers' claims to a specified bundle of goods. On this reading, the force of Hutchison's complaint against Engels, that he failed to admit that his criticism of Rodbertus for neglecting the information-yielding role of prices applied equally to his own scheme, is greatly reduced; essentially, there is no scope for the appearance of excess demands or supplies where quantities demanded as well as quantities supplied are centrally decided upon. It should however be emphasized that Marx and Engels were

proposing a "war economy" not for an emergency situation, with which Robbins was concerned, but permanently, or at least into the foreseeable future once their scheme had been put in place.

There are, of course, complexities, and we shall devote a brief word to them. For example, the planners would make allowances in their cost calculations, as shadow or accounting returns, for interest and rent based on the productivity contribution of land and capital and not only for labor ("Outlines of a Critique of Political Economy" 1844 etc).[6] And differential skills would be taken into account. Understanding Dühring as denying that differential values emerged as a result of compound labor, Engels pointed out in *Anti-Dühring* how fortunate it was "that fate did not make him a manufacturer, and thus saved him from fixing the value of his commodities on the basis of this new rule [of treating all labor equally] and thereby running infallibly into the arms of bankruptcy" (1878; MECW 25: 185). (It helps to remember that Engels was a successful, if unhappy, entrepreneur.) Under Engels's program, planners would ascribe "greater values" to productions of compound labor, though pay differentials would not be recognized: "In a socialistically organized society, these costs [of training] are borne by society, and to it therefore belong the fruits, the greater values produced by compound labour. The worker himself has no claim to extra pay." Now Marx had insisted in his "Critique of the Gotha Programme" on the need to recognize "unequal individual endowment and thus productive capacity of the workers as natural privileges" (1875; MECW 24: 86). But he intended specifically recognition of differentials reflecting "natural" characteristics, while Engels had in mind acquired characteristics involving training paid for by society as a whole. Acquired skills did not justify differential pay; natural advantage did (1878; MECW 25: 99). Claims beyond that were an "absurdity," considering the enormous range of individual character differences. And he cited *Capital* itself (1867; MECW 35: 70), to the effect that the idea of equality "already possesses the fixity of a popular prejudice." To find a denial of natural differences, we must go to the Enlightenment writers – including, of course, Adam Smith on the philosopher and the street porter, brought back to life in our day by David Levy and Sandra Peart (2008). Here we have another paradox to add to the roster.

4. My first two samples of anomaly – more accurately *apparent* anomaly – are drawn from allocation theory. I turn now to macro issues, specifically to Keynes. Ricardo, of course, was Keynes's *bête noire*: "If only Malthus, instead of Ricardo, had been the parent stem from which nineteenth-century economics proceeded, what a much wiser and richer place the world would be today! We have laboriously to re-discover and force through the obscuring envelopes of our misguided education what should never have ceased to be so obvious" (Keynes 1933: 144). This from the *Essays on Biography*. In *The General Theory*, J.B. Say and J.S. Mill are

[6]Marx, it will be recalled (above, 250–254), had cautioned that returns to land – and doubtless the point might be made with respect to capital too – could not be designated in physical terms. But this is the case in a market system, whereas the problem of measurement would be greatly reduced in the command scheme with its narrow bill of goods.

held responsible, along with Ricardo, for the proposition "that supply creates its own demand; – meaning by this in some significant, but not clearly defined, sense that the whole of the costs of production must necessarily be spent in the aggregate, directly or indirectly, on purchasing the product" (Keynes 1936: 18).

Now it so happens that Keynes's culprits were innocent of the charge against them. Ricardo, after all, sought to minimize the threat to general output and employment generated by monetary deflation when approaching the Return to Gold. Thus in *The High Price of Bullion* (1810–1811) he insisted on extreme caution in such an exercise: "I am well aware that the total failure of paper credit would be attended with the most disastrous consequences to the trade and commerce of the country, and even its sudden limitation would occasion so much ruin and distress, that it would be highly inexpedient to have recourse to it as the means of restoring our currency to its just and equitable value. . .. Before therefore they can safely pay in specie, the excess of notes must be gradually withdrawn from circulation" (Ricardo 1951–1973, 3: 94). Yet more striking is the recognition that permanent devaluation might be preferable should the excess of the market over the mint price of gold be of massive dimensions: "I never should advise a government to restore a currency, which was depreciated 30 pc^{t}., to par," he wrote to John Wheatley of 18 September 1821. "I should recommend ... that the currency ... be fixed at the depreciated value by lowering the standard, and that no further deviations should take place. It was without any legislation that the currency from 1813 to 1819, became of an increased value, and within 5 pc^{t}. of the value of gold, – it was in this state of things, and not with a currency depreciated 30 pc^{t}., that I advised a recurrence to the old standard" (Ricardo 1951–1973, 9: 73–74).[7] And in the Commons debate of 12 June 1822 on resumption, Ricardo spelled out the same opinion: "If, in the year 1819, the value of the currency had stood at 14*s*. for the pound note, which was the case in the year 1813, he should have thought that upon a balance of all the advantages and disadvantages of the case, it would have been as well to fix the currency at the then value, according to which most of the existing contracts had been made. . ." (Ricardo 1951–1973, 5: 208). Also in 1822, in *Protection to Agriculture*, this theme is repeated: "If, indeed, in 1819, or immediately preceding 1819, gold had been at 5*l*. 10*s*. an ounce, no measure could have been more inexpedient then to make so violent a change in all subsisting engagements, as would have been made by restoring the ancient standard" (Ricardo 1951–1973, 4: 223).[8]

[7]Wheatley, who himself supported devaluation, had misunderstood Ricardo, and Ricardo wrote to correct the misunderstanding.

[8]Ricardo's concern with the "short run" in the context of resumption is reflected also in the general case made for a paper currency. One major argument relates to the greater flexibility of operation by the monetary authorities in dealing with short-run difficulties. In 1811, Ricardo rejected any policy of checking note issues by imposing formal quantitative limits, precisely because of the need for flexibility (Ricardo 1951–1973, 6: 67). The concern with flexibility is apparent in the *Economical and Secure Currency* of 1816: "Amongst the advantages of a paper over a metallic circulation, may be reckoned, as not the least, the facility with which it may be altered in quantity, as the wants of commerce and temporary circumstances may require" (Ricardo 1951–1973, 4: 55). The advantage of credit elasticity in satisfying, rapidly, increases in the demand for liquidity –

One may insist on distinguishing between a demand contraction that is the "cause" of a depression, and one that merely accompanies it, the "cause" lying elsewhere. Yet this contrast in no way dilutes the profound significance of Ricardo's recognition of excess commodity supply and general unemployment due in the first instance to credit contraction. It is difficult to conceive a more dramatic refutation of Keynes's representations of Ricardo in the *Essays in Biography* and the *General Theory* than the approval of devaluation should that be required to avoid severe deflation.[9]

Keynes misrepresented his predecessors disgracefully.[10] And this is not the full story, since he managed further to distort the record by representing Malthus as precursor of the "true" doctrine, despite Malthus's "conservatism" regarding the Gold Standard and muted perspective on counter-cyclical monetary policy (Hollander 1997: 1003–1004). Ricardo went far beyond Malthus in the clear preferences given to devaluation if required to avoid severe deflation.

The problem is with Keynes, not with the classics.[11] Hayek, commenting on Keynes's "apparent ignorance" of the Ricardo position on the return to gold, wrote

having in mind both fluctuations in "confidence," which affect the acceptability of non-monetary media of exchange, and the finance of expanding trade – is weighed heavily in the pamphlet, the distinction between legitimate and illegitimate accommodation even leading Ricardo to describe the advantage of paper money in terms of the opportunities created for "the judicious management of the quantity" thereby according a "degree of uniformity, which is by no other means attainable . . . to the value of the circulating medium in which all payments are made" (57–58). Ricardo's Parliamentary intervention of June 1822 confirms the foregoing perspective. He there objected to an assertion that note issues cannot be deficient in a convertible system, and focused specifically upon the short-run deflationary effects of a monetary contraction (Ricardo 1951–1973, 5: 199–200).

[9]As for J.B. Say, his recognition of aggregate-demand fluctuations as a source of depression and unemployment is apparent from his rejection of what came to be called the Treasury View, on the grounds that the capital constraint is a flexible one, such that expansion of any one sector is not necessarily at the expense of another: "On insiste trop sur ce principe que l'industrie est bornée par l'étendue des capitaux, et n'est bornée que par eux" (Say 1843 [1828–1829]: 345). In fact, the holding by firms of surplus funds was the *rule*: "Un pays renferme une immense quantité de petits capitaux inoccupés que les circonstances mettent en lumière." This is striking enough. But there is also allusion to the consequences for general output and employment of aggregate-demand fluctuations that characterize advanced manufacturing: "Il y a dans tous les pays où l'industrie est très-developpée, des moments où l'ouvrage ne va pas, et où la classe ouvrière toute entière est en souffrance. Ce malheur [tient] . . . à la nature des produits manufacturés qui sont en général exposés à de grandes vicissitudes dans la demande qu'on en fait" (90). We have too Say's interpretation of the French industrial depression of 1813, as the outcome of a contraction of demand for manufactures emanating from the agricultural sector (Say 1814, 1: 148). And there are allowances that in periods of deflationary expectations the concern of investors to avoid loss of interest characterizing quiescent times gives way, generating excess demand for money to hold (164). The same general perspective is, of course, found also in J.S. Mill's celebrated "Of the Influence of Consumption of Production" composed in 1830, and later in the *Principles*.

[10]On various aspects of Keynes's treatment of the Classics, see also Ahiakpor 2003.

[11]It is not the classical literature that is problematic. The so-called Law of Markets or Say's Law was never intended as a denial of short-term excess supply as Keynes represented it; but was designed to counter the proposition as expressed by Malthus or Chalmers or Sismondi or Smith

of "the surprising gaps in his knowledge of nineteenth-century English economic theory (and economic history)" (von Hayek 1978: 199, 231). "I ask myself often," he continued, "how different the economic history of the world might have been if in the discussion of the years preceding 1925 one English economist had remembered and pointed out this long-before published passage in one of Ricardo's letters" – that to Wheatley of 18 September 1821– that he "should never advise a government to restore a currency which had been depreciated 30% to par."

Now, as I made clear, it is not a matter of one letter. Ricardo's actual position emerges in a wide range of private and public pronouncements, so that it is scarcely credible that Keynes could have been unaware of it. And that my disbelief is confirmed, and Hayek's charge against Keynes of total ignorance is proven unwarranted, can easily be demonstrated. That Keynes was aware of Ricardo's position on the dangers of a Return to Gold at par emerges in *A Tract on Monetary Reform* (Bonar 1923), where Ricardo is said to have spoken "on the issue between deflation and devaluation . . . in clear tones the voice of instructed reason," citing as evidence the speech of 12 June 1822 and the passage from *Protection to Agriculture* cited above (Keynes 1971 [1923]: 124). But here lies the conundrum: why did Keynes not refer to all this in his campaign of 1925 against the decision by the Chancellor of the Exchequer, Winston Churchill, to return to gold at par? It is saddening to read his account of nineteenth-century experience, which neglects to mention that Ricardo had distanced himself from the so-called "orthodox party" of his day: "The course of events in England during the nineteenth century is instructive. The wars of Napoleon brought an inconvertible managed currency for a period of more than 20 years, just as the Great War has brought us the same thing for a period of more than 10 years. . .. Then, as now, the orthodox party proclaimed that the one thing necessary was to restore gold convertibility; and a little more than a hundred years ago the deed was proudly done. The results were shocking. We suffered 20 years of successive credit maladjustments and crises, the most disturbed and troubled we have ever known, barely escaping revolution" (*The Nation and Athenaeum,* 21 March 1925; Keynes 1981: 339).[12]

(in some contexts) that secular output is necessarily accompanied by a deficiency of purchasing power, thereby precluding sales at unchanged prices and profits. In the absence of land-scarcity problems enhancing the real costs of producing the wage basket and thereby encroaching on profits, there were no limits to secular output expansion.

[12]A broader mystery remains, how it came about that Ricardo's views were apparently lost from sight quite generally. Thus it has been proposed that "any decision not to return to gold at the prewar parity carried very great political costs, given the unanimity of informed business, financial and political opinion in such matters," and that this was one of the factors entering into Churchill's decision to return to gold at $4.86 (Moggridge 1969: 66–67). But Keynes was not, in fact, the only one to have access to the true Ricardo. Bonar cites the relevant passages from *The High Price of Bullion* and the Wheatley letter in his "centenary tribute" to Ricardo's Ingot Plan (Bonar 1923: 281–282, 293). Further study of those in the know, and the use – or non-use – made of such knowledge, is in order. Also promising dividends would be a systematic study of responsible authorities in Ricardo's day, who – apart from Ricardo himself – made a case for devaluation over severe deflation, to the end of evaluating the strength of the "orthodox party." Wheatley, as we have noted (see Note 7), is one candidate.

Keynes does, later in the year, mention Ricardo favorably in two respects: "Mr. Churchill has done what was expected, and the experience of a hundred years ago has repeated itself. With one improvement; – Ricardo's Ingot Plan, rejected then, has been adopted now, and the public are not to have back their sovereigns" (*The Nation and Athenaeum*, 2 May 1925; Keynes 1981: 357). Secondly, Keynes commended Ricardo's recommendation for "an appreciable margin between the buying and selling prices for gold, namely £3 17s. 6d. for the former and £3 17s. 10 1/2 d. for the latter," which if adopted would have provided some "limited protection" against the Bank's having to accept "unwanted gold in unlimited amounts" (*Economic Journal*, June 1925; Keynes 1981: 377–378). But these allowances – which somewhat lift the veil over what Keynes actually knew of Ricardo – provide scarcely a hint of the truth of the matter spelled out only 2 years earlier, and they are swamped by the reference to "the orthodox party" which "[t]hen, as now ... proclaimed that the one thing necessary was to restore gold convertibility."

I cannot imagine that amnesia played any part in Keynes's failure to reinforce his campaign. Two years are nothing for a young man of 42. The matter remains a mystery to me. But even as things stand, one may perhaps be allowed to suggest that since Keynes already declined in 1925 to make proper use of his knowledge of Ricardo, it is less surprising than it otherwise would be that a decade later in the *Essays on Biography* and again in the *General Theory*, he should have entered his positively deformed reading. The direction had been set in the 1920s and merely taken a step further. I would only hope to avoid having to rationalize the matter in terms of a deliberate distortion of the truth by Keynes in order to aggrandize his stature as innovator. Is there perhaps an alternative suggestion?

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Hollander on Anomalies in the History of Economic Thought: Some Comments

David Laidler

The history of economics is often treated as a rhetorical arsenal, a storehouse full of weapons available for deployment in current battles. Economists should not be too apologetic about this, because they certainly did not invent argument from authority. But we should at least try to get our history right if we are going to use it this way. As Samuel Hollander's erudite and entertaining paper shows only too clearly, even the most scholarly among us – could there be a more distinguished trio of historians of economic thought than Lionel Robbins, George Stigler and Terence Hutchison? – can slip up when cherished beliefs are at stake, let alone such a notoriously agile and unscrupulous debater as John Maynard Keynes. My main duty as discussant of this paper, then, is to say that its moral is utterly convincing. When we find what looks like an anomaly in someone's writings, we should ensure that it is really there, rather than in our own careless or prejudiced reading of them. The specific comments that follow are strictly secondary to this central message.

Smith on Usury

First, to Adam Smith on usury: Hollander is right to hint that these ideas are more likely to appear anomalous to those who want to deploy the rest of Smith's economic thought in defense of the virtues of unregulated markets than to others. This is not to go quite so far as to concede that the ideas in question are convincing (to me at least), but Hollander is also right to stress that they are the result of careful reflection on Smith's part, not of a careless slip when dealing with a topic that didn't much interest him.

The fact is that Smith consistently took what is, to anyone well versed in today's microeconomic theory (though not I suspect in modern behavioural psychology) an odd but nevertheless systematic view of behaviour towards risk. He thought of agents as being driven by a particular kind of risk preference – a proclivity to believe that the odds are stacked in their own favour – and not just in capital markets either. For example, when he discusses the factors that influence relative wages and profits (Smith 1776, Book 1 Ch. X) he makes a great deal of the "overweening conceit, which the greater part of men have of their own abilities" and its consequence that "the chance of gain is naturally over-valued" while "the chance of loss is frequently undervalued." And this, he thought was true not just in the labour market, but in business ventures and lotteries too. Smith's discussion of these matters, copiously illustrated with examples, runs for six closely argued pages – pp.122–128 – not just a

sentence or two, and his later discussion of speculators and projectors, and how usury laws might rein in their behaviour, is in exactly the same vein.

More generally, of course, a prohibition on usury was by no means the only government intervention in the market-place that Smith supported. In the financial sector he also argued for the prohibition of small bank-notes, not to mention a legal requirement of specie-convertibility-on-demand for larger ones; and elsewhere he defended agricultural protection as well as the Navigation Acts. When one considers the importance of banking, agriculture and merchant shipping to the eighteenth century British economy, it is hard to think of Smith's vision for it as entailing a free trader's paradise! And it is also worth recalling that even his case for the market itself rested on its capacity for promoting the division of labour rather than – his comments on the motives of butchers and bakers notwithstanding – for enhancing opportunities for maximizing agents to exhaust the gains available to them from voluntary and mutually beneficial exchange.

Engels, Marx and the Market

In short, today's advocates of the market should be careful about how they deploy Smith in their own defense, and they probably should not wear ties bearing his profile either lest they be taken for dangerous interventionists (In the interests of full disclosure – yes I did once own and sometimes wear one). Perhaps, in the light of Hollander's second anomaly, they should consider sporting lapel-pin portraits of Friedrich Engels and Karl Marx instead. Among Hollander's three topics, this is the one I know least about, and yet, as someone who has not read carefully in this literature beyond the pages of *Capital, Volume I* (Marx 1867) I found much of what he had to say here unsurprising. Who could read this book without realizing that its author regarded competitive capitalism, for all its proneness to cyclical instability, as a formidable engine of economic growth, or that there are good reasons why historians of macro-economics routinely to refer to a Marx-Schumpeter tradition in the sub-discipline's development? I'm bound to say, though, that aware though I also was of Marx and Engel's contempt for such "utopian" socialists as Karl Rodbertus – only a "name" to me – I was surprised to see how much they relied on analyzing the demand side of market mechanisms in general and prices in particular to justify it, and downright startled to have their resulting affinities to those other later Austrians, von Mises and von Hayek, drawn so mischievously to my attention.

And yet, I wonder if Hollander's defense of Engels and Marx against Hutchison's accusation of inconsistency is ultimately satisfying. Hollander suggests that, though they didn't give any systematic and detailed account of what their true – as opposed to utopian – socialist economy might look like, there is enough in their writings to indicate that they had in mind what he calls a rather simple "war-economy" in which the need for decentralized allocative mechanisms fades into the back-ground. Maybe, and I'm certainly not qualified to argue the point, but I can't

help wondering whether such an economy – which clearly has no attractions at all for Hollander himself – does not bear too much resemblance for comfort to the one that Lenin and Stalin tried to create in the twentieth century, and whether attributing too firm a grasp of its nature to Engels and Marx is the best way to defend their reputations as great nineteenth century economic thinkers. Perhaps, then, those lapel pins should go into the drawer along with the Adam Smith tie.

Keynes on Some Classics

Fortunately, there is no need to wonder what distinguishing sartorial symbols the modern Keynesian should display, because the current crisis seems to have made Keynesians of everybody. Even so, Hollander's paper surely warns us that our Keynesianism, whether old or new-found and however sincere, had better be selective. Whatever his merits as a theorist – I discuss some of these at another session at this conference – Keynes' comments on the writings of his predecessors, including David Ricardo, were more often than not misleading, and surely willfully so on some occasions, as Hollander hints. Everything that Hollander has to say about Ricardo's views on the dangers of falling prices, particularly but not only when associated with sudden monetary contraction, is true. His sometime student Timothy Davis (2005) has recently documented the facts of this case thoroughly and persuasively for a new generation of readers and Hollander's regret that Keynes usually ignored these facts is well justified. But two qualifications that soften his indictment in this instance nevertheless might be added.

First, the supply and demand theory of the long run value of specie that underlay Ricardo's qualms about the deflationary effects of even a smoothly executed return to convertibility, and that led him to devise his ingot plan as an alternative to the restoration of a gold coinage, preferable because it would economize on the monetary system's demand for specie, is not to be found in the *Principles* (see Ricardo 1817, pp. 14–15, 86–87). There, a more thoroughgoing cost-of-production theory of natural price is deployed, and the authority of that great book did tend to distract the attention of later readers of his works from Ricardo's much more carefully nuanced and down-to-earth policy commentaries. If, moreover, Ricardo himself could overlook the nuances in question as he saw his *magnum opus* through three editions, perhaps we can forgive Keynes for falling into a similar oversight between the *Tract* (1923) and the "Economic Consequences of Mr. Churchill" (1925).

Second, when we come to the specific question of Keynes' preference for Malthus over Ricardo, we need to remember that the co-ordination failure emphasized in the *General Theory* (Keynes 1936) was not associated with the behaviour of the price level in the market for currently produced goods and services but with the role played by the rate of interest in the market where resources are allocated over time. In the light of this distinction, one can understand why Malthus' "oversaving" theory of the "general glut," which Ricardo had opposed, might have

seemed to Keynes more immediately related to his own concerns than Ricardo's speculations about the danger that sudden monetary contractions could create what we would nowadays call an excess demand for money. Certainly this would be quite consistent with the amount of attention that Keynes paid, for example, to John A, Hobson's under-consumptionism in the *General Theory* and with his neglect of the monetary explanations of the depression then recently expounded by, say, Ralph Hawtrey (1932) or Irving Fisher (1933).

Concluding Comment

But none of these comments detract in the least from Hollander's basic theme, namely that those who deploy history as a weapon in debates about current issues should at least try to get it right – or at least not to get it obviously wrong – and that historians of economic thought have a part to play in helping them with this task.

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Reason and Reasonableness in Keynes: Lessons from *The Economic Consequences of the Peace* 90 Years Later

Maria Cristina Marcuzzo

Abstract The crisis that has hit the world economy has made once again topical the principles that Keynes advocated to address the loss of market confidence and the decline in production and employment. A re-reading of the *Economic Consequences of the Peace* points along a path parallel to the one usually associated with Keynesian policies – deficit spending and public investments – which relies on the concept of *reasonableness*. This paper outlines the context in which the *Consequences* was written and then traces Keynes's approach back to the Bloomsbury background. It is argued that the distinction between reason (or rationality) and reasonableness, to be seen also in Rawls, is one of the characteristic features of Keynes's economic thought which can be used to trace out a parallel between the humiliation of the conquered at Versailles and the debtor mortification inflicted on Lehman Brothers. The conclusion is that the return to Keynes we should wish for is not only a matter of supporting demand in order to avoid general deflation, or reform of the international monetary system to avert the effects of the present world imbalances, but more extensive application of the Keynesian concept of *reasonableness* against the so-called *rationality* of individuals and markets.

The Return of Keynes

Within just a few months from its publication, in December 1919, *The Economic Consequences of the Peace* was enjoying a resounding success (selling about 100,000 copies) and bringing fame to its author. Ninety years later, the argumentation

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and presentation still prove arresting and persuasive, particularly in the present crisis with Keynes back on the world scene, his thought once again seen as a source of inspiration and stimulus for reappraisal.

This *return* to Keynes may be accounted for by his role as the major exponent of an approach to economics based on the conviction that markets and economic behaviour are to be guided by a logic of coordination and rules, rather than left to the pursuit of individual interests and to the freedom resulting from the lack of public intervention and regulation by the institutions.

We owe to Keynes the recipes and remedies to fight mass unemployment, turbulence on the financial markets and in international trade, and the disorder of markets in the absence of coordination among the supranational institutions. The tools he used were those of persuasion and reasonableness, the means he applied to convince and to create scope for freedom of action where others – economists, above all – saw only constraints and the limits of resources. This approach was formulated and brought to ripeness as Keynes thrashed out his ideas over time, from the end of the First World War, during the years of the Great Depression and then of the Second World War, and on to the design of the new world order a few months before his death in the April of 1946.

Keynes was not only an outstanding theoretician but also an economist intent on seeking concrete remedies and solutions in his work for the British Treasury, as a representative of the United Kingdom in negotiations with the United States during the Second World War, and as a freelance journalist and operator on the financial markets. The 1930s saw him a speculator on the foreign exchange and commodity markets; this led him to the conclusion that market instability was an endemic feature of economic systems, to be curbed with interventions coordinated between the supranational institutions and the central banks. He drew up plans to bring order to the functioning of the raw material market, of multilateral exchange between countries and the regulation of international payments.

In his long activity as government adviser and leader of public opinion, Keynes was ever guided by the idea that the economic system was not regulated by immutable and eternal laws, but rather as a result of conventions, convictions and prejudices which the reformer had the task of moulding through persuasion.

The *Economic Consequences of the Peace*, written when Keynes was only 36 (he was born in 1883), anticipates and, in his attitude to the economic problems to be addressed at the end of the war, affords glimpses of an approach that was to mature in his subsequent writings, such as the *Tract on Monetary Reform* (1923), the *Treatise on Money* (1930), and the *General Theory of Employment, Interest and Money* (1936).

The book was a desperate attempt to persuade the public and governments that the Treaty of Versailles was to be modified on the basis of the principle of reasonableness, and not of the victor's revenge, "to avert the misfortunes which impend otherwise" (Keynes 1971b, CWK II, p. xxii). A concluding phrase sums up the intention, namely "The assertion of truth, the unveiling of illusion, the dissipation of hate, the enlargement and instruction of men's hearts and minds . . ." (Keynes 1971b, CWK II, p. 188).

With the crisis that has now hit the world economy, the principles to look to are once again those that Keynes advocated to address the loss of market confidence and the decline in production and employment, and the pages of the *General Theory* or, rather, interpretation of the Vulgate – deficit spending and public investments – are once again being referred to and re-utilised. A rereading of the *Consequences* and some hard thinking about the use of the term reasonable seem to me to point along a path not alternative but parallel to the one described above, and it is the path I wish to traverse in this paper.

To see what sort of guidance we can find there for the present, I will briefly outline the context in which the *Consequences* was written and the reasons behind it (Section "The Carthaginian Peace"), and then trace Keynes's approach back to the Bloomsbury background (Section "The Bloomsbury "Civilisation"); in section, "Reason and Reasonableness" I identify in the distinction between reason (or rationality) and reasonableness, to be seen also in Rawls, one of the characteristic features of Keynes's economic train of thought. Section "Was it *Reasonable and Fair* to let Lehman Brothers Go Bankrupt?" ventures to trace out a parallel between the humiliation of the conquered at Versailles and the debtor mortification inflicted on Lehman Brothers. In section "Some Tentative Conclusions" we briefly conclude that the return to Keynes we should wish for is not only a matter of supporting demand in order to avoid general deflation, or reform of the international monetary system to avert the effects of the present *world imbalances*, but more extensive application of the Keynesian concept of reasonableness against the so-called *rationality* of individuals and markets.

The Carthaginian Peace

In the 1918 armistice the United Kingdom and France included a clause to the effect that Germany was to pay "reparations" to the Allies for the damage inflicted on the civil population. In his first Memorandum, October 1918, to the Treasury, where he had been on leave from teaching at Cambridge University since the outbreak of war, Keynes lost no time in making it clear that, in determining the magnitude of reparations, due account should be taken of Germany's effective capacity to pay. In other words, the country's productive capacity was not to be destroyed, for this was the only way to ensure that Germany could pay the Allies.

Thus there were essentially two accounts to be reconciled: the extent of the damage caused by Germany and the probable magnitude of its productive capacity. While the former could be ascertained with a fair degree of certainty, the latter could only be estimated approximately. But – and this was the point Keynes stressed – there was not a reason in the world why the two sums, i.e. damage undergone and capacity to pay, should coincide, and, on the basis of Keynes's estimations, they did not coincide at all.

The UK General Election of December 1918 saw the triumph of Lloyd George, and a parliament of nationalists was formed which found itself in perfect accord

with the revanchism of the French allies under the leadership of an implacable Clemenceau. France and the United Kingdom were both set on making Germany pay the cost of the war, and not only the war damage, as established by the armistice[1]: if there was anything left to discuss, it was how to share out the spoils, while a severely depleted Italy could only hope that reparations might get the country back on its feet. Dominating the whole scene, now, was the United States, without whose contribution there could have been no victory for the Allies, and the philosophy of President Wilson, who, with his 14 points and the promises of the League of Nations, had set the terms of the armistice.

In Paris the "Four" victors (United States, France, United Kingdom and Italy) and their delegations were called upon to give concrete content both to the principles of self-determination and freedom for the new nations and to the claims for reparations to be made by the defeated to the victors – a hard and contradictory task.

Such was the scenario opening before Keynes's eyes when he arrived in Paris on 10 January 1919 as the representative heading the delegation of the British Treasury, sent to handle the financial aspects of the transition to peace.

The first opportunity to gauge the difficulty was a journey to Germany with the representatives of the US, Italian and French Treasuries to meet a German delegation. The meeting took place on a train. The German delegation was headed by the President of the Reichsbank, but it was another member of the delegation who made a strong impression on Keynes, namely Carl Melchior, a partner of the German Warburg Bank. Keynes was later to write a splendid memoir on him, published posthumously (Keynes 1971c, CWK X, pp. 389–429).

Keynes found the French at the Conference insufferable. He found them arid, incompetent and absurd in their demands on Germany, reserving particular loathing for their finance minister, Klotz. In March 1919, in an effort to persuade Lloyd George to revise demands for reparation, Keynes reassessed the ratio between what was being demanded and what might realistically be expected of Germany, but the British delegation showed scant response. By the month of May the draft of the Peace Treaty drawn up by 58 economic experts with the deliberations of the Council of Four (United States, France, United Kingdom and Italy) was ready, and Keynes was shocked. He decided to resign within the space of 2 or 3 weeks, bereft of any illusions about the possibility of obtaining "substantial modifications" (in his own words) to the terms contemplated.

And in fact, when the Treaty of Versailles was signed on 28 June 1919 Keynes had already left Paris for the peace and comforting familiarity – unorthodox, but typically Bloomsbury – of Charleston, the country house where Vanessa Bell, Duncan Grant and David Garnett enjoyed a *mènage à-trois*, and had started work on his book. It took 3 months to finish; Keynes drafted it during the summer, and as

[1]As has been pointed out (Amato and Fantacci 2009, p. 185, my translation): "Moreover, the unilateral alteration of the conditions previously agreed upon constituted, for Keynes, unscrupulous violation of that sacredness of international conventions in the name of which the Allies had declared war on the invader of neutral Belgium".

the text took shape he read passages from it to his Bloomsbury friends (Moggridge 1992, p. 321). I will return to the subject in the following section.

The theme of the *Consequences* is how the war had damaged the delicate economic mechanism thanks to which Europe had been able to live in prosperity before 1914, and how the Treaty of Versailles had not repaired it but completed its destruction: "My purpose in this book" – wrote Keynes – "is to show that the Carthaginian Peace is not practically right or possible" (Keynes 1971b, CWK II, p. 23), going on to point out (Keynes 1971b, CWK II, p. 40):

> I am mainly concerned in what follows, not with the justice of the treaty – neither with the demand for penal justice against the enemy, nor with the obligation of contractual justice on the victor – but with its wisdom and with its consequences.

Here I would like to dwell on the words "wisdom" and "consequences" as the yardstick Keynes always adopted to evaluate any intervention in the economic sphere, particularly in times of crisis or major upheavals. The word "wisdom", vague as it may seem, actually refers to a guiding principle of human behaviour that breaks away from individual utilitarian calculation, which brings apparently some but occasionally illusory advantages. It is a principle invoked to curb the forces that threaten to disrupt the social order, and, rather, to favour the settlement of conflicting interests with the logic of social civility.

Keynes based his analysis of the consequences of economic decisions or measures on the interrelation between, on the one hand, the production of wealth and its distribution, and, on the other hand, the social organisation upon which it rests. Disruption of the orderly, progressive increase in individual and collective well-being generates obscure and irrational forces that undermine security and freedom within society.

In the *Consequences*, as in the *General Theory* and in his addresses to the British Parliament calling for approval of the Bretton Woods agreements – that is, the architecture of the international monetary system in the aftermath of the Second World War – Keynes would invariably arm himself with these means to alert against the risks and dangers of high unemployment, growing foreign debt drastically squeezing consumption, mortifying national identity on sanctioning the subjection of one country to another.

Keynes never abandoned the ideal of a civilisation based on individual freedom but not at the expense of collective good; in vain, all too often, he sought to demonstrate that this was not practicable if founded solely on the pursuit of private interest – whether of individuals or of nations – following a *homo homini lupus* rationale in a market without rules or an international organisation devoid of institutions deputed to settle conflicts.

In the *Consequences* the need is summed up thus (Keynes 1971b, CWK II, p. 60):

> . . . particular interests and particular claims, however well founded in sentiment or in justice, must yield to sovereign expediency.

Let us now look more closely into Keynes's reasoning in relation to the issue dealt with in the *Consequences*, namely the Treaty of Versailles, which brought an

end to one of the bloodiest wars Europe had seen, and how the victors set about addressing the economic and social devastation in the aftermath to set things aright.

The conceptual and political crux of the peace of Versailles lay in the victors' demands that the defeated should bear the cost of the war, to sap the strength of a powerful and dangerous enemy forever by crippling the sovereignty and economic influence of Germany and, at the same time, burdening the country with the cost of reparations, *as if* its productive and financial potential had remained intact at the pre-war level.

But let us see in detail how Keynes came to this conclusion. His estimates of the war damage – deriving from close, painstaking examination – are summarised in the following figures (all in millions of pounds sterling): Belgium (500), France (800), United Kingdom (570), Other Allies (250) for a total of 2,120. Set not only on claiming reparation for material damage, France and the United Kingdom, riding the wave of hate and vengefulness, were also determined to include in the reparations of pensions and benefits to be provided at home, bringing the overall bill to about 8,000 million pounds sterling.

In defence of such a sum, which Clemenceau and Lloyd George rhetorically held sufficient to satisfy the public sentiment and their electorates, was the hypocritical conviction that this was also what Germany – whose economic power was to be overthrown – was effectively able to pay.

But, according to Keynes's estimates, how much would Germany have in fact been able to pay? Taking into account the immediately transferable wealth (gold, ships and foreign securities), property present in territories ceded or surrendered on the basis of the armistice, and exports yielding foreign currency to pay the Allies, the figure came to no more than 2,000 million, and thus the 8,000 million demanded by the Treaty were certainly "not within the limits of reasonable possibility".

The force of Keynes's arguments is inescapable: a peace based on the principle that "*Germania delenda est*" was neither just nor, above all, practicable, not only as a matter of human justice, which does not authorise nations to "visit on the children of their enemies the misdoings of parents or of rulers" (Keynes 1971b, CWK II, p. 142), or of the political consequences that humiliation of the defeated can entail for the victors, too, but also on account of the jeopardy unleashed on the overall economic order. Depriving Germany of sovereignty, its foreign possessions and internal productive capacity meant in practice preventing the country from meeting the demands for war damage reparation. Deprived of its colonies, of trade relations, merchant fleet, ten percent of its territory and population, a third of its coal production and three quarters of its iron production, with two million killed in the war, its currency reduced to a seventh of its value, and a huge public debt, how was it possible for Germany to pay an indemnity calling for a level of economic activity actually higher than had been achieved by the country before the war?

The sheer folly of the demand – ferociously advanced by France (i.e. by Clemenceau), feebly opposed by the United States (i.e. by Wilson) and cynically supported by the United Kingdom (i.e. by Lloyd George) – lay in the misappraisal of Germany's effective capacity to pay and the short-sightedness of statesmen

whose preoccupations related not to the future of Europe but solely to "frontiers and nationalities, to the balance of power, to imperial aggrandizements".

The consequences of the political and economic destruction of a country were not seen to extend to the inevitable impact on its trading partners, who would also be sucked into a vicious circle of stagnation.

Characteristically, Keynes's analysis entails identification of the remedies – in this case, essentially a matter of lightening the burden placed on Germany to allow for recovery in production and trade (and the other countries needed to be able to benefit from Germany's recovery to sustain their own); settlement of inter-ally indebtedness to avoid internal constraints on the victorious countries in their policies to relaunch their economies, and an international loan to help boost all the economies (Keynes 1971b, CWK II, p. 169):

> . . . an economic system, to which everyone had the opportunity of belonging and which gave special privilege to none, is surely absolutely free from the objections of a privileged and avowedly imperialistic scheme of exclusion and discrimination.

And yet today Keynes's *remedies* strike us as far from having any chance of being taken up as they were at the time. Asking the United States to forgo the repayment of loans granted above all to France and the United Kingdom, to sustain the war effort as precondition to ask these two countries to forego reparations from Germany, and actually proposing and inter-ally loan to get the economies moving after the devastation of 4 years of destructive fury, attests to Keynes's great economic wisdom, but also, perhaps, to a certain political naivety.[2] On the other hand, it might be seen as an example of that appeal to "reasonableness" that could be interpreted in modern terms of reciprocity (as in Rawls, for instance) or, more probably, associated with that concourse of ideas upon which Bloomsbury thrived.

The Bloomsbury "Civilisation"

Bloomsbury is a district of London where there lived – in many cases living together – a group of friends who shared a lifestyle and a passion for art and literature, holding social conventions and the morals of their time in contempt.[3] At the core of the group were Vanessa, Virginia and Adrian Stephen, Clive Bell (who married Vanessa), John Maynard Keynes, Lytton Strachey, Duncan Grant

[2]According to De Cecco "at the Paris conference strangling economic conditions were imposed on Germany . . . [but] there was no intention to respect them . . . what happened at the peace table was dictated not by the stupidity or wickedness of the protagonists, so much as the need to give the masses which their political classes had drawn into the war proof that the sacrifices had not been in vain" (De Cecco 1983, pp. 18–19, my translation). Keynes, according to this thesis, became aware only "dimly of the scandalous 'political exchange' that was taking place" (p. 20).

[3]In the pungent description by the most authoritative biographer of Bloomsbury, Michael Holroyd, "all couples were triangles who lived in squares" (Holroyd 1967, p. 413).

(with whom Vanessa had a daughter, who eventually married David Garnett), Desmond MacCarthy, Roger Fry and Leonard Woolf (who married Virginia). Then there were the other, outer members, like the writer E. M. Forster, James Strachey (brother of Lytton and translator of Freud), the painter Dora Carrington (who lived with Lytton Strachey) and many other exponents of twentieth-century British culture.

These friends met regularly, exploiting the occasion of dinners, receptions and travels to discuss common ideas rooted in the absolute value attributed to aesthetics and bearing witness to a *religion* that saw the highest form of human expression in art. Roger Fry and Clive Bell, drawing upon certain aspects of the philosophy of G. E. Moore, theorised the principles of "Civilisation" based on "reasonableness and a sense of value" (Clive Bell (1928) *Civilization*, p. 54, cited in Goodwin 2006, p. 223).

A cornerstone of this "civilisation" consisted of the life of the imagination, which Fry and Bell identified with artistic activity. We may, however, also interpret it as an activity that surpasses the constraints and limitations of biological and material existence through the transformations and sea-change that art – but also science – can achieve. There is an echo of this in one of the concluding sentences of the *Consequences*: "setting in motion those forces of instruction and imagination which change opinion" (Keynes 1971b, CWK II, p. 188).

During the First World War Keynes came under heavy criticism from his Bloomsbury friends – many of them conscientious objectors – for working for the government and for a war that had come about to defend a world and lifestyle they detested. We must also bear in mind that Keynes often exploited the privilege of his position to help his friends obtain exoneration from military service and to find jobs for them in the civilian world as a sort of refuge from the war. And it was this book, written and read within the sphere of the Bloomsbury group, that reconciled Keynes's two commitments: to the world of his friends and to public life within the institutions.

As we have seen, most of the book was written at Charleston during the summer of 1919. We know the date when he started – 23 June – from a letter by Keynes to Oscar Falk (Skidelsky 1983, p. 376), and of the end of the first draft –11 October – (Skidelsky 1983, p. 382).

The book was greatly appreciated in Bloomsbury. Lytton Strachey wrote to Keynes on 16 December 1919, 4 days after its publication (quoted in Dostaler 2007, p. 150):

> As to the argument it is certainly most crushing, most terrible … To my mind the ideal thing would be to abolish reparation altogether – but of course that is not practical politics.

Virginia Woolf wrote that it was "a book that influences the world without being in the least a work of art: a work of morality" (quoted in Goodwin 2007, p. 275), but even more significant is her letter to Benedict Nicholson, of 24 August 1940: "Maynard is Bloomsbury. He wrote the [Economic] Consequences of the Peace" (quoted in Goodwin 2007, p. 290).

What characterised Bloomsbury, as defined, for example, by Quentin Bell (1968), was the ideal of reason in the service of enjoyment of the pleasures of life, trusting in the exchange of different points of view – in a word, placing their faith in "rational conversation" as a means of fending off the irrational urges prompted by religion, nationalism or superstitions. This, essentially, was what belonging to Bloomsbury meant, not only for this particular book, but for Keynes's entire output.

And yet in *My Early Beliefs* (the essay read in 1938, but published posthumously – as Keynes had expressly wished it to be – and addressed to the Bloomsbury friends),[4] Keynes stated that he had subsequently abandoned his "juvenile" creed, incubated in the sphere of the Apostles (the forerunners of the Bloomsbury group), and in particular the conviction that "human nature is reasonable" (Keynes 1971b, CWK II, p. 447). According to R. E. Braithwaite, however, Keynes had no intention of including the moral principles of Moore's *Principia Ethica* in his outgrown convictions (Braithwaite 1975, p. 245), least of all the tenet that an action can be judged to be just solely in the light of its consequences ("good as a means"). This interpretation of Keynes's position finds confirmation in the celebrated dictum in the *Consequences* which we have already met (Keynes 1971b, CWK II, p. 40, italics added):

> I am mainly concerned in what follows, not with the justice of the Treaty . . . but with its wisdom and with its *consequences*.

Thus it was not in reason – the Bloomsbury creed – that Keynes continued to place his trust, for it does not always succeed in guiding behaviours which may be prey to the obscure and irrational forces of both individuals and markets, but in reasonableness as moral criterion, as judgment of the consequences of choices.

If Keynes subscribed to a consequentialist ethic, just how this sat with his rejection of utilitarianism and whether or not he continued to embrace Moore's ideas unwaveringly are questions beyond our scope here. I merely wish to point up the idea that behaviour guided by "wisdom" with a view to the consequences is indeed that "reasonable and fair" behaviour to which Keynes attributes the value of right choice and moral duty. In the words of A. Carabelli, who studied the issue in depth (Carabelli 1994, p. 219):

> . . .the problem of right conduct or moral duty in ethics is by Keynes, so to speak, dissolved into that of reasonable action . . . Right action or duty is simply reasonable action. Keynes maintains that what matters is the reasonableness, not the absolute rationality of action.

[4]The essay was published together with *Dr Melchior: A Defeated Enemy*, in a book entitled *Two Memoirs*, in 1949. David Garnett, who wrote a brief introduction, recalls that "Over a long period, we met together two or three times a year, dined at a restaurant, and after dinner revived our memories of the past listening to one, or more often two, memoirs read aloud by different members of our company" (Keynes 1971c, CWK X, p. 387).

Reason and Reasonableness

At the end of the Second World War, when the Treasury was again getting to grips with war finances and post-war economic conditions – but now also as negotiator with the Americans – Keynes took a position strikingly similar to the approach he had taken in the *Consequences*. As a principle of justice and wisdom, he asked the United States to waive payment of the debts incurred by the United Kingdom, which had "long borne the costs alone". He suffered a resounding and indeed crushing defeat, failing in his efforts while also having to get the British Parliament to ratify conditions far worse than he had himself anticipated (Marcuzzo 2008). But let us take a brief look at the facts.

A fortnight after cancellation of the Lend–Lease programme[5] – subsequent to the Japanese surrender – Keynes returned to Washington in September 1945 for his fifth mission as Treasury envoy; he had outlined his strategy to secure US aid in a Memorandum of 18 March of that year, evoking the scenarios opening up for the future UK economy as *Starvation Corner*, *Temptation* and *Justice*. *Starvation Corner* described the effects of efforts to be financially independent of the United States through a policy of rationing and controls following a line of economic autarchy and isolationism; *Temptation* was the choice to ask the United States for a commercial loan in return for a commitment to multilateralism and dismantling imperial preferences, but it was the third solution – *Justice* – that Keynes saw as the only "reasonable alternative" (Keynes 1971d, CWK XXIV, p. 291). The Americans were, as an act of justice, to grant financing in the form of a "free gift" allowing the United Kingdom to return to normal peacetime conditions of production and consumption, and to embark upon multilateralism in payments and international trade (Carabelli and Cedrini 2010).

If the repayment of UK debts had been negotiated on purely commercial bases, as the Americans were set on demanding – and eventually they had their way – the United Kingdom would have to squeeze internal demand drastically, and this would lead to deflation with worldwide repercussions. As we know, such consequences were to some extent avoided thanks to the Marshall Plan and massive American aid for the reconstruction of Europe.

The argument in favour of the "free gift" followed much in the line of the *Consequences* reasoning (Keynes 1971d, CWK XXIV, pp. 291–292, italics added):

> It is only by a more comprehensive settlement, which attempts to offer everyone what is *reasonable*, and so far as we can make it, fair, that the financial consequences of the war can be liquidated. This is the aim, namely, that as between the partners to the war, its financial consequences, in so far as they affect future economic intercourse between them, should be so far as possible liquidated.

[5]The US programme (voted in 1941) to supply the UK with what it needed "not in exchange for money but acknowledged by some 'consideration' to be negotiated later" (Moggridge 1992, p. 652).

In this Memorandum of March (circulated in revised form on 15 May at the Treasury) Keynes once again takes up disclosure of the truth, awareness of the consequences and the arms of persuasion against the obscurity of politics, ignorance of realities and entanglement of interests.

The negative conclusions to draw from a possible American refusal also echo the judgment of the allies' intransigence on reparations passed in the *Consequences* (Keynes 1971d, CWK XXIV, p. 293):

> The Americans would have lost the sense of magnanimity for a financial benefit which is useless to them and even perhaps injurious. This variant would only appeal to those who believe that their duty to God and to mankind requires that every action must be at least dressed up to look like "business".

As we have seen, Keynes systematically applied the term *reasonable*, often in contrast with the reasons of the victor or creditor, to a guideline *not* characterised by utilitarian calculation, which may prove only apparently to be in the individual interest. Thus reasonable action is guided by judgment, taking into account contingent, mutable circumstances as far as our knowledge can encompass the facts (Carabelli 1994, p. 219).

The same term was used by Rawls in defining the characteristics of a plural and *just* society. In his book *Political Liberalism* we find this definition (Rawls 1993, p. 58):

> The reasonable is an element of the idea of society as a system of fair cooperation and that its fair terms be reasonable for all to accept is part of its idea of reciprocity.

But how exactly are we to take the term *reasonable*? Habermas interprets it as distinguishing between those who accept the principle of *fairness and cooperation* and those who act *rationally* on the basis of their *own* (i.e. individual) conception of what is *good and just*. Thus being *reasonable* is a moral quality lacking in those who behave in a solely rational way.[6]

This, according to Habermas, is the source of the distinction between moral and ethical questions (Habermas 1995, p. 125):

> Questions of justice or moral questions admit of justifiable answers – justifiable in the sense of rational acceptability – because they are concerned with what, from an ideally expanded perspective, is in the equal interest of all. Ethical questions, by contrast, do not admit of such impartial treatment because they refer to what, from the first-person perspective, is in the long run good for me or for us – even if this is not equally good for all.

The sense Keynes attributes to the term *reasonable* shows a strong analogy with the quality described by Rawls and interpreted by Habermas as *moral,* but it is anchored to the structure of his economic theory. In fact, Keynes takes the example of the fallacy of composition to show why the *rational* pursuit of individual interest does not guarantee the collective good, in this case identified as full employment.

[6]"What rational agents lack is the particular form of moral sensibility that underlies the desire to engage in fair cooperation as such, and to do so on terms that others as equals might reasonably be expected to endorse" (Rawls 1993, p. 51).

For example, attempts to reduce real wages or increase the saving of individuals on the basis of an individual rationale will not achieve the aim if undertaken by all, since aggregate prevails over individual effect. However, the impasse of failure to achieve the aggregate effect of full utilisation of resources can be remedied with a set of direct and indirect instruments designed to overcome individual inertia and generate the level of demand necessary to raise the level of employment.

In the case of the reparations and debts consequent upon the two world wars, the fallacy of composition is manifested in the form of a deflationary potential for *all* the economies – a level of demand kept drastically low within a country to satisfy the *reasons* of the victor or creditor. Thus lack of reciprocity or reasonableness leads to consequences not only morally reprehensible but also economically disastrous for anyone who has looked for guidance solely from the individual point of view.

In his introduction to the Italian edition of the *Consequences*, Marcello De Cecco applied Keynes's categories during the 1973–1974 crisis to denounce the policies of the International Monetary Fund, which, faced with the enormous debt run up by the international private banking system, required the "principal debtor countries to adopt policies entailing brutal deflation of the internal demand" (De Cecco 1983, p. 21). In my rereading of the *Consequences* I would like to add another example of victor policy, suggesting comparison with the episode that sparked off the latest crisis.

Was it *Reasonable and Fair* to let Lehman Brothers Go Bankrupt?

A number of authoritative commentators see in the decision to let Lehman Brothers go bankrupt the origin of the spate of devastating consequences on production, employment investment and consumption, and thus on exports (especially in Germany and Japan) that was only stemmed by the turnabout in public intervention policies.

The collapse of New York's banking giant on 15 September 2008 was the most spectacular bankruptcy in the history of the United States, and probably of the world, with $613 billion worth of banking debts and 155 of bond debts. The shock to the financial markets brought Standard & Poor's 500 Index plunging in the sharpest annual fall since 1938, froze the credit market, forced Goldman Sachs to apply to Warren Buffett for an investment of $5 billion in preferred shares and triggered a run on Treasury Bills that set yield slumping below zero for the first time (Stacy-Marie Ishmael in the *Financial Times*, May 4, 2009).

Comparing the Lehman Brothers bankruptcy with the reparations forced on the country that lost the First World War (as was the case of Germany in 1918) or the demand that the country that won the Second World War repay her debts (like the UK in 1945) is indeed audacious and perhaps farfetched, given the different scale on which social and economic ills and the tragedy of so many human lives

entailed by the wars are measured. And yet the consequences of the decision – apparently taken on the basis of economic rationality – of letting Lehman Brothers crash are of a systemic and moral order such as to suggest reconsidering the matter with the categories applied by Keynes on those two occasions.

We might even use the scheme of the *Consequences* and, if only we could, imitate the style of the celebrated portraits of Lloyd George, Clemenceau and Wilson to tell the story, with its protagonists (Paulson, Geithner, Bernanke, Fuld), of how Lehman Brothers was eventually left to crash on that fateful week-end of September 2008.

Two days after the publication of the figures for the third quarter, showing losses amounting to close on $4 billion in the Lehman Brothers' balance sheet, the Federal Reserve convened an urgent meeting in its New York premises, inviting all the major investment banks of Wall Street. It was Friday 12 September. Hosting the meeting were Hank Paulson (Dartmouth and Harvard MBA), US Treasury Secretary, and Tim Geithner (Dartmouth and Johns Hopkins MA), President of the New York Fed. It was immediately made clear that the Bush administration held that it was not up to the taxpayers but rather to the Wall Street banks to throw the rope to haul Lehman Brothers up from the precipice.

Subsequently Paulson defended himself asserting that given the Fed statute the government could not grant loans without collateral (which Lehman Brothers could not provide), and the point was also borne out by Bernanke, Chairman of the Federal Reserve, in an interview a few weeks later.[7]

However, accounts of the meeting reported by the press and emerging from the official statements give a rather different picture of Paulson's reasons *why* Lehman could not be saved. As he himself put it to the journalists on that Monday 15 September when application was submitted for recourse to Chapter 11, the US bankruptcy law which allows firms using it to restructure in receivership: "I never once considered it appropriate to put taxpayer money on the line". The US government would not come in to save firms on the brink of bankruptcy because it "would just invite more foolish risk-taking. It would create a 'moral hazard'".[8]

Moral hazard applies to that behaviour which seeks to maximise gains, characterised by risk propensity or scant care about avoiding or minimising losses, encouraged by the conviction that State intervention would be inevitable in the case of failure.

But let us return to that Friday 12. In the Lehman Brothers premises the CEO Dick Fuld (University of Colorado and New York Stern Business School) was waiting in his office convinced that the game was going in his favour. Six months before, JP Morgan had saved Bear Stearns with a Fed loan (Tim Geithner playing a

[7]"A public-sector solution for Lehman proved infeasible, as the firm could not post sufficient collateral to provide reasonable assurance that a loan from the Federal Reserve would be repaid, and the Treasury did not have the authority to absorb billions of dollars of expected losses to facilitate Lehman's acquisition by another firm" (at http://www.usnews.com/blogs/the-home-front/2008/10/15/ben-bernanke-why-we-didnt-bail-lehman-out.html).

[8]E. Thomas and M. Hirsh. Paulson's Complaint. *Newsweek*. May 24, 2009.

leading role), and in early September the Fed had taken over control of Fannie Mae and Freddie Mac, the two giants of the real estate mortgage market, also on the brink of bankruptcy. If they did not manage to find a buyer – for weeks Fuld had been desperately searching for one – after the South Koreans withdrew from negotiations, it was widely believed (not only by Lehman, but by many banks the world over) that the Fed or the government would certainly intervene. Fuld was not invited to the meetings, and his vice Bart McDade was leading the Lehman delegation (Sorkin 2009, p. 306). Fuld was continuing his quest in the market environment. One possibility was to make a proposal to the Bank of America, and Fuld immediately set about contacting the Chairman, Ken Lewis, but at the very same time Lewis was finalising an agreement with John Thain, Chairman of Merrill Lynch, to acquire that bank. He would not call Fuld back; Lehman Brothers was not able to offer the system of retail brokers that the Bank of America was interested in. Subsequently it came to light that it had been Paulson who prompted Thain – a Goldman Sachs work associate – to meet Lewis for the possible acquisition of Merrill Lynch. It is worth adding that an inquiry of the Committee on Oversight and Government Reform is now (June 2009) in progress,

> . . .investigating the events surrounding Bank of America's acquisition of Merrill Lynch and the role the Federal Reserve Board (the Fed) and the Department of the Treasury played in that transaction.[9]

On Sunday morning Fuld played his last card with Barclays, the big British bank, but there was the complication that nothing could be ratified without the shareholders' assembly; Paulson no longer answered Fuld's phone calls. So it was that when McDade brought an end to the "weekend of fear" with the Fed and government top officials that Sunday, bankruptcy had to be declared before the European and Asian markets opened the following day, and there was nothing left to do.

Thus Hank Paulson, nicknamed "the hammer" – a non-smoking teetotaller, Christian Scientist and keen bird watcher, of the Harvard and Goldman Sachs tribe, got the better of Dick Fuld, nicknamed "the gorilla", a floor trader with state-school education who attended MBA evening classes, and a gambler with a penchant for risky bets. Was this a clash between two ethical codes, two world views,[10] or simply the liquidation of a rival in the world of investment banking that was to be restructured? This was the indictment launched by Dick Fuld, who stated before the Waxman Committee[11] on 6 October 2008:

[9]At www.oversight.house.gov.

[10]At the time of writing (7 June), the magazine *Time* carried out an opinion poll among its readers on the 25 people to blame for the financial crisis, and among them appeared the names of Fuld and Paulson; of the two, the more blameworthy according to the number of votes was the Treasury Secretary.

[11]Statement of Richard S. Fuld before the United States House Committee on Oversight and Government Reform, Causes and Effects of the Lehman Brothers Bankruptcy, October 6, 2008 (at www.oversight.house.gov).

> On the same day Lehman Brothers prepared to file for bankruptcy, the Federal Reserve significantly broadened the types of collateral all banks were able to pledge to the Federal Reserve to create additional liquidity, the lifeblood of our system, and the Federal Reserve also adopted, on a temporary basis, the type of exemption that Lehman Brothers had applied for earlier. Had these changes been made sooner, they would have been extraordinarily helpful to Lehman Brothers. A few days later, the Federal Reserve took expedited action to approve applications of Goldman Sachs and Morgan Stanley to become bank holding companies.

It was Ben Bernanke (Harvard and MIT), who had studied the Great Depression bank crashes in depth and upheld the effectiveness of the New Deal regulations,[12] who prevented total collapse by opening the Fed coffers – with loans amounting to $1 trillion – and persuading Paulson to go before a recalcitrant Congress to gain consensus for exceptional measures.

The week-end after the one so fateful for Lehman Brothers, AIG was *saved* by the government with an outlay of $85 billion and control of an equity stake of 80% of the capital. Just a month later, the considerations that had stood in the way of saving Lehman Brothers did not apply, or no longer applied. Bernanke submitted two arguments to justify intervention, in this case, by the Fed:

> In the case of AIG, the Federal Reserve and the Treasury judged that a disorderly failure would have severely threatened global financial stability and the performance of the U.S. economy. We also judged that emergency Federal Reserve credit to AIG would be adequately secured by AIG's assets.[13]

With regard to the first point, the Fed and Treasury deemed that in the case of Lehman Brothers bankruptcy would not have "severely threatened global financial stability and the performance of the U.S. economy"; unfortunately, the argument proved ungrounded and the suspicion remains that the judgment was prompted by another motivation, namely reprisal against the debtor, given an exemplary answer at the level of market law rationale.

Questionable, too, is the second point, i.e. that Lehman Brothers lacked sufficient collateral to obtain credit, if it is in fact true that when it crashed Lehman had book equity amounting to $26 billion, as also emerges from the evidence given by Luigi Zingales to the Waxman Committee:

> The Lehman CEO will likely tell you that his company was solvent, and it was brought down by a run. This is a distinct possibility. The problem is that nobody knows for sure.[14]

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[12] "It might be argued that the federally directed financial rehabilitation – which took strong measures against the problems of both creditors and debtors – was the only major New Deal program that successfully promoted economic recovery" (Bernanke 1983, p. 273).

[13] At www.usnews.com/blogs/the-home-front/2008/10/15/ben-bernanke-why-we-didnt-bail-lehman-out.html.

[14] According to Zingales, "the doubts about the value of its assets combined with the high degree of leverage created a huge uncertainty about the true value of this equity. It could have been worth $40 billion or negative 20" (United States House Committee on Oversight and Government Reform, at www.oversight.house.gov).

The case of Lehman Brothers – the only investment bank left to go bankrupt in the USA or Europe – has yet to be studied, its darkest corners scrutinised, above all in terms of the *reasons* of its protagonists.[15]

Some Tentative Conclusions

The distinguishing feature of the Keynesian approach is a conception of economics as extension of possibilities, as opposed to behaviours guided by particular interest, proposing remedies to safeguard the general interest as condition for prosperity and social harmony.

Forging his approach under the enlightening influence of Bloomsbury and Moore's *Principia*, Keynes combined the criterion of consequentialist justice with criticism of the fallacy of composition, which he saw in classical economic theory, to contest the equation: individual interest = collective good.

The return to Keynes repeatedly called for in the emergency of the economic earthquake that began in 2008 and the economic crisis persisting into 2009 has taken the form of large-scale public intervention, extraordinary injections of liquidity and abandonment of the rhetoric about the superiority of the market.

There is, however, an aspect of Keynes's approach that does not seem to have received as much attention. Economic rationality seems to authorise demands to bring debtors to book, imposing indiscriminate sacrifices, ignoring the pleas of the weakest, invoking rigorous laws and threatening social protection and security. By contrast, Keynes's reasonableness appeals to judgment on the basis of the circumstances, to exercise of the imagination and creativity in seeking solutions characterised by analysis of the consequences from the overall point of view.

Foregoing exorbitant war reparations from Germany and cancelling the UK debts – solutions proposed as conditions for a *common* future of prosperity – are examples of the philosophy that Keynes promoted and pursued. The same principle of *reasonableness* should have guided the decision to save Lehman Brothers, abandoning the logic of uncertainty about the true value of its assets and the idea of sending a message to the investment banks or simply defeating a formidable competitor. Consideration of its consequences for the stability of finance and economic growth should been allowed for.

Comparing the Lehman Brothers crash with the decisions taken at Versailles and in the Anglo-American negotiations of 1945 serves the purpose of drawing upon the lesson of the *Consequences* and Keynes's teaching. The contexts are manifestly

[15]The conclusion of a recent and very well documented account of the events is: "it cannot be denied that federal officials – including Paulson, Bernanke, and Geithner – contributed to the market turmoil through a series of inconsistent decisions. They offered a safety net to Bear Stearns and backstopped Fannie Mae and Freddie Mac, but allowed Lehman to fall into Chapter 11, only to rescue AIG soon after. What was the pattern? What were the rules? There didn't appear to be any..." (Sorkin 2009, p. 535).

different, but we can see prevailing the same logic of confrontation between personalities (Wilson, Clemenceau and Lloyd George in the first case; the American Morgenthau, Secretary of Treasury, White, Treasury Director of Research, and Keynes in the second; Fuld, Paulson and Geithner in the third), and contrasting interests (the Allies against Germany, the Americans against the British, the Treasury and the Fed against Lehman) rather than a solution that was and should have been seen as *reasonable*.

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Comments on Professor Marcuzzo's Paper

Moshe Justman

The theme that Professor Marcuzzo has placed at the center of her fascinating paper, the distinction between rationality and reasonableness, recalled discussions I had some years ago with our departed colleague and my good friend Ehud Zuscovitch, who regularly taught a course on the History of Economic Thought (from Aristotle to Adam Smith) in our department. Ehud received his economic education in France and became a Directeur de Recherche in the CNRS as well as Professor of Economics at Ben Gurion University, which like all university economics departments in Israel is dominated by the Anglo-American tradition. He first introduced me to the distinction between the rational French and the reasonable British, and Professor Marcuzzo's threefold application of this distinction, between rationality and reasonableness, in this power illustrates its potential scope.

Professor Marcuzzo first, and most persuasively, applies it to the events John Maynard Keynes describes in The Economic Consequences of the Peace, the peace negotiated at Versailles after the First World War. There were certainly other forces at work, too, which shaped the negotiations at Versailles – the French having repeatedly suffered at the hands of the Germans were more apprehensive of German reconstruction. And, of course, there was some popular thirst for revenge after a long and bitter war, which the elected officials who led the national delegations could not ignore. (Civil servants, like Keynes, have the privilege of greater dispassion.) But looking back on these events it seems clear that the neither the French nor the British delegation, led by the Welsh Prime Minister Lloyd-George, were acting reasonably: they failed to see what they should have been able to see even at the time, what seemed evident to Keynes, that the peace negotiated at Versailles was unreasonable for Britain and France because it was inconsistent with *their* economic interests, as only an economically viable Germany could realistically be expected to make substantial reparations.

Applying this distinction to the disagreement between the United Kingdom and the United States regarding the economic consequences of the Second World War seems less straightforward. In this case, the essential underlying difference between the parties was not between reasonableness and rationality but between two different implicit perspectives on the war. In the British view, which Keynes argued, the war was a partnership between Britain and America to defend the cause of freedom against the totalitarian threat of the Axis powers – a partnership in which Britain had borne a disproportionate share of the costs. It was therefore only fair, or "reasonable" (perhaps in a slightly different sense of the word) that the United States forgive British debts. "If the tables were turned", the unspoken argument runs, "and it were the United Kingdom rushing to the rescue of the United States, we would forgive your debts. It is only reasonable that friends thus share risks and not exploit a temporary advantage to the full".

Of course, the United States saw the war in a different light altogether. The war in Europe was precisely that, a war in Europe, in which the United States was helping its friends (and pursuing its interests), but a war which certainly posed no threat to its own territorial integrity. As America saw it, Britain was waging a war it risked losing badly and the United States stepped in, as a friend, sacrificing its material resources and the lives of its soldiers in Britain's aid; and lending Britain the resources it needed to continue the war – without immediate consideration of payment. The implicit notion of reciprocity that underlies Keynes' call for reasonableness, were it stated explicitly, would have been laughed out of the room. This was clearly a one-way street and would continue to be so in the foreseeable future.

Indeed, the Americans might have argued with equal if not greater force that Britain was not being reasonable. Rather than offer compensation to the United States for the losses it incurred to save Britain from the Nazi threat, it was asking the United States to foot the bill for Britain's part in the war, as well. What the Americans offered instead was to roll over the debt, extending much needed credit for the rebuilding of Europe through the Marshall plan. While this was certainly less favorable for Britain it was not detrimental to American interests, and hence not an unreasonable position for the United States to take.

Versailles was unreasonable for the Allies because it was ultimately bad for them, in a way that could be foreseen at the time. The same cannot be said of the economic consequences of the Second World War for the United States. The Marshall plan was a great success – the years after the war were economically prosperous for the United States and the Marshall plan also laid the foundation for the political alliance that the United States formed to counter the threat of Soviet expansion. The United States' refusal to forgive British debt after the war was not "unreasonable" in the sense that Versailles was unreasonable. There was no reason at the time to expect it to have an unfavorable impact on American interests, and indeed it had no such effect.

The third case Professor Marcuzzo turns her attention to is the United States government's decision not to bail out Lehman Brothers. This is much nearer actual events and so more difficult to analyze, as it is not yet clear what exactly transpired. There is a clear principle involved, that markets cannot function efficiently unless they are subject to the discipline of the marketplace – unless the rules of the game are kept and those who fail are required to bear the cost of their failure. Rationality requires that rules be kept; reasonableness, in this context, may be likened to an inner voice telling us that every rule has exceptions. And there were good reasons, too, for seeing this as an exception, primarily the fear of grave external repercussions that widespread financial failure would have on the American economy at large and on the world economy. Indeed, there were precedents of such bailouts on the part of the United States government in the past. Was this the time to apply the rule or was this the exception? Would a more flexible approach to Lehman Brothers have averted the crisis or lessened its consequences?

There are two reasons why I would hesitate to label the actions of the United States government in this matter as unreasonable. The first is that we do not know now and certainly could not have known at the time what might have happened if

Lehman Brothers had been bailed out. There were too many things going on at once, too much uncertainty and too little information. The second is that Treasury officials were making earnest efforts to avert the fall of Lehman Brothers by brokering a bailout through the market, as had been done in the case of Bear Sterns. They were not able to do so in the very short time frame they had to work in, but they were not indifferent to Lehman Brothers' plight or oblivious to the serious consequences that its failure might have, though with hindsight it might be argued that they underestimated their gravity. Unfortunately, as Henry Paulson argues in his recently published version of these events, the government did not have the authority to intervene directly, e.g., by infusing government funds to purchase Lehman Brothers' illiquid assets, and Congress was not prepared to issue such authority until there was an actual, palpable crisis for all to see. Such direct involvement was not politically feasible when it might have made a difference. And for good reason. There are moral hazard issues in allowing politicians to bail out investment bankers other than in obviously exceptional circumstances. The voting public is right to view such interventions with suspicion. In sum, the decision not to bail out Lehman Brothers may well have been wrong but given what was known at the time and the political realities of representative government it seems harsh to attribute the failure of Lehman Brothers to an unreasonably inflexible laissez-faire position on the part of the United States government.

There is another, secondary line of argument running through Professor Marcuzzo's description of the events leading up to Lehman Brothers' failure, which merits at least a brief comment: the "tribal" allegiances that pitted Ivy League graduates against those who attended less exclusive schools. I am not in a position to judge the practical import of this dimension, but there is an element of irony that arises from her description. It was not that long ago – a few decades back, when investment banks were privately held companies – that the divisions in Wall Street ran along religious lines, between Christian firms, such as JP Morgan or Merril Lynch, and Jewish firms, such as Goldman Sachs or Lehman Brothers. That Professor Marcuzzo can describe the Christian Scientist Paulson as belonging to the Goldman Sachs "tribe" shows how far the United States has progressed in removing old prejudices.

Professor Marcuzzo has written a fascinating, thought-provoking paper. The distinction she chose as the theme of her analysis opens up many new directions for thought, allowing us to draw illuminating parallels among these three very different events and see each in a new light.

The Marshallian Roots of Keynes's General Theory

Michel De Vroey

Abstract The aim of this paper is to elucidate Keynes's Marshallian lineage. I argue that the result of bringing out the Marshallian antecedents of the *General Theory* highlights Keynes's failure to achieve the theoretical project he was striving at, namely to demonstrate an involuntary unemployment result in the arising of which nominal wage rigidity would play no role.

In the first part of the paper, I reexamine Marshall's theory of value. This section's main conclusion is that no theory of unemployment is to be found in Marshall's writings. In the second part of the paper, I study the literature spanning from Marshall to Keynes, focusing on Beveridge, Hicks and Pigou, in order to see whether the lacuna present in Marshall's writings happened to be filled. Documenting the emergence of the notion of frictional unemployment, I come to the conclusion that its arising went along with little theoretical elaboration. The third and last part of the paper is a critical reflection on the *General Theory*. I start by making the point that Keynes's theory of effective demand ought to be viewed as an extension of Marshall's analysis of firms' short-period production decisions. This enables me to bring out the decisive role played by the wage rigidity assumption in Keynes's reasoning. I claim that, except for this assumption, the differences between "effective demand *à la* Marshall" and "effective demand *à la* Keynes" are minor. I close my analysis of Keynes's reasoning by showing that no real removal of the nominal rigidity assumption is to be found in Chapter 19 of the *General Theory*.

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Introduction

Many authors (e.g. Clower 1984; Leijonhufvud 2006; Hayes 2006; Lawlor 2006) have defended the view that a correct understanding of Keynes's *General Theory* (1936) requires a central place to be given to his Marshallian lineage. While agreeing with these authors, I differ from them as far as the implications of their views are concerned. It is true that reading the *General Theory* in this way is enlightening. However, it does not follow that Keynes's theory is thereby reinforced, as several of these authors seem to take for granted. On the contrary, I shall argue that such a reading points to Keynes's failure to achieve the theoretical project he was striving for, namely to demonstrate an involuntary unemployment result in the origins of which nominal wage rigidity would play no role.

In the first part of this paper, I re-examine Marshall's theory of value.[1] Three specific points are dealt with: Marshall's account of the working of the market day (the corn model); his conceptualization of time; and his analysis of firms' optimizing production decision in the short term. This section's main conclusion is that no theory of unemployment is to be found in Marshall's writings. In the second part of the paper, I study the literature of the period between Marshall and Keynes in order to see whether the lacuna in Marshall's writings was filled in this period. Documenting the emergence of the notion of frictional unemployment, I come to the conclusion that its elaboration was not accompanied by much theoretical elaboration. This means that, when Keynes started to write the *General Theory*, unemployment theory was almost non-existent. The third and last part of the paper is a critical reflection on the *General Theory*. Its aim is to assess the implications of anchoring Keynes's theory more firmly in the Marshallian tradition. I start by developing the point (already made by Clower) that Keynes's theory of effective demand ought to be viewed as an extension of Marshall's analysis of firms' short-period production decisions. This enables me to bring out the decisive role played by the wage rigidity assumption in Keynes's reasoning. I shall claim that, except for this assumption, the differences between "effective demand *à la* Marshall" and "effective demand *à la* Keynes" are minor. I close my analysis of Keynes's reasoning by showing that, contrary to what is usually claimed, no real removal of the nominal rigidity assumption is to be found in Chapter 19 of the *General Theory*.

Before entering into these questions, a preliminary methodological remark is useful. Leijonhufvud has recurrently observed (e.g. in Leijonhufvud 2006) that a distinction should be drawn between a theory and a model. To him, a theory is a set of beliefs about reality, propositions claiming to tell the truth about some real-world occurrences. In turn, a model is a formal representation of these beliefs or a part of them. Usually, it takes a mathematical form but reasoning in prose or with the

[1]There is more than one way in which to be Marshallian. While many present-day authors like to emphasize the institutional and evolutionist aspects of Marshall's work, I shall stick to Marshall the neoclassical value theorist – that is, mainly to the ideas that were developed in Book V of the *Principles*, a fine recasting of which can be found in Frisch (1950).

support of graphs is also possible. Here the aim is to draw logical inferences, to defend the validity of the reality-geared proposition through a demonstrative procedure. My aim in this paper is to assess unemployment as present in Marshall's and Keynes's models even if, following the usual practice, I often use the term "theory" (e. g. Keynes's theory of effective demand) when in all rigor I should say "model".

Marshall[2]

Marshall's Time Framework

Marshall was keenly aware that "man's powers are limited" while "almost every one of nature's riddles is complex". "Breaking up a complex question, studying one bit at a time, and at last combining his partial solutions with a supreme effort of his whole small strength into some sort of an attempt at a solution of the whole riddle" was his strategy (Marshall 1920: 366). This partitioning process, he claimed, should proceed along two lines, dividing the economy into separate industries, on the one hand, and dividing time into three categories — the market (the unit period of exchange), the short period and the long period – on the other. This led Marshall to separate three equilibrium concepts associated with these three time categories. Each of them could be the subject of a separate analysis: market-day equilibrium (in short, market equilibrium), short-run equilibrium and long-run equilibrium. Marshall engaged in these separate analyses but, as is well known, his theory evolved on robust grounds only for the short-run equilibrium aspect. He also argued that the relationship between these categories should be viewed as a gravitational process.

The Lack of Any Rationing (and hence Unemployment) Result in Marshall's Theory of Value

Marshall's main interest when constructing his theory of value lay in what he called the study of normal equilibrium, the centre of gravitation for market outcomes. Nonetheless he must be credited for having addressed the issue of market-day equilibrium, the outcome of the working of markets on a daily basis, in Chapter II of Book V of the *Principles*. Let me retrace Marshall's reasoning in this chapter.

From the outset, the reader is provided with information about the market supply and demand schedules enabling him to calculate what Marshall calls the "true

[2]Here I draw on De Vroey (2007).

equilibrium" – 700 hundred quarters traded at the unit price of 36 shillings.[3] Marshall suggests that this is the result of a bargaining process between agents, the "haggling and bargaining of the price around the 36 shillings mark". Eventually, he claims, the price of 36 shillings will impose itself. What is the underlying mechanism? Scrap the rhetorical effects, and it turns out that the attainment of market equilibrium results from agents' ability to form correct conjectures about equilibrium values or, in other words, from their being as knowledgeable about market conditions as the outside economist. In short, it must be assumed that agents hold perfect information. Under this assumption, all sellers will be ready to trade at a price above the equilibrium price, but they will never find trading partners from the other side of the market; the converse is true for purchasers. As a result, trade will occur at the equilibrium price only.

However, Marshall is aware that this assumption is too heroic. Hence his next move is to show that the same result comes close to being realized when the assumption of perfect information is removed or, more precisely, limited to one side of the market. To this end, it is necessary to assume that the marginal utility of money is constant, which, in turn, requires expenditure made in the market under study to represent only a small proportion of total income. Now market equilibrium is attained gradually through successive false trading without income effects being generated. The end result is almost the same as in the perfect information case. The quantity of corn traded and the price of corn in the last transaction are the same as in true equilibrium, but agents end up with different money balances.

Although a testimony to Marshall's cleverness, this last step of his reasoning cannot win the day because the idea of a constant marginal utility of money (or income) is *ad hoc*, and cannot be generalized. Actually, Marshall fails to refer to it later on, and falls back, be it only implicitly, on the perfect information assumption. An important conclusion follows: the Marshallian market always features a match between market supply and demand, i.e. market clearing.

Three implications ought to be drawn. First, whenever the perfect information assumption is adopted, the idea that duration matters ceases to be relevant. On any market day, equilibrium can be arrived at quickly or slowly, yet this hardly matters. Applying Occam's razor, we ought to consider the formation of market equilibrium to occur in logical time, i.e. instantaneously. In other words, once the perfect information assumption predominates, the idea that equilibrium follows from negotiations between sellers and purchasers turns out to be just a rhetorical varnish.

Second, I must raise the question of whether Marshall's account of the corn market can be extended to the labor market. My answer is "yes". At the end of his chapter on the corn market model, Marshall admits that the constant marginal utility of money assumption is inappropriate when it comes to the labor market. Moreover, his scattered remarks in the *Principles* about labor pertain to the particularities of

[3]This shows that Marshall, unlike Walras, was not interested in demonstrating the logical existence of equilibrium. Rather, he wanted to elucidate how agents' interactions could end up making these equilibrium values effective.

the demand for, and supply of, labor rather than to the functioning of the labor market. He never argues that the labor market functions differently from the corn market. The bottom line must be that, to Marshall, the labor market operated on the same principles as the corn market, in which case it would not be an exception to the market clearing principle. In other words, there is no room for the notion of unemployment in Marshall's value theory. There is one exception, but it is trivial (and not even considered by Marshall). It follows from assuming an exogenous wage floor. If this is above the market-clearing level, unemployment rises.

It is not that Marshall remained silent on the topic of unemployment. It is just that he had limited interest in it. As Matthews noted:

> The social problem that disturbed *his* conscience was poverty; and poverty might have a number of causes, of which unemployment was only one (Matthews 1990: 33).
>
> Cyclical unemployment was *par excellence* a "Vol. II" subject, along with business cycles generally. It does get some treatment in the *Principles*, but to a large extent Marshall's views have to be pieced together from his various writings. Those are often fragmentary or aphoristic (Matthews 1990: 35).[4]

My third remark is that the permanent realization of market clearing in Marshall's analysis does not preclude it having some room for frictions. It is just that these frictions should not be viewed as causes of market non-clearing. Since this point has been an important source of confusion, it is worth delving into it.

In Marshallian theory, the study of market equilibrium cannot be separated from the more fundamental equilibrium concept, which Marshall calls "normal equilibrium", towards which market equilibrium outcomes are assumed to gravitate.[5] States of divergence between the market equilibrium price and the normal equilibrium price can be considered to be states of disequilibrium. This means that Marshall's theory features the possibility of co-existence between market clearing and disequilibrium, a trait that makes little sense with respect to the Walrasian approach.

To look more closely at this point, about which confusion exist, I will use Marshall's well-known fishing industry example (Marshall 1920: 307). In this passage he studied the reaction of suppliers to changes in the demand for fish. I will confine my attention to the effect of an increase in demand of moderate duration. Marshall supposes that the new equilibrium price and quantity will quickly be arrived at, without being more precise. In so far as production takes place in advance, and as the change in demand is unanticipated, disequilibrium will be present at least on the market day when the shock manifests itself. If the change in variable capital can be made before the next period of exchange, the new equilibrium will be reached in this next period. But the adjustment process can

[4]In his 2006 book, which devotes a chapter to "The treatment of labor markets in Marshallian economics", Lawlor praises Marshall for emphasizing "the non-deterministic influence of social institutions at work in labor markets" and for his account the actual institutional details of wage practices (Marshall 2006: 69). But when it comes to unemployment, Lawlor is compelled to admit that "Marshall had something of a blind spot" (Marshall 2006: 61).

[5]At present, normal equilibrium is often (misleadingly) called "long-period equilibrium".

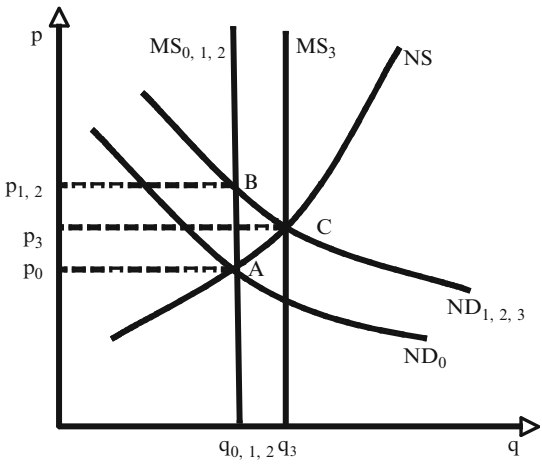

Fig. 1 Disequilibrium with market clearing

also be longer, because putting variable capital to work can be a time-consuming process. In this case, disequilibrium will extend over several periods. The scenario is illustrated in Fig. 1.

Starting from a state of equilibrium at t_0 (A), a change in normal demand (ND) of a moderate length occurs at t_1. As to supply, a distinction has to be drawn between market-day supply (MS), which is vertical due to fish's perishable nature, and short-period normal supply (NS) expressing firms' optimal plan when they can change their variable capital. The initial result of the change in demand is that at t_1 the market equilibrium price rises to p_1. At B, the market is in disequilibrium, since the short-period normal equilibrium is not attained. Note, however, that market clearing prevails. *Normal* supply and demand do not match but *market* supply and demand do. Assume that it takes 2 weeks for the new optimal quantity of variable capital to deliver the new planned production, so that the market remains in the state of disequilibrium at t_2. The short-period equilibrium is reached on the third week at point C. When the departure from permanent values comes to an end, the market returns to A. This slow adjustment to normal equilibrium (accompanied by an instantaneous adjustment to market equilibrium) can be considered to be due to frictions. However, it should not be assumed that such frictions are a *cause* of market rationing.

Firms' Equilibrium in the Short Period

The perfect information assumption also underpins Marshall study of firms' optimal short-period production decisions in a given industry (to be found in Book V, Chapter 5 of the *Principles*). This analysis bears on firms' individual equilibrium, i.e. the determination of their optimal trading plans. As is well known, each firm's optimizing plan is to equalize marginal revenue and marginal cost. To

get this result, firms must decide about both the supply of outputs and the demand for factors. When they establish their supply curves (their marginal cost functions) they need to make conjectures about the cost of their inputs, in accordance with the possible varying levels of demand. To this end, the firms need to estimate the magnitude of a series of variables, which, at the time of their decision-making, are still virtual. The wage rate is one of them. That is, the firms need to make conjectures about the labor market outcomes. The fact that Marshall's jumps at once from individual equilibrium (optimal planning) to interactive equilibrium (the industry equilibrium) means that he implicitly assumes that these conjectures are correct. If not, he should have entered into an analysis of what goes on when they are wrong. Thus, the determination of normal equilibrium in a given branch first occurs as a thought-experiment in the minds of firms' managers, which is only later implemented as an objective observable market experiment. This implies that firms hold perfect information.[6]

The following quotations from Marshall's *Principles* can be adduced in support of my interpretation.

> We assume that the forces of supply and demand have free play; that there is no close combination among dealers on either side, but each acts for himself, and there is much free competition; that is buyers generally compete freely with buyers, and sellers compete freely with sellers. *Though everyone acts for himself, his knowledge of what others are doing is supposed to be generally sufficient to prevent him from taking a lower or paying a higher price than others are doing*. This is assumed provisionally to be true both of finished goods and of their factors of production, of the hire of labor and of the borrowing of capital. We have already inquired to some extent, and I shall have to inquire further, how far these assumptions are in accordance with the actual facts of life. But meanwhile, this is the supposition on which I proceed; I assume that there is only one price in the market at one and the same time (Marshall 1920: 341; my emphasis).

Here Marshall has no qualms about assuming perfect competition and perfect knowledge.[7] The implications of the sentence in italics should be clarified. If agents refuse any price other than the single market-day equilibrium, it means that they know the equilibrium price. The question to be addressed then becomes, how this may be true. Again, omniscience is the only possible answer.[8]

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[6]The underlying reasoning is as follows. Assume that firms incorporate an incorrect nominal wage (i.e. a market non-clearing value). Since market supply and demand always match in Marshallian analysis, in the labor market as in other markets, a discrepancy would arise between the actual wage (the market-clearing wage) and the wage incorporated by the firms. This would result in their engaging in a change of behavior. But then Marshall's reasoning cannot be considered to describe an equilibrium state. For this to be the case, firms' conjectures about the labor market have to be correct.

[7]Admittedly, he wrote almost the opposite in other passages of the *Principles*, (e.g. Marshall 1920: 540–541).

[8]Perfect information à la Marshall should not be ascribed to Marshall exclusively. It is, for example, also present in Jevons' *Theory of Political Economy*: "It is the very essence of trade to have wide and constant information. A market, then, is theoretically perfect only when all traders have perfect knowledge of the conditions of supply and demand, and the consequent ratio of exchange" (Jevons 1970 [1871]: 143). The same idea was taken up by later Marshallian

Unemployment Theory Between Marshall and Keynes

The previous section has shown that there is no value-theoretical study of unemployment in Marshall's *Principles*. In this section, I examine whether post-Marshall but pre-Keynes economists improved on this state of affairs. As an exhaustive examination of the literature is beyond the scope of this study, I shall content myself with surveying a few key works. The authors discussed below are Robbins (1926), Dobb (1928), Beveridge (1912), Hicks (1963) and Pigou (1933).[9]

Robbins and Dobb

In 1926, Lionel Robbins published a 90-page book entitled *Wages: An Introductory Analysis of the Wage System under Modern Capitalism*. As its title makes clear, it made no claim to present cutting-edge research; its aim was purely pedagogical. The topics addressed were the nature and measurement of wages, wages and the cost of labor, the determination of wages, fluctuations in wages, trade unions and wages, and the state and wages. Robbins's discussion is institutional and down-to-earth with no theoretical reference.

Maurice Dobb's essay, *Wages*, came out in 1928 in the *Cambridge Economic Handbook* series edited by Keynes. Subsequently, it underwent several revisions and reprints. According to Lawlor (2006), it can be considered as the expression of the orthodox Cambridge view of the time. Lawlor finds this book praiseworthy for its " wealth of institutional details" (Lawlor 2006: 63). It provides a detailed description of distinct methods of wage fixing and contracts between employers and employees. Dobb also offers a historical account of wage formation from classical to marginalist economics, with Marshallian supply and demand analysis being viewed as the apex of this evolution.

For the purposes of my inquiry, the striking common feature of these two pieces lies in what they omit, the issue of unemployment. We have here two authors, one of them with a socialist inclination, writing on wages at a time where unemployment had becoming a looming social problem, who felt no need to address unemployment in their discussions.

Beveridge

In his book, *Unemployment:A Problem of Industry* (first edition 1908; third edition 1912), Beveridge criticized existing theory for having neglected to address the issue

economists such as Stigler (1965 [1957]: 252) and Knight (1921: 76 ff.). None of these authors had the feeling that this assumption was too heroic to make.

[9]For a similar discussion of other authors, in particular Clay and Cannan, see Casson (1983).

of unemployment (without explicitly mentioning Marshall in this respect, except for one reference). It is noteworthy that he takes it for granted that unemployment is *frictional* unemployment.

> The weakness alike of theory and practice in regard to unemployment in the past has been the assumption that this adjustment was already substantially secured; in other words that the force of friction might be neglected (Beveridge 1912: 216).[10]

Beveridge emphasizes three specific imperfections of adjustment, the analysis of which is the main object of his book: changes in the industrial structure, fluctuations of industrial activities, and the need for a reserve of labor. The reserve of labor is mainly for casual work, such as dock work. It is present in trades which experience a high volatility of activity, where a permanent reserve is needed to meet these fluctuations. "The men forming these reserves are constantly passing into and out of employment" (Beveridge 1912: 13). The common factor underlying these different imperfections is the plurality of labor markets.

> Why should it be the normal condition of the labour market to have more sellers than buyers, two men to every job and at least as often two jobs for every man? The explanation of the paradox is really a very simple one – that there is no one labour market but only an infinite number of separate labour markets (Beveridge 1912: 70).[11]

According to Beveridge, the solution to the problem is as straightforward as its diagnosis: the labor market needs to be better organized, that is to become more centralized.[12] "There shall be known centres or offices or Exchanges, to which employers shall send or go when they want workpeople, to which workpeople shall go when they want employment" (Beveridge 1912: 198).

Beveridge's book is an excellent work, still impressive today. It provides the reader with a wealth of data at a time where statistics were scarce. It studies institutional aspects in a detailed way. However, it is hardly a theoretical piece. It may well have introduced the notion of frictional unemployment but it fails to explain it theoretically.[13]

[10]Unlike Keynes, who opposed the notions of involuntary unemployment and frictional unemployment, Beveridge considered frictional unemployment to be involuntary (Beveridge 1912: 3).

[11]Excess supply is also deemed to be due to increases in population (Beveridge 1912: 70).

[12]In the case of dock workers, "The total number of men practically required to do the work without delay (and by consequence the number of reserve labourers) is, in fact, increased by every barrier to free movement from one wharf to another, and can correspondingly be decreased by everything tending to the organization of the whole ten [wharves] into a single labour market" (Beveridge 1912: 78).

[13]For a different analysis of Beveridge's theory see Dimand (1999a, 1999b).

Hicks

Hicks's book, *The Theory of Wages,* published in 1932 is a theoretical essay.[14] It addresses issues such as the equality between wages and the marginal product, or the coexistence of wage increases and unemployment. Here, I will focus on Hicks's explanation of unemployment. Note first that he (and for that matter all the authors surveyed) took it as a fact that real-world labor markets are usually not in a state of equilibrium (Hicks 1963: 42), an opinion with which most economists concurred, both at the time and in subsequent decades, until Lucas radically questioned it in the 1970s.

Hicks's book is a convoluted piece unceasingly weighing the pros and cons of theoretical propositions. Its main message is that "pure theory", i.e. Marshallian theory, should not be applied too hastily:

> If a labour market could be found which was genuinely in equilibrium, so that every employer could go on employing the same men, and every man could go on working for the same employer, without either party having any incentive to make a change; and if then the employers' opportunities of profitably employing labour were suddenly reduced, or the number of labourers available suddenly increased, unemployment would result. If the new conditions remained unchanged indefinitely, then, under competitive conditions, this unemployment must lead to a fall in wages, going on until the excess of labor was absorbed. But these artificial conditions, although they may serve as a convenient model for analysis, are not a description of what really happens (Hicks 1963: 56).

Hicks's main interest was in what really happens, and how this involves departures from pure theory. For example, theory states that wages must decrease in the presence of unemployment. It can be observed that this does not happen. Why is that so, asks Hicks. He suggests three reasons. First, an irrepressible level of unemployment always exists because of the presence of "unemployable" workers whose efficiency is subnormal and who are long-term unemployed. A second reason lies in the existence of a non-competitive labor market where trade unions play a central role. Third, even when the economy is in a stationary state, frictional unemployment is present:

> For although the industry as a whole is stationary, some firms in it will be closing down or contracting their sphere of operations, others will be arising or expanding to take their place. Some firms, then will be dismissing, others taking on, labour; and when they are not situated close together, so that knowledge of opportunities is imperfect, and transference is attended by all the difficulties of finding housing accommodation, and the uprooting and transplanting of social ties, it is not surprising that an interval of time elapses between dismissal and re-engagement, during which the workman is unemployed (Hicks 1963: 45).

[14] A second edition was published in 1963. In the latter, Hicks admits that 1932, the blackest year of the Great Depression, was not a lucky date for the appearance of his book. Operating at a high level of abstraction, it had nothing to say about the situation of the time, and this was certainly shocking. Moreover, the book was published on the eve of the appearance of Robinson's book on imperfect competition and Keynes's *General Theory*, which were to radically change economists' vision.

To Hicks, frictional unemployment is an equilibrium phenomenon. Firms have no interest in profiting from the existence of unemployment to cut wages. Such attempts would ultimately prove futile. "By reducing wages he [the employer] has reduced his chances of getting good workmen; and sooner or later he will find that he suffers" (Hicks 1963: 46).

Hicks's analysis calls for two observations. First, as the author of *Value and Capital* (1939), Hicks is rightly considered an important contributor to economic theory. However, drawing on his book on wages, published in the same decade, it is striking to note what limited faith Hicks had in economic theory (although he did not seek to repudiate it). His reasoning can be summarized in the following three steps: (a) pure theory has little room for unemployment; (b) unemployment is nonetheless an undeniable fact of life; (c) there are discrepancies between the pure theory model and reality – explaining unemployment involves resorting to factors relating to the interstices between them. To all intents and purposes, this amounts to foregoing providing a theoretical explanation of unemployment.

My second observation is that even the little room that Hicks grants unemployment in pure theory is unwarranted. Look at the following quotation summarizing his standpoint:

> Wages, say the textbooks, tend to that level where demand and supply are equal. If supply exceeds demand, some men will be unemployed, and in their efforts to regain employment they will reduce the wage they ask to that level which makes it just worthwhile for employers to take them on. If demand exceeds supply employers will be unable to obtain all the labour they require, and will therefore offer higher wages in order to attract labour from elsewhere (Hicks 1963: 4).

At first glance, everybody will accept this statement. To me, however, it is flawed as it betrays Marshallian value theory (as analyzed above). Hicks errs because of his failure to separate the formation of *market* and *normal* equilibrium. As far as market equilibrium is concerned, false trading ought to be excluded as soon as the constant marginal utility of income cannot be applied, which is the case for the labor market. The implication is that market disequilibrium has only a virtual existence, being eliminated before becoming effective. Hicks is thus wrong to take for granted the existence of unemployment in Marshallian theory as a result of the slow adjustment of wages. Whenever present in the Marshallian framework, slow adjustment pertains to the formation of normal equilibrium but not of market equilibrium. This is the point that Hicks and the textbooks have missed. He falls prey to the mistake, pointed out earlier in this paper, of believing that slow adjustment can explain unemployment. We may suspect that, if an economist as sharp as Hicks could misunderstand Marshallian theory in this way, so too would the majority of other economists at the time.

To summarize the discussion on Hicks, we can conclude that, except for his slow adjustment argument, he viewed unemployment as a topic that could not be dealt with by pure theory. In a sense, he was right: I have shown that unemployment cannot be integrated into the standard Marshallian model. However, he failed to take the next step of understanding that other theoretical formulations (search theory) could be pursued more profitably.

Pigou

Pigou wrote a short introductory essay, entitled *Unemployment,* in 1914 and, later, a more elaborate book, *Theory of Unemployment* (Pigou 1933). Keynes took it as his foil in the *General Theory*, as if his book was the perfect incarnation of classical orthodoxy. As noted by Hicks in his IS-LM paper, Pigou's book was new and difficult, and "to most people, its doctrine seemed quite as strange and novel as the doctrines of Mr. Keynes himself" (Hicks 1967: 126).

Like the other authors examined here, Pigou takes it as a compelling fact of life that an excess supply of labor is the normal state of affairs. As a result, all his analyses are conducted on the premise that labor demand plays the active role in the determination of employment (and hence of unemployment as well): "The quantity of employment is equal to the quantity of labour demanded" (Pigou 1933: 9).[15] That is, firms determine employment unilaterally. For all his having been Marshall's favorite pupil, Pigou failed to realize that this premise departed from Marshallian orthodoxy by a long way. The orthodoxy would rather have it as: the quantity of employment is determined by the intersection of the supply of and demand for labor. Pigou is not explicit about the reasons behind the permanent excess supply of labor. Most commentators think that Pigou only examined fixed real wages.[16]

Pigou's book is frustrating. The reader expects to learn about unemployment and, in view of the time when it appeared, about massive unemployment, but Pigou fails to deliver. Moreover, there is a gap between the book's title and its contents. The book comprises about 300 pages. Two hundred forty of these are devoted to the study of the short-period elasticity of the real demand for labor, and of the factors affecting this demand, a rather excruciating read. The motivation for this study is to ascertain what changes in wages are required to make the demand for labor (and hence employment) increase. This may be a fine motivation, but it leads Pigou into laborious detours. It is only in Part V of the book, starting on p. 247, that the subject of the causation of employment and unemployment is broached. At this juncture it is doubtful whether readers will find the substance of what Pigou has to say to be worth all the preliminaries. In effect, Pigou's views look trivial and insufficiently elaborated:

> With perfectly free competition among workpeople and labour perfect mobility, the nature of the relationship [between the real wage and demand] will be very simple. There will always be at work a strong tendency for wage-rates to be so related to demand that everybody is employed. Hence, in stable conditions everybody is employed. The implication is that such unemployment as exists at any time is due wholly to the fact that changes in demand conditions are continually taking place and that frictional resistances prevent the appropriate wage adjustment from being made instantaneously (Pigou 1933: 252).

[15]Pigou qualifies this statement by adding that the number of unfilled vacancies should be subtracted from the quantity of labor demanded.

[16]See Klausinger (1998: 54). Further references can be found in this article.

Frictions are thus declared to be the cause of unemployment. The impression is given that once this cause is uncovered, nothing more remains to be done. There is no need to theorize frictions. While frictions are the culprit, wage policy is an aggravating factor:

> There is reason to believe that the goal at which wage policy aims is sometimes, in some centres of production at all events, a wage-rate substantially higher than the rate which, if adopted everywhere, would yield nil unemployment (Pigou 1933: 253).

Like Hicks, Pigou's reasoning evolves at a high level of abstraction. His book has little relevance to the problems that were plaguing the western economies at the time. When he eventually comes to utter a few remarks about the post-war period, Pigou could hardly have been more orthodox:

> Our general conclusion then must be that, as a remedy for the heavy unemployment of the post-war period, a mere correction of wage inequalities would probably have proved, not merely unavailing, but actually harmful. This would have been so even were labour perfectly mobile, and in actual conditions the argument is *a fortiori*. To reduce unemployment from the side of wages it would have been necessary, after wage inequalities had been reduced and labour appropriately redistributed, *also* to reduce the average rate of real wages (Pigou 1933: 270).

It is true that Keynes's criticism of Pigou was often off-target. Nonetheless, Keynes's annoyance with Pigou's book is understandable. Not only did it defend retrograde policy conclusions, but also, like Hicks's writings, it presented a poor image of the economic profession as consisting of people engaged in abstract work and unable to come to grips with the problems that were plaguing the times.

Conclusion

The pitfall of a retrospective reading of past works is that one tends to be too hard on past authors. Things that look obvious to us seem to have escaped their attention. This being so, I am nonetheless struck by the rudimentary character of pre-Keynesian theories of unemployment. Explanatory factors are brought forward and commented upon, but any attempt to demonstrate them is lacking. What is called theory only consists of expressing opinions about reality, and a general discourse around and about the subject. Moreover, when reading books like Pigou's and Hicks's, it is hard to imagine that they were written in the midst of the Great Depression. Read in isolation, Keynes's diatribe in Chapter 2 of the *General Theory* against economists, comparing them to Candides cultivating their gardens while proclaiming that everything is for the best in the best of all possible worlds, may look like a somewhat bewildering piece of rhetoric. But Keynes's outburst becomes more comprehensible when related to the theoretical literature of the time, which was indeed disconnected from what was happening in reality.[17] Beyond doubt, there was something wrong with economic theory. The issue of unemployment had to be considered afresh.

[17]Hence my contention that the most interesting of the early works is Beveridge's book, because it pursues the modest aim of providing a detailed descriptive account of the working of markets.

Keynes's *General Theory*

Reconstructing Keynes's Theoretical Project

Keynes's *General Theory* (1936) is a complex book, intertwining different types of arguments developed at distinct levels of abstraction. Most commentators agree that Keynes's aim in this book was to demonstrate the theoretical existence of involuntary unemployment. The latter, he recognized, had found no room in economic theory in spite of its compelling real-world existence. Bridging this gulf was the task he set himself. The line he took was to state that involuntary unemployment resulted from a deficiency in aggregate demand, which was itself the result of insufficient investment.

The broader research program in which this aim was embedded can be called "Keynes's program". I view it as consisting of the following four objectives, to be fulfilled jointly:

1. Demonstrating the existence of involuntary unemployment
2. Demonstrating that wage rigidity is not its cause
3. Giving a general equilibrium or interdependency explanation of the phenomenon while adopting a perfect competition framework
4. Demonstrating that demand stimulation rather than wage deflation is the proper remedy for the problem

The usual reasoning in Keynes's time was that unemployment was the result of excessive wages or of wages that adjust too slowly. Another feature of traditional theory was that it was put forward against the background of a Marshallian analysis in which one market, in this case the labor market, is considered in isolation from the rest of the economy. Keynes wished to escape from this framework in two ways. On the one hand, he wanted to exonerate excessive wages from any responsibility for the existence of involuntary unemployment. On the other hand, he believed that its cause had to be looked for outside the labor market. What Keynes was actually striving for was to move the analysis of unemployment from a partial to a general equilibrium framework (although this terminology did not exist in his time). However, his willingness to adopt an interdependency perspective should not be interpreted as adhesion to the Walrasian general equilibrium approach. In Keynes's time, Walras's views were little known in Cambridge and, for better or worse, Keynes did not think that Walras's theory would be of any help in his own project. Moreover, Keynes did not want to join the imperfect competition line of argument, which was emerging at the time in Cambridge. He wanted to formulate his argument in terms of perfect competition, possibly because he associated imperfect competition with collusion, unions, etc. His concern was to bring to the fore something deeper, namely the idea that unemployment was possible even when the labor market was functioning in a perfectly competitive way, without either frictions or market power.

Different Kinds of Unemployment?

In Chapter 2 of the *General Theory*, Keynes claims that different types of unemployment exist, including frictional unemployment and involuntary unemployment. He also gives the impression that economic theory is on solid ground as far as the explanation of frictional unemployment is concerned.[18] What is needed, according to Keynes, is the consideration of an additional type of unemployment, involuntary unemployment. Fulfilling this task is the main purpose of his book.

Does the alleged real-world co-existence of frictional unemployment and involuntary unemployment extend to theory? That is, did Keynes propose a theory in which these two types of unemployment co-exist? The answer is "No". When it came to developing his theory, Keynes only considered one kind of unemployment, involuntary unemployment, i.e. unemployment caused by a deficiency in effective demand. Either involuntary unemployment is present or there is no unemployment at all, and the Marshallian state of a match between supply of and demand for labor exists. Hence the involuntary adjective is unnecessary.

Effective Demand à la Keynes Versus Effective Demand à la Marshall

Keynes introduces the notion of effective demand in Chapter 3 of the *General Theory*. He defines it as the intersection between aggregate demand and supply, both of which are a function of the quantity of goods produced, and hence of employment. He claims that involuntary unemployment is present whenever the employment level associated with effective demand is lower than full employment. Hence his central claim is that the cause of involuntary unemployment lies in a deficiency of effective demand, which is underpinned by what he calls the "fundamental psychological law" of a decreasing marginal propensity to consume out of income.

I fully agree with Clower (1997: 42) when he claims that Keynes's theory of effective demand merely restates in aggregate form a Marshallian partial equilibrium supply and demand model. More precisely, Keynes's aggregate supply/demand analysis is an extrapolation of Marshall's analysis of firms' optimal short-period production decision, which was discussed above. That is, the formation of effective demand is accounted for in the same way as the formation of the representative firm's equilibrium values. It occurs first in entrepreneurs' minds as the result of a thought-experiment on market outcomes. The entrepreneurs make conjectures about the aggregate supply price and the aggregate

[18]My survey has cast doubt on this view. Nothing worth being called a theory of frictional unemployment existed at the time.

demand price functions, and derive effective demand as their intersection. As in Marshall's model, there is no problem of realization. In other words, it is implicitly assumed that these conjectured values actually arise once the economy starts to unfold. None of this would happen if the entrepreneurs did not have perfect information. The heroic character of the assumption is even stronger in Keynes's model than in Marshall's, since the exercise now bears on the whole economy.

Of course, there are some differences between Keynes's and Marshall's models, but these are minor. Keynes considered proceeds (i.e. price times quantity) as the dependent variable rather than price alone. He also took employment, not the quantity produced, as the independent variable. Finally, he reasoned as if his analysis concerned the whole of the economy, or at least its manufacturing sector, but this makes no difference because the economy is viewed as a single sector. This change is purely semantic.

The conclusion to be drawn is that Keynes's theory of effective demand is less original than it appears to readers who are unfamiliar with Marshallian theory. This raises another question: what explains that, unlike "effective demand *à la* Marshall", "effective demand *à la* Keynes features a market non-clearing result?"

My answer runs as follows. The sole element of Marshallian reasoning which Keynes abandoned, is the view that the aggregate supply price function incorporates the cost of inputs at their market-clearing values, at least as far as labor is concerned. Instead, he assumes that the wage rate upon which firms elaborate their supply price function is a "false" (i.e. non-market-clearing) wage. To retain the assumption that firms are correct in their conjectures (as stated above, if this assumption is dropped, a theory of learning is needed). It must be concluded that their incorporation of a false wage into their conjecture follows from their correct anticipation that this is indeed what will happen in the labor market. As to the factor explaining the prevalence of the false wage, there exists only one candidate: exogenous wage rigidity. Therefore, all the claims to the contrary notwithstanding, it is difficult to escape the view that Keynes's effective-demand reasoning is based on a fixed-wage hypothesis. The reason for unemployment lies in the labor market, and no fuss should be made about effective demand being the cause of unemployment. The existence of unemployment logically precedes the determination of effective demand rather than the other way round.

That Keynes adopted the nominal-wage rigidity assumption in Chapter 3 is beyond dispute. But he stated that its introduction served only a simplifying purpose, and that it would be removed later in the book without harming his theory. To settle the issue, one needs to assess Chapter 19 of the *General Theory*, where this removal is allegedly made. This will be done later. At this juncture, I will simply note that the introduction of this assumption is intriguing. What an oddity to have brought it into the picture! Keynes was well aware that the wage-rigidity assumption was the main, if not the only, rival to his own preferred explanation. It is unusual to introduce a rival explanation as a simplifying device in one's own argument. In order to remove any ambiguity, would it not have been safer to avoid this explanation altogether, albeit at the price of a more complicated demonstration?

The Time Framework of Keynes's Analysis

In the *General Theory*, Keynes treads in Marshall's footsteps by deciding to analyze the short- and the long-period separately. I have already discussed the rationale for this strategy: the theory's *explanandum* is too complex to be studied as a whole. But it has several drawbacks. Two of them are visible in the *General Theory*. The first is a lack of consistency. Keynes uses the perfect-information assumption to study the short period, but when it comes to the long period he goes for the radical-uncertainty assumption. The issue of how the short and long periods interact is left unaddressed.

A second ambiguity pertains to the time framework adopted by Keynes when developing his aggregate-demand deficiency. He argues that this is a short-period analysis. From Hicks's inaugural paper onwards, IS-LM models have followed suit — "thus I assume that I am dealing with a short period in which the quantity of physical equipment of all kinds available can be taken as fixed." (Hicks 1967: 127). But is this so obvious? The point is to see what the "short period" means. Does it designate a single period of exchange (Hicks's Monday or, in reference to Marshall's model, the single day on which the corn market was studied) or a short sequence of periods of exchange? The latter is the Marshallian definition of the short period. Returning to the *General Theory*, if the issue to be solved is the emergence of involuntary unemployment, the period of exchange (or market day) ought to be the relevant time framework for studying it. In effect, involuntary unemployment must necessarily occur on a given market day. I must insist on this point. Were we just wanting to make a descriptive assessment, we might look at quarterly statistics and observe, for example, that unemployment increased by a certain percentage over a given quarter. But such a statement will not do when it comes to a theoretical discourse (here understood as modelized reasoning).

Any Salvation from Chapter 19?

If the analysis above is valid, one cannot but be skeptical about Keynes's ability to deliver on his Chapter 3 promise to lift the nominal wage rigidity assumption later in the book without impairing the aggregate-demand deficiency claim. This task, he claims, is performed in Chapter 19. To Patinkin, an eminent Keynes scholar, this chapter constitutes the apex of the *General Theory*, proving "that, the many contentions to the contrary notwithstanding, the analysis of this book does not depend on the assumption of absolutely rigid money wages" (Patinkin 1987: 28).[19] Keynes's claim can be summarized as follows: it can be concluded that too high

[19]Many commentators have trod in Patinkin's footsteps. To give just one example: "It is true that Keynes assumed a fixed money wage for the first 18 chapters of the book, but this, as he explained, was just 'to facilitate the exposition'. In Chapter 19, entitled 'Changes in Money Wages' he relaxed the assumption and argued that it made no difference to the conclusions of the previous 18 chapters" (Howitt 1990: 72). See also Trevithick (1992).

wages are not the cause of involuntary unemployment as soon as it has been demonstrated that a decrease in the wage ends up increasing unemployment. In order for employment to increase, it is necessary that the decrease in wages causes either an increase in the marginal efficiency of capital or a decline in the interest rate, but neither of these effects is bound to occur.

As a prerequisite of gauging Keynes's claim, I need to clarify whether it relates to the real world or to a fictitious theoretical universe. As to the issue of whether decreases in nominal wages succeeded in decreasing mass unemployment during the Great Depression, most economists agree that they did not. For my part, I want to insist on the point that what is at stake is the ability to generate this result in a theoretical model. In this context, it is crucial that the time assumptions are precise. This is where Keynes errs.

Two separate adjustment processes need thus to be disentangled, the adjustment within the market day and the adjustment across market days. Whenever the object of analysis is effective demand, the impediment to adjustment caused by wage rigidity occurs within the market day. This is the type of wage rigidity that needs to be removed and replaced with the flexibility assumption. But then this is the very thing that Keynes does not do in Chapter 19. The issue that he actually tackles relates to the adjustment process across market days – will employment increase if wages decrease from one market day to the next? This question does not address the wage formation process on a given market day. Thus, it must be presumed that the assumption made earlier in this respect still prevails: the labor market is under the spell of an exogenous wage floor. That is, an exogenously rigid wage is assumed at each trading round, but it may be different across these rounds. Would employment increase if the exogenous wage floor were lower on market day t_2 than on market day t_1? This is the question addressed in Chapter 19. Now, Keynes may well have clinched a point by stating that this can fail to happen but this is hardly tantamount to removing the point-in-time rigidity assumption, the task that needed to be addressed to exonerate nominal rigidity from causing involuntary unemployment during a given period of exchange. So, contrary to what Keynes, Patinkin and others have claimed, the rigid wage assumption, as introduced in Chapter 3 and pertaining to a given period of exchange, is not removed in Chapter 19.

Concluding Remarks

The aim of this paper was to assess the implication of trying to anchor Keynes's theory more firmly in Marshallian theory. Many other authors have emphasized the need for such an anchorage, but they have assumed that it would reinforce the validity of Keynes's argument. I have shown that the contrary is true. This anchorage leads to the conclusion that Keynes was unable to achieve his program, the main stumbling block being to demonstrate simultaneously the existence of involuntary unemployment and the exoneration of wage rigidity as its cause. However, we should not blame Keynes

for his failure. The task was beyond the reach of any economist in the 1930s, in view of the state of the discipline at the time. To have started the quest in the way he did was already a considerable feat. The claim made in this paper is not that Keynes was wrong. It is rather that he was unable to rigorously modelize his intuitions.

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Marshall, Models, and Macroeconomics: Comments on Michel De Vroey's The Marshallian Roots of Keynes's General Theory

David Colander

Michel and I both agree that the correct interpretation of Keynes's work involves seeing Keynes as a Marshallian. Where we differ is on what it means to be a Marshallian. Michel interprets *Marshallian* as "one who uses Marshallian models"; I interpret *Marshallian* as "one who uses the Marshallian method". A Marshallian in my sense would use models in quite different ways than a Marshallian in Michel's sense.

Because Michel interprets Marshall as someone who uses Marshallian models, much of his discussion is about Marshallian models, such as Marshall's market clearing labor market. He judges Keynes's work within this "model" interpretation of Marshallian, and finds that Keynes essentially added nothing to the Marshallian models in terms of an explanation of unemployment. He argues that Keynes did not extend Marshall's theory, because Keynes did not develop a macroeconomic theory of unemployment based on Marshallian models. He ultimately concludes that seeing Keynes as a Marshallian does not reinforce the validity of Keynes's argument, but rather shows its weaknesses and undermines Keynes' contribution.

I fully agree that Keynes did not develop a macroeconomic theory of unemployment based on Marshallian models. However, I do not see that failure as undermining Keynes' contribution. In fact, I suggest that since Keynes used the Marshallian method, his goal in presenting Marshallian models was *not* to develop a theory of unemployment or a macroeconomic theory using Marshallian models. Rather I see his goal as simply to explore how a vision of the economy grounded in his work on true uncertainty would fit in those models. If it had been possible to develop his ideas in Marshallian models in any straightforward manner, Keynes, having a high regard for Marshall's abilities, would have fully expected that Marshall would have already done it.

Instead of seeing Keynes's *General Theory* as an attempt to develop a theory of unemployment within Marshallian models, I see Keynes' *General Theory* as an attempt to view the macroeconomy with a different vision than Marshall implicitly used as background for his models. Elsewhere (Colander 1996, 2006), I have called this vision a complexity vision, and have argued that the novel elements of the *General Theory* involved an exploration of how that complexity vision might help in explaining persistently and high unemployment and the depression that the economy was experiencing at the time. This complexity vision involved giving up Marshall's "one thing at a time-hold everything else constant" partial equilibrium micro modeling method, and replacing it with a "everything is interrelated–all things at the same time" – macro modeling method. Doing so, Keynes saw that the

assumption of a unique long-run stationary state that served as an implicit backdrop for Classical and neoclassical (both informally in Marshallian and formally in Walrasian) models, was problematic, and that in the short run, many different aggregate outcomes to the economy due to aggregate coordination failures were possible. In such a vision, Marshall's micro models had no macro foundation. Given the highly complex interdependencies of agents' decisions, nothing inherent in a model that included those interdependencies guaranteed the achievement of a desirable aggregate equilibrium in the short run.[20] This meant that you couldn't assume an aggregate full employment level of aggregate activity as a backdrop for models as Marshall had implicitly done, and as Walras had explicitly done. Instead, you had to develop an aggregate model that would determine the aggregate level of activity that would be achieved by the economy and embed micro models within the appropriate macro model. Classical economists had not done this, and thus did not have an acceptable theory.

Within this complexity vision an aggregate economy of interdependent agents would likely have serious coordination failures. Thus, unemployment of aggregate resource was a *macro* phenomenon that could have little to do with micro issues. Issues of unemployment could not be reasonable analyzed in ceteras paribus models. Whether in some abstract long run these coordination failures would be solved and a desirable aggregate equilibrium would be reached was irrelevant to Keynes, because society would never wait that long, and allow that particular long run to occur by the system.

Having arrived at that alternative complexity vision after a long struggle to escape the alternative unique equilibrium (stationary state) vision, Keynes faced the problem of how to convey his new vision to other economists. To do so, he naturally tried to relate it to models that they understood; much of the *General Theory* involves Keynes' attempts to do so. But these attempts should be seen as attempts to explain his vision, not as an acceptance of the alternative vision implicit in the existing micro models, Keynes's arguments did not rise or fall on his ability to translate his vision into existing micro models; that is why when the models and the vision came to different results, Keynes accepted the vision in his discussion. He could do so because he used the Marshallian method, which sees models as aids to intuition, not as the holders of truth. Consider Keynes's description of the use of models.

> Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world. It is compelled to be this, because, unlike the typical natural science, the material to which it is applied is, in too many respects, not homogeneous through time. The object of a model is to segregate the semi-permanent or relatively constant factors from those which are transitory or fluctuating so as to develop a logical way of thinking about the latter, and of understanding the time sequences to which they give rise in particular cases. Good economists are scarce because the gift for using "vigilant observation" to choose good models, although it does not require a highly specialized intellectual technique, appears to be a very rare one (Keynes 1938).

[20]My interpretation of short run is a period of time that was shorter than society would need to give up the economic system, in which case the model would no longer be relevant.

This description places intuitive judgment above models. As Michel, and many others, have pointed out, Keynes' translation of his vision into Marshallian models is confusing and involved many mistakes. Unfortunately, later economists built on those models, and lost sight of the Marshallian method as they embedded Keynes's ideas into models. Put simply, they choose the wrong models. As that happened, Keynesian economics lost its grounding in the Marshallian method, and was replaced with a neoKeynesian/Neoclassical synthesis that focused on models in place of judgment. As that happened Keynes' complexity vision was lost and macro theory went astray.

Why didn't Keynes and Keynesians object to this transformation? Some, such as GLS Shackle (1949) and Paul Davidson (1978) did, but most Keynesians were more interested in policy, not theory, and were willing to accept models that came to what they considered the "right" policy conclusion even though they were intellectually unsatisfying.

Conclusion

In conclusion, seeing Keynes as a follower of the Marshallian method is important both in understanding his contribution, and in understanding how later economists lost sight of it. Keynes' contribution to Marshallian economics was twofold. First he developed a vision of the aggregate economy that saw it as possible that it could end up for long periods of time at highly undesirable equilibria. That was an enormous contribution to theory that was unfortunately lost. Second, he saw that it was likely that to get out of those undesirable equilibria in a politically socially accepted period of time, government action might be necessary. That was an enormous contribution to policy. True, he did not develop an acceptable scientific theory of how such a complex system would operate, nor did he have acceptable models to convey his vision to others. But for those who were willing to see the vision, it was there.

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Was Patinkin a Keynesian Economist?

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Abstract Don Patinkin regarded himself a Keynesian economist, in the sense that he did not believe that the automatic market mechanism of price change efficiently leads the economy to its full-employment path. In his 1956 *Money, Interest and Prices* Patinkin advanced an interpretation of Keynesian macroeconomics as disequilibrium economics. However, he was perceived as an anti-Keynesian economist by a substantial part of the profession. The present essay discusses why was that so, based on an investigation of the "central message" of Patinkin's theoretical framework and on unpublished archive material.

Frankly, I have never been able to understand why my *Money, Interest, and Prices* was regarded in some circles as an anti-Keynesian work. I have always regarded it as a work that strengthened the Keynesian view by diverting attention from the pointless semantic issue of whether there could or could not exist a situation of "unemployment equilibrium", to the real question of the efficacy of price flexibility in restoring full employment. (Letter from Don Patinkin to James Tobin, 1 Jan 1992; Duke University, Don Patinkin Papers).

Introduction

Don Patinkin (1922–1995; for general information about his life and work see Liviatan 2008 and Boianovsky 2008) clearly regarded himself a Keynesian economist, but, as the opening quotation shows, he was painfully aware that his major work was seen by part of the profession as an elaborated (re)formulation of classical (or neoclassical) macroeconomics. Indeed, John Hicks (1957, p. 278), in his

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controversial review of Patinkin's 1956 book, claimed that it was just a modernized version of what Keynes used to call "classical" theory. In the same vein, after Patinkin became in the 1970s a leading historian of the Keynesian revolution, Joan Robinson (1979) wrote that "Professor Patinkin, who at first totally rejected Keynes's *General Theory*, now regards himself as an authority on it".

Even economists who took seriously Patinkin's disequilibrium approach to unemployment, such as Paul Davidson (1967, p. 562), rejected its supposed implication that unemployment is just a transitory phenomenon, instead of a ubiquitous, persistent and "normal" one. Axel Leijonhufvud (1968, pp. 325–329) – in a book celebrated as a seminal contribution to disequilibrium macroeconomics – discussed at length Patinkin's "neoclassical critique" of Keynes, based on the real balance effect. Leijonhufvud (p. 161 n.) referred only in passing to Patinkin's ([1948] 1951) priority on the claim that a purely static interpretation of Keynes's unemployment theory is impossible. When Leijonhufvud (1974, p. 167) eventually came to refer to Chapter XIII of *Money, Interest, and Prices* – after Barro and Grossman (1971) had acknowledged that chapter as one of the foundations of disequilibrium macroeconomics – he described it as a "strange interlude" in the book. Another illustration of the way *Money, Interest, and Prices* has been received by some economists is provided by J.-M. Grandmont (1983, p. 1; quoted by Rubin 2005, p. 47) reference to Patinkin as one of the protagonists of the "neoclassical counter-attack" on Keynesian economics.

The sample provided above indicates that Patinkin was criticized by Keynesian economists of various persuasions. The title-question of this paper begs another question: what are the criteria for being a Keynesian economist? There is no brief answer to that; for the purposes of the present paper, "Keynesian economists" are those who believe that the automatic market mechanism of price change does not efficiently lead the economy to its full-employment path. That was the definition suggested originally by Patinkin (1948), which raised Dennis Robertson critical reaction in correspondence in the early 1950s (see Boianovsky 2006, pp. 222–224).

David Laidler (1973, p. 263) was one of the first to notice – in his review of Patinkin (1972b) – that Patinkin was often misunderstood as an exponent of the "classical" tradition of convergence to full employment which supposedly had been kept alive by Milton Friedman and others at the University of Chicago. Patinkin's careful reading of pre-Keynesian economics, together with his painstaking treatment of the wealth effect, may have played a role in that assessment. As pointed out by Laidler (1973), the widespread description of Patinkin's contributions to monetary macroeconomics as anti-Keynesian was a "gross caricature", which overlooked the fact that he was consistently trying to "carry forward the work of Keynes and his predecessors, rather than to undermine it".

In order to understand the divergent signs in the reception of *Money, Interest, and Prices*, one should apply to that book the concept of "central message", originally devised by Patinkin to study multiple discoveries in economics. According to Patinkin (1982, pp. 16–17, 82–86; 1983), one should distinguish between the systematic component of an author's thinking and the random component, that is, between the "central message" the writer intended to convey and the "noise".

Patinkin (1982, p. 17) suggested that in order to determine the central message of an author's work, the historian of thought should act as if (s)he was passing a hypothetical regression line through the empirical evidence represented by the text. Commentators (see e.g. Johnson 1962, p. 338; Fischer 1993, p. 15; Mehrling 2002, p. 161) have quoted the subtitle of *Money, Interest, and Prices* – "An Integration of Monetary and Value Theory" – as evidence of its main theme. Moreover, many (e.g. Weintraub 1979, p. 63) have referred to the last paragraph of the introduction, where Patinkin summarized the general theoretical results of the book.

> The propositions of the quantity theory of money hold under conditions much less restrictive than those usually considered necessary by its advocates and, *a fortiori*, its critics. Conversely, the propositions of Keynesian monetary theory are much less general than the *General Theory* and later expositions would lead us to believe. But this in no way diminishes the relevance of Keynesian unemployment theory for the formulation of a practicable full-employment policy (Patinkin [1956] 1965, p. xxv).

Patinkin's integration of money into the "real" general equilibrium Walrasian system through the explicit introduction of the real balance effect in the excess demand functions for goods, bonds and money led to a rigorous restatement of the long-run neutrality of monetary changes and of convergence to full-employment market equilibrium. However, convergence may be delayed by interest-inelasticity and ensuing destabilizing distribution and expectations effects absent from the Walrasian system, which brings in Keynesian theory as the economics of "unemployment *dis*equilibrium" (Patinkin 1965, Chapter XIV.1). Some commentators have concluded from those passages that "accepting the Patinkin argument forces the view that Keynes could *not* have been saying anything of particular importance in theoretical terms", but was confined to the practical result that policy intervention (mainly via fiscal policy) was necessary to shorten the length of the disequilibrium period (Weintraub 1979, p. 67). Similarly, Leijonhufvud's (1974) reference to Chapter XIII as an "interlude" indicates that Patinkin's path breaking discussion of the connection between aggregate demand constraint and the demand function for labor was regarded as just a "noise" by part of the literature. Hence, to Patinkin's (1989, p. xvii) disappointment, for a long time his 1956 model of macroeconomic disequilibrium and involuntary unemployment – which he would regard as the "most novel and important contribution of the book" (see Boianovsky 2006, p. 195) – had little repercussion in the macroeconomic literature. In the following I discuss why that was so, and how Patinkin's overall contributions to Keynesian economics and its history are related to his dynamic interpretation of involuntary unemployment as disequilibrium phenomenon.

The Quantity theory and the Long-Run Neutrality of Money

Patinkin's intellectual formation at the University of Chicago in the 1940s may be seen as the result of the tension between the influence of the economics department

of that university, where he studied under Jacob Viner, Frank Knight, Henry Simons and Lloyd Mints, and the Cowles Commission for Research in Economics, where as a graduate student and research fellow he interacted with Jacob Marschak, Lawrence Klein, Trygve Haavelmo and others (see Mehrling 2002; Boianovsky 2002; Patinkin 1995a). Marschak, director of research of the Commission between 1943 and 1948, was also chairman of Patinkin's Ph.D. thesis committee at the University of Chicago. Patinkin's doctoral dissertation "On the consistency of economic models: a theory of involuntary unemployment" was submitted in the spring of 1947, after a brief period of intense research at the Cowles Commission. Its main theme was overdetermination and inconsistency of systems with homogeneous equations, called the "Patinkin problem" by members of the Commission. Whereas the economics department was predominantly "classical" (Oskar Lange, who taught mathematical economics and business cycle theory to Patinkin, was an exception), researchers at the Cowles Commission were engaged in the pioneer econometric estimation of large scale Keynesian macroeconomic models. Another relevant interlocutor was Milton Friedman, who returned to the Chicago department of economics in 1946 (where he had taken his M.A. degree in 1933).

Patinkin's 1947 dissertation was formed by two parts, on the "classical system" and "involuntary unemployment" respectively. The main results of part I were published in revised form in two *Econometrica* articles in 1948 and 1949, whereas part II came out, with even more extensive changes, in the *Economic Journal* in 1949, after Patinkin had tried unsuccessfully to publish the dissertation as a book with the University of Chicago Press. In 1948 Patinkin came back to the idea of publishing a book on *The Theory of Employment*, under the immediate impact of the success of his *American Economic Review* article on "Price Flexibility and Full Employment". The book project, designed as a collection of essays, was soon abandoned after Patinkin moved to Israel in February 1949. In 1952, under an invitation to contribute to a volume on Keynesian economics after Keynes, Patinkin started working on a paper on "Keynesian Economics and the Quantity Theory" (Patinkin 1954), which eventually led him to resume his plans for a book – but this time it was meant to be mainly a book on monetary, not employment, theory (see Boianovsky 2006, p. 199). Patinkin (1954) argued that, under the assumptions of full-employment and absence of money illusion, the main proposition of the quantity theory – the long-run neutrality of money – is valid irrespective of the forms of the aggregate demand and money demand functions. The phrase "long-run neutrality of money" encapsulates the notion that a change in the quantity of money causes a proportionate change in the absolute price level, with no permanent effect on relative prices, the rate of interest, real income and real balances. He would later recall how in the 1954 essay he had

> Enthusiastically demonstrated the erroneous nature of Keynes's contention that absence of speculative demand for money was a necessary condition for the validity of the quantity theory... In retrospect, perhaps I was too enthusiastic: for when [*Money, Interest, and Prices*] finally appeared, its emphasis on the quantity theory had become so great, that its title (in sharp contrast to that of my dissertation) did not even contain the word

> "employment". I remember that at the time I was not happy about this, for I regarded the discussion of the theory of unemployment in Chapters XIII–XV of the book as constituting its "second major theme" [see Patinkin 1965, p. xxv]. I also remember toying with the possibility of adding "employment" to the title, but realized that this would make it sound most presumptuously similar to the title of another book, published 20-odd years before it (Patinkin 1995a, p. 384).

While discussing the notion of "central message", Patinkin (1982, p. 83) suggested that one of the ways to identify an author's central message is by looking at the title of the work, as illustrated by Keynes's *General Theory of Employment, Interest and Money*, which was about everything but prices. Patinkin (1982) then asked: "Is one accordingly justified in inferring that my *Money, Interest, and Prices* deals with everything but employment?" His answer was that "for obvious reasons" (see above) the word "employment" had not been included in the title, but that the announcement in the introduction of employment as "major theme" of the book should have made clear that it was part of the central message.

However, the view that *Money, Interest, and Prices* was primarily about the long-run neutrality of money became gradually dominant in several quarters. For many economists, Patinkin's emphasis on the long-run properties of a Walrasian general equilibrium monetary system set him apart not only from Keynesian theory, but also from a substantial tradition within the quantity theory itself. As suggested by Robert Nobay and Johnson (1977, pp. 471–472, 476), the main concern of the "classical phase" of the development of monetary theory was the demonstration of the long-run neutrality of money. The issue was central to Keynes's attack on "classical theory", which explains why it resurfaced in the work of Patinkin and a few others after Keynes. According to Nobay and Johnson, monetary economics had moved, even before the publication of the *General Theory*, to the study of the conditions required for monetary equilibrium, that is, the (Wicksellian) conditions for short-run neutrality of money. In contrast with the "classical phase", there was a growing concern with the analysis of the dynamics of the working of the quantity theory in monetary disequilibrium, a tradition that continued in the work of Friedman and others after Keynes, and eventually led to the monetarist interpretation of prices and output changes over the business cycle.

Money, Interest, and Prices was not about the business cycle (see Fitoussi and Velupillai 1993) – neither was the *General Theory* for that matter. Incorrect expectations of price changes, monetary contracts and wage lags etc do not play a meaningful role in the theoretical framework of the book, as Patinkin (1995b, p. 123) would regret years later, when he pleaded "guilty to having earlier placed too much emphasis on the long-run neutrality". In fact, it was in his 1972a piece about the "short-run non-neutrality of money in the quantity theory" that Patinkin discussed for the first time the short-run effects of monetary changes on both output and the velocity of circulation of money, with extensive references to Irving Fisher, Henry Simons, A.C. Pigou and D.H. Robertson. (Patinkin had dealt in *Money, Interest, and Prices* with long-run non-neutrality associated to distribution effects called "forced savings", caused by changes in the distribution of real income, bond

holdings and money holdings among individuals.) He came back to short-run non-neutrality in his 1987 entry on the "neutrality of money" written for the *New Palgrave*, revised in Patinkin (1992).[1] Patinkin (1992, p. 18) pointed out that short-run non-neutrality is a feature of both Keynesian and monetarist approaches to monetary theory, albeit for different reasons. Patinkin's approach to involuntary unemployment in *Money, Interest, and Prices*, on the other hand, was not based on money illusion (which would break the long-run neutrality of money proposition) or on short-run non-neutrality of monetary changes. A case in point is the introduction of money illusion into the labor supply function. Contrary to some interpretations, its effect is not to generate excess labor supply and unemployment, but to bring about a shift downward of the full-employment yardstick if the price level falls because of a reduction in aggregate demand (Patinkin 1965, Section XII.2).

Money and the Rate of Interest

One of the corollaries of the long-run neutrality of money proposition is that the rate of interest, like other relative prices, is determined by "real", as opposed to "monetary" factors. This, of course, implies rejection of Keynes's argument that the rate of interest is decided by money supply and liquidity preference. Contrary to many interpreters of the Keynesian system (see e.g. Hicks's IS-LM model), Patinkin clearly separated Keynes's theory of interest from his theory of employment and effective demand. In particular, he argued that rejection of Keynes's theory of interest left the theory of effective demand – which Patinkin admired – intact. Patinkin (1965, p. 375) claimed that Keynes (1937, p. 250) himself had stated that his theory of employment – as expressed by the notion that equality between investment and saving is brought about by income changes through the multiplier mechanism, not via changes in the interest rate – was developed before and independently of his theory of interest.

In late 1957 Patinkin was invited to give a couple of lectures at the London School of Economics, which were scheduled for March next year. Patinkin's lectures were based on a long essay titled "A Restatement of Keynesian Economics", which he finished in December 1957 and submitted next year for publication in the *Economic Journal*. The essay included many references to Hicks (1937, 1957), and it was certainly produced as an indirect reply to Hicks's negative review of

[1]Long-run neutrality of money was a topic that attracted Patinkin's attention, which led him to investigate, together with Otto Steiger, the origins of the terms "neutrality of money" and "veil of money". Patinkin and Steiger (1989) found the earliest use of "veil of money" in a book by D.H. Robertson in the 1920s. If I may strike a personal note, that is how I started to correspond with Patinkin in the early 1990s (as a graduate student in Cambridge), when I let him know that I had come across "veil of money" in books by Böhm-Bawerk and Irving Fisher written at the end of the 19th century. With characteristic interest, Patinkin encouraged me to write a note about it, which became my first published international article (Boianovsky 1993).

Money, Interest, and Prices. Roy Harrod, editor of the *Economic Journal*, rejected the paper largely because of its length and asked for a shorter version (letter from Harrod to Patinkin, 3 Dec 1958). After some hesitation, Patinkin (letter of 9 Mar 1959, Don Patinkin Papers) eventually decided to submit a "short and direct" rejoinder to Hicks, which was published in that same year (Patinkin 1959). The 1957 typescript is remarkable for its careful discussion, in terms of the IS-LM model, of the implications for Keynesian theory of the introduction of the real balance effect, and of the rejection of the double assumption that money demand becomes infinite at a minimum rate of interest (the liquidity trap) and that the speculative demand for money is independent of changes in the price level. Patinkin (1957, p. 48) concluded that "from what has been said here, it will be evident that I do not feel the fundamental contribution of Keynesian economics to lie in its monetary theory; for this we must turn instead to its employment theory".

Keynes's proposition that an increase in money supply will reduce permanently the rate of interest only applies if money–wages and prices are constant, or if the speculative demand for money function features money illusion (that is, it is not influenced by the price level). At first Patinkin (1950–1951, p. 56) leaned towards the first interpretation, suggesting that, whereas the rigid money–wage assumption was "*not* necessary" for Keynes's theory of unemployment, it was part of his interest rate theory. In *Money, Interest, and Prices* (pp. 278–280), probably under the influence of Robertson (1940, pp. 151–152), Patinkin came to emphasize that, since liquidity preference should express demand to hold a real (not nominal) amount of money, its permanent role was to affect the price level, not the rate of interest.

Patinkin (1957, Section 7) tentatively advanced what he called a "sociological" explanation of why monetary theories of interest are generally more pervasive in developed than in underdeveloped countries. One reason is the fact that an underdeveloped economy is also underdeveloped in its securities market on which the bulls and the bears can play. A more fundamental reason is that capital shortage is conspicuous in underdeveloped economies, like the Israeli one.

> The economist in an underdeveloped country (and I am speaking as one) is brought into daily contact with the fundamental problem of capital shortage that besets all plans for progress. For him interest is first and foremost the price that must be paid for this scarce resource. And, from the opposite viewpoint, it is the price that can only be paid by virtue of the productivity of capital. Correspondingly, it is difficult for him even to entertain the notion that interest is a purely nominal phenomenon whose level can be arbitrarily determined by the proper monetary machinations (Patinkin 1957, p. 43).

Hence, the apprehension of the fact that interest is ultimately due to the productivity of capital is more immediate in underdeveloped economies than in developed ones. Moreover, as far as developed countries are concerned, monetary theories of interest tend to be more acceptable in times of depression, when the productivity of capital becomes "a dim memory" without relevant effect on the public's expectation about the future level of the rate of interest (Patinkin 1965, p. 380). (This is dealt with in more detail in Boianovsky Unpublished.)

Patinkin's overall assessment of Keynes's contributions to monetary economics and the theory of interest was not on the whole negative, though. As often pointed out by Patinkin (see e.g. [1968] 1972, p. 135; [1974] 1982, Sections 1 and 2), Keynes's liquidity preference theory fully articulated for the first time the dependence of money holdings on the rate of interest and, by that, provided the essential starting-point for the explanation of money demand as part of the optimum-portfolio approach. Keynesian monetary theory went astray, however, in its claim that the dependency of money demand on the rate of interest implied that the rate of interest itself would be decided by the demand and supply of money – this unwarranted implication reflected a confusion between what Patinkin called individual and market experiments. Nevertheless, as Patinkin (1950–1951, Section 8) was the first to point out, Keynes broke new ground when he correctly argued that shifts in liquidity preference between money and bonds will affect permanently the rate of interest. An increase in money demand, as explained by Patinkin, will leave the interest rate unaffected only if it is neutral, that is, at the expense of both money and commodities. Keynes implicitly assumed that the increase in liquidity preference is solely at the expense of bond holdings, which changes the relative liquidity of money and bonds. The upshot is that, under these conditions, the rate of interest raises, the price level falls, and the real amount of money in the economy becomes higher (see also Patinkin 1965, Section X.4). That important point had not been recognized by pre-Keynesian economists, as acknowledged by Robertson (1953–1954) with reference to Patinkin's demonstration (see Boianovsky and Presley 2009, Section 6).

Effective Demand and Employment

Patinkin's 1957 restatement of Keynesian economics concluded with a plea for a dynamic interpretation of Keynes's employment theory. The introduction of the real balance effect into the commodity market makes it impossible to interpret Keynesian economics in terms of long-run unemployment equilibrium, since prices will be falling continuously. From a disequilibrium perspective, in a short-term context one should take into account the effect of falling prices on expectations, confidence etc, which may turn the adjustment process into a long and politically unacceptable one. Patinkin then reaffirmed the conclusion of his 1948 *AER* paper, that Keynes had made a revolution in economic policy, not theory. Keynesian economics had not, in Patinkin's view, successfully challenged the theoretical proposition – included in incipient form in "classical" pre-Keynesian economics but fully elaborated in its final form in *Money, Interest, and Prices* – that long-run equilibrium is described by full employment of labor and capital. From Patinkin's perspective, the only way to make sense of the notion of equilibrium is to frame it in terms of the Walrasian system, even if complemented by the Hicksian notion of temporary equilibrium under the assumption that trade takes place sequentially

(the week device) – which brings in price expectations, usually assumed to be unit-elastic by Patinkin.

Patinkin's disapproval of equilibrium concepts distinct from the Walrasian one is behind his emphasis on the real balance effect. He had attempted in the 1947 dissertation to make precise the idea of Keynesian long-run equilibrium with unemployment, but the framework was not fully satisfactory and was soon abandoned by him (see Boianovsky 2002, 2006; Rubin 2002, 2005). Interestingly enough, the "Pigou effect" (as was called then) was rejected in the dissertation (Boianovsky 2006, pp. 217–218), to be incorporated later in the 1948 *AER* article under the impact of Friedman's criticism in correspondence and conversations in Chicago (Rubin 2005, Section 2).[2] However, differently from Friedman, Patinkin (1948, p. 558) stressed that data for the American economy during the Great Depression indicated that the large increase in real balances had not prevented the sharp contraction in output in that period. Indeed, Patinkin complained that Friedman's reliance on the efficiency of the Pigou effect (as shown in some of Friedman's writings at the time) was unwarranted, and challenged him to produce empirical evidence that deflation could bring the economy back to full employment within an "acceptable" interval. "What empirical evidence can you adduce in support of such faith? I still think the experience of the... period 1929–1932... is a strong evidence against your position" (letter from Patinkin to Friedman, 7 Nov 1949; quoted by Backhouse 2002, p. 190, and Rubin 2005, p. 55).

Given the standpoint that economic downturns and involuntary unemployment are disequilibria phenomena, the next stage is to analyze the determination of output in periods of aggregate demand constraint. According to Patinkin, the concept of aggregate supply of goods was the missing link in the economics of unemployment in the 1940s and 1950s. He tried to fill the gap in his 1949 piece about "Involuntary unemployment and the Keynesian supply function", which did not attract as much attention as his other papers about monetary theory at the time, but raised a few reactions that were enough to put the topic in the macroeconomic research agenda in the mid and late 1950s (see Boianovsky 2002).

Although Patinkin (1949) contained an early formulation of his disequilibrium approach, it was only in Chapter XIII of *Money, Interest, and Prices* that he would put forward his claim that under involuntary unemployment both workers and firms are off their respective labor supply and labor demand curves. The route from part II of the 1947 dissertation to that famous chapter was anything but linear, as discussed in detail elsewhere (Boianovsky 2006). The focal point of Patinkin's (1956) unemployment model was his diagram of a kinked labor demand function, which illustrated the inability of firms to sell their equilibrium output, and so to employ the equilibrium amount of labor. Patinkin's main interlocutor during the elaboration of

[2]As pointed out by Patinkin (1956), Knut Wicksell ([1898] 1936) had already considered the role of the real balance effect in monetary economics. Wicksell influenced Patinkin not so much with the introduction of real balances in excess demand functions, but with the stability analysis of the price level, which would become an important feature of *Money, Interest, and Prices* (Boianovsky 1998).

Chapter XIII in 1954–1955 was Nissan Liviatan, his former student at the Hebrew University. Their correspondence makes clear that Patinkin was aware of some of the analytical problems entailed by the "weird kink", especially in the Walrasian general equilibrium context of price flexibility in which it was embedded. Those problems, however, did not prevent Patinkin from arguing that the application of his notion of "spillover effect" to the labor market was the best way to clarify the idea of involuntary unemployment. The puzzle of the influence of output on the amount of demand for work – even though labor demand is a function of real wages only – could be solved, according to Patinkin, "by leaving the static boundaries, which will allow us to think about points off the demand curve, by leaving Marshall's partial analysis and going to Walras's general equilibrium where the influence on other markets of excess supply in one market can be examined" (letter to Liviatan, 13 Jan 1955).

Patinkin's disequilibrium model of unemployment was conspicuous for its absence in the 1957 lectures at LSE. One may surmise that Patinkin had decided to leave it out of his "restatement of Keynesian economics" because it was new and controversial. Interestingly enough, that model was counter evidence to Patinkin's own conclusion that – since long-run convergence to full-employment equilibrium remained unchallenged – the novelty of Keynesian economics did not extend to economic theory, just policy. Another possible reason why the disequilibrium model is absent from the 1957 London lectures may be the fact that, although it confirmed and reinforced the analytical relevance of Keynes's concern with involuntary unemployment, it differed in important aspects from Keynes's (1936) own exposition. Indeed, Patinkin had maintained, since the 1947 dissertation – against both classical and Keynesian economists – that under unemployment conditions there is no reason to expect that the real wage rate is equal to the marginal product of labor (see also Boianovsky 2006, p. 213). In contrast with Keynes, Patinkin's (1965, pp. 324, 340) disequilibrium approach did not imply that employment and real wages are inversely related.

Differently from Leijonhufvud (1968), who claimed that the "economics of Keynes" – as opposed to "Keynesian economics", which he associated with the neoclassical synthesis – was essentially disequilibrium economics with absence of market clearing in both labor and good markets, Patinkin made clear that Keynes's (1936) treatment was distinct. Indeed, when Patinkin (1976a, p. 94) embarked in the mid 1970s in a long and detailed historical investigation of the development of Keynes's thought, he pointed out that, despite's Keynes goal of integrating monetary and value theory, the Cambridge economist had "not really developed a theory of the demand for labor consistent with the state of unemployment qua market disequilibrium".

Patinkin's careful study of Keynes's theory of effective demand led him to withdraw his previous charge that Keynes had overlooked the role of the aggregate supply function. According to Patinkin (1982), the "central message" of the *General Theory* was the proposition that a decline in output caused by excess aggregate supply will reduce supply more than demand and bring the economy to equilibrium at less than full employment. The equilibrating effect of the contraction in

aggregate income is the core of Keynes's theory of effective demand, based on the assumption that the marginal propensity to consume is less than one. That was the main analytical difference between Keynes (1936) and the pre-Keynesian discussion of business cycles and short-run monetary disequilibrium based on the Wicksellian cumulative process – a similar point had been made by Lloyd Metzler (1946), with whom Patinkin interacted in Chicago in the mid 1940s. According to Patinkin (1982), Keynes's central message could not be found in other authors who had also discussed the theory of effective demand before 1936, such as Michal Kalecki (Patinkin, however, overlooked the case of the American economist Frederick B. Hawley, examined in Boianovsky 1996).

Eventually, Patinkin (1982) concluded that the notion of aggregate demand constraint could be used even when the goods market clears through price changes, as in Keynes's formulation. Keynes's presentation of the theory of effective demand was marred by his modeling of the representative firm as a giant firm, and the related assumption that the aggregate expenditure function represents entrepreneurs' expectations of demand – which is inconsistent with the Marshallian theory of firm in perfect competition. Patinkin (1982) showed how Keynes's short-run equilibrium is determined through changes in prices and production, under the correct assumption that the aggregate demand curve is not perceived by the representative firm, whose only relevant short-run expectation is the price of its product. Interestingly enough, this is close to Patinkin's formulation in the first (1953) draft of *Money, Interest, and Prices*, later abandoned by him, about the determination of equilibrium when short-run price expectations of firms are confirmed (Patinkin 2006 [1953]; see also Boianovsky 2002, Section 6, and references there cited; and 2006, Section 4).

By the mid 1970s, Patinkin's earlier assessment that the Keynesian revolution was mainly about policy was gone. He would deny that there is an exact matching between policy recommendations and theoretical frameworks. "The real Keynesian revolution took place not in the sphere of economic policy (where changes were already occurring in the 1930s), but in that of economic theory. I suspect the real change wrought by Keynes's *General Theory* was in the conceptual framework from which he viewed the problems of employment, interest, and money" (Patinkin [1974] 1982, pp. 168–169). The revolution was, after all, about economic theory. This change of mind probably reflected not only Patinkin's ongoing research about Keynes's central message, but also the status of disequilibrium macroeconomics at the time, which had finally penetrated the research agenda of macroeconomics after years of neglect (see Backhouse and Boianovsky Unpublished).

The Crisis in Keynesian Economics

In July 1976 Patinkin drafted a research proposal titled "A Reexamination of Keynesian Economics", which consisted of three topics (1) history of the Keynesian revolution; (2) the theory of unemployment; and (3) inflation and

unemployment. In the following years Patinkin published extensively on the first topic, but not so much on the others. The planned second topic was intended as an investigation of some unsolved problems that arose in connection with the interpretation of the relationship between unemployment and the profit maximizing behavior of firms advanced in *Money, Interest, and Prices* and further discussed by Robert Clower, Leijonhufvud, Barro and Grossman and others. "I am not too satisfied with the solutions that these economists have offered, and so am planning to return to the question with the hopes of being able to advance the analysis further". Among the analytical questions relevant to an economy in disequilibrium that Patinkin mentioned were the reinterpretations of the role of *tâtonnement* and of Walras Law. Patinkin (1987) did contribute a substantial entry about Walras Law for the *New Palgrave* – where he argued, against Clower, that Walras Law applied also in disequilibrium conditions with quantity constraints – but did not add to the original treatment of unemployment put forward in Chapter XIII of his 1956 book. After the mid 1970s the discussion of disequilibrium macroeconomics became dominated by the more technical approach of the fixed-price literature represented by Jean-Pascal Benassy, Jacques Drèze and Edmond Malinvaud. Patinkin kept himself out of that literature, since, as he observed at a 1987 conference, "you can develop a whole complicated mathematical theory of quantity constraints, but in most of the literature I don't see the person going back to ask why the quantity constraint exists".

The final topic of Patinkin's 1976 research proposal was directly related to what was then perceived as a "crisis in Keynesian economics", caused by the simultaneous occurrence of significant inflation and unemployment in the leading industrial countries. For the first time since the beginning of Patinkin's career as an economist in the late 1940s, Keynesian economics was no longer dominant. Patinkin referred to contributions with respect to the Phillips curve and the natural rate of unemployment by Edmund Phelps, Friedman, Tobin and others, and wrote that "I hope to be able to make further contributions to this literature". However, this did not happen. Instead, Patinkin's energy from the mid 1970s on was almost completely absorbed by his work as historian of monetary macroeconomics, especially the Keynesian revolution. In fact, it is as part of his essays in history of thought that we may find many of Patinkin's (mostly critical) reactions to developments in macroeconomic since the appearance of Friedman's extremely influential 1968 presidential address on the natural rate of unemployment (see also Leeson 2000). Another important source is the new introduction to the second, abridged, edition of *Money, Interest, and Prices*, which contains Patinkin's (1989) reactions to some developments in monetary macroeconomics since the publication of the second edition in 1965, as well as a summing up of his own contributions made since that date (e.g. on Walras Law, and on money and growth) and an extension of his full-employment model to an open economy.

The tone of the new introduction was distinct from the introduction to the first 1956 edition (which was kept in 1965), reflecting the defensive position of Keynesian economics in the mid 1980s. Instead of stressing the generality of the quantity theory of money and classical economics vis-à-vis Keynesian theory, Patinkin

(1989) criticized key concepts of new classical macroeconomics, such as market equilibrium, rational expectations and Friedman's natural rate of unemployment. It is worth noting that Patinkin was not against the concept of the natural rate as such. He considered the expectations-augmented Phillips curve, with its implication that there is no tradeoff between unemployment and inflation in the long-run, a clear improvement over the original one (Patinkin 1990, pp. 21–22). Indeed, Patinkin (1989, p. xxxviii) recalled that, before Friedman (1968), he used to teach the Phillips curve to his students at the Hebrew University as a relation between unemployment and nominal, not real, wages, which ignored one of the very assumptions of *Money, Interest, and Prices*, that is, the absence of money illusion.[3] Hence, the notion of the natural rate of unemployment was not just consistent with Patinkin's (1965, Chapters X–XII) full-employment model, but also made it more general. For, whereas Patinkin's full-employment model had been formulated in terms of the levels of wages and prices, Friedman's 1968 model re-elaborated it in terms of rates of change of wages and prices (see Dixon 1995, p. 65).

> As these [price] expectations are adjusted to the rate of inflation, the tradeoff disappears, and the economy returns to the "natural rate of unemployment", which is the term Friedman coined to denote what had hitherto been called "frictional unemployment", and which is accordingly the rate that corresponds to "full employment". What must, however, be emphasized is that though this has generally been the long-run effect of inflation, there has generally also been a prolonged short-period during which a tradeoff has taken place. Thus the expectation-augmented Phillips curve can be incorporated into the Keynesian system as interpreted above: namely, as a dynamic system whose level of unemployment varies over time (Patinkin 1990, p. 22).

The analogy between the formulation of *Money, Interest, and Prices* and Friedman's 1968 address was apparently complete: the long-run equilibrium of the natural rate and the short-run transition when price expectations are not fulfilled could be seen as the counterparts of long-run full employment and unemployment disequilibrium dynamics, respectively. Of course, at close inspection the analogy is far from perfect, since Friedman's short-run cannot be described as involuntary unemployment. Accordingly, and more importantly from Patinkin's (1990; see also 1989, p. xx) perspective, the very term "natural" rate of unemployment was misleading, since it implied that the automatic working of the economic system would take the economy to its long-run equilibrium level of employment – an implication that was at variance with the empirical evidence and denied the relevance of Keynes's research program as a whole.

Despite the misgivings about some implications of the natural rate concept and doubts about the empirical validity of the stability of the demand curve for money, Patinkin (1990, pp. 22–23) suggested that over the years the distance between Keynesians and monetarists had narrowed. Moreover, neither provided a

[3]However, Patinkin 1974, pp. 128-29, had defended Keynesian economics from Friedman's charge by pointing to the econometric work of L. Klein and others in the 1950s, where the rate of change of money-wages depends on on the rate of unemployment and the rate of inflation; see Boianovsky 2002, section 5.

satisfactory explanation and correspondingly policy proposals for the macroeconomic problems of the 1980s. "Thus, what we have really been confronted with in these years is not only a 'crisis in Keynesian economics', but a 'crisis in macroeconomics'" (Patinkin 1990). New classical macroeconomics *'a la* Robert Lucas was not the best alternative either. Although the assumption of rational expectations had rightly emphasized the relevance of the credibility of government policy, it had also led to the "extreme" notion of the complete neutrality of anticipated policy (that is, a vertical short-run Phillips curve), an implication Patinkin (1989, p. xxvi; 1990, p. 23; 1992, p. 19) rejected on the basis of work by Stanley Fischer and John Taylor on long-term wage contracts.

The second assumption of new classical macroeconomics, according to Patinkin (1990, p. 23) was market equilibrium, which he described as "the antithesis of Keynesian economics" and criticized as contradictory with the facts about unemployment. The alternative analytical approach is that of market disequilibrium generated by quantity constraints. Patinkin (1990) suggested that "one of the major issues of macroeconomics today is the choice between these two approaches". The problem with disequilibrium macroeconomics, in Patinkin's (1989, p. xviii) view, was that, although it was consistent with prolonged periods of unemployment, it apparently violated the "assumption of rational economic behavior", which had led to its rejection by new classical macroeconomics. Patinkin, however, believed that then emerging new Keynesian economics had been able to "rationalize the seemingly irrational". The assumption of imperfect competition (which Patinkin had resisted to incorporate into *Money, Interest, and Prices*; see Boianovsky 2006, Section 5) – together with the "efficiency-wage", "insider-outsider" and other approaches to real wage rigidity – had apparently made acceptable Patinkin's original notion that the actual level of employment is determined by the aggregate demand for output instead of real wages (Patinkin 1989, p. xix).

Interestingly enough, Patinkin (1984) had been asked to speculate about what Keynes would have thought of modern macroeconomics. Although not fond of that kind of exercise, he answered, on the basis of textual passages from Keynes, that the Cambridge economist would have rejected both assumptions of rational expectations (because it was inconsistent with Keynes's views about absence of probability calculus under uncertainty) and market equilibrium (because of Keynes's distrust in the efficacy of the market-equilibrium process). The same applied to the contention (by Robert Barro and others) that the market would not allow a condition of persistent unemployment, since contracts could be made which would make everyone better off:

> Because of the uncertainty of how others react to our actions, the actual world for Keynes was one that – in a macro context – could readily lead to the "globally irrational" results of the prisoner dilemma; not to the rational results of the Walrasian auctioneer (Patinkin 1984, p. 101).

That was also Patinkin's world, and it is no surprise that his own opinion about some developments in macroeconomics in the 1970s and 1980s was quite close to the judgment that he imputed to Keynes.

Final Remarks

The answer to the question posed in the title of this paper is that Patinkin was a Keynesian economist, in the sense that he regarded aggregate demand the relevant determinant of the level of activity over relatively long time spans. Indeed, Patinkin (1965, p. 340) considered himself to be "more Keynesian than Keynes", since decreases in employment were not connected with increases in the real wage rate in his disequilibrium framework. Differently from Keynes's Mashallian approach to economic theory (see Patinkin 1982; Leijonhufvud 2006), Patinkin sustained that the Walrasian framework was the best way to understand unemployment as the outcome of the interaction between markets in general disequilibrium. Being a disequilibrium phenomenon, involuntary unemployment could not be a feature of the long-run equilibrium of the economy, which brought about in some quarters the view that *Money, Interest, and Prices* essentially belonged in the classical economic tradition. That perception was reinforced by Patinkin's rejection of Keynes´s theory of interest as a monetary phenomenon. Patinkin's (1956) reference to unemployment theory as a "second major theme" of his *Money, Interest, and Prices* suggests that the book may have not just one, but two "central messages". Whereas the long-run neutrality results of the book caught most of the attention in the 1950s and 1960s – as a sort of antidote to the Keynesian domination of macroeconomics at the time – Patinkin would later emphasize his unemployment analysis, when new classical macroeconomics became increasingly influential in the 1970s and 1980s.

Alternatively, the book was interpreted as the culmination of the so-called "neoclassical synthesis", a term created by Paul Samuelson in the 1950s to express the unity between Keynesian aggregative macroeconomics and the microeconomic theory of resource allocation, once Keynesian policies had succeeded in taking the economy to its full-employment equilibrium. After Samuelson, the term became increasingly associated with the interpretation of Keynesian economics in a general equilibrium framework, reflecting the concern with the microeconomic foundations of macroeconomics (see Weintraub 1979, Chapter 4). Although this meaning of neoclassical synthesis to some extent fit the scope of *Money, Interest, and Prices*, the phrase was also used to describe the IS-LM interpretation of the *General Theory* as a constant money–wage economy. That was Leijonhufvud's (1968) meaning, who called it "Keynesian economics" as opposed to "the economics of Keynes".

From that perspective, Patinkin did not really belong to the neoclassical synthesis – a term he did not use – since he always tried to distance himself from the interpretation of unemployment based on rigid wages and prices. Patinkin's sense that the microfoundations of aggregate supply should be a central concern of macroeconomic research has proved to be right, even if a significant portion of macroeconomics has parted company with his original interest in involuntary unemployment (see de Vroey 2004). The careful study of the history of economic ideas in general (and of Keynes's thought in particular) was fully integrated within Patinkin's research strategy (see Backhouse 2002). However, at the same time Patinkin (1990, p. 25; 1984, pp. 101–102) pleaded that Keynes should be read as one of

the true classics in our discipline, not to invoke his authority to judge attempts to advance the state of macroeconomics. The controversial character of macroeconomic theory, reflected in the variety of alternative explanations of macroeconomic phenomena, resulted from what Patinkin (1989, p. xvi; 1992, p. 18) called *la condition scientifique* of economics. Reflecting his Cowles Commission background, Patinkin defined such condition as the inability to choose among alternative hypothesis, that is, "to reach definite conclusions about theoretical questions on the basis of empirical studies". However, there was no doubt in his mind that involuntary unemployment was a real world phenomenon, and that Keynes (1936) had been the first to identify it analytically.

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A Commentary on Mauro Boianovsky's "Was Patinkin a Keynesian Economist?"

Michel De Vroey

Mauro Boianovsky's paper on Patinkin is an excellent piece of intellectual biography bringing out the complex itinerary of one of the great interpreters of Keynes. I hope that one day Boianovsky will have the opportunity to extend it into a full-blown book. As I have no basic qualms with his general account, my comment will focus on one particular point, namely my disappointment at discovering that his paper fails to deliver on its promising title, "Was Patinkin a Keynesian economist?"

This question touches on a real issue: we have here an author, who from the beginning of his scientific career up to its end has considered himself a disciple of Keynes, yet recurrently faced criticism about the anti-Keynesian character of his contribution, leveled not only by Post-Keynesians but also, and more surprisingly, by authors such as Clower and Leijonhudvud. I expected Boianovsky to address this conundrum but he did not so. Here is then an outline of the paper that he could have written!

There must be good reasons for questioning the Keynesian character of Patinkin's work, reasons that are absent when it comes for example to the writings of Davidson or Leijonhufvud. I see at least two such reasons. The first is Patinkin's endorsement of the Pigou effect, re-coined as the real balance effect. It constitutes a central piece of his monetary theory but it has a devastating effect as far as Keynesian theory is concerned since it results in the collapse of the claim that involuntary unemployment can exist when the economy is in equilibrium. By taking such a stance, the "Keynesian" Patinkin comes close to shooting himself in the foot. Anti-Keynesian authors were soon to perceive the importance of this point. The second reason lays in Patinkin's decision to read Keynes through Walrasian rather than Marshallian lenses.[4] True, Keynes wanted to study an economy as a whole, his point being that unemployment originated in other parts of the economy than the labor market. But this did not make him a Walrasian economist. In other words, the Walrasian approach should not be granted a monopoly over general equilibrium while this is the standpoint adopted by Patinkin.

Small wonder then that Patinkin appears to be a split intellectual personality forced to engage in intellectual contortions. I am thinking, first, of his assertion that

[4] "Thus a basic contribution of the *General Theory* is that it is in effect the first practical application of the Walrasian theory of general equilibrium : 'practical' not in the sense of empirical . . . but in the sense of reducing Walras' formal model of *n* simultaneous equations in *n* unknowns to a manageable model form which implications for the real world could be drawn" (Patinkin 1987, p. 27). "The analysis of the *General Theory* is essentially that of general equilibrium. The voice is that of Marshall, but the hands are those of Walras. And in his IS-LM interpretation of the *General Theory*, Hicks quite rightly and quite effectively concentrated on the hands" (Patinkin 1987, p. 35).

the real balance effect plays a central theoretical role but is of little empirical relevance and, second, of his limiting the existence of involuntary unemployment to the duration of the adjustment towards equilibrium process, his disequilibrium theory of involuntary unemployment. None of these two solutions is satisfactory. The argumentation for or against the existence of involuntary unemployment ought to proceed at the abstract theoretical level and not by resorting to vague empirical assertions. As to slow adjustment towards equilibrium, this feature has no room in Walrasian theory as it ought to be assumed that the tâtonnement process takes place in logical time.[5] A last contortion is Patinkin's interpretation of the *General Theory* twisting Keynes's text in order to having it corroborate his own disequilibrium claim.

A further point is that the question making the title of Boianovsky's article cannot be tackled in earnest without addressing the definition of the "Keynesian" adjective. Boianovsky deals with this point in too loose a way by stating that "Keynesian economists are those who believe that the automatic market mechanism of price change does not efficiently lead the economy to its full-employment path" (Boianoski 2010, p.). My own viewpoint is that a distinction should be drawn between two meanings of the Keynesian term.[6] First, it may refer to a vision as to the working of the market economy and its associated policy conclusion. Against this aspect, being Keynesian means to believe that the market system can fall prey to market failures, which can be remedied upon through demand activation policies. Defenders of the free market system are anti-Keynesian in this policy cause conception. Second, it may refer to a precise conceptual apparatus drawn from a lineage going from Marshall to Keynes and, next, to the IS-LM model. Being a Keynesian in this sense means to use the Marshall–Keynes–Hicks framework – to all intents and purposes, the IS-LM model. Economists using any other conceptual apparatus are non-Keynesian in this second sense. This is the case for works following the Walrasian line (Patinkin, Barro and Grossman) or the Marshall–Chamberlin (imperfect competition models) line. Note that no normative connotation should be attached to being or not being Keynesian in either of the two senses. The purpose of the distinction is just to remedy upon the prevailing semantic confusion.

Once this distinction is drawn, the question forming the title of Boianovsky's paper is easily solved. Patinkin was Keynesian as far as the first criterion is concerned but non-Keynesian against the second criterion. At first, this may look a bizarre combination but upon scrutiny it is not so. Certainly, Patinkin is not isolated in holding such a mixed position. His mirror image is Friedman who had few qualms with Keynes's conceptual apparatus while, obviously, being anti-Keynesian from the aspect of the policy cause defended. Modern neo-Keynesian authors are in the same position as Patinkin. They hold a Keynesian vision of the market economy and favor demand activation but they have abandoned the

[5]Cf. Donzelli (2007).

[6]Cf. De Vroey (2009, 2004).

Marshall–Keynes–Hicks conceptual line. The difference between Patinkin and them is that he adamantly wants to attach his reasoning to Keynes's, at the price of the above noted contortions, while they just do not mind.

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Keynes, Robbins and the Nature of Economics

Amos Witztum

Abstract Robbins's *Essay* went through two rounds before Keynes's *General Theory* came out. It aimed at defining and delineating the subject matter of economics. Yet, in spite of Keynes's revolutionary sentiments, there is no reference to the *Essay* in the GT. Given that much of the *Essay* has been adopted by most modern economists, examining the GT in terms of Robbins's conception may provide an interesting perspective on Keynes's contribution. We find that there are difficulties in the ethical neutrality of Keynes's agenda; in his approach to postulates as well as in the way he treats equilibrium. But by examining these three aspects we find it difficult to uphold the view of Keynes as a revolutionary. Instead, we find a considerable amount of methodological confusion which may explain the diverse interpretations emanating from his work. We also find that some of these confusions could have been resolved had Keynes taken a better look at the real classical school. We find that while many of Keynes's concerns exited before him at least in Marshall and Mill whom he tends to refer to repeatedly, he did ignore the way in which real classical economics (i.e. Mill and Smith) treated them. We will show that Keynes was referring to two different types of equilibria in classical analysis: a "good" one and a "bad" one. This led both Mill and Smith to emphasise the "endogenous" role of government. Keynes's novelty was that his misconception emphasised the "exogenous" role of government. In this way, Keynes did a disservice to those who have always doubted the ability of markets to regulate themselves.

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Introduction

When Keynes published his *General Theory* (GT) in 1936, Robbins's *Essay on the Nature and Significance of Economic Science* had gone through two rounds (1932 and 1935). Yet, in spite of Keynes's "revolutionary" feelings,[1] there is curiously no reference in the GT to, perhaps, one of the most important attempts to identify the main stream of economic analysis.

On the face of it, one may say that there is no obvious reason to refer to a methodological discussion in a book which is theoretical in nature. But the GT was not just a theory, it was a new theory; ostensibly, a departure from an existing paradigm, which, if there was a purpose to Robbins's *Essay*, had not yet really been properly formed.

Robbins's *Essay* was an attempt to delineate and define the subject matter of economics at the time when debate was raging about its nature and significance.[2] Keynes's GT effectively opened yet another frontier but in spite of being clearly methodological in nature, the GT is devoid of any attempt at positioning the new theory in the context of these debates.

This is particularly crucial given the way in which the debate about Keynes's own contribution has developed.[3] Backhouse (2006) provides a good summary of the history of this debate but in a nutshell, it was mainly about methodology. Townshend (1937), Shackle (1973) and Robinson (1974) picked up on Keynes's own response to the debate in the *QJE* to claim that the contribution of the GT was to introduce time and expectations into the market mechanism of all goods. Coupled with Keynes's insistence on the irreducibility of uncertainty by probability, this meant the introduction of a non-equilibrium approach to economic analysis.

Leijonhufvud (1968) agreed that the contribution of the GT was about market failure but not just because of time and expectations. It was a failure because of the absence of an "auctioneer" and thus, the GT is not merely about non-equilibrium but rather about disequilibrium economics. Together with time and expectations this would be extended into an analysis of inter-temporal disequilibrium.

In turn, a whole literature of disequilibrium macroeconomics developed [Solow Stiglitz (1968), Barro and Grossman (1971) and Benassy (1986)] but from the point of view of the methodological debate, as captured by Leijohnhufvud, this was not really a proper modelling of disequilibrium. These models were simply Walrasian

[1] In a letter to G B Shaw, Keynes writes: "I find myself to be writing a book on economic theory, which will largely revolutionise. . .the way the world thinks about economic problems" (Skidelsky 1992, 520); also quoted in Davidson (2007, 18).

[2] Robbins himself quotes contemporary competing definitions from within the Anglo Saxon tradition (Marshall, Davenport, Beveridge and Pigou) not to mention the continental strands emanating from the Austrian School, the School of Lausanne, German Historicism and American Institutionalists.

[3] I am looking here mainly at the period prior to the development of stagflation where the debate was mainly concerned about the interpretation of the work itself without further external input about the discussions on its validity.

models with rationing and as such they did not, in his view, capture the spirit of the GT.[4]

On the other hand, there were also the less "revolutionary" interpretations like Clower (1965) and Patinkin (1965) where methodology was not at the heart of the discussion. For them, the main contribution of the GT was the role of money in exchange. Due to the fact that people exchange goods with money and labour with money, the presence of unemployment may trigger a co-ordination of decision failure.

Strangely, when examining the nature of the debate which seems to be emanating from the GT, one cannot avoid the feeling that we have already been there. For instance, on the matter of equilibrium and disequilibrium there are considerable similarities between the arguments about the nature of an "unemployment" allocation and some elements in the great debate between the Lausannian and the Austrian schools. More worrying is the sense that even on some important substantial points, Keynes was not really saying anything new.[5] As Backhouse notes, the IS-LM model – a clearly neo-classical synergy – closely follows the GT and thus suggests a much less dramatic departure from standard economic analysis. Moreover there is this rather peculiar claim which Keynes makes at the end of the GT, where he says that he only wanted to correct a premise of the "classical school" but once we reach the state of full employment, the classical schools "comes to its own" (GT, 378).

All of this suggests that throughout the GT Keynes was not fully aware of the position of his own theory within the broader context of the development of economic analysis. Robbins, on the other hand, is acutely aware of the raging intellectual battles and attempts to provide some order in it.

The purpose of this paper, therefore, is twofold. Firstly, I would like to examine how well does Keynes's analysis (which appeared 4 years after the publication of the *Essay*) follow the delineation suggested by Robbins. Secondly, I would examine the originality and integrity of Keynes's theory in view of some of these schools which Robbins so easily discarded.

From Robbins's perspective, Keynes's theory does not seem to fit into the framework of an economic theory. It is not ethically neutral, its postulates are not as obvious as they may appear and the difficulties with the concept of equilibrium completely derail the ability of economics to comment on efficiency. At the same time, Keynes theory can be viewed as a challenge to Robbins's ideal. As such, it seems that the Keynesian story reverts back to the real classical economists (notably, Smith and Mill) who distinguished between a "good" and a "bad" equilibrium in addition to being aware of the process by which the economy may reach its equilibrium. The implication of the distinction between "good" and "bad"

[4]A more localised and focused attempt at interpreting Keynes were concentrated on Chap. 2 (and 19) of the GT or the possibility of non-clearing labour market. However, here too, the focus of attention has been primarily methodological. Hoover (1995) provides a summary of this line of investigation.

[5]See, for instance, Laidler (2006).

equilibrium allows us to distinguish between what I would call exogenous and endogenous roles of the government. As Keynes believed that even at the "bad" equilibrium, those who participated in the system were efficiently allocated, the question which he poses is really about participation and as such, he considered it the subject of exogenous government activities. In other words, the "bad" equilibrium was not really bad and it was simply that society had to choose between two "good" equilibria. Government intervention is thus exogenous as it only shifts the axis on which the economy rotates.

However, if we conceive the two allocations as a "good" and "bad" equilibria, then the role of government becomes endogenous in the sense that it must intervene in the initial conditions as well as influence the way markets operate and become part of the system not only when there are clear market failures. This is particularly acute when in the world of incomplete markets; there are market failures across the board.

We also note that given the difference between the economic problem perceived by Keynes (which he, in fact, shares with neo-classical economics in spite of Robbins's protestations) and that of the real classical school suggests that the problem of unemployment has the same methodological function as the decline in labour when wages fall below subsistence. For Keynes, the failure of the system meant involuntary unemployment but for the classical school, it meant something even more disturbing, involuntary employment.

Robbins and the Science of Economics

The purpose of Robbins's *Essay on the Nature and Significance of Economic Science* was to carve a niche for economics as an independent discipline and to maintain its scientific nature by divorcing it from anything remotely ethical. By offering a clear, almost mechanical, definition of what constitutes the subject matter of economics, he was fending off historicism, which opposed the breaking up of social phenomena into well defined and separate areas of investigation.[6] By relying on a notion of the *Verstehen Doctrine*, he was hoping to ward off the institutionalists' demand for direct empiricism as a way to formulate the postulates of economic analysis.[7]

[6]See a discussion of this point in Hodgson 2001, pp. 207–208.

[7]By Verstehen Doctrine, in this context, I refer to Robbins's suggestion that we are able to understand human action on the basis of introspection. "We do not need controlled experiments to establish [the postulates'] validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious" (p. 79). Weber (1947) is a key figure in the development of the concept. For a more general discussion of the doctrine see Blaug (1980), pp. 47–49, and in connection with Robbins, see a discussion in pp. 86–91.

At the heart of this conception, therefore, we find the followings main tenets:

1. The subject matter of economics is the tension between scarcity and wants (the means-ends problem)[8]
2. Economics is based on axioms (abstractions) which are derived from experience and which lead to statements about reality[9] (hence, the "scientific" nature of the subject)
3. Economics is not concerned with ends but only with the means available to achieve those ends. It is thus, value free[10]

Irrespective of how well received was Robbins's *Essay* at the time, these tenets are familiar to the modern reader and would be easily found in most contemporary textbooks. In this respect, Robbins represents the turning point in the debate about methodology and as such, a relevant reference point to any new developments like the one proposed by Keynes. Moreover, the significance of Robbins's work to today's conception of economics makes the issue a contemporary one and not merely a historical debate.

Both the first and the third tenets have an important role in what perhaps was Robbins's main objective: to divorce economic analysis from any value system or ethics. On the face of it, Robbins makes a simple claim: People may have different ends but they will always need to allocate resources. If economics were to deal with all such allocations, it would have been value free. However, there are two main problems to this approach which are significant to the analysis of Keynes's contribution. Firstly, economics would not have a well define criterion of economic performance as these would depend on the objective people seek to achieve. Secondly, the idea of efficiency in the sense of waste minimisation would not necessarily be part of economic analysis. Taken together, these two difficulties make economics less, rather than more, well defined or analytical. Clearly, this could not have been Robbins's intention. Nor is it, in my view, a correct reading of Robbins and the sources which inspired him, and in particular, Wicksteed.

There are three fundamentals which appear to help Robbins achieve the isolation and independence of economic analysis. Firstly, he insists on divorcing the definition of the subject from any particular objectives and in particular, the one he perceives to dominate the English tradition (in which, of course, he includes

[8]"From the point of view of the economist, the conditions of human existence exhibit four fundamental characteristics: The ends are various. The time and the means for achieving these ends are limited and capable of alternative application. At the same time, the ends have different importance" (Robbins 1935, p. 12).

[9]Section 2 of Chap. 4 is devoted to show that economic generalisations are not historical or based on experiments. It is here that Robbins appeals to the *Verstehen* doctrine according to which we should base our axioms on some notion of introspection. "In the light of all that has been said the nature of economic analysis should now be plain. It consists of deductions from a series of postulates, the chief of which are almost universal facts of experience present whenever human activity has an economics aspect. . .." (Robbins 1935, pp. 99–100).

[10]To which Section 4 of Chapter 6 in Robbins (1935) is devoted.

Marshall). By insisting that economics is not about wealth creation he offers a view, which still resonates today, that economists have nothing to say about the objectives of human action. Instead, economics is merely concerned with finding solutions to the problem of choosing appropriate means to achieve certain ends when there is scarcity.[11]

This step is essential in the Robbins scheme as it facilitates both the universalisation of economics and its value free agenda. The former is based on the absence of any substantive boundaries to the application of economic analysis except the presence of scarcity. By implication, this is also a licence for what some people call today "economic imperialism". As scarcity is prominent in almost all aspects of the social sciences, there are no reasons why economic analysis should not be applied across all disciplines.

The value free agenda is also assisted, as by removing particular objectives (like wealth creation) from the economic analysis of human behaviour, economics can avoid the debate about the moral significance of these objectives. Hence, according to Robbins, current debates about whether growth maximisation is a legitimate social objective instead of, say, happiness, lie entirely outside economic analysis. Clearly to advocate full employment would be equally outside the domain of economic analysis. In other words, economics does not distinguish between solutions in terms of what they are trying to achieve. Instead, economics can only comment on whether that which has been achieved was achieved by the best means or the minimal opportunity costs. However to be able to say as much we would need to know what the real opportunity costs are and for this, we would need to know the prices at a competitive equilibrium. Notwithstanding whether competitive equilibrium is a sufficiently universal concept, for Robbins, economic was merely about the comparative static of equilibrium outcomes: "Equilibrium is just equilibrium" (p. 143) and is, according to Robbins, independent of what we are trying to achieve.

But there is a far more immediate problem with Robbins's approach. If indeed "there are no economic ends. Only economical and uneconomical means of achieving given ends" (p. 144), then economics is about the minimisation of cost (both at the individual and social levels). However, is the minimisation of costs not the logical dual of maximising wealth? Wicksteed (1933) claims that by avoiding wastage, economics is about creating as many means as possible to achieve whatever ends individual/society wish to achieve.[12] But unless we create a distinction between productive and unproductive economic goods-which, of course,

[11]O'Brien (1990, p. 155) claims that in microeconomics Wicksteed (1933) was the primary source of influence on Robbins. However when we examine Wicksteed's conception of the subject, the difficulties with separating ethics from economics become apparent. It is easy to see in his writing (in particular, in pp. 182–185) why such a position is not as obvious as Robbins, and Wicksteed, try to portray. I will deal with this further, below.

[12]"The tendencies of modern thought and the conditions of modern life have combined to sever the consideration of the administration of resources from the discussion of the ultimate ends . . . it has therefore become usual to treat Political Economy as concerned with increasing the communal means rather than securing the communal ends;"(Wicksteed 1933, p. 15).

neither Robbins nor Wicksteed subscribed to- this is virtually the same as output maximisation. Why then, is the accumulation of means not an objective which is open to ethical scrutiny?

The second fundamental in Robbins's argument is the implicit presumption that there is always a potential co-ordinated outcome when individuals solve their end-means problem in the face of scarcity. In economic terms, this means that there will always be equilibrium irrespective of which ends people seek to promote.

Now equilibrium is a highly charged concept. This is particularly so when many attribute to Robbins Austrian tendencies. However, while one would find Austrian ideas in Robbins, the fact that he followed Wicksteed (1933) [who followed von Wieser (1967)] suggests a broader agenda than the one adopted by a leading Austrian. In his *Notes and Recollections* von Mises claims that Wieser, a follower of Menger, has moved so far away from anything Austrian that one can say that he "was a member of the Lausanne School" (von Mises 1978, p. 36). It is therefore not surprising that both in Wicksteed and Robbins (as well as Wieser)[13] one can find a far greater commitment to general equilibrium-in a more Walrasian sense- than the mere nebulous idea of spontaneous order. Robbins clearly adopts the idea of general equilibrium as can be seen in his Essay (Robbins 1935, pp. 67–69), in his discussion of stationary equilibrium (Robbins 1930), and in his analysis of costs (opportunity costs) (Robbins 1934). O'Brien (1990) quite explicitly admits that "The focus of analysis [being] upon equilibrium-*general* and not *partial* equilibrium" (p. 158).[14] If indeed, as O'Brien implies, Robbins's microeconomics is very much the same as the modern one, it is not at all surprising to find a Walrasian concept of equilibrium at its heart.

But the presence of equilibrium in Robbins does not necessarily mean that it is the same as that of Walras.[15] It does not necessarily contradict the view according to which the main interest lies in the process by which prices converge to equilibrium even if they never do. Equilibrium, in this broad sense, can be interpreted as a

[13]See Salerno (2002).

[14]Blaug (1990) criticises O'Brien for attributing general equilibrium to Robbins. The reason for this is that he objected to the potential connection between Robbins and Walras. For Blaug, Walrasian equilibrium is a historical aberration. However, Robbins's connection with the Lausanne school cannot be denied and in particular, as considered it as important influence on Wicksteed. Moreover, I will later try to demonstrate that Robbins could not have meant any other form of equilibrium but this does not mean that the Walrasian idea is incompatible with more process-based analysis. The problem, in my view, is more with Blaug than with Walras (see a discussion in Witztum 2008).

[15]Though when Robbins says that "Instead of dividing our central body of analysis into a theory of production and a theory of distribution, we have a theory of equilibrium, a theory of comparative statics and a theory of dynamic change" (Robbins 1935, p. 68), it is difficult to see how could he have objected to Walras. The truth of the matter is that he has not at all objected. In the introduction to Wicksteed's *Common Sense*, Robbins says that Wicksteed "was deeply influenced by the work of those who carried the application of mathematical methods furthest – by the work of Walras and Pareto" (Robbins 1933, xviii).

logical limit, or the benchmark, and it is in this way, which Robbins (and Wicksteed) conceived it.[16]

The reason why the universality of equilibrium under any sort of motivation is important in Robbins's ethics–economics scheme is that once the objective of wealth creation has been dismissed, there is not much left for economists to say. We saw that they would like to say something about cost minimisation. The other thing which they may say is to explain the relationship between prices. The two are, of course, related as one cannot properly account for opportunity costs without competitive equilibrium prices. Indeed, as Robbins says quite explicitly, the only thing, which economics can say, is to describe this equilibrium and to describe what would happen if there were a departure from complete freedom (pp. 143–144). In other words, economics only describes the co-ordinated outcome that would emerge under all possible sets of motives and characters but cannot comment on the significance of any *institutions* in resolving any particular economic problem. In other words, economics can either prescribe, for which it would need a well defined problem, or describe, for which is needs a reference point.

Keynes and Robbins's Criteria

While it is easy to see that from Robbins's perspective, Keynes's analysis fails to satisfy some of the criteria which, in his view, define the subject matter of economics; on others it is not entirely clear. Notwithstanding the difficulties which may arise from Robbins's definition, the fact that much of it has been adopted by modern economics may help in explaining some of the difficulties in the relationship between Keynes and economics which go beyond the substantial issues. It may also shed some light on the complexity of relationship between micro and macro economics from a perspective which is not usually analysed.

The Means-End Test

Robbins defines economics as the investigation into the solving of means-ends problems in the face of scarcity. Economics is not interested in the ends but only with the 'efficient' use of means: "[T]here are no economic ends" writes Robbins, "[o]nly economical and uneconomical means of achieving given ends" (p. 144).[17]

[16]This, quite interestingly, corresponds well to Robbinson distinction between historical and logical time.

[17]Also: "Economics . . . is concerned with that aspect of behaviour which arises from the scarcity of means to achieve given ends. It follows that Economics is neutral between ends; . . . it] is not concerned with ends as such" (Robbins 1935, p.24)

As far as Keynes is concerned there is a certain ambiguity regarding his contribution which can be captured by the difference between Chapters 1 and 24 of the GT. In Chapter 1 Keynes makes a clear claim that the purpose of his work is to "contrast the character of my arguments and conclusions with those of the *classical* theory" (GT 3) and to "argue that the postulates of the classical theory are applicable to a special case only and not to the general case" (GT 3).

This may appear sufficiently neutral from an ethical position as Keynes does not seem to have any objective other than to correct the theoretical apparatus of the classical school.

However, Robbins does not find the "classical school", or more specifically Marshall as neutral in terms of its objectives as he would like it to be. He cites Marshall as one of the examples of a value laden approach to economic analysis: "Economics" writes Marshall, "is a study of the mankind in the ordinary business of life; it examines that part of individual and social action which is more closely connected with the attainment and with the use of material requisites of well-being" (Marshall 1952, Principle, 1). Hence, according to Robbins, this kind of economics is about the causes of material wealth, or about wealth creation, and is, therefore, subject to ethical scrutiny. Whether the analysis of wealth creation is motivated by an observation that people actually seek to do so or as an objective imposed from above, it remains an ethical issue. All the recommendations emanating from such an analysis would be subservient to the general goal which could easily be described as the service of greed. Robbins objection to this depiction of economics was based on both the descriptive part and the ethical one. Firstly he argues that many of the concepts in the domain of economic life have nothing to do with material wealth (like the wages of orchestra players) and secondly, that such an objective is open to ethical objections and as such, undermines the scientific nature of the subject.

On the other hand, in Chapter 24, Keynes reveals the real objectives of his theory:

> "The outstanding faults of the economic society in which we live are its failure to provide for full employment and its arbitrary and inequitable distribution of wealth and incomes." (GT 372)

Here Keynes clearly states a purpose for his economic analysis even though the title of the chapter suggests a reverse causality (from the analysis to the objective). Yet, given that his starting point was the generalisation of a theory which is focused on the study of wealth creation (or output maximisation) Keynes cannot escape the attribution of this objective to his theory. Therefore, Keynes's GT, is clearly about solving the problem of unemployment (i.e. output maximisation). Moreover, Keynes also believes that his solution to this problem will also help in resolving a second problem (distribution of income).[18] So in terms of

[18]Mainly through the conclusion that the possibility that re-distributive taxes may affect savings will not harm the economy in a state of unemployment.

Robbins's claim that economics is only about the means to an end, Keynes seems to be a clear departure.

If the purpose of economic analysis is indeed to find ways to ensure that there is full employment, one can no longer claim that economics is value free. This is particularly true if in addition, one has pre-conceptions about the "right" distribution of income and wealth.

From an ethical perspective it is by no means obvious why it is that society should wish all its members to be fully employed. In fact, this is far more complex as Keynes does not really want everyone to be employed. He only wants that those who wish to be employed would be able to do so.[19] However, the objective of eliminating involuntary unemployment is even more ethically questionable. There are three possible interpretations to Keynes's requirement. Either, that all things which individuals want should be provided for, or, that all things which people *rationally* want should be provided for, or, from a very different perspective, that everyone should be able to access social wealth (or income).

Keynes does not offer a proper explanation to what are the origins for his expectation that society should provide full employment. It cannot seriously be the first. It is difficult to conceive of an ethical system which would say that in the presence of scarcity, each individual is entitled to whatever they desire. If it is the third, then this is clearly an ethical objective which is derived from some "right" which people may have to work. So could it be the second reason? Namely, should society ensure the absence of involuntary unemployment because each individual is entitled to that which he *rationally* desires and is this a value free objective?

If it is, this is exactly what lies at the heart of the efficiency concept embedded in neo-classical economics (which Keynes refers to as the classical school). But it is not obvious that efficiency and equity are synonymous. In the end, the reason why economists tend to believe, in the context of the two welfare theorems, that efficiency is consistent with whatever society may wish to achieve – and thus, in Robbins's spirit neutral – is due to the consequentialist nature of their conception of ethics. If one person can be made better off without making anyone worse off what could possibly be the grounds for objecting to it? I do not wish to get into a complex discussion of this point but it is evident that not all ethical theories are consequentialistic.[20]

Hence, it is far from being self-evident that it is a meaningful social objective to allow all those who wish to be employed to find employment. Hence, not only is Keynes identifying an objective, or an economic problem, which is ethically

[19]It is more complex as it is no longer a question of a universal right but merely the ability to exercise rational choices.

[20]Sen (1970) proposed the impossibility of a Paretian liberal. However, is approach was flawed for reasons which are beyond the scope of this paper. However, it is not so difficult to imagine an ethical system where procedures matter more than outcomes. In such a world, efficiency and equity would by no means be compatible. I discuss some of these points in Witztum (2008) in the context of Robbins's claim for the neutrality of economic analysis.

complex, he provides no reason why it is that we should follow it. Perhaps Keynes's youthful fascination with G.E. Moore (1903) had overwhelmed him in the sense that he can, intuitively identify the good in exactly the same manner in which he can identify that which is beautiful.[21] For Robbins, on the other hand, this was neither appropriate nor relevant to what he considered the subject matter of the science of economics. The efficiency of competitive equilibria merely suggests that *whatever* society sought to achieve had been achieved by the best means possible in a world of scarce resources.

There is, perhaps, a way in which one may find Keynes's work consistent with Robbins's definitions. That is if we consider the GT as a policy document. While on the face of it this seems somewhat far fetched, it is not entirely unthinkable.

It is true that Keynes considered himself a revolutionary and that the GT is supposed to be a *general theory*. But if we consider the peculiar set-up of his theory it is not entirely clear that this is what he had achieved. In fact, Keynes identifies two worlds. One, where "classical model" allocates properly all those who are employed but cannot deal with those who are unemployed and the other is a world of full employments:

> "To put the point concretely, I see no reason to suppose that the existing system seriously misemploys the factors of production which are in use.... When 9,000,000 men are employed out of 10,000,000 willing and able to work there is no evidence that the labour of these 9,000,000 men is misdirected. The complaint against the present system is not that these 9,000,000 men out to be employed on different tasks, but that tasks should be available for the remaining 1,000,000 men." (GT 379)

Coupled with the idea that in full employment, the "classical" theory "comes to its own", what we have here is two points of equilibrium,[22] or, perhaps, two allocations generated by competitive institutions. In terms of allocating resources, they both seem to be working well and in exactly the same manner. Yet in the one case, some people remain outside of the system while in the latter they are all included.

Notwithstanding Keynes's explanations to why this is the case, in terms of what Robbins calls "theory" he has not changed anything. At the two points we have systems which allocate resources in the best means possible with regard to those who are in the system. The role of the economists, according to Robbins is to do just that. It is, however, a question of policy whether society prefers one allocation over another. Here, as I said, earlier, Robbins is quite happy to allow for ethical considerations to influence the discussion. It is, perhaps, for this reason that Robbins did come round Keynes's idea about fiscal policy but refused to accept Keynes's monetary theory.

[21] See an excellent discussion in Raffaelli (2006).

[22] I am aware of the question of whether the unemployment equilibrium is at all an equilibrium and I will discuss this a bit later.

Postulates

"The propositions of economic theory", writes Robbins, "like all scientific theory, are obviously deductions from a series of postulates" (Essay 78). These postulates must be based on *experience*: "We do not need controlled experiments to establish their validity; they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious". (Essay 79)

We must also note that Robbins's acceptance of the idea of the rational utility maximiser as one of economics postulates is well documents in Chapter 4 Section 5 (Essay 90–93).[23] It means that the agent behind Robbins's system is the opportunistic agent who would always choose the best means to an end regardless of what is the end.

The same is obviously true of Marshall who, for Keynes, represents that classical school.[24] However, Keynes does not really contest this postulate of classical economics which I take to mean that he accepted it as one of the "obvious" axioms, to use Robbins's language. Yet, one of the main novelties of Keynes' theory was, of course, the use of aggregates as the main tenets of his analysis. Presumably, there should be some connections between the uncontested postulates of classical economics and Keynes new structure of aggregates.

One would have thought that Keynes would offer a connection between the acceptable tenets of classical economics and the new structures which he proposes. In particular, as one would expect Keynes to be aware of the famous philosophical problems of the "cunning of reason".

For instance, in Chapters 8 and 9 of the GT Keynes lays down the reason why a community would have a marginal propensity to consume which is less than unity. It is interesting to note that in spite of his eight motives, his argument does not seem very different from what Adam Smith calls the tension between present and future enjoyments. However, while Smith attributes these motives to individual agents he is aware of the fact that the consequences of their collective pursuits may not be consistent with this balanced approach. After all, the outcomes of the competitive system may lead to stationary or even declining states. Elsewhere, Witztum (2009a), I show how a form of a prisoner's dilemma may emerge when, in a Smithian framework, agents seek to maximise the rate of growth of their own surplus. The outcome could be quite disastrous.

Moreover, as we said before, Keynes does not really reject the postulate of the rational utility maximiser. However, such an agent is someone who makes decisions which are based on prices, Yet, Keynes's rejection of the classical postulate

[23]O'Brien (1990) reiterates this by saying: "Thus economics was essentially to be about choice models. Rational behaviour, which amounts to acting consistently in accordance with the ranking of different possibilities , lay behind demand" (p. 158).

[24]See, for instance, a discussion in Marshall, principles, 92–93.

that interest rate represents the price of present consumption[25] is inconsistent with this postulate. In Chapter 13 of the GT Keynes describes how the decision about consumption is based entirely on the motives he mentioned in Chapters 8 and 9 and not at all on the interest rate which here, is the mechanism deciding the form of savings. He does acknowledge in Chapter 9 that the interest rate may have small effects on the marginal propensity to consume as a higher rate may increase the cost of present consumption and should, therefore, increase savings.[26] However, in the famous paradox of thrift Keynes counter this argument based on the behaviour of the aggregates. So we have here more than one paradox. While motive (3), which Keynes calls Improvement suggests that rational agents should want to save more when the interest rate rises (i.e. the price of present consumption goes up), Keynes creates a very peculiar reasoning as to why this will not affect the marginal propensity to consume. His claim is that:

> "The rise in the rate of interest might induce us to save more, *if* our income were unchanged. But if the higher rate of interest retards investment, our incomes will not, and cannot, be unchanged. They must necessarily fall. . .". (GT 111)

This basically means that the decisions about consumption are made by taking into account the equilibrium outcomes. Both intuitively and in terms of Robbins expectations for a postulate to be self evident, this is clearly not good enough. This is clearly an example of how the transition from the rational utility maximiser which directs the allocation of resources of which Keynes approves, to the aggregate poses serious difficulties.

Yet, Keynes does not offer a proper transformation from micro to macro which, I hasten to say, is not the same as that which most contemporary economists consider as the "micro foundations" of macro economics. So the Robbinsian question we face is as follow: are Keynes's postulates "so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious"? I doubt this very much. Robbins was driven by the idea of introspection and *verstehen* which are both based on our ability to experience these sentiments are motives. But while we may recognise all these elements which Keynes enumerates as the subjective factors in our individual experience, we have no way of telling whether this translate into a collective demand for consumption. More to the point, for someone like Keynes who was acutely aware of the role of expectations, the idea that what may drive us individually may not translate into the collective form should have crossed his mind. If consumers have expectations regarding the behaviour of other agents, would these not affect their actual choice of actions?

In the spirit of Keynes's own declaration on the purpose of his work, Davidson (2007) claims that the proper way of interpreting Keynes is indeed as someone who accepted most of the axioms of classical economics with the exception of three.

[25]"[classical economics] regarded the rate of interest as the factor which brings the demand for investment and the willingness to save into equilibrium"(GT 175).

[26]See GT 110.

Firstly, of course, there is the presumption of the neutrality of money; secondly, the gross substitution axiom and, thirdly, the ergodic axiom.[27]

The removal of only the first and the third of the "classical" axioms should have completely upset the market mechanism of allocating resources. If people have money in their utility function and if they do not have a reliable way of predicting the future, the mechanism of exchange in all markets would have been transformed. This is indeed the reading of Keynes which was adopted by Shackle (1973) and Robinson (1974). The removal of the third could have completely destroyed the idea of *laissez faire* according to which the free interaction of self-interested individuals can achieve a co-ordinated outcome which is socially beneficent.

Yet if this is indeed what Keynes intended, it is contradictory to all his declarations about the relationship between his own theory and that of classical economics. Surely if these axioms were removed, Keynes could not have claimed that those who are employed are properly allocated and in full equilibrium, classical economics comes to its own.

So we have a rather peculiar situation here. On the one hand, Keynes axioms which may be obvious, a la Robbins, lose their credence as Keynes ignores the problem of aggregation. On the other hand, while people may sympathise with the removal of the axioms mentioned by Davidson, this would make Keynes's theory incomplete and incoherent. If decentralisation yields an inefficient equilibrium (if at all) with and without full employment, what exactly is the purpose of seeking full employment?

Equilibrium

From the above it seems evident that we cannot really declare the postulates over the aggregates as clear but we can find sympathy – in terms of the self-evident nature of the postulates – with Keynes implied suggestion to remove the three postulates adopted by the classical school. While it is clear that Keynes was aiming at the labour market, in particular with regard to the gross substitutability axiom,[28] we cannot limit its application as the nature of these classical postulates – like all other postulates – is universal. If there is no money neutrality, no ergdoicity and in particular, no gross substitutability, there may not be equilibrium at all and even if there is, it may not be efficient.

As I said before, the existence and efficiency of equilibrium are essential to Keynes as they can justify the limits of his theory. If he realised the significance of the removal of these axioms, he should have started from the beginning rather than from the position in which he compares two classical based allocations. If we accept

[27]See Keynes (1921) and Davidson (2007, 26–34).

[28]Even though the most explicit dealing with it is in Chap. 17 of the GT where he is dealing with the essential properties of interest and money.

that Keynes wished to remove all three axioms than we must decree that his analysis is erroneous in the sense that the foundation of his analysis did not exist and the problem which he was trying to solve could be the least of society's concerned.

If, however, we believe Keynes's declarations then it is clear that this was not his objective and that he did not universally remove these axioms but thought that the may not be true in certain circumstances. This too, is extremely awkward because it means that the axioms are not sufficiently general and hence, are not really axioms.

From Robbins's perspective the question of equilibrium is quite significant. If economics is merely about means rather than end and the only thing economists can say is whether a certain action or outcome are efficient, economists would need to know the opportunity costs. This is also essential for Keynes's assertion according to which those who are employed are efficiently allocated.

However, we know that prices reveal the true opportunity costs only in an efficient competitive equilibrium. This means that even though the economy may not be in equilibrium at all time, there should be, in principle,[29] a co-ordinated outcome to all types of aims and objectives which people may seek to achieve.

In Robbins (1934), the connection between the evaluation of cost and equilibrium is clearly stated:

> "If we reflect upon the way in which equilibrium is established. It is surely obvious that it is only through regard for cost in the value sense that any harmony between technical displacements and prices can be conceived to come about. It is only in equilibrium that such a harmony exists. . ..It is not merely true of the Austrian approach. . .. [It] is as essential a condition of equilibrium in the Walrasian system". (Robbins 1934, p. 4)

In other words, we have here both the Austrian story of process which, Robbins claims, requires that people consider costs in terms of prices, and the Walrasian story according to which only in equilibrium will prices reflect the true opportunity costs. The ease in which Robbins swings between the two is more than a proof, in my view, that he was less convinced of their incompatibility than most Austrian writers. But more to the point, he could not have been more explicit about the fact that proper cost minimisation cannot be achieved outside of the Walrasian equilibrium.

Moreover, Kirzner (1999), in claiming Wicksteed-and by implication Robbins-to the Austrian says that: "Here we see Wicksteed, in an Austrian fashion, seeing the decisions of market participants not as the implications of equilibrium conditions somehow assumed already to exist, but as the initiating cause for (and stages) the process of equilibrium itself". (Kirzner 1999, p. 111). Namely, that which distinguishes the Austrian story from the Walrasian one is that in the Austrian approach, equilibrium is not determined by a-priori exogenous conditions but rather by the process in the market place. However, there is nothing clearer than Robbins's own statement that "[w]e enquire rather concerning the conditions of equilibrium. . .*given certain data*" (Robbins 1935, p. 67 *my italic*). And: "Now, of course, it is of the essence of the conception of equilibrium that, *given his initial resources*,

[29]In logical terms.

each individual secures a range of free choice, bounded only by the limitations of the material environment and the exercise of similar freedom on the part of the other economic subjects" (p. 143, my italic).[30] By no stretch of imagination can this be interpreted as an Austrian narrative.

It seems to me quite obvious that Robbins is describing a Walrasian equilibrium where equilibrium prices depend on exogenous variables like taste and initial distribution of resources. He then goes on to say that society may choose not to allow such freedom to individuals (a different end). But the role of the economist is not to judge society's choice but merely to say that – by referring to the potential competitive free equilibrium as the benchmark – the actual allocation does not satisfy the cost minimisation principle (that a policy is or is not "economical"). In other words, Robbins thinks that economists must always refer to the benchmark of efficiency (which solves the implicit problem of plentiful of means) to evaluate any existing equilibrium. However, by saying this, Robbins is contradicting the most important edifice of his structure: that the means-ends examination is value free.

Nevertheless, without the problem we mentioned earlier regarding the change of axioms, the Robbins story could have sat well with the Keynesian one at least on the matter of equilibrium. If we accept the superficial reading of Keynes according to which he was comparing a state of quasi-equilibrium[31] (with unemployment) with a state of equilibrium, then his theory would have fitted the Robbinsian criteria. The benchmark of efficiency is there and the economy is in the process of movement around that point of equilibrium. Exactly in the same way as in Robbins, convergence may not happen (yet equilibrium will influence the movement of economic variables and would serve as the base from which opportunity costs are extracted), in Keynes, we may get stuck in a quasi-equilibrium point, or points.

However, this line of reasoning would fail if we accept that the changes in the axioms of the classical school mean the absence of any equilibrium.[32] In such a case, economics could no longer be ethically neutral and we would have returned to square one by asking the question, is unemployment the greatest social concern?

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[30]Robbins makes it even clearer when he states that from the fact that people rank their preferences (taste), we can derive equilibrium: "From this elementary *fact of experience* we can derive the idea of the substitutability of different goods, of the demand for one good in terms of another, of an *equilibrium distribution of goods* between different uses, of *equilibrium of exchange* and of the formation of prices" (emphasis added p. 75).

[31]By quasi-equilibrium I wish to circumvent the important question of whether the state of unemployment is an equilibrium or disequilibrium. I believe that Keynes was confused about this matter as he clearly thought that there is an efficiency equilibrium in the unemployment case concerning all those who are employed.

[32]It is important to note that the absence of equilibrium to which I refer here is not the case of disequilibrium analysis as conducted by Solow and Stiglitz (1968), Barro and Grossman (1971) and Benassy (1986). This approach was duly criticised by Leijonhufvud (1968) is being merely a Walrasian equilibrium with constraints. Disequilibrium always implies the logical possibility of an equilibrium even if it would never materialise. The removal of these axioms from across all markets may mean that equilibrium is the wrong reference point. As such, it would logically undermine the entire Keynesian project.

Keynes and Real Classical Economics: From Exogenous to Endogenous Government and from Involuntary Unemployment to Involuntary Employment

Thus far we examined how Keynes's economics would fit Robbins's conception of that which defines the subject matter and methods of the discipline. We noted that there are difficulties in the compatibility of Keynes's theory with Robbins's tenets on almost all major accounts: the problem of the neutrality of the end; the problem with the familiarity of the postulates and finally, the problem with equilibrium.

However, we can somewhat reverse the perspective here and argue that Keynes's difficulties in fitting Robbins's framework reflect a genuine concern about the way in which economics had been done since the 1980s of the nineteenth century and as such, serve as a rebuke of Robbins's and the neo-classical approach. If this was his intention, it is very difficult to learn it from the GT.

To reduce the complexities of the issues, it is possible, in my view, to identify two key elements which lie at the heart of Keynes's complaint:

(a) The existence of two allocations, both of which are competitive a la "classical" school and allocate efficiently among those who participate but one of them has less participants.
(b) People act on the basis of expectations but there is no ergodicity.

The first one is quite obvious and suggests the need for government intervention to move from one allocation to the other. As in both cases the mechanism of allocation is considered by Keynes as appropriate, government's intervention must remain *exogenous* so that it does not interfere with the working of the markets. The second key element is a bit more complex. In the most general terms it suggests that there may not be equilibrium in *any* market as expectations would normally not match and people would consistently make mistakes. This should hold both in the short and in the long run. We can add to this the other two axioms which Keynes implicitly removes (neutrality of money and gross substitutability) and this could bring down the axe on any co-ordinated outcomes as we know them.

It is clear that Keynes was concerned with the first element but it is not entirely clear how to deal with the second one which, in my view, is the source of all the confusion regarding his contribution. In a narrow interpretation we can suppose that the abolition of the three axioms is only relevant to the labour and money markets. This would explain Keynes repeated claims about the relevance of "classical" economics for those who participate in the economic system together with the possibility of involuntary unemployment.

However, from a logical perspective this is a complete travesty. Axioms are universal and one would need further convincing why the question of expectations, ergodicity and the absent of gross substitutability is only relevant to some markets and not to all of them. Had Keynes taken his own criticism seriously, he would have needed to develop a whole new theory of markets and exchange and a replacement

for the concept of equilibrium. Keynes has not done this and in this respect, has therefore failed to act on his concerns.

It is clear that the difficulties which element (b) highlights are about the existence of equilibrium as an argument against the possibility of involuntary unemployment. But by removing key axioms of modern ("classical") economics he was compelled to throw the baby with the bathwater. In essence, Keynes only wanted to argue that *the co-ordinated outcome of decentralised decision making may sometime produce outcomes which are socially not desirable and we therefore would need government interference to correct them.*

But to make such a claim Keynes need not have disturbed the idea of equilibrium. Instead, he could have simply followed the real classical economists like Smith and Mill who have always argued that the competitive co-ordinated outcome may be harmful to society. In other words, Keynes needed to distinguish between a "good" and a "bad" equilibrium. In neo-classical economics, unobstructed competitive equilibria[33] are always consistent with whatever it is that society desires. Therefore, Keynes needed to partially break the concept of equilibrium to allow the two allocations to emerge and for government to help in rectifying the situation. However, as I have argued above, this is logically almost impossible.

Lessons from Mill

Laidler (1999, 2006) has already identified ideas in Keynes which were not really new and in particular with respect to Say's Law and the role of money in exchange. Indeed, in J.S. Mill we can find a clear statement which very much resembles the problems which bothered Keynes. To being with, in as much as the aggregate demand – as a determinant of employment – is concerned, it is interesting to note that Mill's (and Smith's) wage-fund doctrine is a very similar construction. It is true that many confuse (including, occasionally, Mill himself) the wages-fund with capital and therefore, combine employment with savings. However, this is not the real case as the wages-fund includes the demand for all types of labour productive or otherwise. The demand for unproductive labour does not come out of savings and therefore, the overall demand for labour cannot be affected just by savings:

> "To this, however, must be added all funds which, without forming a part of capital, are paid in exchange for labour, such as the wages of soldiers, domestic servants, and all other unproductive labourers. There is unfortunately no mode of expressing by one familiar term, the *aggregate* of what has been called the wages-fund of a country". (Mill 1909, pp. 343–344 emphasis added)

[33]In the absence of incompleteness the first and second welfare theorems hold. The idea of incompleteness would not have helped Keynes much as in such a case, government could not have taken us from a "bad" equilibrium to a "good" one but rather from one "bad" equilibrium to another.

So Mill was clearly searching for the equivalent of Keynes's aggregate demand and like Keynes he did not believe savings to be the only thing which would increase the demand for labour. It is true that Mill then suggests that the demand for productive labour is the greater part of the whole and thus, he felt that equating the wages-fund with capital was fine as long as one bore in mind that it is not just capital. It seems that he himself forgot about this when he talks about savings and employment.

But not only that, Mill was also aware of the difficulties of adjustments. When he argues that wages are determined by the ratio between the wages-fund (aggregate demand) and the number of workers seeking employment, he qualifies this statement:

> "The demand for labour in any *particular* employment is more pressing and a higher wages are paid, when there is a brisk demand for the commodity produced; and the contrary when there is what is called stagnation; the workpeople are dismissed and those who are retained must submit to a reduction in wages; those in these cases there is neither more nor less capital than before". (Mill 1909, p. 344 emphasis added)

Mill is saying here two things which are relevant to Keynes. Firstly, he recognises that even for a given stock of capital (aggregate demand), there may be some conditions where the individual components of the aggregate demand would be different for the same whole. The demand for one commodity may slack. "A manufacturer" write Mill, "finding a slack demand for his commodity, forbears to employ labourers in increasing a stock which he finds it difficult to dispose of" (Mill 1909, p. 344). This means that for any given wages-fund, it is still possible that the demand for some commodities would fall. This, he believes would lead to a fall in demand for labour and the dismissal of workers while those who remain will experience a decline in wages. Notice here that labour is dismissed *and* wages are falling yet labour is not re-instated. Its re-allocation is not the mechanism of adjustment.

The reason for this is that Mill recognises the specificity of the labour supply and what he talks about is the labour in a specific occupation which would become unemployed even though there may not be any changes in the average wages which would reflect the ratio between the total wages-fund and the total supply of labour.

Indeed, Mill recognises that this would not be a long run situation as the capital made idle in the declining industry would be employed in a rising industry and will thus, increase the wages of this *particular* occupation. However, note that this does not mean that employment would be restored only that the average wages would return to its original level. Hence, Mill was well aware of the possibility of involuntary unemployment except that in his case, this is not a failure of the level of aggregate demand but rather of its composition.

It is interesting, and curious, to note that Marshall too discussed the specificity of labour in a very similar way and notes that "[t]he vertical movement from one grade to another is seldom very rapid or on a very large scale" (Marshall 1952, 217). However, Mill is far nearer to Keynes by saying that the adjustment mechanism would be the movement of capital rather than labour while in Marshall, he does

believe that in the long run, the shift across grades of occupation would be sufficient to plug gaps in the disaggregated labour market.

But to a great extent, this is not really the issue. For Keynes and neo-classical economics (and also embedded in Robbins), the economic problem is that of output maximisation. Hence, involuntary unemployment which reduced output is a major source of failure. For classical economics, where the problem was that of growth maximisation, a decline in the labour force is the equivalent methodological infliction to that of involuntary unemployment.

For Mill, this was an endemic problem of the competitive system. "In this country" he writes about England, "there are few kinds of labour of which the remuneration would not be lower than it is, if the employer took the full advantage of competition" (PPE 343). In other words, if competition ruled supreme, the wages would have been so low that even employers feel that they cannot be so mean and deprive workers from what may be considered as a "reasonable wage".[34] This means that competitive wages would be below subsistence and while in the long run this may raise the average wages due to a fall in workers' number, it is an immoral position to take with regard to the current labour force.

For Mill, competitive equilibrium, if left to itself, is a social evil for parallel reasons which prompt Keynes to write his treaties. It arises from concerns for the unemployed (in Mill's case, to those who earn wages below subsistence) and the fairness of the distribution of income. He writes:

> "If the choice were to be made between Communism with all its chances, and the present [1852] state of society with all its suffering and injustices; if the institution of private property necessarily carried with it as a consequence, that the produce of labour should be apportioned as we now see it, almost in an inverse ratio to the labour -the largest portion to those who have never worked at all, the next largest to whose work is nominal, and so in a descending scale . . .;if this or Communism were the alternative, all the difficulties, great or small, of Communism would be as dust in the balance". (Mill 1909, p. 208)

Obviously, that which concerns Mill in the working of competition in a world of private property is the fact that people are not remunerated in relation to their contribution. The reason why this is sufficient to make him discard of this form of organisation is that it violates the freedom of individuals. Not to be able to enjoy the fruits of one's labour, or faculties, is an impediment to the development of individuality.[35] It is almost the same as current justifications of inequality, which

[34]While it is true that Mill believes that workers are partly to blame for the competitive low wages (due to their number) he distinguishes between the question of population which has to do with future workforce and people who are currently in the labour market. Here, he puts the blame on the capitalists who do not devote sufficient sections of their surplus to hire productive labour. This may sound as a call for savings (increasing the capital) but we must view this in terms of the problem. While any increase in the use of surplus would increase demand for labour, the need to increase the productive employment is associated with increasing future surpluses and is therefore associated with growth rather than output maximisation.

[35]See a full discussion in Witztum (2005).

claim that not to allow entrepreneurs to enjoy the benefits of their innovations will stifle innovation.

The question, which immediately arises, is whether this is due to the unfortunate distribution of property or that it is inherent to such a system. Allow me to quote at some length:

> "That all should indeed start on perfectly equal terms is inconsistent with any law of private property: but if as much pains as has been taken to aggravate the inequality of chances arising from the natural working of the principle, had been taken to temper that inequality by every means not subversive of the principle itself; if the tendency of legislation had been to favour the diffusion, instead of the concentration of wealth – . . .; the principle of individual property would have been found to have no necessary connexion with physical and social evils. . .". (Mill 1909, p. 209)

In simple terms this means that the only way for private property and competition to be consistent with the principle of justice is through active interference in the natural distribution of wealth and income to which it gives rise. An interference, which will not only be concerned with the re-distribution of wealth bur first, and foremost, be concerned with the distribution of income. "To judge of the final destination of the institution of property, we must suppose everything rectified which causes the institution to work in a manner opposed to that equitable principle, of proportion between remuneration and exertion. . ." (Mill 1909, p. 209).

So in the case of Mill, the "bad" equilibrium is equilibrium of "laissez-faire". It is equilibrium without government. In such equilibrium, not only is reward inversely correlated with effort but wages would normally go down to below subsistence levels. The reasons why this equilibrium is "bad" are both equity and efficiency. The possible dwindling of the labour force due to wages below subsistence means lower growth. But low wages (as the ultimate notion of inverse relationship between effort and reward) also mean the lack of personal development. This, according to Mill prevents people from developing their individuality and thus, prevents them from becoming really free.

The "good" equilibrium would be the one where government is active in the system. But this activity does not come from manipulating demand (either directly or through monetary policy) but rather through active involvement in the distribution of ownership and income, and the provision of education which is not provided by any market mechanism.[36]

In spite of Keynes's initial claim that he is concerned with the "arbitrary and inequitable distribution of wealth and income" the only thing he can say about taxation is that his "new" theory would allow the government to use taxes without worrying too much about the effect it may have on savings (GT 372). In other

[36]Mill argues that there can be no "consumer sovereignty" when it comes to education and by implication, he would have opposed to any market driven provision of it. For more details, see Witztum (2005).

words, unlike Mill, Keynes's government is very much outside the system while Mill's government has to be more active to ensure that the "good" equilibrium emerges.

Lessons from Smith

But while Mill was well known for his "socialist" inclinations, no one can blame Adam Smith for such leanings. Yet, even the "father" of Anglo-Saxon "laissez-faire" seemed to have come up with the two types of equilibrium and he too, considered government involvements as crucial.

In Smith's case we have an economy where people exchange surpluses. The collective surplus of the economy is what Mill calls the wages-fund which, as we noted earlier, really corresponds to Keynes's aggregate demand. However, Smith does not take an aggregate perspective. For him, there are three possible outcomes for competitive interactions: growth, stagnation or decline.[37] In terms of the methodological equivalent to involuntary unemployment, clearly decline is the case where the equilibrium fails. That which identifies the symptoms of either state is the reward for labour. "The liberal reward of labour" writes Smith, "as it is the necessary effect, so it is the natural symptom of increasing national wealth" (Smith 1976, p. 91). Also note, "[n]o society can surely be flourishing and happy, of which the far greater part of the members are poor and miserable. It is but equity, besides, that they who feed, cloath and lodge the whole body of people, should have a share of the produce of their own labour as to be themselves tolerably well fed, cloathed and lodged" (Smith 1976, p. 96).

But what is it that determines these outcomes? According to Smith there are two main usages of the surplus, the demand for productive and unproductive labour. As all industries are dependent upon each other[38] there is a need to co-ordinate the demand for productive labour. It is easy to show that all three possible states can emerge as an equilibrium outcome.[39] Both the stationary and the declining cases can be seen as the methodological equivalents to the Keynesian problem. Here, in all three cases the competitive allocation works. But in the stationary and declining cases it means that wages are at subsistence level or below which means that the labour force will not grow and the economic problem would not be resolved.

This, of course, leads Smith to expect governments to interfere. But the kind of interference which he has in mind is far more focused on "endogenous" than "exogenous" activities. The mere division of book 5 of the *Wealth of Nations* between those duties of the sovereign which are more or less "exogenous" (defence and justice) and those which are endogenous suggest the importance of the latter.

[37]"The progressive state is in reality the cheerful and hearty state to all different orders of society. The stationary is dull; the declining, melancholy" (Smith 99).

[38]See a lengthy discussion in (Smith 378).

[39]For a full exposition of the argument, see Witztum (2009a).

Only 34 pages are devoted to the "exogenous" rules while 90 are devoted to the "endogenous" ones. These "endogenous" duties begin with general requirements of directly building and maintaining infrastructures, continues with the need to provide support for special industries (like, for instance, to help facilitate trade when there is a greater risk involved but there are social benefits from having the trade[40]) and ending with education. In a famous passage Smith decries the terrible effects of excessive division of labour which is the engine of growth in commercial societies:

> "The man whose whole life is spent in performing a few simple operations, of which the effects too are, perhaps, always the same. . .has no occasion to exert his understanding, or to exercise his invention. . . [he] generally becomes as stupid and ignorant as it is possible for a human creature to become. . .His dexterity at his own particular trade seems, in this manner, to be acquired at the expense of his intellectual, social, and martial virtues. But in every improved and civilised society this is the sate into which the labouring poor, that is, the grater body of the people, must necessarily fall, *unless government takes some pains to prevent it*". (Smith 1976, p. 782)

Very much like Mill we see an emphasis on the provision of education which is non-market driven as well as regulation to ensure that people do not end up in this de-humanising state to which commercial society drives its agents.[41] These trends which Smith identifies have much greater significant to growth than Smith is willing to explicitly admit. As knowledge is an important element in innovation, Smith argues that in early stages, each individual knew a lot but collectively there was not as much knowledge as there is in commercial societies. However, in the latter, each individual – due to extreme specialisation – knows relatively little and innovation, then depends on those who specialise in taking an overview of knowledge. This means that there must be some form of mechanism to allow those who specialise in knowledge to transmit it to specific applications. It is not entirely clear that there is an obvious *market mechanism* for it. As such, again like in Keynes, society needs to move from one equilibrium to another but this can only be done by government's direct actions.

Involuntary Employment

So within the classical school we see that competitive equilibrium is not a promise for resolving the economic problem. This means that we distinguish between those equilibria which solve the problem from those which do not. However, this is not an invitation for a Deus Ex Machina in the shape of the government to resolve the problem. Instead, government should become involved in the system to help achieve the desirable outcome.

[40]See a discussion in Witztum (2009b).

[41]Another excellent example to Smith's clear preferences to state action as opposed to spontaneous provision is the case of the debate about Standing armies versus Militia. See a discussion in Montes (2009).

However, that which created so many difficulties with Keynes misreading of these possibilities, is his concern about the presence of involuntary unemployment. I have argued that such a situation may also arise in Mill's disaggregated view of the labour market and in this sense, there is no novelty in Keynes claim. In Mill, this was not in itself the cause for government intervention but as such a situation would reflect a morally repugnant distribution of income, it is inevitably associated with the "bad" equilibrium.

But while for Keynes, only the presence of involuntary unemployment was a cause for concern, for classical economists like Smith and Mill there was a greater concern which is, what I would call *involuntary employment*.

We already saw what Smith thought that it meant for people to live in a society with an extended division of labour. Having a job while suffering complete degradation of one's humanity, seems to suggest that people are employed under duress. Given that they have no other means of accessing the social output, they work because they have to and not because they want.

Mill too felt the same:

> "If the bulk of the human race are always to remain as at present, slaves to toil in which they *have* no interest, and therefore, *feel* no interest – drudging from early morning till late at night for bare necessaries, and with all the intellectual and moral deficiencies which that implies – without resources either in mind or feelings – untaught, for they cannot be better taught than fed; selfish, for all their thoughts are acquired from themselves; without interests or sentiments as citizens and members of society, and with a sense of injustice rankling in their minds, equally for what they have not , and for what others have. . ." (Mill 1909, p. 373)

So the real problem of commercial societies is not so much the unemployed as it is the employed. Both Smith and Mill felt that individuals should earn more than their subsistence as growth requires this as well as the development of their human capacity. Mill thought that in the process they will become co-operative and realise the folly of material wealth which would remove the need to grow but as long as competition rules, growth is of the essence. So the "bad" equilibrium was characterised by its demeaning effects on the working population.

By focusing the concern on the unemployed while assuming the working of competition for the employed, Keynes has drastically re-directed the focus of classical attention. In his defence one should hasten to say that the onslaught of neo-classical economics is the real cause in the shift of attention which, together with Robbins's demand for neutrality, has turned economics into a fanciful detached and useless exercise. But shifting the spot light to the wrong place is not necessarily the kind of remedy society needed.

The problem is far more complex when one considers the fundamental flaws of the modern narrative. If we begin our story by a society comprised of individuals who are fending for themselves in an autarkic manner, we would find that the idea of specialisation and trade suggests that people take a risk in so doing. Modern economics claims that there is no risk as everyone benefits from specialisation and trade. However, the reason why this is true is simply because there is an embedded assumption that individuals can say no. Namely, if market prices are such that it

would not be one's interest to abandon autarky, one could always choose to stay where they are. Of course, in the sterile world of identical agents this would not happen but it could easily happen in a society comprise of heterogeneous agents.

If the option of autarky is no longer available, individuals are not really in the position to say no to what the market offers. Therefore, when they move, as Marshall puts it, from one grade to another, they may be moving from an occupation which they like to one which they do not like. Should people spend most of their lives doing something which is dictated by markets and is incompatible with their wants? Can we really be sure that in a heterogeneous society, everyone really benefits from specialisation and trade?

In this respect, the question of involuntary unemployment should have been contrasted with involuntary employment. If society could swap those who wish to work in a particular profession with those they do not, everyone would benefit. However, for this to happen, society must ensure that people have a choice as far as labour is concerned. This, I am afraid, is the single most important question which Keynes agenda has removed from social consciousness and which, to my mind, is the most important one.

Conclusions

We started our investigation by examining Keynes's theory in the light of the Robbins's *Essay on the Nature and Significance of Economic Science* and we found that on all major accounts there are difficulties in including Keynes in Robbins's scientific project.

We found that there are difficulties in the ethical neutrality of Keynes's agenda; in his approach to postulates as well as in the way he treats equilibrium. However, the comparison to Robbins's methodology was useful as it helped to identify the historical position of Keynes as very much part of the neo-classical agenda which Robbins was trying to scientifically cleanse.

From a methodological perspective, the debate about Keynes seems to have been stretching from the one end where Keynes seemed to have argued the complete abolition of equilibrium analysis to the other where Keynes as viewed as merely tweaking with the margins of neo-classical economic analysis. It is quite clear that Keynes did not mean the former extreme as he has not really offered any insight into the significance of those axioms he attributed to individuals and which would destroy equilibrium analysis. Moreover, as he was discussing aggregates, it is not entirely clear how his attributes to individuals translate into an aggregate. On the other hand, the tweaking of the equilibrium system creates a logical inconsistency which is not very easy to bridge.

As the comparison with Robbins alerted us to the possibility that Keynes's agenda is more in line with Marshall and modern economics than he would have been willing to admit, we note the transition from classical to neo-classical as the cause of some of the confusion regarding Keynes's work.

While we found evidence that in Mill the concept of aggregate demand driven unemployment exists, it became apparent that the cause of unemployment was not the level of the demand but its internal composition. Moreover, unemployment would not have been resolved by the lowering of wages as the adjustment mechanism would only restore the average wages which could leave a labour market with some occupations fully employed and highly paid and others, underemployed and underpaid.

However, we have shown that the real difficulty of the labour market in the eyes of classical economists like Mill and Smith is not necessarily the unemployed as it is the employed. Given the difference between the classical and neo-classical economic problem, there is a methodological equivalence between wages below subsistence in the classical school and unemployment in the neo-classical one. Hence, it was clear that classical economists recognised the fact that competitive markets may fail to solve the economic problem; something which has, at the time, alluded neo-classical economics.

As a result, we identified the presence of a "bad" and "good" equilibrium. This is something which Robbins would have never accepted as for him, ethics was outside economics. Nevertheless, the distinction seemed relevant and significant in allowing a different interpretation of the Keynesian story.

In modern economics there may be many equilibria which are efficient and the role of government, from outside the system, is to move the economy from one place to another without interfering with the working of the markets. For classical economists, the move from a "bad" to a "good" equilibrium requires a much more involved government. We distinguish between the two by calling the former "exogenous" government which the latter, "endogenous".

Moreover, as the nature of "bad" equilibrium highlighted the plight of those who are employed it meant the problem of involuntary employment seems more pressing from that of the involuntary unemployment. Clearly, to resolve these issues there is a greater need for an "endogenous" government. To this effect, Keynes's contribution has deflected attention from those important insight emanating from the real classical economics by focusing both on the unemployed and the exogenous government. Robbins's implicit denial of Keynes's place in the science of economics was, in the end, unjustified.

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Keynes According to Robbins: A Comment on Witztum

Mauro Boianovsky

The differences between Lionel Robbins's and John Maynard Keynes's respective approaches to economic theory and methodology have been occasionally mentioned in the literature (see e.g. Gruchy 1949; Wright 1989; Davis 1991), but an in depth discussion is still wanting. Amos Witztum's critical assessment of Keynes's 1936 *General Theory* in the light of Robbins's 1932 *Essay on the Nature and Significance of Economic Science* provides an opportunity to tackle the issue upfront. However, by attempting to reconstruct what Robbins would have thought about the scientific status of the *General Theory*, Witztum largely misses the opportunity. In particular, the conclusion that Robbins "implicitly denied Keynes's place in the science of economics" is not warranted by Robbins's *explicit* acknowledgement of the relevance of the *General Theory*, especially after he gave up the Austrian business cycle framework – which had informed his 1934 book on the *Great Depression* – and endorsed several (although not all) tenets of Keynesian employment policy (see Robbins 1947, 1971).

Despite some important methodological differences, Robbins and Maynard Keynes belonged to what Mark Blaug (1980) has aptly called the long "verificationist" tradition in economic method, which was started by J.S. Mill in the 1830s and prevailed until Terence Hutchison (1938) and especially Milton Friedman (1953) brought in Popperian falsificationism to the economic realm. For both Robbins and Keynes, data could and should be used to test whether a theory was applicable to a particular situation, but not to decide the overall validity of the theory itself. Significantly, Robbins (1938, p. 347) quoted a passage from volume 2 of Keynes's 1930 *Treatise on Money* to support his own view about the key role of empirical verification in economics. And defended Keynes from the charge (presumably made by Hutchison 1938) of neglecting economic data: "it is curious that the author of this passage and the distinguished contribution to the applied theory of money in which it occurs, should have been recently been singled out for public censure for 'unscientific' methods of procedure". The two economists parted company when it came to the relation between the science of economics and the objectives of economic policy, which Robbins regarded as strictly separated. True enough, Keynes did not discuss Robbins's methodology in the *General Theory* – as one might expect, he did refer to Robbins's (1934) instead. However, in an oft-quoted letter he wrote Roy Harrod in 1938 about Harrod's presidential address published that same year, Keynes claimed that "against Robbins, economics is essentially a moral science and not a natural science. That is to say, it employs introspection and judgments of value" (Keynes 1973, p. 297).

Keynes's view of economics not as pure science, but as an applied, moral science (see also p. xxii of the preface to the *General Theory*) reflected his Cambridge Marshallian background (Gruchy 1949; Colander 2009), against which

Robbins (1932) put forward his alternative definition of the subject matter of economics.[42] Indeed, as pointed out by Richard Wright (1989), the circumstances surrounding Robbins's participation in the Committee of Economists – set up by Keynes in 1930 to produce a diagnosis and remedies to the economic depression – played an important role in prompting him to write his 1932 and 1934 books. Robbins, who at the time disagreed with Keynes's diagnosis and solutions, was unwilling to be bowled over by "the complexity and great aesthetic beauty of the theorems propounded by Mr. Keynes" (Howson and Winch 1977, p. 60, quoting from Robbins's answer to a questionnaire drafted by Keynes). In particular, Robbins was critical of the kind of interplay between theory and policy argued by Keynes, which played a key role in what Hugh Dalton (1986) called Robbins's "feud" with Keynes throughout the 1930s. From that perspective, it is hardly surprising that the *General Theory* does not pass the Robbinsian 1932 test as a scientific contribution to economics, as much as a substantial part of modern welfare economics for that matter (see Atkinson 2009).

However, as it is well-known (Robbins 1971, pp. 150–156; O'Brien 1988, pp. 115–117), although Robbins remained faithful to his 1932 methodological principles, he would recant his 1934 Austrian-like business cycle theory and its policy implications. He would regard "this aspect of my dispute with Keynes as the greatest mistake of my professional career, and the book, *The Great Depression* . . . as something which I would willingly see forgotten" (Robbins 1971, p. 154).[43] Robbins's change of mind is reflected in his support and active participation in the elaboration of the famous White Paper on Employment Policy and in the Bretton Woods agreement during the Second World War, when he worked alongside Keynes in the Economic Section of the Treasurer. As pointed out by Robert Skidlesky (2003, p. 651), the "Keynes-Robbins axis" became crucial for the success of domestic and foreign British economic policy in the years 1943–1945 (see also Robbins 1971, Chapter 9). Although Robbins did not fully convert into Keynesianism – he was very close to Dennis Robertson and accepted many of Robertson's critical points about Keynes's framework (see Boianovsky and Presley 2009) – he did eventually take in the notion that fluctuations of aggregate demand

[42]As suggested by Robbins (1932, p. 15), his definition of economics - as "the science which studies human behavior as a relation between ends and scarce means which have alternative uses" - was influenced by continental economic thought. Wicksell was a likely source: "By an *economic* phenomenon or activity is meant every systematic endeavor to satisfy a material need, or, more precisely, one which seeks with the available means to achieve the greatest possible result, or a given result with the least possible means" (Wicksell ([1901] 1934, p. 2). Robbins was familiar with Wicksell's writings and was responsible for the English translation of the *Lectures*, for which he wrote an introduction.

[43]While reviewing Harrod's biography of Keynes, Robbins (1951) acknowledged the unique impact of the *General Theory* on economic theory and policy. In his article on Malthus, Robbins (1967, p. 260) observed that Keynes, like Malthus before him, rebelled against the interpretation of economic depressions in terms of theories which "omitted ingredients essential to a reflection of contemporary reality".

"must not be left to look after themselves" and that it is an "important function of government" to pay attention to such matters (Robbins 1971, p. 188).

A few years upon his return to the LSE after the Second World War, Robbins (1952) became engaged in the production of one of his most important investigations into the history of thought. His book about the theory of economic policy in classical economics cleared several issues about the economic role of government that had not been discussed in any detail in his 1932 *Essay*. In view of Witztum's contrast between the Keynesian and the classical respective treatments of economic policy, it is worth noting that Robbins (1952, pp. 37–38) stressed the similarities between Maynard Keynes's ([1926] 1972) and Adam Smith's formulations of the economic role of the state. Notwithstanding the distinct contents of Keynes's later agenda (e.g. control of aggregate investment) as compared to Smith's, Robbins called attention to their "*formal* similarity", which he did not regard an accident but an indication of the essential "continuity of thought in the tradition of economic liberalism concerning the positive nature of the co-operation between the state and the individual". In his LSE lectures delivered in 1979–1980 Robbins wondered whether Keynes had involuntarily appropriated Smith's definition of the functions of the state, given that the wording was almost the same (Robbins 1998, p. 153).

Witztum's suggested distinction between (classical) "endogenous" and (Keynesian) "exogenous" role of the state may be found already under another guise in Richard Musgrave's (1959) well-known classification of the allocative, distributive and macroeconomic stabilization branches of the government, which are not opposed to one another. In the same vein, the notion of "involuntary employment" and its perverse effects on labor supply and growth is not necessarily incompatible with Keynes's concept of involuntary unemployment, as illustrated by Barro and Grossman's (1971, pp. 90–91) treatment of the determination of output and employment under conditions of excess demand for labor and goods, which they distinguished from involuntary unemployment associated with excess supply. Hence, the supposed unscientific character of the *General Theory* remains unproven.

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Johnson's Conversion from Keynesianism at Chicago

Russell S. Boyer

Abstract "Johnson's Conversion from Keynesianism at Chicago," Russell S. Boyer.

Johnson arrived at Chicago in 1959 identifying himself as a Keynesian, but during his period there he began to speak in derogatory terms about Keynes and about Keynesians. This paper analyzes the role that Friedman and Mundell played in this conversion. Our argument is that Johnson moved towards "the monetarist position," but he denied that this was due to Friedman's influence. Mundell's thinking followed a similar path, both in his economics and in his assertion of independence from their distinguished colleague. The effect of these claims is to hide the fact that Friedman's impact on their work was much greater than has generally been recognized.

Introduction

Harry Johnson's thinking concerning macroeconomic topics evolved markedly during the years 1959–1977, while he was at the University of Chicago. His move from Keynesianism towards "the monetarist position"[1] over this period has

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[1]Johnson (1972a, 5, 7) defined "the monetarist position" as encompassing the general conclusions at which one arrives by applying ". . . commonplace . . . monetary theory . . .." This reasonable set of precepts is distinguished from ". . . monetarism . . ., the particular formulation of the monetarist position embodied in the past works of Milton Friedman. . ." (Johnson 1972a, 66). As we show below, monetarism was rejected repeatedly, and in ever more strident terms, as Johnson reworked this material over a five-year period. He felt that monetarism was on the decline, whereas monetarist research by Mundell, although controversial, held out substantial promise (Johnson

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been noted by numerous observers, including such well-known economists as Bhagwati (1977) and Polak (2002).

What has not received sufficient attention is Johnson's (1977) claim that this evolution was independent of any influence from his distinguished colleague, Milton Friedman. Such a claim, although supported explicitly by Laidler (1984) and in a nuanced way by Leeson (2000) and Tobin (1979), runs counter to conventional thinking, because the usual view is that it was precisely such influence which caused Johnson dramatically to switch his allegiance between these rival camps in macroeconomic thought.

We argue here that the impact of Friedman's thinking on the work of both Johnson and Mundell was far greater than has generally been recognized. The basic themes of both the Optimum Currency Areas Argument, and the Monetary Approach to the Balance of Payments can be found in Friedman's celebrated essay, "The Case for Flexible Exchange Rates". (Friedman 1953) Since neither Johnson nor Mundell has acknowledged Friedman's contributions in these areas, his influence on their work has not received much attention. Given the extreme assumptions which typically go into the research which falls into these categories (zero capital mobility, purchasing power parity, full employment, and no sterilization), these rubrics, rather than being progressive research agendas,[2] are more usefully seen as having the effect of denying Friedman the credit that he is due for these ideas.

The Johnson–Mundell view is that in the aftermath of the Keynesian revolution, they restored monetary elements to a prominent position in open economy macro analysis.[3] The bulk of their work was done at the pre-eminent center for research in money and banking at the time, the Department of Economics at the University of Chicago, which was dominated by the world's foremost monetarist, Friedman. To claim, as Johnson (1977) and Mundell (1967b) do, that this work was carried out without influence from their distinguished colleague, is problematic, and thus the focus of the present paper.

Johnson's Transition from Keynesianism to the Monetarist Position

Johnson arrived at the University of Chicago in 1959 as part of the "minority group," a set of members of the Department which acted as a counterweight to Friedman's dominant position in it. Johnson's strongly-held doctrinal preferences at that time have been remarked on by commentators of all ideological stripes,

1972b, 336). Laidler's (1984, 603) claim that Johnson at times labeled himself "a 'monetarist'" appears to be of dubious validity.

[2]See Chipman (1980, 141–142) and Hahn (1977) for a similar point of view.

[3]See the narratives which appear in Frenkel and Johnson (1976) and in Obstfeld (2001).

including Johnson himself (as is attested by the typescript, titled "A Keynesian Looks at Chicago," (Johnson 1960)[4] of a talk delivered at a cocktail party thrown by the Political Economy Club at 8:30 pm, Friday, 28 October 1960, on the campus of the University of Chicago). So, for example, Bhagwati and Frenkel (1987, 357) note that Johnson was "... invited as a 'Keynesian' economist..." to take up the chair at the University of Chicago. This was an appointment which had been offered to (and briefly accepted, before being rejected, by) Samuelson, a move which aroused strong animosity from Friedman and Stigler (Hammond and Hammond 2005, 46).

It appears that in Chicago's hiring at the time, personality characteristics played as big a role as policy adherences. The appointment rejected by Samuelson was accepted by the next economist to whom it was offered: Metzler. Despite the fact that he was just as staunch a Keynesian as was Samuelson, his hiring met little resistance. Tragically Metzler's full productivity was short-lived as an operation on a brain tumor in 1950 substantially diminished his mental capacity.

Johnson was hired in 1958, after Friedman campaigned for him starting in 1953. They had met first at Cambridge University in that year, and Johnson's warm welcome to Friedman contrasted with the frosty reception which everyone else extended to him.[5] Johnson and Friedman appear to have had cordial relations until at least 1957, when Friedman asked Johnson to contribute a book to the Cambridge Economic Handbooks series of which he was a co-editor (along with Claude Guillebaud). After this point, Harberger seems to have been Johnson's main contact and champion in the Department, and relations with Friedman became more problematic.

Moggridge (2008, 138) argues that Johnson took his "... first large step away from orthodox Cambridge Keynesianism ..." (Laidler 1984, 598) while he was still at Cambridge, when he cited with approval some quantity theory results. Friedman's year-long visit to Cambridge (1953–1954) or the quarter which Johnson spent at Northwestern University (April–June 1955) would have been obvious occasions on which Friedman could have exerted influence on Johnson. For example, in the spring of 1955 Johnson attended workshops at the University of Chicago on Thursday evenings. Nonetheless, Johnson continued to identify himself as a Keynesian at least until October 1960.

Johnson appears to have delighted in attacking authority figures, as is exemplified by his scalding assessments of Meade's (1951) monumental books on international trade and policy (Johnson 1951). In writing these reviews as a 28-year old, Johnson had created quite a stir among the pillars of the establishment in the UK. Moggridge (2008, 139–145) deals with this episode and remarks both on the strains it caused there, and on the belated commentary which Mundell writes about it.[6]

[4]Roughly a quarter of this typescript is excerpted in Moggridge (2008, 205–207). These pages include, in addition, paraphrasing of the rest of the paper, consideration of its contents, as well as an analysis of the reactions of various members of the Department and the Business School to this episode.

[5]This discussion follows the coverage of these matters by Moggridge (2008, 123–125).

[6]Compare Mundell (1968a, 113) with the earlier version of this paper in Adler (1967), where the reviews of Meade's books are not mentioned.

After Johnson had accepted Chicago's offer, and before he had arrived on campus, he was ready to show Friedman that he would not defer to him. In an *Economic Journal* article, written jointly with Bhagwati, who claims that the relevant portion "was almost wholly Harry's contribution" (Bhagwati 1977, 224), Johnson criticized a proof by Egon Sohmen supporting a claim by Friedman about stability in the foreign exchange market. Harry writes ". . . these sentences illustrate the extent to which *non sequitur* and *ignoratio elenchi* can be accepted as valid argument if the conclusion purportedly reached is of sufficient propaganda value." (Bhagwati and Johnson 1960, 93)

Friedman responded by telling Johnson "that he ought to be thoroughly ashamed of himself and that he owes you [Sohmen] an apology . . ." (Friedman 1960a) Johnson had been well aware that his paper would cause a strong reaction. He had written to Harrod, concerning publication dates, that "I should be sorry to see it delayed as late as June 1960 for one particular reason which is that Bhagwati and I will both be at the University of Chicago for this coming year and the last part of the article is a pretty hard-hitting criticism of some strongly-held Chicago views. I should like the article to appear before he has left Chicago." (quoted in Moggridge 2008, 204).

Another opportunity for Johnson to criticize Friedman arose only 5 months later, when Johnson was asked to give a talk at a cocktail party thrown by the Political Economy Club at the University of Chicago, whose typescript is cited above. This document serves as an illustration both of the nature of the relationship between Johnson and Friedman at this point, as well as of Johnson's macroeconomic thinking. The presentation was viewed by the "minority group" (which included at the time Metzler, Downs, Johnson, and Harberger) as an attack on the current thinking among the majority of the members of the Department.[7] Therefore it was directed at Friedman and Stigler, and their followers. In contrast, the names mentioned in it are: Keynes, Veblen, Knight, Marx, Lange, and Metzler.

Further salient episodes in the relationship between Johnson and Friedman include the sniping portions of Johnson's (1965) review of Friedman and Schwartz (1963), and Johnson's Ely Lecture in December 1970 (Johnson 1971). In the latter publication Johnson questions the validity of Friedman's characterization of the development of monetary theory at the University of Chicago during the inter-war period. Specifically, Johnson reiterates many of the objections which Patinkin (1969) had raised about Friedman's narrative.[8] Friedman's first reaction to this lecture was that it was libelous.[9]

[7]See the letter from Downs in Johnson's Papers, Regenstein Library, University of Chicago (Box 13, Writing 1960, November 3, 1960).

[8]The relationship between Johnson's and Patinkin's work in this episode is transparent. Johnson is voicing, in stronger terms, the substantive points which were entirely due to Patinkin. We argue below that a similar relationship existed between Mundell and Johnson's work, but it is less apparent that Johnson is acting as spokesman for Mundell's somewhat derogatory view of Friedman's contributions. See Dornbusch (2001) for instances of such a view.

[9]See Moggridge (2008, 347).

These fragile personal relationships hide the fact that in his thinking Johnson was moving ever closer to Friedman's position. This was something that Friedman noticed, and about which he wrote "Harry was originally a very strong Keynesian who was converted to monetarism."[10] Thus in 1971 Johnson described the Monetary Approach to the Balance of Payments, for which he took substantial credit, as being a "monetarist approach;" that is, an application to open economy macro of insights derived from the monetarist position. By using the adjective "monetarist" Johnson is acknowledging what is quite apparent about this framework. Namely, that it is consistent with Friedman's thinking on macroeconomic topics. As becomes clear from Johnson's (1977) re-jigging of his nomenclature a few years later, he is treading very close to Friedman's turf, but is unwilling to acknowledge the debt that he owes to him.

Similarly, in a closed economy analysis, Johnson (1969a, 30) "covers much of the same ground" as Friedman's Optimum Quantity of Money Argument. Nonetheless, Johnson does not cite this essay in the body of his own paper because it ". . . was submitted for publication . . . [before he] . . . came across Milton Friedman's . . ." work. Since this paper (Friedman 1969) was available in manuscript form to graduate students in September 1967 (when it was used as a text for Friedman's teaching of Economics 330), it is quite surprising that Johnson had not become acquainted with it earlier.[11]

While the basic summary of the evolution of Johnson's thinking about macroeconomics during this period is quite simple and widely recognized, there are some dissenters. For example Tobin (1979, 454) says of these years that "The influence of Chicago on Johnson's thought is . . . hard to assess". More definite is Laidler's (1984, 593) assertion that "The evolution of Harry Johnson's macroeconomics had very little indeed to do with the fact that he held a chair at the University of Chicago". He comes to this conclusion looking at a narrow category of publications. This is in line with the commission which he had received from the *Journal of Political Economy*, which asked him to confine his attention to Johnson's closed economy research. Looking just at Johnson's published work, Laidler focuses on his views of the effectiveness of monetary policy, as captured by the Phillips Curve relationship. It is on the basis of this evidence that Laidler concludes that Johnson's macroeconomic thinking did not evolve due to influences at the University of Chicago.[12]

[10]Letter from Friedman (1995), dated April 19, quoted in Leeson (2000, 746).

[11]Hynes (2001, 629) appears to accept Johnson's claim without question.

[12]Johnson's contribution to the Monetary Approach to the Balance of Payments, "which he viewed as the crowning achievement of his career" (Bhagwati and Frenkel 1987, 355) is covered in less than half a page out of a total of 147 pages, which the *Journal of Political Economy* published in an issue memorializing Johnson in August 1984. In that issue there is no discussion of Johnson's research on open economy macro matters. Since this was the largest component of the research which he undertook while at the University of Chicago this omission is conspicuous.

Tobin (1979, 453) has noted that "... Harry had troubles when, as in monetary theory, his interests intersected Friedman's," but then goes on to say that "In international economics he was able to carve out a rewarding independent role". Specifically, "His 'monetary theory of the balance of payments' commanded his attention in his last years ... Harry regarded the new theory as his most important recent work ..." (Tobin 1979, 456).

The import of Laidler's interpretation is unmistakable. Contrary to the view of the economists quoted above, his claim is the Friedman had a negligible impact on Johnson's thinking in terms of monetary economics. This is a claim of which Johnson would have very much approved, because it suggests that he was doing independent work, even if much of it was "well known at Chicago." (Laidler 1984, 592) This is the interpretation which Johnson and Mundell put forth, with ever greater conviction.

Friedman's Work and His Interaction with Johnson and Mundell

Friedman "clearly identified the revival of interest in money with himself". (Leeson 2000, 747) This is, of course, the chief lesson that comes from the book he edited in 1956, *Studies in the Quantity Theory of Money* (Friedman 1956), which focuses on the macro behavior of closed economies. But his interest in the open economy, as well, is apparent from two of his major works, namely his celebrated essay (1953), and his *Monetary History* co-authored by Anna Schwartz (1963). These publications have a completely different view from Metzler (1948, 212), who describes the minor role assigned to monetary elements in balance of payments theories at the time, as the Keynesian Revolution reached the zenith of its influence.

Friedman (1953, 171) lays out in a logical order first the nature of the Humean price-specie-flow mechanism, and then the effects of recent central bank actions such that the process was thwarted, starting approximately 30 years before his essay appeared in print. He writes, "Traditionally, of course, monetary reserves have not been used as the primary method of adjusting to changes in external conditions but as a shock absorber pending changes in internal prices and incomes. A deficit has been met out of monetary reserves in the first instance, but the proceeds or even a multiple of the proceeds have been, as it were, impounded; that is, the stock of money has been allowed or made to decrease as a result of the decline of monetary reserves, with a consequent rise in interest rates and downward pressure on internal prices. . . . Since the end of the First World War, nations have become increasingly unwilling to use reserves in this way and to allow the effect to be transmitted directly and immediately to internal monetary conditions and prices ... Especially after the Great Depression completed the elevation of full employment to the primary goal of economic policy, nations have been unwilling to allow deficits to

exert any deflationary effect."[13] This discussion is pure Humean thought, but it is also the Monetary Approach to the Balance of Payments, as we document more fully below in the section titled "Friedman's Impact on the Monetary Approach to the Balance of Payments".

Since Johnson and Mundell were working in this area (Johnson joined Friedman's Money and Banking Workshop soon after arriving at Chicago, and ran the workshop during Friedman's sabbatical year (1962–1963)),[14] the question is, What was the relationship between these two bodies of work, and did they communicate concerning issues in which they had a common interest? Was one group consistently responding to claims by the other? Was there an attempt to find common ground between the two groups? The appearance is that, to the contrary, Johnson's thinking was not influenced by his association with Friedman (Laidler 1984, 593). As Dornbusch (2001, 22) has noted, there were at least two quite distinct cohorts at this point, with little interaction between them. But appearances can be deceiving, and we argue below that Friedman's thinking had a major impact on the work done by Johnson and that by Mundell.

Friedman never cited any of Johnson or Mundell's monetary research, in part because his publications in this area preceded theirs by many years. Similarly, Johnson made comments on Friedman's closed economy analysis, but never seemed to be aware of what was contained in the open economy work. For example, in characterizing the differences between his own work and that of Friedman, Johnson makes the claim (Nobay and Johnson 1977, 480) that monetarism assumes that the money supply is exogenous, whereas in an open economy under fixed exchange rates it is endogenous. This claim does not take into account that Friedman and Schwartz (1963, 89) had made precisely this point in the context of the differences between fixed and flexible exchange rates.

It would be remarkable if the money/macro analysis that Johnson and Mundell engaged in at the pre-eminent center for research in money and banking at the time, the Department of Economics at the University of Chicago, was done without any influence from the dominant figure there, who himself was recognized as the most important monetary economist alive. Nonetheless, this is a claim that Johnson and Mundell make, because they refuse to be viewed as "lesser lights" at the University (Johnson 1977, 4). They say that it was just an accident of geography that they happened to be at the same institution as Friedman. Even if it is true that a mere coincidence had brought them into such proximity, isn't it still likely that Friedman's insights could have helped them in their research?

[13]A footnote at this point in Friedman's essay spells out the automaticity of the effects of this mechanism in the Humean case. "Under a pure gold standard, these effects follow automatically, since any international claims not settled otherwise are settled by gold, which in the case of a deficit, is bodily extracted from the monetary stock . . ."

[14]Hynes (2001, 625) says about Johnson's participation in the workshop ". . . he would make only an occasional comment. This was not a particularly supportive environment . . ."

Friedman's views on monetary mechanisms in both closed and open economy settings were formed in the late 1940s and early 1950s, long before Johnson and Mundell arrived at the University of Chicago. Therefore Friedman did not have much to gain from such communication. In contrast, the generally-held view is that Johnson and Mundell's ideas were evolving very rapidly towards Friedman's position. Perhaps they did not engage in discussions with him because this would have made clear that little of their work in this area was original. In order for their work to be seen as having been done independently of Friedman, at the very least they needed to limit their interactions with him.

Johnson's Varying Views of Friedman's Influence on His Work

Johnson's assertion that he had learned little from his interaction with Friedman is laid out in his later accounts (Nobay and Johnson 1977; Johnson 1977). The specific claims concerning the Monetary Approach to the Balance of Payments are: the approach was developed long before Mundell and he had arrived at the University of Chicago; and most of the work done after their arrival was carried out when they were not present at that institution, with the London School of Economics being mentioned specifically.[15]

These claims to independence are rendered implausible by two incontrovertible facts: first, in earlier incarnations of his narrative, Johnson (1972a) had conceded that their view is very monetarist, being consistent with the quantity theory of money; second, the timing of his conversion from Keynesianism coincided with his move to Chicago. We shall deal below with further evidence which contradicts Johnson's view. The essential point to note at this juncture is that Johnson changed his nomenclature concerning his discussion of the Monetary Approach to the Balance of Payments in order to assert independence. A re-writing of the substance of his memoirs was undertaken by Mundell, and it had much the same effect.

That the bulk of the work on the monetary approach was carried out in Chicago can be seen from the list of contributors to the Frenkel and Johnson (1976) volume. All the authors in that book were affiliated at least for a time with the University of Chicago. Furthermore, five years earlier, Johnson (1972a, 83–84) had stated categorically that the Monetary Approach to the Balance of Payments "has been very largely the work of my colleague R.A. Mundell and our students at the University of Chicago . . ." The footnote attached to this sentence in the text points to Chapters 8 and 9 of *International Economics* (Mundell 1968a) as being the key contributions to the monetary approach. According to the extended introductory footnote in the latter chapter, the material in it dates from "the spring of 1965 . . . [and] . . . the

[15]This specific claim is contradicted by Johnson (1976, 263) writing only a year earlier: ". . . [the content of this] . . . chapter draws heavily on the ongoing work of the University of Chicago Workshop in International Economics. . .."

Frank Graham Memorial Lecture (given at Princeton in April 1966 ...) ..." (Mundell 1968a, 134). With these dates this work was definitely done after Mundell had arrived at the University of Chicago. In contrast, the confusion concerning the dating of the drafting of the material in Chapter 8 becomes an important part of our analysis below.

As we have already noted, Johnson had been thinking along quantity theory lines starting in 1956. As an example of this, in a publication in the late 1950s (Johnson 1958, 157), he writes that "... a balance-of-payments deficit implies *either* dishoarding by residents, *or* credit creation by the monetary authorities – either an increase in V, or the maintenance of M. Further ... a deficit associated with increasing velocity of circulation will tend to be self-correcting ...".

Johnson's claim to independence from Friedman is reflected in the fact that his work is rarely cited by Johnson, even in places where it would be apt.[16] For example, Johnson (1972a, 80) omits Friedman's name from those who were familiar with the notion of the inflation rate's being *given* in the case of small countries on fixed exchange rates.[17] But in fact Friedman makes quite clear in his essay that one of his reasons for espousing flexible exchange rates is the primacy of internal policy which he endorses. (Friedman 1953, 180) Also, he notes that the rate of inflation in a fixed rate system is determined by those countries which have the most expansionary policies, with the other countries needing to move in step because of the "harmonization of monetary and fiscal policy" which is required in such an exchange rate regime. (Friedman 1953, 198–200).

Throughout his 1971 de Vries Lecture Johnson used the expression "monetarist," an approach which at this time he heartily embraced. While using this expression he gave little credit to Friedman for the application of this framework, including him, in contrast, in "a new and eccentric vocal minority espousing a new theory called monetarism." (1972a, 51) Instead, Johnson made the ironic comment (1972a, 68) that "The Quantity Theory of Money: A Restatement" (in Friedman 1956, 3–21) is "a much clearer and more enlightening statement of Keynes' essential contribution to the development of monetary theory than the statements of Keynes and his followers." Clearly Johnson was trying to wrest away from Friedman some of the credit given to him for the development of these ideas for both closed and open economy analysis. Leeson (2000, 757) has noted that Johnson's purpose was bifurcated. He wished to propagate "... international monetarism while denigrating Friedman's domestic version." What Leeson does not mention is that Johnson's international application is the same as the view that is presented on pages 164–167 and 170–173 of Friedman's (1953) essay, which amounts to an

[16]Johnson's (1972a, 84) uses an unusual expression concerning the motivation for the Monetary Approach to the Balance of Payments. Namely, it "... harks consciously back to David Hume's analysis ..." These words suggest that there was only a select group of individuals with whom Johnson was willing to engage on this subject.

[17]Instead, Johnson at this point lists the names of numerous economists, in alphabetical order of the names of their (unlisted) native countries.

extended account of the Humean view of the mechanism of international adjustment.

By applying the term "monetarist," in light of the general readership's association of Friedman with this term, Johnson was merely confirming his own conversion to this way of thinking, rather than garnering any credit for himself in connection with purported original thoughts on the subject. So Johnson's strategy here backfired. As a result of this failure to erode Friedman's reputation, Johnson in his subsequent publications lashes out at the idea of monetarism. Johnson asserted that ". . . the evolution of the monetary approach to the balance of payments had . . . proceeded independently of the debates between monetarists and Keynesians." (Nobay and Johnson 1977, 480).

In his Horowitz Lecture, Johnson distinguished carefully between "monetarism" and his own work. "It is important to emphasize . . . [that] . . . the development of the monetary approach to the balance of payments has been confused in many so-called minds with something called 'monetarism,' . . . [which is embraced by] . . . those who emphasize the necessity of proper monetary policy for the stabilization of the economy, led but by no means dragooned by Milton Friedman." This discussion goes on to point out that many readers are "confused" and "unthinking" about the connection between monetarism and the monetary approach to the balance of payments, and as a result the two are viewed as intertwined because of "guilt by association." (Johnson 1977, 3–4) These same sentiments are presented in Johnson (1975, 221), where they are voiced in the strongest terms: "There has been a noticeable tendency to dismiss the new approach as merely an international economics application of an eccentric and intellectually ludicrous point of view of a contemporary lunatic fringe referred to as monetarism".

Johnson's emotional derogation of those who endorse monetarism, as well as those who make the obvious connection between monetarism and the Monetary Approach to the Balance of Payment[18] (as Johnson himself, hair-splitting aside, had done just five years prior), is a dramatic breaking of the silence about Friedman's contribution on the subject. It suggests that the former silence had not been inadvertent. To have worked in monetary theory at the University of Chicago, where a monetarist was the dominant figure, and not to have mentioned his work, as part of the resurgence of interest in monetary ideas after the hiatus of the Keynesian Revolution, is very strange. It is implausible to argue that this omission was unintentional. The striking aspect of both Mundell (1967b, 1968b), and Frenkel and Johnson's (1976) discussion of the origins of the monetary analysis of an open economy is the *absence* of Friedman's name, for the open economy analysis he did in his 1953 essay and in his work with Anna Schwartz.

The effect of the previous silence and of the claims made later are the same: both argue that all credit for the Monetary Approach to the Balance of Payments belongs to Mundell and Johnson, since their work was independent of Friedman.

[18]Among those whom Johnson is deriding as being "so-called minds" are Grubel (1976) and Whitman (1975).

Mundell's Re-Writing of His Narrative

Mundell's relationship with Friedman's essay follows a similar path. Mundell's initial publication in this area is a precise formalization of the argument in the Friedman essay: namely "to analyze the difference between an economy with fixed exchange rates and flexible prices, and an economy with flexible exchange rates with fixed prices." (Mundell 2002, 6) Although his conclusion at first blush appears to be that Friedman's intuition on this matter is "entirely erroneous" (Mundell 1960a, 227), in fact careful consideration confirms that Mundell agreed with Friedman's view that it is only the dynamics of the two exchange rate regimes that cause them to differ. Their comparative static properties are identical, a point which Meade (1951, 190) had made earlier.

In subsequent publications, Mundell again does not do justice to what Friedman had written. So, for example, Mundell (1961b, 657, 663) wrote that Friedman was a proponent of flexible exchange rates, whose argument depended on money illusion. In fact, Friedman's position is the same as Mundell's, and Friedman had *already asked* the key question which Mundell later posed concerning choice of exchange rate regime, and had *answered* it in precisely the terms which Mundell uses. And two further works, which we consider below (1961a) and (1967b), deal with exactly the same questions as Friedman asks, such that they are best interpreted as formalizations of his argument. As he did with Fleming's (1962) work published at this time, Mundell does not cite Friedman in these publications.

Further evidence of this arises in Mundell's re-writing of the narrative of the circumstances of his "sharp move back to classical, money-oriented models, the context of the classical economy." (Brooks 1982, 139) In an interview with John Brooks for *The New Yorker* magazine in April 1982, Mundell claimed that this "return-to-the-classics" (Mundell 2002, 13) occurred "[a]round 1965." The paper which shows this change of heart is Mundell's (1960b) major contribution to the Monetary Approach to the Balance of Payments, which is singled out by Johnson, and appears as Chapter 8 of *International Economics*.

The question then becomes, When was this paper (titled originally "International Disequilibrium and the Adjustment Process," (Mundell (1960b) [and published as Mundell (1967b)]) and subsequently titled "Barter Theory and the Monetary Mechanism of Adjustment" (Mundell 1968b) first drafted? In identifying his "return to the classics" as occurring "[a]round 1965" Mundell was using the date when he presented this paper to a World Bank Conference (in late July 1965). The date for the appearance of the original published version of this paper is 1967, in the conference volume edited by John Adler (1967).

Note that since July 1965 is the date of Mundell's first appointment as a faculty member at the University of Chicago (as Ford Visiting Professor of International Economics), it would appear that he was caving in to Friedman's point of view without so much as even engaging in an initial debate. Alternatively, it makes Mundell appear to be chameleonic. At the IMF he wrote in Keynesian terms; heading to Chicago he switched to the classical framework.

Mundell's subsequent narrative of these events portrays himself as having much greater independence of mind. Starting in 1992, Mundell's memoirs claim that his return to classical economics took place in the spring of 1963; and, he asserts, that is when he presented these ideas to a seminar in the Research Department of the IMF. Apparently Marcus Fleming "really disliked" (Mundell 2002, 13) this change of heart on Mundell's part.

This account lacks plausibility in that in the spring of 1963 Mundell was actually writing the first draft of his most-cited paper [which assumes perfect capital mobility (Mundell 1963)] which is explicitly Keynesian in its structure. He continued to turn out research in this mode for at least another 18 months. At a minimum, this shows that he conducted research in both modes *simultaneously*. Indeed, this very paper in question, Barter Theory, says that it uses "a simple model simultaneously classical and Keynesian in spirit." (Mundell 1967b, 461) This hardly seems consistent with a "sharp move back to classical ... models."[19]

Furthermore, there is no evidence to support much of what Mundell writes here. For example, the IMF has no record of his leading such a seminar, even though they have details of three of his other presentations between May 1962 and July 1963. An early version of this paper (Mundell 1966) [dating from approximately March 1966 when Brookings Institution was set to publish Mundell's collected works (Mundell 1967a)] includes the original drawings of its figures. These diagrams are on yellowed paper, and the typewriter which was used, in a clumsy labeling of them had a European font. Finally, the sentiments in the introductory paragraphs of this paper are repeated, almost verbatim, in Mundell's (1961a) *Kyklos* publication which appeared in 1961.[20] All this evidence points to this paper's having been written sometime in 1960, when Mundell was in Bologna.[21]

Mundell's questionable dating of the drafting of this paper has led many economists to provide their own timing of Mundell's purported sharp change in

[19]Frenkel and Johnson (1976, 10), after repeating Mundell's claim, that Barter Theory "... introduces the connecting lines between ... classical and Keynesian concepts and methods of analysis ...," asserts that this same paper is "... the closest to a classic statement of the difference between the Keynesian approach and the monetary approach that can be found in the phase of Mundell's transition from one to the other approach."

[20]The title of this piece, "The International Disequilibrium System," is very similar to the original title of the Barter Theory paper.

[21]Barter Theory clearly must have preceded *Kyklos*, in terms of the timing of their drafts. The reason is that *Kyklos* asserts that the Hume adjustment process does not work. In order to make such a claim, that process first needs to be modeled and assessed. Given the vagaries of the publication outcomes, ironically Mundell's *oeuvre* has the claim that the process does not work appear in print (1961) before the article which claims that the adjustment process operates automatically (1967). This mis-ordering is corrected in Mundell's (1968a) collected works. There, not only does *Kyklos* come after Barter Theory, it is in a completely different section of the book. The re-titling of the Barter Theory paper hides the fact that it and *Kyklos* are intimately related, were written virtually simultaneously, and convey the ideas in two adjacent paragraphs which appear on the same page in Friedman (1953, 171).

modeling strategy. Thus, Frenkel and Johnson (1976, 32) view the transition as having taken place after 1964; Obstfeld (2001, 7), as mid-1960s; and Dornbusch (2000, 203), as late 1960s. All these narratives see Friedman as having had a minimal impact on Mundell's research, and are based on the idea that Mundell's Barter Theory paper was written in 1963 or after. But, in fact, all available evidence shows otherwise. Any shift in Mundell's position signaled by this paper's drafting is going to be mis-dated to the extent that the date of the drafting is misperceived. Specifically, since this paper was drafted before (rather than after) all of the Keynesian papers which Mundell produced at the IMF (1961–1963), Frenkel and Johnson (1976, 10) provide an incorrect assessment of Mundell's impact on the Monetary Approach to the Balance of Payments. The impression one gets from their version of the events is that Mundell's work in this area was of great novelty, that it represented a different view of macroeconomic theory, and that it was done well before he arrived at Chicago. This appears to validate Mundell's claim that his contribution to the approach was accomplished independently of Friedman's influence. If, instead, the paper is seen as leftover work from Mundell's days in Italy, then the paper was by that time an old manuscript which had not yet seen the light of day in terms of publication.

While it is true that the work was done before Mundell's arrival in Chicago in 1965, it is not correct to see this paper as independent of Friedman's influence. Mundell had been a post-doc at Chicago during 1956–1957, when he had conversations with Friedman about open economy macro and exchange rate regimes. He took a particular interest in Friedman's (1953) celebrated essay, as is apparent from his citation of it in Mundell (1960a, and 1960b). The first of these articles includes a number of citations of this Friedman work, and one of these provides the quotation of a whole sentence from it. As we have noted above, the second article repeats ideas about the choice of exchange rate regimes which appear in Friedman.

Since Mundell cites material from various pages throughout this essay, it seems likely that he, at least at times during this period, was aware that the ideas behind the Monetary Approach to the Balance of Payments and Optimum Currency Areas are included in that work.

Mundell's neglect of Friedman's contributions in this area can be easily seen in the various drafts of the introduction to his Barter Theory paper. He writes that the purpose of the paper is to build "... bridges between ... the barter and the monetary models, on the one hand, and between the classical and Keynesian traditions in international economics, on the other ... [because it] ... is ... self-evident ... that they do not yet exist ..." In introducing these ideas Mundell says that his model " ... owes its origin to David Hume ...," and he goes on to mention the names of numerous economists, including 12 from the twentieth century: Viner, Meade, Johnson, Keynes, Hayek, Myrdal, Tinbergen, Frisch, Leontief, Samuelson, Hicks, and Mosak.

In what appears to be a direct swipe at Friedman, Mundell describes Mosak's work as being different from all the other economists mentioned because it is not in a Keynesian mold. Mundell's (1968a, 112) explanation for the exceptional nature of Mosak's work is that "... he was working in a different milieu (Chicago) in the

1940s where Keynes' ideas were not regarded as so novel …" Does not this description fit Friedman's work even more closely, and is not the relevance of Friedman's work to that of Johnson and Mundell an order of magnitude greater? Furthermore, the quantity theory of money is mentioned at least five times in this paper. Since no name was more closely associated at that time with this framework, it is noteworthy that Friedman's name does not appear in the original version of the paper.[22]

Friedman's Statement of the Optimum Currency Areas Argument

As I have recounted elsewhere (Boyer 2009), the Optimum Currency Areas Argument appears in virtually identical terms in Friedman (1953) and in Mundell (1961b), so that Friedman's essay provides the first definitive statement of it (although Friedman calls it "mixed system").[23] The reason that this is not well-known in the profession *is that the most accessible form of Friedman's essay is an abridged version,* for which the "mixed system" argument has been elided. This version appears in the American Economic Association's *Readings in International Economics*. (Caves and Johnson 1968)

We have provided examples above showing that the strategy employed by Johnson and Mundell was to use Friedman's ideas without acknowledgment, as is seen in their use of the expressions "quantity theory of money" and "monetarist" without citation. This strategy culminated in the spring of 1969, when Johnson was invited to contribute to the Hobart Papers, presenting a lecture on the alternatives which the UK faced, subsequent to the devaluation of sterling. Johnson chose as the title for this essay "The Case for Flexible Exchange Rates, 1969." This title is identical to that of Friedman's celebrated 1953 essay on exchange rate regimes, but with the current date appended to it. Johnson graciously notes in the first footnote that "The title acknowledges the indebtedness of all serious writers on this subject to Milton Friedman's modern classic essay …" (Johnson 1969b, 12) Johnson then includes a full citation of both the original publication and the reprint in Caves and

[22]Friedman's name does appear, but without citation [for work that was published as Friedman (1960b)], at the end of the version published in Mundell (1968a, 130), merely in the context of questioning whether the Friedman monetary growth rule is appropriate in a fixed exchange rate setting.

[23]As Cesarano (2006) has pointed out, Lerner (1944, 1947) asks the key question concerning Optimum Currency Areas. Since he deals specifically neither with asymmetric shocks nor with the difficulties inherent in the maintenance of a fixed exchange rate regime, he should be credited only with an abbreviated stab at this problem. In contrast, Friedman (1953, 192–196, 198–201) deals with all these issues. Mundell (1961b) then is seen as merely a reiteration of Friedman's argument.

Johnson. While it is noted that the reprint is abridged, the extent and nature of the abridgement are not indicated. This is strange since the elision included precisely the topic which Johnson's paper covers, namely, What are the appropriate criteria for a country to use in choosing between a rigidly fixed exchange rate and a fully flexible exchange rate regime?[24]

Stranger still is the way in which Johnson's article proceeds. At no point is Friedman's name mentioned in the body of the text. Consequently the reader is baffled as to whether the material being presented is to be interpreted as Johnson's original thoughts on the subject, or as Johnson repeating Friedman's ideas. If the latter, are these words purporting to represent Friedman's contemporaneous ideas or those of 16 years prior as applied to contemporaneous circumstances?

The only name that appears in the body of Johnson's paper is Keynes. The gist of the argument is that Friedman's essay had contributed little of value, because "The case for flexible exchange rates on these grounds has been understood and propounded by economists since the work of Keynes and others on the monetary disturbances that followed the First World War." (Johnson 1969b, 13) In contrast, the important recent contribution to exchange rate thinking was not done by Friedman, in Johnson's view. Instead, the key "development in the theory of exchange rates … [is] … the theory of optimum currency areas, developed by R.A. Mundell and R.I. McKinnon." (Johnson 1972b, 13).

For much of his essay, Johnson directs his attention to what should be done with sterling, which is appropriate since this paper was delivered in front of an audience in London. However, Friedman (1953, 192–196) also discussed sterling and the sterling area in his essay. Johnson should have, therefore, made a comparison with what Friedman had said. This is all the more the case, because it was in this context that Friedman had raised the issue of Optimum Currency Areas (although his term for the idea was "mixed system").

Certainly a possible interpretation of what Johnson was doing in this article was to make the Mundell position seem reasonable relative to that of Friedman. He is portrayed as being irrationally attached to flexible exchange rates, and adamantly opposed to fixed exchange rates. Friedman pointed out numerous times in various lectures that this was not an accurate statement of his point of view. His position was essentially identical to that of Mundell: both opposed pegged-but-adjustable exchange rate regimes, however Friedman was more consistent in pointing out that this implied that central banks under rigidly fixed exchange rate regimes would need to end their attempts at independent stabilization policy.

[24]Laffer (1973) writing at the time notes that the key pages in Friedman (1953) which deal with this question are 191–200. All the material in these pages was either deleted or revised for the abridgement. This material constitutes about two-thirds of all the alterations which one finds in the Caves and Johnson version.

The Historical Origins of the Monetary Approach to the Balance of Payments According to Johnson and Mundell

We have pointed to the role played by the classical adjustment process in Friedman's discussion of balance of payments problems. Since at the time money played only a minor role in most other presentations of the mechanism of open-economy re-equilibration, to omit Friedman's name from a narrative of the historical origins of the modern revival of the price-specie-flow mechanism leaves a conspicuous gap in the account. Nonetheless that is the way in which Johnson (1977) and Mundell (1967b) proceeded.

A narrative which fails to include a discussion of the impacts of the Keynesian revolution[25] and the monetarist counter-revolution spearheaded by Friedman, raises the question immediately as to why there was a revival of interest in the 1960s in the mechanism laid out in an essay (Hume 1898 [1752]) that had been written over 200 years before. There had not been such interest in the preceding 50 years, since Taussig (1966 [1927]) and his students had failed to find a convincing quantity theoretic explanation which yielded empirically useful models of adjustment in open economies. Viner (1957 [1937]) had refined the thinking to such a point that Hume was no longer the relevant reference to cite in this context.

We are told that this approach is novel, that in 1969 it was "newly emerging." As late as 1974 the approach is described as both "new" and as having ". . . a long, solid, and academically overwhelmingly reputable history . . ." (Frenkel and Johnson 1976, 9, 29) If the latter is true, should there not be a consensus about which are the important contributions in this area? Should there not be discernable links among these contributions? Should any single contribution not be seen as adding new insights to those that had preceded it? And should not these contributors be viewed as thereafter embracing only this approach, abandoning rival frameworks which had become outdated with the new insights?

That there is no uncontroversial history of the monetary approach is apparent from the many versions that have been published, and the fact that there is widespread disagreement on the documents which are central to it. In some cases, particular economists have had constantly shifting views of which individuals had developed the key insights. Thus in 1968, Mundell (1968b, 5) uses the expression "monetary approach" in its application to balance-of-payments analysis, and cites only one paper: Johnson (1958).

Johnson must have been flattered by this claim, especially since it noted the superiority of this work over the traditional elasticities and absorption approaches, because it ". . . is valid even when there are capital movements . . .."[26] This assertion

[25]Strangely Frenkel and Johnson (1976, 9) refers to ". . .the 'Keynesian Revolution' in monetary theory. . ." but not to the monetarist counter-revolution. This, too, seems to be an attempt to minimize the impact of Friedman's interest in monetary factors in the post-war environment.

[26]Read Mundell's (Vane and Mulhearn 2006, 95) reassessment of Johnson's contribution, in order to determine whether Mundell's initial praise was likely to have been sincere.

is reprinted in Chapter 10 in Mundell's (1968a, 150) *International Economics*. Johnson returned the compliment by pointing to the important pieces on the monetary approach as being Chapters 8 and 9 in that same volume. The original forms of these two chapters (Mundell 1967b) have a total of three citations: one to a book on Zen Buddhism; one, a self-citation; and one to a little-known paper by Prais (1961). So the view that Johnson made a major contribution seems defective in that that work cites no earlier work that conforms with Mundell's references, and, in turn, it is not cited frequently by others until after Mundell's championing of it. These points raise doubts as to whether Johnson (1958) is an "essential document" in the monetary approach's development.

In 1971 Johnson (1972a, 84) claimed that the key originators of the modern revival of this framework were Holtrop and Koopmans (and Mundell (1960b), as well, continues to be singled out as a major contribution), although Johnson admits that this view is grounded ". . . on the basis of a hunch derived from the literature, rather than of scholarly research." By 1974 this hunch had proved to be a dead-end. These names do not appear in the list of 52 references which Frenkel and Johnson (1976) provide, in what should have been a definitive statement, being the first chapter in the eponymously titled book. In 1975 Johnson (1977, 4) has a different view, and sees the key contributor as being Meade. (In this account Mundell as well continues to be an important figure, but for reasons different from his being associated with the Dutch economists indirectly, through Polak, during his period of service at the IMF.)

Without Friedman there is no simple line connecting the various contributions in this area. Johnson (1958) conspicuously lacks a citation to any writing before the War, and therefore can hardly be considered a link in the narrative. But Mundell is part of a completely disjoint set of references, so there are at least two channels by which the monetary approach developed.

In addition, one must take account of the input provided by the Research Department at the IMF. Their claims, which they press with equal vigor, distinguish between two different views of the origins of the monetary approaches, Keynesian and Johnsonian, and neither of these corresponds with the narrative as told by Frenkel and Johnson, as will now be shown.

Polak and Helliwell's Competing Views of the Origins of the Monetary Approach

Polak (2002) has his own view on this subject. According to him there are two monetary approaches: Keynesian and Johnsonian. The Keynesian approach is evolutionary, and his own work (Polak 1957) is the key document. The Johnsonian approach is revolutionary, and is based on Johnson (1958).

The Keynesian monetary approach, according to Polak, evolved from the Cambridge economists. The list of contributors which he provides is disjoint

from what one finds in any other publication describing the origins of this approach. The names included are Kahn (1931), Machlup (1943), and Holtrop (1957). It is true that Johnson at one point gave credit to Holtrop, but not with sufficient specificity that he appears in the definitive list in Frenkel and Johnson.

Polak's outline of the Johnsonian version of the monetary approach refers only to Johnson (1958). About this paper Polak says "His discovery of the monetary approach as a completely new starting point for balance of payments analysis seemed to have had the liberating force of an epiphany … The 'essentially monetary' epithet made its first appearance in Harry Johnson's (1958) 'basic article' … devaluation remained 'the standard question' … on which the Chicago School would demonstrate its version of the monetary approach …" (Polak 2002, 26–27).

There are a number of deficiencies in this view of the development of the monetary approach. First, contrary to Polak's epiphany claim, Johnson was unaware that he had revolutionized balance of payments analysis until being told by Mundell (1968b) that he had done so. Before this time, Johnson's 1958 paper had languished. So it was after a delay of more than ten years that Johnson refers to the monetary approach as being a newly-emerging way of thinking.[27] Johnson's paper contains two equations, both of which are obvious, and had been used in discussions of the absorption approach previously. There is no separate equation specifying equilibrium in the money market. The "model" in the paper has never been used in any subsequent discussion. Although a distinction is drawn between stocks and flows, it is not applied consistently.[28]

In addition, although Polak's statement of the key requisites of the monetary approach seems reasonable, we disagree with the way in which he applies them to the evidence. Polak is not correct as to the first appearance of the expression "essentially monetary" in this context. Friedman (1953, 166) writing about responses to fluctuations in trade patterns notes that "Adjustments are required continuously, and many are called for by essentially monetary phenomena, which, if promptly offset by a movement in the exchange rate, would require no change in the actual allocation of resources." This shows that by Polak's criterion, Friedman is a prior modern statement of the monetary approach, not only because the crucial wording appears there, but also because he makes the important point that devaluation is monetary policy under another name. In contrast, Johnson (1958, 162–163) distinguishes forcefully between a "policy of expenditure-reduction … through … monetary restriction … and … expenditure-switching … policies … [such as] … devaluation." If "expenditure-reducing" and "expenditure-switching" constitute a useful method of categorization, then Johnson must be arguing that monetary policy and devaluation are quite different initiatives. Later Johnson (1976, 274) seemed to recognize that there was an inconsistency here, but he continued to perpetuate it.

[27]Moggridge's (2008, 146) discussion of this paper shows that work on it began no later than 1954.

[28]Hynes (2001, 628) considers Johnson (1958) to be a seminal paper, despite its numerous shortcomings, because its "…message was quite subversive."

Finally, Polak saw Mundell as not contributing anything useful to the approach, and this viewpoint must be given serious consideration, since Polak was Director of Research at the IMF during the time period when Mundell was there. Since some dating of Mundell's (1960b) contribution sees this work as being written at this point, one would think that Polak would be an authoritative source on this matter.[29] Polak's claim is that the derivative nature of Mundell's contribution can be easily discerned by focusing on the relevant dates. Polak (2002, 19, 27) targets Johnson's view of this matter. That view is faulted as being "rather different (and no doubt audience-pleasing)" in that (in a lecture in Amsterdam) he highlights the Dutch contribution and, in that connection, "Mundell's period of service in the research department of the International Monetary Fund . . ." Polak makes the valid point that if Johnson (1958) is viewed as part of the early work on the monetary approach, then that dating of the approach is "long before Mundell moved to Chicago, indeed before he joined the Fund staff in 1961 – which suggests that Johnson's genealogy of the Chicago monetary approach . . . was unduly modest." Polak's conclusion is that Johnson (1958) and Polak (1957) are the early valuable contributions. Ironically, Polak, the man who was Director of Research at the time, dismisses as of minor significance at most the piece of work that Chicago insists is an essential document, and which Mundell dates to his period of service in that Research Department: his Barter Theory paper.

My assessment is the same as Polak's: the role of Barter Theory has been misinterpreted. Specifically, my view is that Barter Theory did play a significant role in the monetary approach, but that due to questionable dating on Mundell's part, the usual interpretation of this contribution is incorrect: in fact, it was drafted much earlier than the timing given in most narratives. As to significance, many more chapters in Frenkel and Johnson (1976) use the template that is provided by Mundell's (1960b) "essential document" than use Johnson (1958).

Furthermore, the IMF did have a role to play in this approach, according to the consensus. As we have noted, one of the mystifying aspects of this subject is how few of the purported main contributions reference any of the others. The only piece which attracts multiple citations is Prais (1961) (with Mundell's (1967a, 448) reference being the first appearance in a publication). But this only deepens, rather than resolves, the mystery. Prais deals for the most part with the fixed- price models exemplified by Polak (1957). Since the bulk of subsequent work in this area instead assumes price flexibility, which was a key component of the price-specie-flow mechanism, it is unclear what relevance Prais's work has to the evolution of this literature. This is especially true concerning the popular assumption of purchasing power parity, which is used extensively in the monetary approach. Prais's model is incompatible with such a view.

[29]The facts, that Polak does not have a high regard for Barter Theory nor does he gauge its impact as being important, are consistent with the absence of any record at the IMF that a seminar presentation on this paper took place during Mundell's stint there.

Helliwell, too, is deferential to Johnson's views on the monetary approach. Helliwell notes that "Johnson did more than anyone else to define the boundaries and establish the claims of the monetary approach to the balance of payments." (Helliwell 1978, S63) Nonetheless, Johnson's claim that his narrative of the history of the subject is based on "... a hunch derived from the literature rather than of scholarly research." (Johnson 1972a, 84) is the sort of tenuous evidence which Johnson used late in his career, and Helliwell notes that it "... reflects a departure from the well-referenced and careful analytical style that is usually a hallmark of Harry Johnson's exposition of balance-of-payments issues." (S64).

In keeping with our view that the use of this approach was a device to allocate credit for existing results, rather than a progressive research agenda to derive new results, Helliwell highlights some of its distinctive aspects. "One of the most interesting features of the monetary approach to the balance of payments, especially in the post-1973[30] development and defense of it by Harry Johnson, has been the adversarial tone of the analysis. This tone is surprising because there would appear to be little in the way of professional resistance to consideration of the consequences and difficulties inherent in sterilization policies, and hence to serious questioning of the assumption of national monetary independence commonly adopted in textbook analysis."(S63) He goes on to say, "It must remain something of puzzle why so much stress was placed, by Johnson and others, on the extent to which the monetary approach differs from what is described, without much supporting detail, as the 'traditional approach.' The puzzle becomes even deeper when one notes that the same papers and authors (e.g. Meade 1951 and Mundell 1968a) that were regarded as leading exponents of the Keynesian policy approach are also seen as providing the essence of the monetary approach." (S64) By 1974 Johnson was seeing Meade (rather than the Dutch economists) as being the main influence on Mundell. It seems unlikely that Meade and Mundell were both the source of the traditional approach and the ones who overthrew it. To have both roles raises the question as to with which view their loyalties lay.

Helliwell's footnote at this point continues the expression of puzzlement. "Another slightly puzzling feature of some of the papers utilizing the monetary approach is the extent to which an air of controversy is built up by reference to unnamed detractors who are said to have made extreme counterclaims. For example, in the introductory chapter of Frenkel and Johnson (1976), a brief account 'implicitly disposes of various, and often ill-informed, criticisms that have tended to have been made of it, and which amount essentially to red herrings across the trail of scientific study and understanding.' References to the sources of the red herrings are not included; this too reflects a departure from the well-referenced and careful analytic style that is usually a hallmark of Harry Johnson's exposition of balance-of-payments issues." (S64).

[30]The precise date of writing of this portion of Frenkel and Johnson (1976) is unclear. Since Johnson's first stroke occurred in October 1973, Helliwell seems to be alluding to work that was written afterwards. To focus on such work may not be an even-handed way of dealing with Johnson's overall impact in this area.

Friedman's Impact on the Monetary Approach to the Balance of Payments

The monetary approach reached its high-water mark with Frenkel and Johnson (1976) and the publications which appeared soon thereafter. By 1985 it had been abandoned, as is apparent from the survey volume edited by Kenen (Jones and Kenen 1985).[31] Although the expression appears there, it does so without the distinctive features which had set it apart a decade earlier. Not surprisingly, it is Frenkel (Frenkel and Mussa 1985) who continues to plow this material, with other authors using the expression in a perfunctory manner.

With its output arriving in such a concentrated way (over such a short period of time), it is easy to see that the model contained in Barter Theory is the central template for this approach, and therefore that that paper is the key document. Thus the question arises as to whether this paper merely replicates mathematically the ideas that are expressed verbally in Friedman's essay. In order to make this case we need to introduce a second paper by Mundell which follows Friedman's thinking very closely. This paper asserts that "Hume's law is inapplicable." (Mundell 1961a, 153) Since this paper asserts the exact opposite of Barter Theory (which looks at the automaticity of balance-of-payments re-equilibration), it points to two logical inconsistencies. The assertion that the law is no longer operative logically requires that the Humean mechanism on which it relies be modeled. Since this publication does not present a Humean model, one looks instead to the mechanism that appears in Barter Theory. In that paper one finds such a theoretical construct: "The model owes its origin to David Hume." (Mundell 1967b, 445). But the question arises as to this publication, Why would anyone, intending to apply the construct's insights to issues in current macroeconomic performance, model a process which is no longer operative?

Logical consistency in this matter can be restored if one follows the evidence which shows that this paper was drafted in 1960, but was not published until 1967. Being the earlier of the two drafted papers, Barter Theory is logically consistent with Mundell (1961a), in the same way that the first paragraph on page 171 of Friedman (1953) precedes the second paragraph. *Both* Friedman and Mundell's accounts start off by showing how Hume's mechanism would work in the absence of offsetting actions by the central bank. *But both accounts also go on to say* that due to a recent change in behavior on the part of the central bank, this automatic mechanism has been thwarted, so that balance-of-payments problems continue to fester.

In the event, the actual order of publication of Mundell's papers makes his position untenable. If "Hume's law is inapplicable," there is little reason to model the mechanics of an obsolete process, and even less to claim that the process in its

[31] A similar assessment is made by Blejer et al. (1995, 709).

original form has any relevance, let alone that the mechanism which it describes has any possibility of success.

We now point out how Barter Theory (Mundell 1967b, 441–462) is very similar to Friedman (1953, 164–167, 170–172) in virtually every way. This is true as to the views expressed, the assumptions made, the results derived, and the conclusions drawn.

These similarities will be spelled out by covering the following points which these two papers have in common:

There Is a Separation of Real Effects from Monetary Ones

Friedman's essay, immediately after the introduction, in the first paragraph of the substantive material, makes the distinction between "'real' conditions" and "monetary conditions." Examples of the former are "weather, technical conditions of production, consumer tastes." Examples of the latter are "divergent degrees of inflation or deflation in various countries." It is noted that real shocks are given relatively little attention in the essay since their "... effects on the relative price structure are likely to be much the same whether exchange rates are rigid or flexible and to raise much the same problems of adjustment in either case ..." (159) The focus in the essay instead is on shocks which are "essentially monetary phenomena." (166).

Barter Theory too makes this distinction, looking for cases of adjustment for which "... the separation of real from monetary phenomenon [*sic*] is exact." (454) It is pointed out that in the "... dynamical, short-run, disequilibrium theory monetary elements assume a role of first-order importance in the adjustment process. But after the adjustment process was complete money turned out to be a mere veil, with no influence upon the nature of [*sic*] position of long-run equilibrium." (442).

The Adjustment Occurs Automatically

Friedman notes that "... monetary reserves ... have ... been used ... as a shock absorber pending changes in internal prices ... A deficit has been met out of monetary reserves in the first instance, but the proceeds or even a multiple of the proceeds have been, as it were, impounded; that is, the stock of money has been allowed or made to decrease as a result of the decline of monetary reserves, with a consequent ... downward pressure on internal prices ..." A footnote points out that "Under a pure gold standard, these effects follow automatically, since any international claims not settled otherwise are settled by gold, which, in the case of a deficit, is bodily extracted from the monetary stock ..." (171).

Barter Theory portrays the same mechanism: "An over-issue by the banking system would quickly bring its own corrective, as specie flowed out and forced the banks to take back the redundant currency . . . The nominal quantity of money was thus determined in a single economy by international considerations . . ." (443) Thus, classical economists provided a ". . . demonstration of the automaticity of balance-of-payments adjustment . . . To have perceived the essential truth of these propositions, which even today exhibit a fundamental truth, was a supreme intellectual achievement of classical economic analysis . . .."[32] (442–443).

These Ideas Are Presented in the Context of a Specific Monetary Shock: Foreign Deflation[33]

Friedman deals with foreign deflation as a prime example of a monetary shock, covering it explicitly at least three times, and dealing with the issue implicitly a roughly equal number of times. Such a shock ". . . if promptly offset by a movement in the exchange rate, would require no change in the actual allocation of resources." (166) ". . . [A] decline of 10% in every internal price in Germany . . . would clearly have identically the same effect . . . as . . . a decline of 10% in the dollar price of the mark, with all internal prices unchanged . . ." (165) In contrast, under fixed exchange rates because "[w]age rates tend to be among the less flexible prices . . ., an incipient deficit that is countered by a policy of permitting or forcing prices to decline is likely to produce unemployment rather than, or in addition to, wage decreases . . . In this way, it offsets the incipient deficit. But this is clearly a highly inefficient method of adjusting to external changes. If the external changes are deep-seated and persistent, the unemployment produces steady downward pressure on prices and wages, and the adjustment will not have been completed until the deflation has run its sorry course." (165).

Barter Theory introduces six diagrams, but employs them for just one exercise: a monetary shock for which a 50% devaluation would restore equilibrium, with no change in the values of real variables. A 50% decline in foreign prices is an example of such a shock. It is noted that an alternative path to the same equilibrium, in terms of the values of real variables, can be followed without any change in the value of the exchange rate. "Obviously a deflationary process would reduce the [domestic]

[32]Note that these sentiments appear also in Mundell (1961a, 153), but with quite a different conclusion: ". . . this analytical separation . . . is . . . a[n] . . . artificial distinction . . . [which] . . . is now out-dated . . ."

[33]Such a topic has often been the subject of analysis in international financial tracts. Laursen and Metzler (1950, 283–284) look into the international transmission of the business cycle under flexible exchange rates. If these cycles take the form of price movements, they say that there is "general agreement" on the shielding provided by flexible exchange rates, based on an argument that is "extremely simple." Friedman and Mundell are deriving their conclusions from this simple argument.

price level and the money supply … [by 50%] … under the automatic fixed exchange rate adjustment process. But the deflation involved may be a painful process, and certainly would be if rigidity of factor prices meant that deflation would imply unemployment …" (454–455).

Devaluation Is Monetary Policy by Another Name

Friedman notes that "Adjustments are required continuously, and many are called for by essentially monetary phenomena, which, if promptly offset by a movement in the exchange rate, would require no change in the actual allocation of resources."(166) Since devaluation can offset a monetary shock, such as a monetary policy initiative, it must itself be a monetary phenomenon, and therefore is monetary policy by another name.

Barter Theory shows that when the problem is that money supply and prices are out of line, of the same proportion, against foreign trading partners, due to a monetary shock, then equilibrium can be restored through devaluation. Therefore devaluation and monetary policy are the same, only with different names. (454–455).

The Automatic Mechanism Described by Hume Has Been Thwarted by Sterilization

Friedman says about balance-of-payments surpluses and deficits, "Since the end of the first World War, nations have become increasingly unwilling to use reserves in this way and to allow the effect to be transmitted directly and immediately to internal monetary conditions and prices. Already during the 1920s, the United States, to cite one outstanding and critical example, refused to allow its surplus, which took the form of gold imports, to raise domestic prices in the way the supposed rules of the gold standard demanded; instead, it 'sterilized' gold imports. Especially after the Great Depression completed the elevation of full employment to the primary goal of economic policy, nations have been unwilling to allow deficits to exert any deflationary effect." (171).

Turning now to the companion piece to Barter Theory, we find Mundell (1961a) making a strikingly similar statement of the same idea: "The decline in automaticity dates from the first attempts of central banks to adjust the domestic supply of notes to … the needs of trade … instead of the requirements of external equilibrium (the bullionist principle) … the abandonment of the bullionist principle became widespread only after the revolution in Federal Reserve policy during the 1920s, and especially after legal or de facto recognition in post-war years of full employment as a primary goal of public policy …" (153).

These quotations show the extensive overlap between this portion of Friedman's essay and Mundell's Barter Theory paper, as well as his paper in *Kyklos*.[34] *Mundell does not cite Friedman in either of these papers.*

Conclusion

This paper has recounted how in 1959 Harry Johnson took up the "Keynesian" chair in the Department of Economics at the University of Chicago, initially offered a dozen years previously to Samuelson, and occupied earlier in the decade by Metzler. Since it is widely recognized that, at least in his open economy macroeconomic analysis, Johnson switched to a monetarist point of view during his stay there, and since the Department was "pretty much dominated by Milton Friedman," it is natural to see Friedman as having converted Johnson to his cause. This is a view that Johnson resisted, and with ever greater vigor over time as it portrayed Mundell and him as being the "lesser lights" at Chicago.

The resulting equilibrium seemed to suit the purposes of all three economists.[35] Although he appears to have been somewhat oblivious to the *specifics* of what they were doing, Friedman was flattered that the Chicago Keynesians were imitating him. Johnson got what was, in effect, credit for open economy macro-analysis which should have gone, first and foremost, to Friedman, and only then to Johnson, for having *popularized* Friedman's 1953 approach; and Mundell was able to claim that although his ideas were very *similar* to Friedman's he had arrived at them independently, via a different route.

[34]Further similarities between Friedman (1953) and Mundell (1961b, 1967b) are (1) It is a matter of indifference for the process of adjustment whether most of the changes in money income occur through movements in the price level or through movements in real output. This is stated explicitly in Mundell (1961a, 154), and in passing in Mundell (1967b, 445, 453, 454, 461). Both publications thus encompass both the classical and the Keynesian points of view. Friedman (1953, 164) sees both possibilities as leading towards re-equilibration, as the title to this subsection ("Changes in Internal Prices or Income") indicates. (2) Capital movements are not an important part of the adjustment process. Friedman (1953, 166) says "interest-induced capital movements. . .come into operation only incidentally to the adjustment of internal prices." Mundell (1967b, 444–445) assumes that there is no capital market in the economy which he is analyzing; therefore ". . . wealth is held primarily in the form of money and goods . . ." Mundell (1961a, 165) sees the automatic adjustment process being at work, independent of the degree of capital mobility, including the extreme case when it is zero.

[35]Perhaps "equilibrium" mischaracterizes the situation, since others have viewed it as unsustainable. Dornbusch (2001, 22, 23) noted that there were tensions in the Department, if only because Mundell is an "*enfant terrible*" who "could play the bad boy with success." This was reflected in a lack of communication between members of the Money and Banking Workshop (run by Friedman), and the International Economics Workshop (Johnson and Mundell). One may speculate that Mundell's abrupt departure in 1971, to an institution (the University of Waterloo, Ontario, Canada) which was not in the same league as the University of Chicago, was not lamented by Friedman.

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Appendix

A Comment on Russell S. Boyer's

"Johnson's Conversion from Keynesianism at Chicago"

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To put it provocatively, Professor Boyer's paper can be viewed as a conspiracy theory: Two Canadian Keynesians came to Chicago, stole Milton Friedman's open economy ideas and jointly covered it up. One of them, Johnson, literally added insult to injury.

I usually don't like conspiracy theories because most of them blame the Jews for everything that is wrong in our world. However, this is a different kind of conspiracy theory, because here the Jew is the victim rather than the evil perpetrator. Presenting this paper *in Israel* is a very wise choice indeed!

I am grateful that the organizers let me comment on this paper because it was a rare opportunity for me to take a break from my usual research and look at my own intellectual roots. Although I've never been to the Department of Economics at the University of Chicago, no place has influenced my education, research, and career like that department. Chicago graduates taught me at Tel Aviv University (Leonardo Leiderman, Assaf Razin, and Jacob Frenkel) and at the University of Rochester (Alan Stockman). My work in monetary theory was most influenced by Neil Wallace (mostly through his student Per Krusell – my Ph.D advisor). My idea of working on both monetary theory and monetary history was obviously inspired by Wallace, who seems to have taken the idea from Friedman. Perhaps because I was taught to think like a Chicago graduate, I've been hired as an economist *only* by Chicago graduates (Leonardo Leiderman at the Bank of Israel, Leonardo Auernheimer at Texas A&M University, and Avi Weiss at Bar Ilan University). Perhaps many economists can tell a similar story.

Given the great importance and influence of Chicago, Professor Boyer's contribution – if true – is very big indeed for the intellectual history of our profession. Nobody that I know of has made these claims before, at least not so forcefully. I found on *Jstor* real-time reviews of those Johnson and Mundell books that Professor Boyer refers to. None of the reviewers mentioned Friedman. Is it really the case that in international economics "it's all in Friedman?"

Professor Boyer made a meticulous and impressive research of primary sources, including talk transcripts, obscure references, drafts, personal letters, and editorial correspondence. He has a great advantage in personally knowing the people he is writing about. My situation is quite different. I never met any of them, I am new to (economic) history, and this is my very first attempt at history of economic thought. However, I am trained in law. I shall therefore play the attorney for Mundell and Johnson in an imaginary plagiarism lawsuit brought by Friedman and his attorney

Boyer, and let the readers be the jury. As is the case with most lawyers, I do not necessarily believe some of the points I make below.

My defense starts with an acknowledgment that Mundell's and Johnson's ideas did appear in Friedman's 1953 chapter. The defense argues that Mundell and Johnson were merely negligent and sloppy with regard to the citation. There was no malice or intent: They did not plagiarize intentionally and did not cover it up. To make my point it is sufficient to consider as an example Professor Boyer's allegations regarding optimum currency areas:

1. Mundell misrepresents Friedman on the cause for sticky prices and wages.
2. Mundell mentions the sterling area for motivation, like Friedman, but without citing Friedman. He doesn't cite Friedman in the actual analysis.
3. Johnson covered it up by removing Friedman's contribution from Caves and Johnson's *Reading in International Economics*.

Let us examine these issues closely.

Sticky Prices and Wages

Friedman writes in p. 165: "internal prices are highly inflexible . . . Wage rates tend to be among the less flexible prices." Friedman also writes in p. 202 about "institutional rigidities in internal price structures." Mundell, referring to Friedman's argument, writes: "The argument is based on money illusion"[36] and, in more detail, "money illusion in the bargaining process between unions and management (or frictions and lags having the same effects)."[37] I don't see how Mundell misrepresents Friedman. One could argue that whenever I sign a nominal contract without effective indexation I display money illusion.

Mundell Not Citing Friedman

Mundell does cite a line from Friedman's p. 165. Friedman's optimum currency area discussion is in p. 193 in Section IV.D, and most of it is in an 18-line long footnote. That discussion is almost entirely about the sterling area, with a little generalization at the end of the footnote. In contrast, Mundell's discussion is general and he only gives the sterling as a one-line example. Also note that Mundell does cite later work by Meade and Scitovski and does not claim to be original on everything. I argue that Mundell probably read the content of Friedman's footnote but forgot where he read it. Alternatively, Mundell forgot that he ever read it

[36]Mundell (1960), p. 227.

[37]Mundell (1961), p. 663.

anywhere. These things happen. You read something, it gets buried in the back of your head, and it reappears in your head much later as a new, original idea.

Johnson Editing Friedman

Johnson kept almost word for word Sections I and II of Friedman's chapter but deleted Sections III and IV almost entirely. There are exceptions: One paragraph in Section I was deleted, and one subsection of Section IV was absorbed in Section II.

I argue that this is easily justified by considering the titles of these sections (my emphasis):

I. Alternative methods of adjusting to changes affecting international payments.
II. Objections to flexible exchange rates.
III. *Special problems* in the establishment and operation of a flexible exchange rate system.
IV. *Some examples* of the importance of a system of flexible exchange rates.

Those "Special problems" include the role of the European Payments Union, the IMF, gold, and the sterling area. These issues were not necessarily that important when Johnson edited the chapter in 1968. Those "Some examples" include the 1950s rearmament, also not interesting by 1968.

The subsection of Section IV which was included by Johnson and absorbed in Section II is part of Friedman's Monetary Approach to the Balance of Payments. This does not seem like a cover-up! Finally, it should be noted that even after deleting these sections Friedman's chapter is still longer than the average chapter in that volume. Johnson's editing seems entirely reasonable and is consistent with a non-conspiratorial interpretation of the facts.

I would like to make three more comments.

General Tributes

Johnson titles his paper "The case for flexible exchange rates, 1969." I see this as the greatest tribute possible, far more important than giving page numbers and detailed references. This is just like we use the terms "Keynesian," "Walrasian," and "Cobb–Douglas".

Similarly, Mundell writes in the preface to his 1968 book: "I had the good fortune in the three years from 1954 to 1957 to benefit from personal association with ..." and here he cites nine people, including Friedman.[38] He adds: "Their influence on my work will, it is hoped, not have escaped detection." Such a general

[38]Mundell (1968), p. vi.

tribute especially makes sense, instead of exact references, when it comes to someone in your own department with which you have interacted on a daily basis for a long time. This has been done by others. In his famous *The Historian's Craft*, Marc Bloch (1954) dedicates the book to his closest colleague Lucien Febvre. He writes that he cannot determine about many of the ideas in the book whether they came from himself or Febvre or both.[39] Similarly, in discussing his latest book, economic historian Russell Menard made a similar comment when he was asked to clarify a general tribute he gave to a colleague at the beginning of the book. He honestly did not know who was responsible for the ideas in the book.[40]

Friedman Not Citing Others

Friedman himself is sloppy when it comes to citations. He does not come (to this imaginary lawsuit) with clean hands. The 1953 chapter has only two references to others, even though the entire chapter is composed of claims and counter-claims, which are surely not all his own. Maybe Friedman himself instituted a sloppy standard in the profession or in his department. Johnson later claimed that Friedman's quantity theory merely reformulated Keynes' liquidity preference with different and confusing names and red herrings.[41]

Personal Interest?

Professor Boyer was a student at Chicago in those years. It might help the reader to know the nature of his relationship with them during his graduate studies and perhaps afterwards as well. Was any of them his advisor, for example?

The defense rests.

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[39]Bloch (1954), pp. v–vi.

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Part II
Models, Pedagogy, Policy and Crisis

The Keynesian Revolution and IS-LM: From Enigma to Conundrum

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Abstract This paper attempts to place the IS-LM model in the framework of the Keynesian revolution, a matter still subject to conjecture and debate. Substantive questions relating to this issue are posed, and suggestions are made regarding how to resolve them – rather than attempts to answer them per-se by reference to the conventional wisdom. Cognitive and operational levels of analysis are specified, with focus upon questions relating to *continuity*, *cardinality*, *conflict* and *consensus* regarding the relationship between the model and the Keynesian revolution. The *plasticity*, *metamorphosis*, *dynamization* and *canonization* of the model are then dealt with, again, in the context of its relationship to the Keynesian revolution.

Introduction

Over two decades ago, in the first Hicks' Lecture, Solow observed that "to a large extent, the IS-LM model for almost 50 years has been Keynesian economics", adding the caveat "though only a part of Keynesian economics it is fair to say" (Solow 1984, p. 14). Prior to, and following this lecture, which Solow entitled "Mr. Hicks and the Classics", much work appeared on the *nature and impact* of IS-LM. Indeed, one aspect of the divide between the "two Cambridges", could be described as the demarcation between IS-LM of Hansen–Samuelson–Klein–Modigliani vintage, as against the approach to Keynes's *General Theory* according to the Kahn–Robinson–Pasinetti interpretation.

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What was lacking, however, was an account of the *origins and development* of the IS-LM equations and diagrams. A first attempt was made in *Interpreting Mr. Keynes* (Young 1987), which I prefaced by citing the old African adage "what goes around comes around"; a second in my 1995 paper "IS-LM: an inquest"; a third, more recently, in "Is IS-LM a Static or Dynamic 'Keynesian' model?" (Young 2008a); and finally, in my survey of its diagrammatic development, in Blaug and Lloyd's forthcoming volume, *Famous Figures and Diagrams in Economics* (Young 2010).

These works focused on the IS-LM model *per-se*, *as against its place in the context* of the "Keynesian Revolution". Laidler, in *Fabricating the Keynesian Revolution* (Laidler 1999), started to deal with aspects of this. Now, the subtitle of *Interpreting Mr. Keynes* was "the IS-LM Enigma". While an account of the model's origins and development was provided there, and expanded upon in the later works mentioned above, questions still remain regarding its place in the "Revolution", the answers to which are in the realm of *conjecture*, and thus the title of the present paper.

Some of the themes to be dealt with here were addressed by Patinkin, some two decades ago, in another context, in his seminal paper "On different interpretations of the *General Theory*" (Patinkin 1990), in which he counterpointed the IS-LM interpretation with what he called "'The Modern Cambridge School' *cum* Post-Keynesian interpretation" (Patinkin 1990, p. 214, note 6). A decade ago, Vercelli (1999) focused on other themes I shall deal with below, albeit, regrettably, somewhat distorting the evolution of what he termed "first generation IS-LM models" (Vercelli 1999, p. 4) by only dealing with that of Hicks (Hicks 1937a), and not even mentioning the contributions of others, i.e. Harrod, Meade, Reddaway, and Lange (see Young 1987). Vercelli then went on to describe what he called "a second generation of IS-LM models", such as those of Fane (1985) and Koenig (1993a, b), among others (Vercelli 1999, p. 12). However, again, he totally overlooked the *actual* "second generation" of IS-LM models, such as those of Hansen, Modigliani, Klein and their *dynamic* extension of IS-LM, but more about this below (also see Young 2008a).

In this paper, I will – in the spirit of Robinson (1977) – pose a number of questions that should be asked if we want to put the IS-LM story in the context of the "Keynesian Revolution". I will *not* attempt to provide *specific* answers. Where possible, however, I will try to suggest *how* to answer questions raised by what, after over seven decades since the model's inception, still remains, in my view, the IS-LM *conundrum*.

I will divide questions into two areas of concern, first, on the *cognitive*, and then on the *operational* level of analysis. With regard to the former, focus will be upon issues of *continuity, congruence, cardinality, conflict and consensus* within the framework of the relationship between IS-LM and the "Keynesian Revolution". As for the latter, the questions relate to the "extensions" of the model via *plasticity* and *metamorphosis*, again in the context of its relationship with the "Keynesian Revolution", the extensions of the model via *dynamization*, and the issue of *canonization*.

Cognitive Level of Analysis

Contiguity and Congruence

A major question that follows from Solow's observation, cited above, is: what is the relationship between IS-LM and "Keynesian ideas"? I suggest that in order to put this question into historical context and possibly address it, we could consider the somewhat *overlooked* retrospective paper, *in French*, by Hicks entitled "The Theory of Keynes after Nine Years" (Hicks 1945), and its relationship with his earlier "Mr. Keynes and the Classics'" (Hicks 1937a). Indeed, the 1945 paper did not even appear in the bibliography of the recent conference volume on Hicks's contributions to economics, *Money, Markets and Capital* (2008).

Now, perhaps the most oft-cited statement made by Hicks in his 1937 paper is "So the General Theory of Employment is the Economics of Depression" (Hicks 1937a, b a, p. 155). In his 1945 paper, however, Hicks changed his view. Because the relevant passages are not well known, they appear below in translated form. As Hicks put it (Hicks 1945, p. 2):

> . . . In 1937 I myself wrote that that the General Theory, is the economic theory of depression.
>
> *We now know that it is a mistake* [my emphasis]. The theory has been considerably strengthened and it has earned the support of economists who, on its debut, were very skeptical (as Beveridge and to some extent, Robbins). Now we have discovered that is not less applicable to conditions that constitute the other extreme in relation to depression: the boom conditions of a full economy mobilized for war. It is while trying to apply the General Theory to the economic theory of war that the instrument of evaluation of national income has been developed (British White Papers and similar documents in the United States).

Hicks continued:

> Another area in which new applications have been developed is that of international trade . . . *Keynesian ideas* [my emphasis] are now widely applied to the problem of international trade, analytical and historical assessments, as well as from the standpoint of practical politics.

Hicks qualified this by saying:

> The absence of any *formal theory* [my emphasis] of international trade is one of the shortcomings of the most striking book by Keynes.

From the passages cited above, we can readily see that as early as 1945, Hicks *demarcated* his own 1937 SILL approach (IS-LM) from an overall Keynesian approach that emanated from Keynes' "striking book", ranging from the economics of "depression" and "boom" in a mobilized economy under wartime conditions and the application of "Keynesian ideas" to international trade – on both theoretical and practical levels, as manifest in the works of Machlup (1943) and Nurkse (1944) [Hicks 1945, p. 2 note 3].

Moreover, in his 1945 paper, Hicks does not utilize his 1937 SILL diagram, or its related equations, for economic analysis. His diagram is in {Y, I=S} space, with a

vertical income axis and horizontal I=S axis, a savings "curve", as a function of income, originating on the vertical income axis above the origin, rising from left to right, and cutting a vertical investment line (parallel to the income axis) from below. He then adds a "YS" line to represent the locus of points where *all* income is saved (Hicks 1945, p. 4). Hicks then proceeds to present a "sequence analysis" equation system (Hicks 1945, pp. 6–7), similar to those of Samuelson (1941b), Machlup (1943), and Lange (1944) respectively, writing [again translated from the French] (Hicks 1945, p. 7):

The recurrence formula underpinning this sequence analysis is as follows:

$$\begin{aligned} &Y_{n+1} = C_{n+1} + I \quad C_{n+1} = \text{function of } Y_n = Y_n - S_n; \\ &\text{with } S_n = S(Y_n), \text{ taken on the curve S}: \quad Y_{n+1} = Y_n + (I - S_n). \end{aligned}$$

Using his "*Value and Capital* Method" (Young 1987), i.e. reducing an equation system to a two-dimensional diagram, and also applying his *Value and Capital* (Hicks 1939) notion of "weeks", Hicks then presented his 1945 equation system diagrammatically in {Y, I=S} space, and obtained a "construction" which was analogous, as he put it, to the process represented by "the cobweb theorem" (Hicks 1945, p. 7).

The issue of contiguity and congruence regarding the relationship between IS-LM and "Keynesian ideas" was, then, dealt with by Hicks via the *demarcation* of SILL (IS-LM) – both in equation and diagrammatic form – from the equation and diagrammatic system he proposed (Hicks 1945) as a representation of "Keynesian ideas". This follows from "caveats" in his 1937 paper, which were somewhat overlooked by those who later transformed his "little apparatus" (Hicks 1937a, p. 156) into "the core" of modern macroeconomics (Dornbusch and Fischer 1984, p. 100), or at least of its textbooks (e.g. Gordon 1987; Blanchard and Fischer 1989; Mankiw 1992; Blanchard 1996).

As Hicks put (1937a, p. 158), his generalization of "Mr. Keynes theory begins to look very like Wicksell's"; given that the IS-LM "skeleton apparatus" is, as he wrote, "a *slight* extension [my emphasis] of Mr. Keynes' similar skeleton", albeit "a terribly rough" extension, in which "the curves are not really determinant unless something is said about the distribution of income as well as its magnitude". Hicks went on to say that what the curves "express is something like a relation between the price system and the system of interest rates; and you cannot get that into a curve". Finally, he wrote that all questions regarding depreciation and "the timing of the process under consideration" in his 1937 approach "have been neglected" (Hicks 1937a, p. 158).

By counterpointing Hicks's 1937 and 1945 papers, a first approach could be made in attempting to address the question of the relationship between his "little apparatus" of 1937 vintage, i.e. IS-LM, and the overarching nature of a "Keynesian revolution" in economic ideas, as seen in the approaches of *both* Cambridges'.

But even more is involved regarding Hicks's 1945 diagram. Klein, in his widely-read 1947 book, *The Keynesian Revolution*, used what is essentially the Hicks diagram (Klein 1947, p. 76, Fig. 2) to illustrate the "Keynesian model" that provided,

in his view, "a basic theory which replaced the classical savings-investment theory of interest" (Klein 1947, p. 76). Klein's diagram has similar axes and schedules to that of the Hicks diagram, and if the axes in Klein's diagram are rotated, the two diagrams are *identical*.

Now, it should be recalled that Klein's 1947 book is based upon his 1944 MIT Ph.D thesis of the same title. It would seem, then, that *both* Hicks and Klein realized the importance of the *diagrammatic representation* of the "Keynesian model", as Klein put it, that "replaced" the classical model that determined the interest rate, and *both recognized it as a central component* of Keynes's "revolution".

Given this, the question may be asked: Why didn't what I would term the *Hicks–Klein diagram* become more widely recognized and utilized? Could the reason be that it never attained the importance given to it by Hicks and Klein due to the fact that what later became known as the *Hicks–Hansen diagram* (IS-LM) had come to "rule the roost"?

Cardinality, Conflict, and Consensus

Following from the above, the question may be asked: how many "Keynesian Revolutions", and which should be considered as *primus inter pares*? As the saying goes "let us assume, for argument's sake", that we can *count* at least *five* sets of ideas (and models) of the "Keynesian type" in *closed-economy* theory, and can also assign them the term "revolution" – at least with regard to their impact on theory – despite the fact that some sets of ideas can be said to have exhibited some "continuity" with previously held theories, or synthesized "new" with "old" ideas, while others – by projecting external elements onto the structure of what was taken to be *the* Keynesian model – generated *mutually exclusive* approaches. These five sets of "Keynesian ideas", to use Hicks' term (1945, p. 2) can be seen in Keynes's *General Theory* (1936); in the earlier and later Robinson (1937, 1971), Kahn (1984), and Pasinetti (1974) interpretations; in the one and two sector *mathematical models* developed by Harrod, Meade, and Hicks respectively (Darity and Young 1995); in the *neoclassical synthesis* of Hansen (1941, 1947, 1949), Samuelson (1941a, b,1947), Klein (1944, 1947), and Modigliani (1944a, b), and in the set of ideas that has come to be known as the "New Keynesian Macroeconomics". In the latter context, almost two decades ago, Mankiw pronounced Lucas's "obituary" for Keynesian economics (Lucas 1980), as "premature" (Mankiw 1991, p. 1). A discussion of whether Lucas or Mankiw have been, or will be, proven right is beyond the scope and purpose of this essay. Suffice it to say, however, that despite its initial "promise", the "New Keynesian Macroeconomics" of Mankiw and others has, in my view, not *displaced* the dynamic Lucas–Sargent–Prescott approach. Rather, there seems to be an anomalous situation in which a number of "toolkits" co-exist and vie for pride-of-place in macroeconomic analysis.

Thus, while it can be said that each set of "Keynesian ideas", to use Hicks' term, *may* encompass some "revolutionary" aspect, whether *all* constitute *the* "Keynesian

Revolution", or any one is *the* "Revolution", in its own right, *is a question that remains to be answered*.

Now, the issue of whether there was a "methodological Keynesian revolution" has recently been dealt with by Dow (2008). In her view, "there has not been a Keynesian revolution at the level of methodology that is consistent with Keynes's approach to economics" (Dow 2008, p. 12). She went on to say "the methodology of [macro] economics evolved after Keynes, with conventional interpretation of Keynes's macroeconomic innovations playing an important role". But, in her opinion, "the growth of mathematical formalism meant a significant divergence from Keynes's approach" (Dow 2008, p. 12).

How then, can we account for Solow's confirmation, in his Hicks' lecture, of Tobin's view that "IS-LM has become the trained intuition of many of us" (Solow 1984, p. 14)? One possible way of dealing with this question is to *conjecture* that *despite* the *"methodenstreit"* between various interpreters of Keynes *and* their interpretations, a *consensus* was reached between those whose advocated "Keynesian ideas" and those who opposed them, i.e. the "monetarists", at least regarding IS-LM. This consensus enabled *both* groups to engage in economic discourse and debate *via* the IS-LM model.

Thus, for example, prominent "Monetarists" such as Friedman could debate with prominent "Keynesians" such as Tobin in "IS-LM space" (Friedman 1974), while monetarist–oriented policy makers such as Francis, President of the St. Louis Fed, could deal with policy prescriptions also in terms of the IS-LM model. Indeed, as he put it (Francis 1973, p. 3):

> As a Federal Reserve policymaker I must live in the real world. Therefore, advice from my staff that I should support a policy that would shift the LM curve is of very little use to me. As a member of the Federal Open Market Committee I know the actions I can vote for are changes in Federal Reserve holding of Government securities. As President of a Federal Reserve Bank, I can recommend to our Board of Directors that they should submit a change in our Banks' discount rate. I cannot recommend to the Open Market Committee that the LM curve should be shifted one way or another. I can only recommend actions in terms of the instruments at hand.

This ostensible consensus regarding the IS-LM interpretation of the Keynes's *General Theory* also enabled politicians to state that "we are all Keynesians now". There are, however, some possible *alternative* answers to the question of *whether IS-LM actually became* the "trained intuition of many of us", but they are on the operational level, and it is to these that I now turn.

Operational Level of Analysis

Extensions Via Plasticity and Metamorphosis

Two types of "extension", to use Hicks's term, can be identified in the case of IS-LM. The first is via the plasticity of the model, the second by its metamorphosis. Starting from Keynes's *General Theory*, the initial "extension", as Hicks put it in 1937, was his

SILL model, and the mathematical models of Harrod and Meade. Given the plasticity of the model, following this, the next *set* of extensions can be seen in the works of Hansen, Samuelson, Klein, and Modigliani. Further extensions followed, especially as regards the diagrammatic representation of the model (see Darity and Young 1995).

As for *metamorphosis* of the model, one example is evident in the transformation of IS-LM into the AS-AD model, which is ostensibly derived from it. Another can be seen in the emergence of the IS-LM-BP and Mundell–Fleming models in the context of open economy macroeconomics, *although the origin and development of the latter is problematic, to say the least* (see, for example, Boyer and Young 2005; 2010). A further example is the extension of the model, *without LM*, into the Taylor–Romer model (Taylor 1998, 2000; Romer 2000). Indeed, the Taylor–Romer model has, most recently, undergone a *total metamorphosis* into "Keynesian economics without the LM and IS curves", by means of its "dynamic generalization" (Koenig 2008).

The question then arises, given these "extensions", why has the model fallen into disfavor and even disrepute amongst most economists, while *concomitantly*, when wearing their "economics teachers" hats, *most still utilize the model*, rather than better alternatives such as the Taylor–Romer model, at least, as regards representation of monetary policy. An attempt to answer would lead us around to the cognitive level and the reason for *cognitive dissonance* exhibited by economists regarding IS-LM. But that is another element in the conundrum, and the history of this phenomenon beyond the scope of the present paper (see the papers in the volume edited by DeVroey and Hoover 2004; also see Darity and Young 2000, for a discussion of *cognitive dissonance* in the case of IS-LM).

Extensions Via Dynamization

A further question can be posed regarding the extension of the static IS-LM approach into more sophisticated *dynamic* models. Now, the history of AD-AS and Taylor–Romer been dealt with by Dutt (2002) and King (2000) respectively. However, what has not been considered is the extension of the IS-LM model into the realm of dynamic economics; something which I attempted to do only recently (Young 2008a).

Once again let us go back to Hicks's 1937 paper. He ended his paper with what in my view was a *prescient* statement. As he wrote "The *General Theory of Employment* is useful book; but it is neither the beginning nor the end of dynamic economics" (Hicks 1937a, p. 159). Hicks may have been thinking of his own effort, as manifest in what was to become the Mathematical Appendix to *Value and Capital* (Hicks 1939), which was published in booklet form (in French), 2 years earlier (Hicks 1937b), and reviewed by Bowley in the *Economic Journal* (Bowley 1938). However, as I recently noted, Hick's (1937b) booklet only contained one sentence on dynamics (Hicks 1937b, p. 53; Bowley 1938, p. 515; Young 2008b, p. 112).

Now, much attention has been focused on Harrodian and Hicksian dynamics of 1939 vintage (see, for example Young 1987). However as I recently observed (Young 2008a), the *dynamic IS-LM models* of Modigliani, Samuelson and Klein respectively were put on a "back-burner" for the most part. This *richer dynamic* approach was, in effect, with some exceptions, shifted to one side until recently (e.g. McCallum and Nelson 1999; Bénassy 2006, among others), and the *leaner static* approach became "our trained intuition". The question that I would pose here is simply: Why? [As a first attempt to answer- these *dynamic* IS-LM models of *earlier* vintage *may* have exhibited similarities to *modern DSGE* models with wage and price rigidities, the implications of which deserve further study, that is to say, if this is the case, then could it be the *reason* why the *dynamic* IS-LM models of Samuelson, Modigliani and Klein were put to one side?; on the possibility of integration between IS-LM and DSGE models, see Bénassy (2006), p. 2].

Canonization and Dissemination

A final issue that must be resolved emerges in the context of the set of questions that comprise the IS-LM *conundrum* relates to the issue of *canonization* raised by Patinkin almost two decades ago. In his view, the various interpretations of *General Theory* resulted from the fact that it was a "canonical" text that could "attract different interpretations. . .." He said that this was not only the result of "the obscurities and loose ends of the *General Theory"* but, in his view, the outcome of an exegetical process whereby "some interpreters wanted to invoke the authority of a canonical text in support of their prior theoretical. . .and. . .political views. . ." (Patinkin 1990, pp. 233–235).

The final question that could be asked here, then, is: was the dissemination and "extension" of IS-LM the *outcome of the theoretical predilection of its proponents and expositors*, or was the process of its extension simply the result of the *inherent or intrinsic properties* of Hicks's "little apparatus" of 1937, rather than, say, his "construction" of 1945, as representations of Keynes's *General Theory*?

These are only some of possible questions. But I would leave it to the next generation of economists *qua* historians of economics to provide the answers. One can only hope that they will take an interest in probing further into the *development and dissemination* of their "trained intuition" and its relationship to "The Keynesian Revolution".

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Comment on Warren Young's Paper

M June Flanders

Warren Young has given us a thorough review of the IS-LM apparatus. I like what he has done.

I use the term apparatus advisedly. I do not call it a theory, nor does Young. It is, rather, a "device", which is what Hicks himself called it when he developed it in 1937.

He has done us a great service by rescuing Hicks's French revision of the apparatus in 1945 from the anonymity caused by the unilingual bias of the economics profession. It is highly significant that Hicks himself revised his earlier view that the General Theory was simply a special case of a more general construct – that is, the "Economics of Depression".[1]

My reservations about IS-LM are twofold.

First, it is a charming apparatus, but hardly a theory of how an economy behaves. In 1937 Hicks himself referred to his construction as a "little apparatus", though by 1945 he was developing it into a dynamic model, albeit a fairly simple and general one. It may be a useful tool to permit scholars of opposing schools to highlight their differences in terms of a common language, regarding the slopes of the several curves, for example. But it ignores the richness of what Young has called the other Cambridge interpretation of the *General Theory*, and certainly ignores the widely neglected, but in my view enormously important and elegant, interpretation of Shackle, for example, stressing the crucial role of uncertainty in the income-determination model, especially, but not exclusively, in the demand for money.

Hicks, in his discussion in the brief paper, was more aware of the subtleties of interpretation than the subsequent mechanistic applications and interpretations of the "apparatus" recognized. For, as he said, and as Klein (1952) noted, he singled out the liquidity preference function as the hallmark of the Keynesian *Weltanschauung* – which is consistent with Shackle's view of Keynes (1967, 1983).

This implies criticism of the disregard for Hicks's caveats, not only because it trivializes the *General Theory*, as suggested, but also because I find the apparatus a very dubious, even pernicious, pedagogical tool. Students who have been taught to toss about curves or lines of opposite slope and to zero in on their intersection do so with alacrity and frequently fail to comprehend that these curves do not represent simple behavioral functions, like supply and demand. I have observed this among college sophomores and advanced PhD candidates as well. As a teaching device, McKenna's old textbook (1965), for example, with separate but linked diagrams,

[1]As an aside, I was pleased to note that Hicks commented there that the "absence of any formal theory of international trade is one of the shortcomings of the most striking book by Keynes." In fact, of course, it was the only book by Keynes dealing with a closed economy.

gives a much more accessible picture of what in fact is going on – or believed to be going on.[2]

Young speaks admiringly, as well he should, of later applications and uses of the construct, including that of Klein. But on rereading Klein, after 50 years, I remain convinced that the significant point Klein was making was that the full employment S=I might occur only at negative interest rates (85).[3]

Young mentions the "caveats" in his 1937 paper, "which were somewhat overlooked by those who later transformed his 'little apparatus' (1937a, p. 156) into 'the core' of modern macroeconomics or at least of its textbooks. I agree strongly with the thrust of that comment. Hicks's article, as I remember it, specifically states that it is a way of looking at various theories (it is *not* a theory) so that each is a special case of a general statement". . . . in Sheila Dow's opinion, "the growth of mathematical formalism meant a significant divergence from Keynes's approach".

In short, I maintain that the purpose of the Hicks paper was to argue that Keynes and the Classics represented special cases of a general theory, and that this was transformed into a pedagogical device of possibly useful and frequently dangerous value. The true impact of Hicks's brilliant insights into macroeconomics was revealed, in my view, only in his *Value and Capital*, in 1939, but that is another story.

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[2]I remember the first edition, which I have not been able to refer to at present, with pull-out pages at the end showing every component of the reduced form IS and LM curves. Later editions show similar derivations within the text, which are somewhat more complicated for the student to work through, but equally complete and detailed.

[3]When he discusses Hicks, briefly, he has a diagram showing a very steep S = I function intersecting the long horizontal segment of a liquidity-preference function, so that a large decline in the interest rate elicits a small increase in equilibrium income. His mention of "Mr Keynes and the Classics" appears only in what Klein calls his 'Polemical' Chapter, in which he accuses Hicks of rejecting Keynes's "long-run prophecies that the marginal efficiency of capital would decline secularly" (100). Klein was then still in his Marxist stagnationist phase.

The Keynesian Method, Complexity, and the Training of Economists

David Colander

Abstract This paper argues that there are serious problems both with the grand synthesis IS/LM macroeconomics and with the modern DSGE macroeconomics. It argues that the reasons for the problems are not with the models per se but with the way the models are used. Specifically, it argues that both approaches have deviated from the Keynesian method, which in the author's mind, should have been the most important legacy of Keynesian economics. The paper provides a brief history and explanation of how the profession evolved from Keynes' writings to the modern DSGE approach. It argues that the problem was that academic incentives drove the profession away from the Keynesian method and instead directing economists to become more concerned about publication than about understanding. The paper concludes by explaining why the loss of the Keynesian method is of concern.

Keynes is dead; dynamic programming; Keynes is still dead. That's the way Stanford graduate economics students recently summed up what they had learned in their core graduate macroeconomics course (Colander 2007: 152). Graduate students at other top U.S. programs concurred, and in my recent interviews with them the strong feeling among students was that the core graduate macro course provided them little in the way of macro policy thinking and that the course had nothing to do with Keynes. It was a course in which, by design, students learn dynamic stochastic programming.[1] A comment of an M.I.T. student was representative of

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[1]At an AEA session on the core with top economists from Harvard, Chicago, M.I.T., and Columbia, all agreed that macro as it is taught now could be eliminated from the core as long as the core offerings were changed so that students would get the dynamic stochastic control theory in some core course.

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students' view of what they learned about policy in macro. He stated: "Monetary and fiscal policy are not abstract enough to be a question that would be answered in a macro course" (Colander 2007: 169).

As Robert Solow (2007) points out, the situation is a far cry from the core macroeconomics courses of the grand macro synthesis period of the 1950s–1970s period when students studied "Keynesian synthesis" models and macro policy in their core courses. In the synthesis period students learned variations of IS/LM models, and how that IS/LM type of reasoning underlay both large econometric macro models and macro policy. Compared to modern DSGE models, the synthesis models were technically simple, and macro graduate student training of the time was not highly technical. It involved a blend of institutional, historical, and policy training. In this grand neoKeynesian/neoclassical synthesis, macro theory, empirical work, and policy were entwined in a superficially connected, but ultimately unsatisfying, set of models that built differences of policy position on slim reeds such as Pigou effects and wage and price rigidity assumptions. The models could be adjusted to "explain" just about any observation, which meant that, more often than not, researchers' judgments determined the results of the model. By that I mean that it was a synthesis in which one could predict the policy implications of models based on who was doing the modeling. It was hardly a situation that inspired confidence in the usefulness of the models.

As should be clear from the above description I am no fan of the grand synthesis macroeconomics. But I am also no fan of modern DSGE macro as a basis for policy analysis. Robert Solow nicely captured my view of current macro theory when he wrote that modern macro is best seen as a "rhetorical swindle" that the "macro community has perpetrated on itself, and its students" (Solow 2008: 235).

In this paper I (1) explain why I am not a fan of either grand synthesis macro or modern DSGE macro and how both have deviated from the Keynesian method, which I believe should have been the most important legacy of Keynesian economics; (2) provide my explanation of how the profession moved from Keynes' writings to the modern DSGE approach; and (3) explain why I believe the loss of the Keynesian method is of concern.

Losing Sight of the Keynesian Method

The grand synthesis and the DSGE models fail for me because they have given up a central element of the Keynesian method. The Keynesian method takes seriously Mill's famous half truths proposition (Mill 1838) that, at best, any formal model will give us only a partial, highly imperfect, picture of the an economic reality. To move from formal models to policy one must have additional knowledge of economic reality that goes beyond knowledge of the economic model. The Keynesian method held that, at best, theoretical models in economics can only be used to guide researchers' judgment, not to arrive at policy conclusions directly from

theoretical models. It is a method that accepts the limitations of theory, empirical work, and intuition in understanding the economy. It accepts our inability to develop a complete scientific model of the macro economy and sets its goals much lower: provide a set of tools that may help answer certain questions about how the macro economy works.

The Keynesian method has a long history. While Keynes may have differed with Classical economists on many issues, he did not differ with them on what was the appropriate method to use, and his work is best seen within the Millian/J.N. Keynes/Marshallian methodological tradition. My problem with both the grand synthesis and the DSGE approach to macro is that they have given up this method and the advocates of both do not see their models as complements with different purposes, but as substitutes. This makes reasonable discussions of the advantages and disadvantages of the two types of models almost impossible.

Keynes summarizes his method succinctly in the following passage. He writes:

> Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world. It is compelled to be this, because, unlike the typical natural science, the material to which it is applied is, in too many respects, not homogeneous through time. The object of a model is to segregate the semi-permanent or relatively constant factors from those which are transitory or fluctuating so as to develop a logical way of thinking about the latter, and of understanding the time sequences to which they give rise in particular cases. Good economists are scarce because the gift for using "vigilant observation" to choose good models, although it does not require a highly specialized intellectual technique, appears to be a very rare one. (Keynes 1938)

There are a number of aspects of this quotation that deserve highlighting. The first is that the reference to models is plural. In the Keynesian method, there is not a single model of the economy, or even of part of the economy; there are many. The second is that intuition and judgment need to be given a separate role in arriving at a policy conclusion. Keynes accepted that pure theory and analytic methods will get us only so far in understanding the events around us, and in arriving at policy conclusions. For Keynes, choice of models was an art, not a science with a well defined methodology.

The reason this art/science distinction is important is that to practice the art of policy economics usefully, one needs a different set of skills than those necessary for an economic research economist such as a theorist or an applied econometrician whose primary outputs are academic papers for other academics. To be a policy economist, one needs another skill, which might be called intuitive wisdom. It is a skill that includes a knowledge of how institutions actually function, a sense of history, a knowledge of what is happening in other fields of study, an understanding of real-world politics, and a practical sensibility. All these skills are central to economists' role in policy making. It is those skills that allow the policy economist to choose among models.

A follower of the Keynesian method would never move directly from a model to policy unless he or she believed that the model was the relevant one for the question at hand. He or she would keep a meta model of the relevance of various models at

the back of his or her head, and adjust the specific model's conclusions to fit the particular circumstances. In the Keynesian method, models are tools, not rules.[2]

Keeping thoughts in the back of one's head is definitely not formal science; the Keynesian method fully accepts that. For that reason it makes as strict a separation of policy from the formal or pure science of economics as it can.[3] This made what might be called the Policy Separation Proposition central to the Keynes' method: The policy separation proposition holds that if a researcher doesn't have expertise in the specific institutions and real world issues relevant to the problem at hand, then he or she should not make any strong pronouncements about the model's implications for policy as a scientific pronouncement. Such pronouncements should be left to others who have the appropriate training, or be made by the economist with the caveat that he or she is not speaking with the authority of science.

As I stated above, this Keynesian method did not begin with Keynes. It was a standard tenant of Classical economics, and in adopting it, Keynes was accepting Classical methodology. What he objected to in Classical economics was not its method, but its focus on a particular model that centered on long run equilibrium and did not consider potential problems in arriving at the long run equilibrium.

To see how central this method was to Classical economists, consider three Classical economists who took a strong interest in methodology: Nassau Senior, J. N. Keynes, and Lionel Robbins. Nassau Senior writes:

> (The economist's) premises consist of a very few general propositions, the result of observation, or consciousness, and scarcely requiring proof, or even formal statement, which almost every man, as soon as he hears them, admits as familiar to his thoughts, or at least as included in his previous knowledge: and his inferences are nearly as general, and, if he has reasoned correctly, as certain, as his premises.
>
> But his conclusions, whatever be their generality and their truth, do not authorize him in adding a single syllable of advice. That privilege belongs to the writer or statesman who has considered all the causes which may promote or impede the general welfare of those whom he addresses, not to the theorist who has considered only one, though among the most important of those causes. The business of a Political Economist is neither to recommend nor to dissuade, but to state general principles, which it is fatal to neglect, but neither advisable, nor perhaps practicable, to use as the sole, or even the principle, guides in the actual conduct of affairs. (Senior 1938: 2–3)

[2]Keynes writes, "It is a great fault of symbolic pseudo-mathematical methods of formalizing a system of economic analysis. . .that they expressly assume strict independence between the factors involved. . .; whereas, in ordinary discourse. . .we can keep 'at the back of our heads' the necessary reserves and qualifications. . .in a way in which we cannot keep complicated partial differentials 'at the back' of several pages of algebra which assume they all vanish." (Keynes 1936: 237).

[3]There are many different definitions of science; Classical economists were referring to a very narrow definition, which A. C. Pigou called "light-bearing" science, when they advocated strict separation. They had other work which Pigou called "fruit-bearing" science, and that was designed for policy analysis. This work was more engineering than science, and was not to be separated from policy. But it was also not to be given the imprimatur of science. It was a tool to be used in policy analysis, not meant to be treated as fully scientific, but rather as a rough and ready guide for particular issues.

For Senior and for most early Classical economists concerned with methodology, economic science of the time was a branch of logic. In the pure science of economics at the time one did theory, which meant that one developed theorems from almost self-evident principles. But, as Senior makes clear, economic theory was not meant to directly guide policy. To move from the theorems of the science of economics to the precepts of policy-relevant economics, they believed that one had to rely on common sense judgment and institutional knowledge, which involved different skills than did economic theory.

This separation of theory and policy method was strongly advocated by J.M. Keynes' father, J.N. Keynes, in his famous summary of economists' methodology at the turn of the nineteenth century (Keynes 1891). Like Senior, J.N. Keynes saw the pure science of economics as a relatively narrow branch of economics that needed to be strictly separated from the branch of economics that dealt with policy prescriptions. He argued that the two branches of the field of economics had quite different methodologies. He writes that "a definitive art of political economy, which attempts to lay down absolute rules for the regulation of human conduct, will have vaguely defined limits, and be largely non-economic in character" (J. N. Keynes 1891: 83).

Lionel Robbins was equally clear about the need to strictly separate theoretical models from policy. In his Ely Lecture (Robbins 1981), Robbins reflects back on how his famous 1932 essay his 1935 book (Robbins 1935) was incorrectly interpreted by the profession. There he states explicitly that the economics profession needs a separate branch, which he called political economy, to deal with policy. He writes that this policy branch of economics "depends upon the technical apparatus of analytical Economics; but it applies this apparatus to the examination of schemes for the realization of aims whose formulation lies outside Economics. . ." (Robbins 1981: 8).

How the Profession Moved Away from the Keynesian Method

Economists' training up until the 1930s was designed to train economists in both the art and science of economics. This could be done because at the time the formal science of economics was underdeveloped, and for the most part, its theory was heuristic and verbal; formal, mathematically precise theoretical work was only done by a small number of economists. Up until the 1930s mathematical economics was the outlier, not the norm, since at the time an economist's training was not designed to create economic scientists. Similarly, an economist's training at the time involved minimal statistical work. This was reasonable since then empirical statistical work was in its infancy and was not seen as central to economic science.

The underdeveloped nature of both technical theory and statistical work left more time open for training in literature, history, and institutions. It allowed Classical economists to develop an expertise in both the science of economics and in applied policy economics. Because of the limited techniques involved, these

economists could have a firm foundation in each branch of economics. As Robbins stated, even though there was a separation it was reasonable for economists of the day to be interested in both. He writes: "To me at least, it seems difficult to believe that recognition of the distinction between the two kinds of propositions will prevent any man of spirit from being interested in both" (Robbins 1938: 345).[4]

Method Selection Mechanism Within the Economics Profession

The acceptance that it was impossible to directly draw policy advice from models started to change in the 1930s, when economists attempted to more directly relate theory and policy. In micro, a separate branch of theory, welfare economics, developed, which attempted to merge the theoretical and policy branches of economics, drawing policy implications directly from microeconomic models. In macro there was a parallel movement with the development of the consumption function and the IS/LM models.

Explaining why this change occurred is beyond the scope of this paper, but my hypothesis is that the explanation is connected to the replicator dynamics of the economic profession, which in turn is connected to the training the future economist receive, and how they advance in their careers. In my view, these replicator dynamics, far more than prescriptive discussions of methodology, determine economists' method, and it was a change in those replicator dynamics that led to a change in method.

As I stated above, until the 1930s economists were broadly trained, and the profession was not dominated by researchers who had been trained in economics specifically, but rather were trained more broadly in moral philosophy. They chose to specialize in economics after receiving a broad training and their method reflected that training. As economics as a separate discipline and separate academic subject developed, the training started to get more specialized. For economic science, this was an enormous advantage; it allowed the introduction of more advanced analytic techniques, which in turn required more technical training devoted to it. Hence, the 1930s and 1940s saw myriad developments in economic theory.

These improvements in technical training came at a cost, however. As economics training moved away from the general training in theory, policy, history, and institutions that built in the Keynesian method as part of the structure of economists' sensibility, it became more difficult for economists to specialize in both the science and the art of economics. Something had to give, and in the fights over method that occurred in the late 1900s those favoring the art of economics lost,

[4]Keynes is an example of an economist with interests in both branches of economics. He was highly involved in financial markets and in advising policy makers, but because theory was not highly technical, he would also be involved in theory. So he could reasonably discuss policy from an insider's perspective and simultaneously contribute to the cutting edge of economic theory.

which sowed the seeds for a change in method. It did so gradually over decades, through a change in the training economists received. Specifically, slowly, economics training moved away from institutional, literature, and historical studies and toward more technical training in the tools of modeling.

The change in training only became substantial in the 1950s, at which time it accelerated, so that today it almost goes without saying that economics training is designed to train technical scientists in modeling techniques, not to train applied policy economists institutions or political sensibility that would be associated with the art of choosing models.[5] It is just assumed that young economists will pick up the other information should they want to become involved in policy; it is not part of their formal training. This change in training has brought about a fundamental shift in method from the 1930s up to the present day; which, because it has been gradual, has been little noticed by today's economist.

The reason it has taken so long for change in economics to occur is connected to the working lifespan of economists. The productive lifespan of an economist is about 40 years, and their immediate students will also likely reflect their teachers' views. Since students of professors tend to reflect the method they learned from their professors, a well-established method can continue to exist (at decreasing levels of intensity) for up to 80 years after that method is no longer the dominant method of cutting edge economists. Thus, while the Keynesian method started to die out in the 1930s, it took well into the 1980s for it to be largely eliminated as an acceptable method, although one still hears small vestiges of it from older economists (such as myself), well into early 2000s.

I am not arguing that the movement away from the Keynesian methodology was a conscious movement by the profession. My argument is that few economists explicitly consider methodology; they do what they do. Their methods are guided by the incentives they face, not by an explicit choice. Students coming into economics look to see who are considered successful economists and then they copy and adapt those economists' successful approaches into their own approach, while simultaneously incorporating new analytic and empirical techniques into their work. Thus, the advancement procedures economists face are central to their choices about what to study and what methods they employ. Those who do well in the advancement procedures tend to reproduce their approach and determine the direction of the field.

Academic Incentives and the Path of the Keynesian Revolution

The evolution of what is called Keynesian economics can be best understood as a result of the method-selection mechanism described above. If one reads Keynes'

[5]The argument here is not that science is irrelevant to policy. It may well be. The argument is that the primary focus of science is on abstract understanding, and the applicability of the scientific work is secondary and not the driving force behind the research. Someone could be a wonderful economic scientist and a horrendous policy economist, and vice versa.

General Theory (1936), one can come away with a variety of different views of what that work might mean. At a minimum one will get a sense that an economy could get into trouble and could end up at an undesirable equilibrium. But how and why was not clear. It couldn't be made clear since it required a level of sophistication in the mathematics of complex systems that did not exist at the time. The coordination failure issues it raised could only be meaningfully considered within a highly complex dynamic framework, and the appropriate models would involve interacting strategic agents and would likely exhibit highly complex dynamics and multiple equilibria – models that the best mathematicians of the time had not yet developed. Moreover, the empirical tools available to economic researchers at the time were too weak to actually test any of these models in a reasonable scientific way. Even today economists are far from having the expertise to formally model our economy as a complex system, and most economists do not have the technical training to work on the cutting edge of complex systems analysis.[6]

Put simply, picturing the macro economy as a complex system and a depression as the result of the intricacies of complex dynamics may have been Keynes' visionary idea behind the *General Theory*, but it was not a vision that could survive within the evolving institutional framework of professional economics of the time when mathematical economics was only starting to emerge as a separate field, and professional journals that provided a forum for specialized technical discussions were in their infancy. Given such an institutional structure of the economics profession, it is not surprising that Keynes' presentation of his theory was heuristic and that he did not deal with how his ideas could be translated into a precise mathematical presentation.

The closest that economists came to beginning to model the economy as a complex system was work by a small group of economists such as Richard Strotz (Strotz et al. 1953) and Richard Goodwin (1947). They started to work formally to cast Keynes' ideas in the mathematics of non-linear dynamics. The problem for them was that most economists of the time felt uncomfortable dealing with the complex mathematics needed to deal with non-linear dynamics. Put simply, the task was beyond the level of most mathematicians of the time and beyond the level of almost all economists. The problems that theoretical macro posed were simply too hard to crack with the analytic technology then available. Thus, these mathematical economists generated few highly successful students to carry on their professors' work and the non-linear dynamic approach faded away.

Another group of heuristic economists, such as G.L.S. Shackle (1949) and Hyman Minsky (1986), Robert Clower (1965), and Axel Leijonhufvud (1966) worked on developing an intuitive understanding of macroeconomics within that complexity vision. Their heuristic work reflected what elsewhere I have called an educated "consumer's understanding" of the complexity approach. While their

[6]See Colander (2006) for a discussion of this modern work.

work was seen as visionary, it did not develop a significant following since it did not offer a clear path forward to advance it.[7] Their work did not fare well in the competition of ideas in the post Keynesian period because that heuristic work also did not offer a research path for students that would allow those students to thrive in the then-existing replicator dynamics of the profession. Once it was pointed out that the economy is complex, nonergodic, and fundamentally subject to uncertainty, there was not much more to say heuristically.

Thus, my argument is that instead of becoming a complexity revolution that started analyzing the economy as a complex system, the "Keynesian revolution" was quickly translated into a rather mundane set of models that were more amenable to the mathematics available to the cutting edge economists at the time. Only such mundane models could do well in the peer-review replicator dynamics of the time. Thus, Keynesian economics, which could have been the beginning of a complexity revolution in economics, evolved into grand synthesis macroeconomics, which modeled the economy as a unique equilibrium, comparative static, multimarket equilibrium system, in which the only problems were institutional rigidities or wealth illusion.

Elsewhere (Colander 2006) I have discussed how Keynes' ideas were integrated with other developments in economics, and how what was called the Keynesian revolution was not a single directed revolution at all, but instead a multifaceted set of developments in theory, pedagogy, policy, and empirical work, each of which was part of an ongoing evolution in economics that was occurring independent of Keynesian economics, and which would have influenced economic thinking whether or not these developments had been placed under the Keynesian name.[8] These developments reflected the changing technology and changing institutions of the time, and often worked at cross-purposes with each other.[9] Because of its multifaceted nature it is best to think of the Keynesian revolution not as a revolution of like-minded individuals with common goals and views, but to think of it as a collection of developments in economic thinking that were classified under a broad umbrella term "Keynesian," but which could easily have been classified under various alternative headings.

The first of these developments involved theory. In the 1930s there was the movement from a Marshallian partial equilibrium approach to a Walrasian general equilibrium approach. The differences in the mathematics between the two approaches were not all that great, but the difference in method was enormous. Specifically, phrasing questions in general equilibrium involved a major movement

[7]Leijonhufvud moved into computational economics which is currently an important element of the developing complexity approach to macroeconomics (Colander 2006).

[8]Why all these disparate developments came under the Keynesian moniker is a difficult question in social thought, and one that I will not deal with here. I point it out here because in order to understand the history of macro one must recognize these disparate elements of the Keynesian revolution and that many of these developments worked at cross purposes.

[9]It is precisely because the Keynesian revolution involves so many different elements that there has always been, and still is, enormous ambiguity about what precisely Keynesian economics was.

away from the Keynesian method. It replaced the "many model" approach, in which models were aids to intuition, to a "single model" approach where the formal results of the model were given greater priority and not seen as at best a half truth. The back of economists' minds were lobotomized, and the full truth had to be found in the model.

Although Keynes followed Marshall on method, what were called Keynesian models quickly became tied in with Walrasian general equilibrium. This Walrasian general equilibrium modeling approach was quite incompatible with the Marshallian method, but it was a boon to academic researchers whose advancement depended on journal article publication. There were numerous models that needed to be developed, which meant large numbers of articles and dissertations exploring issues such as money neutrality, Pigou effects, multipliers, and alternative specifications of IS and LM curves flowed from this merging of Keynes' ideas and general equilibrium.

A second of these developments was the blossoming of statistical work in the 1940s. Because of analytic developments in statistical theory and computational technology, a new field of econometrics was developing that allowed economists to relate their models to the data in previously impossible ways. This gave some economists the hope of developing models that could be used to actually control fluctuations in the economy. As is clear from his review of Tinbergen (Keynes 1939) Keynes was not enthusiastic about this empirical work. He felt that it was too underdeveloped to shed much light on the problems. Nonetheless what was called Keynesian macroeconomics adopted it whole hog. The combination made Keynesian macroeconomics avant-garde, and gave the results of its model a scientific aura that it otherwise would not have had. It also increased its attractiveness to students enormously, providing both theoretical and empirical dissertations and articles to write.

A third development that was occurring was a change in the pedagogy of economics to accompany the changing method. Models were becoming central to the teaching of economics, and, whereas in the 1930s economics textbooks were broad treatises of reasonable insights about economic events, by the 1960s such books no longer fit the new methods that were becoming standard in economics. The change began in the 1940s. In the United States, for example, Laurie Tarshis (Tarshis 1947) was the first U.S. "Keynesian" text, but it was written in the older literary tradition, and it soon came under explicit attack by anti-Keynesians, such as members of the Veritas Society. It was quickly replaced by Paul Samuelson's text (Samuelson 1948), which was more scientific-looking, and more structured around simple formal models.

Samuelson's text became the prototype for modern economics texts. It created a separate macroeconomics, which led to the separation of the principles course into two courses: microeconomics and macroeconomics. It also introduced students to a macroeconomic framework centered on the AE/AP model and the IS/LM models, models which were further developed and used in upper-level undergraduate and graduate courses. These models served the purpose of introducing students to Keynesian ideas, but they did far more than that. For most students

the textbook models became Keynesian economics, and in doing so replaced other more sophisticated interpretations of what Keynesian economics was about. These simple textbook models helped define the nature of how people understood Keynesian ideas, providing a framework for macro that in many ways was significantly different than the framework that can be found in Keynes' *General Theory*.

A fourth development that was occurring during this period was a changing view about policy and the ability of government to control the economy. Before the Great Depression, laissez faire had strong support among economists, not as an implication of theory, but as a precept in the art of economics that combined broader insights from philosophy and political study with economic insights. The Depression challenged that view and led to discussions of the need for planning, more government control, and greater government involvement in the economy. Then came World War II, and Western democratic governments were seen as having saved our economic and political system. The end result was that, after the war, governments in general had a much better reputation in policy, while the market and laissez faire had a much worse reputation.

Economists were not immune to these changing social moods about government and, quite separately from Keynesian economics, they were exploring an expansion of government policies. Thus we had numerous calls from economists of all political persuasions, such as A.C. Pigou and Henry Simons (Hutchison 1978; Davis 1968) for activist fiscal policy to pull the economy out of the Depression. Initially these discussions were framed in the loose quantity theory/Say's Law framework of the earlier debates, but with the development of Keynesian models, and the Classical and Keynesian monikers, the policy discussion quickly changed from a discussion in which everyone started from the same quantity theory/Say's Law framework, and talked about nuances of institutions, into a discussion in which there were specific differences among economists based on the assumptions of their models. The nature of the policy differences, which were seen as separating Keynesians and Classicals, evolved over time but whatever the policy differences, they followed from the models that both sides used, which suggests to me that policy views were determining model selection and interpretation rather than the other way around.

Whatever their origins, the combination of Keynesian ideas, the analytic structure of a Walrasian type of general equilibrium model, the developing empirical work, and the "Keynesian" multiplier and IS/LM pedagogical models made up an explosive combination, creating a fertile ground for a surface "revolution" in macroeconomics. It offered teachers models to teach; researchers research to do, and empirically minded economists enormous opportunities to apply newly developed statistical techniques. But the combination also presented a problem. Because the models were so closely tied to policy issues, in many ways the science of macro did not progress during this time period, and in some ways regressed, leading people to think they knew more than they actually know.

Somehow, Keynesian economists always seemed to come up with models that showed that government intervention was good, and monetarists always seemed to come up with models that showed that government intervention was bad. This left

some observers with the sense that many of the real issues that differentiated the various positions were going unstated because those real issues were based on reasoning that was too complicated to capture in the analytic models available to researchers at the time, and the empirical tests of the data were too weak to answer the questions being asked. Somehow, no one in the mainstream of the profession wanted to say the brutal truth – the real problems of macroeconomic theory are beyond simple models.[10] One reason the profession could not deliver that verdict is that it had given up the Keynesian multi-model method, which could have held that the synthesis models were simply ad hoc engineering models designed as tools to assist economists' judgment and intuition. Syntheses macro economists were never willing to admit that their models were not scientific theoretical models, but were instead ad hoc models that might be useful for macro if one kept a good intuitive understanding of the macroeconomy at the back of one's head.

It was this failing in the synthesis models – with advocates claiming that these models were more scientific than they were – that led to the development of modern macro and the movement away from the syntheses models. Specifically, modern macroeconomics developed as a reaction to the claims of the synthesis models being a reasonable scientific model of the macro economy. A cadre of what became known as New Classical researchers set out to build such a scientific model. Undermining the synthesis was easy; New Classicists merely pointed out the many inconsistencies and problems with it. They pointed out that forward-looking agents would act differently than the agents assumed in the synthesis model, and that expectations of policy changes would change the structural characteristics of the model. These were not new insights; they had long been recognized but swept under the rug in an attempt to keep the models policy relevant. New Classical economics was not directly worried about model applicability. It was concerned with getting the scientific macro research program back on track.

The insights offered by the New Classical model were not the reason for its adoption. As I stated above, those insights – the advantage of rules, credibility, and the weaknesses of macroeconometric models – had been understood before. The reason it succeeded it that the New Classical research program offered institutional criteria for researcher success. By the 1970s journal articles had become central to

[10]The problems with the synthesis model were identified by many nonmainstream economists, but these were quickly left out of the debate. For example, there were Austrian economists, Fundamentalist Keynesians, Post Keynesians (with and without hyphens), and coordination Keynesians. These dissenting Keynesians pointed out that the synthesis missed the central elements of Keynes' views. But the same institutional pressures that pushed toward the neoclassical synthesis worked against this group of dissidents. As I discuss in Colander (2004), for a view to develop, it must offer institutional advancement for the holders of that view; it must have dissertations for students to write, articles for assistant professors to publish, and textbook expositions that can spread the seeds of the ideas to students. The dissenters failed on almost all of these criteria. In terms of ideas about how the macro economy operated that could be studied by a "scientific researcher" (as economists had come to view themselves), the dissenters had little to say other than that the macro economy was too complicated to model, and that therefore the "neo" models, which assumed away the complications, were not adding much insight into the issues.

economists, allowing for a more technical discussion among smaller subgroups of economists, and more technical models. Similarly, policy issues in macro were conducted by central banks, which had expanded their staffs and were dealing with policy issues on their own. This left the macroeconomists to explore the development of a scientific model.

The rational expectations assumption offered a method of solving models and introducing more technical analytics into the models and thus served the same purpose as did the multi-market analysis of Walrasian economics 50 years earlier. Once the assumption and models became accepted, there were numerous variations of the model for researchers to develop. Just like the early advocates of the neosynthesis advanced 50 years earlier with the development of synthesis models, young New Classical professors advanced in the 1980s with the development of rational expectations models. As the older synthesis macroeconomics and macroeconomists retired, macroeconomics changed: the teaching of graduate economics became the teaching of variations of the dynamic stochastic general equilibrium model, and the graduate course in macro became a course in introducing the students to the techniques needed to solve such models. The specific models used evolved, going through numerous iterations from rational expectations to new classical to real business cycles to the current DSGE synthesis. But by the end of the 1980s, the approach was the only approach learned in graduate macroeconomics.

Modern Macro Policy and Macroeconomic Theory

The modern DSGE approach has had far less success on the undergraduate pedagogical front and policy front. As I predicted back in the 1980s (Colander 1988), the synthesis models have held on in undergraduate macro, and there is now a large disconnect between undergraduate macro, where students are taught variations of the Keynesian synthesis model, and graduate macro, where students are told what they learned in undergraduate macro was all wrong and should be forgotten. They are only taught the DSGE model. The reasons why this bifurcation has occurred have to do with the strength of the alternative models – the synthesis model is seriously flawed as a scientific model, but because of those flaws, synthesis models are much more amenable to intuition being integrated back into them. This means that they can be usefully employed as a framework to hang reasonable policy discussions onto the model, which is how they are used in undergraduate texts (Colander 2004).

My interest in this paper is not with pedagogy, but is with what is happening with policy macroeconomics as practiced in central banks. Earlier Dewey Daane and I (Colander and Daane 1994) observed that central bank policy macroeconomics was only being tangentially affected by the modern macroeconomic revolution and that we expected that limited effect to continue. At the high-level policy level that remains largely true (for example, macro policy during the 2008 financial crisis reflected Keynesian and synthesis thinking much more than it did DSGE thinking).

But as Bussiere and Stracca (2009) and Chari et al (2009) show, it now seems to be changing at the research level at central banks. Today the DSGE model has worked its way into higher-level policy discussions. For example, implicit in estimates of the temporal effect fiscal policy being used by some high-level policy makers are Ricardian equivalence assumptions. The rising influence of DSGE thinking at the research level of central banks has been a surprise to me (although the recent financial crisis may slow or even stop that rising influence).

The reason it is a surprise to me is that I saw research policy macroeconomists, such as those at central banks, as facing different incentives than did academic economists. By that I mean that previously, central bank economists had incentives to be expert consumers of the latest macro theory, not expert producers. As I discussed above I saw the scientific revolution in macro as largely driven by academic incentives. I saw policy economists at central banks advancing by providing useful advice, or at least advice that policy makers saw as useful. The different incentives created a real-world reasonableness in central bank economists that was far less likely to exist in pure academic economists. The best of the policy macro economists, such as Robert Solow, David Laidler, or Charles Goodhart, crossed the lines between academia and policy, and, like Keynes, provided a blending of the two. For central bank policy macro economists the academic economist's models were simply used as a rough framework to structure reasoned common sense. The models were considered something to think about, but not to use as the full truth. The mid-level policy advisers/researchers at the central banks were those subset of economists who had the "vigilant observer" skill that Keynes said was quite rare.

I had assumed that since those differing incentives that had worked throughout the neosynthesis period to differentiate academic and central bank economists would remain, and make central bank economists far less susceptible to the modern theoretical work in macro because those models made so many assumptions to arrive at their results that a "vigilant observer" would not take them seriously for anything other than their general ideas. I was wrong in that assumption.

I now realize that the central bank reasonableness worked well with the IS/LM synthesis models precisely because the synthesis models maintained a foot in both camps. While this attempt to have it both ways undermined the IS/LM model's scientific value, it left economists entering central banks open to retraining. Central banks would train incoming research economists straight from graduate school in its own variation of the Keynesian method (as opposed to any Keynesian model). That training essentially involved teaching the incoming research economists that while the models they learned in graduate school were useful, they contained only half truths, and that the models they had learned needed to be used with intuition

[11]In many ways, the only way I can understand the success of the synthesis model is as a half-way model that allowed economists to massage the model with different judgments. The model could be shaped to arrive at just about any policy conclusion so that the researcher's judgment determined the policy arrived at.

and judgment that was based on institutional knowledge and experience.[11] As the head of research at a major central bank told me in the 1970s, it took central bank research economists about 2 years to "train" incoming economists and separate them from the academic mentality that they had picked up in graduate school. The result of this central bank training was that there was essentially a separate "central bank macroeconomics" that was different than academic macroeconomics, even in the synthesis period.

As I stated above, the reason the academic method and the central bank method could exist simultaneously is that central bank economists faced a different replicator dynamics than did academic economists. Specifically, advancement in central banks did not depend on journal article publication. It depended much more on central bank researchers' ability to pull out relevant insights about policy into briefings and memos. Put another way, central bank advancement depended not only on mastery of the technical models, but also on judgment and intuition – precisely the characteristics that Keynes said were important for practicing economic policy.[12] I had assumed that this "central bank filter" would continue, and would prevent the DSGE training that students received in graduate school from significantly influencing central bank policy in any direct way.

But I am now seeing that my reliance on the reasonableness of existing central bank economists to instill the Keynesian method into central bank economists was probably not warranted. What I did not take into account was the possibility that that ascendency of the scientific method in graduate school would lead to a change in the replicator dynamics of central bank research economists. The earlier central bank training approach worked only as long as those trained in the older methods remained in charge of central bank research departments. But over time, that earlier cohort has left the banks, and we now have research departments in central banks where more and more researchers do not seem to have been acclimated in the same way that earlier researchers were. Some central bank policy researchers don't even concentrate their research on monetary policy or macroeconomics generally, but instead worry about publishing in academic venues on a variety of topics that have little to do with macro.

What I am suggesting is happening is that the moderating influence on theory imposed by central banks is decreasing, and that the introduction of the DSGE model is changing the internal replicator dynamics and incentives of central bank economics research staffs. This means that the graduate training of economists is

[12]It isn't only in macro where this intuition and judgment is needed. It is also needed in microeconomics as Little pointed out when he summed up his message of his study of applied micro welfare economics. He wrote: "Economic welfare is a subject in which rigour and refinement are probably worse than useless. Rough theory, or good common sense, is in practice, what we require. It is satisfying, and impressive, that a rigourous logical system, with some apparent reality, should have been set up in the field of the social sciences; but we must not let ourselves be so impressed that we forget that its reality is obviously limited; and that the degree of such reality is a matter of judgement and opinion" (Little 1950, p 279).

tending to eliminate the Keynesian method not only in academia but also in policy researchers at the central banks.[13] As more scientifically oriented economists, who are only trained in the intricacies of the DSGE model and are not trained in institutions or earlier literature, are coming into the bank, it is harder to retrain them into central bank economists who follow the Keynesian method.

Were advancement in central banks still based primarily on memo writing and briefings, eventually the new research economists coming in would be converted to the Keynesian method, but the selection mechanism of advancement in central bank research departments seems to be changing. More and more, central banks are starting to judge researchers by their ability to publish papers in academic journals, and less by their overall understanding of the macro economy or their ability to write a coherent policy memo. Whereas formerly central banks took academically relevant show dogs and turned them into policy relevant hunting dogs, more and more they are simply changing their advancement policy within central banks to emphasize show dog abilities. When I asked older heads of some central bank research procedures about this change, they agreed that it was happening, and that it was of concern. But they explained to me that it is the only way that they can attract the top academic economists coming out of graduate school.

Now, I am sure some will argue that the DSGE model is not only a useful model for advancing the science of macroeconomics (and I agree; the DSGE model is a much better scientific model than is the IS/LM model); it is also the more useful model for policy guidance. I find that suggestion hard to swallow for two reasons. The first reason the DSGE model is such a poor guide for policy is the same reason that it is a better foundation for a serious scientific model than is the IS/LM model. It is such a highly abstract model that is so far removed from reality that the thought that it could shed much direct light on reality is almost beyond comprehension. Somehow a model of a representative agent, who is infinitely rational and who faces no model uncertainty, is supposed to shed light off and on something as complex as the macro economy is on the face of it absurd. It is that absurdity that underlies Solow's "rhetorical swindle" comment at the beginning of this paper. The absurdity is, however, not the model – the model is reasonable; the absurdity is its direct use for guiding policy, rather than as just background information for guiding policy. The DSGE model does pass even a low level common sense hurdle as a guide for policy. Yet, with the development of computer software such as DYNARE, the DSGE model's influence is spreading.

The second reason is that it does not meet reasonable empirical tests. (If it could be shown that the DSGE model predicted better than do alternative models, the fact that it doesn't meet common sense requirements wouldn't matter.) But the DSGE model doesn't make better predictions. As Juselius, Johansen, and Franchi (Johansen 2006; Johansen and Juselius 2006; Juselius and Franchi 2007) have shown, the DSGE models are not being brought to the data in a reasonable way,

[13]Businesses which have a bottom line to consider use macroeconomists in quite different ways; business economists still rely much more on judgment and intuition than do academic economists.

and that where care is taken in relating the models to the data, claims for their relevance, such as made by Ireland (2004), vanish.

The fact that younger macroeconomists see the DSGE model as the only methodologically acceptable approach is, in my view, scary.[14] It represents a major movement away from the pragmatic educated common sense that used to be the hallmark of central bank economists. It leads to grossly overstated policy conclusions such as the following made by Chari et al. (2009)[15]:

> Macroeconomists can now tell policymakers that to achieve optimal results, they should design institutions that minimize the time inconsistency problem by promoting a commitment to policy rules. However, to what particular policies should policymakers commit themselves? For many macroeconomists considering this question, quantitative general equilibrium models have become the workhorse model, and they turn out to offer surprisingly sharp answers. (p. 9)

Anyone trained in the Keynesian method would be seriously bothered by such a statement. The fact that such statements, and others like it, appeared in the inaugural issue of the American Economic Association's new *American Economic Journal: Macroeconomics,* and, other than from Solow, did not bring about an outcry from the macro policy community is a statement to how far the economics profession has deviated from the Keynesian method.

Conclusion

Incentives and training matter. Keynes once said that policy makers are all the slaves of some defunct economist. That statement does not go far enough. Economists are themselves slaves to the incentives and training in the system, and thus policy makers are ultimately the slaves of the institutional structure which trains and advances economists.

It is time to change that training. It is strange that economists whose models demonstrate the significant benefits of specialization and division of labor seem unwilling to admit and incorporate the need for specialization and division of labor in their own field. Somehow, in their training there is a "one training fits all"

[14]Chari et al. (2009) summarize this generally accepted methodological view when they write "an aphorism among macroeconomists today is that if you have a coherent story to propose, you can do it in a suitably elaborate DSGE model."

[15]Individuals make stupid statements all the time, but ideally, individuals make them in a conference such as this one first (as I am doing) and then through discussion with one's betters, reviewers, and editors, those stupid statements are transformed into nuanced statements that are more defensible. But Chari and Kehoe's (2006) statements made it through all those profession filters and made it into print in AEA journals, without provoking the ire of the mainstream. Thus, my concern about the statement is not with the statements per se, but with the professional elite of the macroeconomics community's response to those statements and others like them. Only Robert Solow made the appropriate response from a Keynesian methodological position.

mentality. They just assume that someone who is great as a producer of scientific models also has the qualities to draw policy inferences from those models. While that may be true for polymaths, it is unlikely to be true for the large majority of economists.

Because of increases in the technical tools of economics, economists can no longer rely on a single type of training. Much more specialization is needed; the training for an economist going into policy economics should differ from the training of someone going into scientific research. Do we really care whether a top policy economist can create a DSGE model or work out the conditions under which a rational expectations equilibrium might exist?

Graduate economics programs today have made preparing scientific researchers their single goal. Consistent with that goal, economics graduate students aren't taught the limitations of models nor are they taught the need for judgment and intuition when applying abstract models to policy. For a long while that didn't matter, since policy institutions selected for those macroeconomists who followed the Keynesian method. That filtering process is now disappearing, and the result is that central bank research departments are now edging away from the Keynesian method. That, in my view, is cause for serious concern.

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Comments on Colander's "The Keynesian Method, Complexity, and the Training of Economists"

Amos Witztum

Colander's paper is a fascinating attempt in understanding how did we get to where we are in Macroeconomics given the Keynesian starting position. The focus of the paper, however, is on methods rather than substance. It is an unusual approach but appears to be quite rich in content and controversy. The paper deals with three main issues: the methodological significance of Keynes's revolution, the process which led to the current dominance of the Dynamic Stochastic General Equilibrium (DSGE) models, and, the question of theory–policy relationship. The general thesis of the paper is that the Keynesian method was more like an ad-hoc approach to economic studies which accepted the existence of many models and combined their choice and use with intuition and some knowledge of the real world. Behind it, however, there is a complex system of the macro economy which combines many aspects of social life. But the complexities of social life were reduced by economists to that which is easier to handle: mathematically sophisticated models. The reason why these models developed is mainly due to the fact that while in earlier years economists were trained by people with broad interests, since the 1930s economists have been trained by increasingly narrow minded economists. Coupled with the ease of establishing one's self academically through mathematical complexities rather than real struggle with the social structures, we have landed in this new world of DSGE models that are as good (or, actually, bad) in explaining things as their predecessors.

I have great sympathy (and agreement) both with the claim that the current state of macroeconomics is lamentable and that the search for ever sophisticated mathematical models is more the outcome of the sociology of the subject than a service of the discipline's intellectual needs. But one can reach similar conclusions without adopting Colander's take on how economics should be done either as an interpretation of the Keynesian method or as a general methodological claim.

Keynes's Method

According to Colander, Keynes would never move straight from a model to a policy recommendation. Nor would he consider only one model. Instead, there will be some kind of a Meta theory at the back of one's mind. When addressing policy issues, one will then be engaged in the *art* of selecting a model which could be useful for the question at hand. This, as Colander argues, would be consistent with the views of Keynes's father as well as Robbins's conception of economic science.

I completely agree that in terms of Robbins's conception of economics, the only way to fit Keynes's work into Robbins's definitions of the subject would be to treat it as a policy document. Moreover, my own feelings are that while Keynes did not offer much more than an ad-hoc theory, it was not of the kind which Colander describes as a Keynesian method. However, there is plenty of textual evidence to suggest that Keynes did not consider his work in this narrow way. Quite to the contrary, the mere title of the book – *The General Theory* – does not seem to support the idea of a modest contribution to resolve an ad-hoc problem. In fact, it seems to hint at much grander objective akin, one could say, to Einstein's contribution to Physics[16]:

> "The classical theorists resemble Euclidean geometers in a non-Euclidean world who, discovering that in experience straight lines apparently parallel often meet, rebuke the lines for not keeping straight – as the only remedy for the unfortunate collisions which are occurring. Yet, in truth, there is no remedy except to throw over the axiom of parallels and to work out a non-Euclidean geometry. Something similar is required in economics." (GT, 16)

Notwithstanding the ambiguity which is revealed in this quote with regard to Keynes's views on classical economics (is it like Galilean relativity or Special relativity in its relationship with the *General* theory?), it does not sound anything like the method which Colander attributes to Keynes. Moreover, as I say in my own contribution to this volume, there can be little doubt about Keynes's own sense of revolution. "I find myself" he wrote to G B Shaw, "to be writing a book on economic theory, which will largely revolutionise…the way the world thinks about economic problems" (Skidelsky 1992: 520). I believe that it would require a great leap of faith to believe that addressing a specific problem in an ad-hoc manner – which is what Colander calls the Keynesian Method – constitutes something which would revolutionise the way we think about economics. After all, it is not really the case that real classical economists like Smith, Ricardo and Mill did not write on policy issues without being completely bound by a restrictive model deduced from first principle.

So it seems that Keynes was clearly aiming at something much greater than the mere solution to the policy issue of dealing with unemployment. I believe that this is enough to disqualify him from following the method which Colander attributes to him.

Still, if we read Keynes as he really is then there may be something in Colander's claim. The fact that Keynes decided not to touch upon the question of economic organisation may fit Colander's argument that he "chose" a model from an existing arsenal and adjusted it so that it would fit the problem he was dealing with. Nevertheless, the discrepancy between the man's intentions and achievements makes it difficult for me to accept that this which he did not really intend should become the defining factor of his legacy.

I must confess that what Colander calls Keynes's method sounds very similar to his own earlier idea about ad-hoc economic analysis. In his excellent paper from 2000 Colander argues that the ad-hoc approach where we address individual problems at a time without a direct reference to any paradigm would be the natural

development once people realise the futility of what he calls "structural simplifications" which represent formal modelling of the economy. In fact, he even suggested a sequence that would ultimately lead to a "Keynesian method." At first, there will be the stage which Solow called "loose-fitting positivism" where the ad-hoc work is based on models which are consistent with the main paradigm but this will give way to "pragmatism" where "the models actually being used for policy purpose were diverging from the underlying formal general equilibrium models at their core" (Colander 2000: 127). In other words, my feeling is that the issue is really Colander's own frustration with current DSGE models rather than a cry over the betrayal of Keynes.

Complexity and the Segregation of Theory from Policy

I fully agree with Colander's analysis of how economists moved from sensible writings to increasingly more complex mathematical models with increasingly more tenuous relationship with their subject matter. I agree with his description of the increased poverty of economics teaching and the senseless rise of quantitative based stature within the discipline.

However, I am not entirely sure that I have been persuaded by his proposed disjunction between theory and policy. On the one hand, Colander accepts that there is a need for a Meta theory and he concedes that this must have also been in Keynes's mind. He rightly deplores the decline of this pursuit into the mundane activity of excessive formalism but he does not offer any insight into how it should be done. Instead, he seems to be more concerned with separating the policy people from the theorists. This, he would like to see happening in both training and in research.

To some degree, Colander argues, the IS/LM synthesis was a good model as it has its legs, so to speak, in both theory and practice. DSGE, on the other hand, seems to him much less institutive and not much better in providing predictions. However, he then goes on to say that "the DSGE model is a much better scientific model than is the IS/LM model" (p. 14). This sounds to me very strange. How can something which is totally useless be of any scientific value? If the model does not offer a reasonable explanation of the world nor capable of predicting it, how can it be a much better model? Is it just because of its mathematical sophistication? All of a sudden, all those things – like senseless and excessive formalism – about which Colander rightly complains – become the essence of the science of economics? I am not at all surprised that if this is his vision of theory, that policy should have nothing to do with it.

But the real question is whether economic science (theory) should develop in such a way as to become incomprehensible for the purpose of policy making.

Because Colander says little about how the science of economics should progress, he wants the people who do policy to be taught about the world more than they are taught about theory.

Unlike Colander, I believe that the real issue is to address the question of how can a theory which is aimed at explaining the economic world be so useless for the purpose of policy making. In other words, the question is not really how to separate policy from theory but rather, how to do theory. Colander only touches upon this in his discussion of complexity. Basically, he claims here (and in his 2000 paper) that the complexity of the world calls for complexity in science. Thus, there are phenomena which are susceptible to traditional economic modelling ("linear dynamics and unique analytical solutions" (Colander 2000: 128)) and those which require a "replicative process" where research begins with a variety of organising principles and dynamics.

I am not entirely sure that I recognise these different phenomena about which Colander writes. It seems to me that he is referring here to the question of the relationship between economics and the other social sciences. I suppose that what he means here is that the phenomena which lend themselves to traditional deductive reasoning are those which can be identified as economics phenomena and the others, the more complex ones, are social phenomena. Personally, I am not sure that I would go along with such a distinction. In some ways, this seems to raise a similar question to the one raised during the famous "*Methodenstreit*": can we separate the economic aspects of social phenomena?

If the answer is no then there is no purpose in pursuing the analytical advancement of economics as a separate discipline. The recipe of "replicative process" should be the only rule and by implications, economics should never be taught as an independent disciplines. While there may be many people who believe this to be true, I am not quite sure that it is. There can be little doubt that economic phenomena are social phenomena. There is also no doubt that one must take into account the other aspects of society when investigating such phenomena. But this does not mean that one cannot conduct a ceteris paribus examination provided that one also has a good understanding of those things which are held fixed and which, over time, must never be kept in the same position. In fact, classical economists like Smith or J S Mill had done exactly this. They created a coherent system of economic analysis which is closely tied together with other aspects of social life. Whether or not their systems lend themselves to a mathematical formulation is neither here nor there in terms of the development of our understanding of how social organisation works. The fact is that they have offered a genuinely complex simplified structure that does not require the separation of policy from theory in such a harsh way as proposed by Colander.

In this respect, Keynes – very much like today's DSGE models – are guilty of the same thing. They have transferred the economic problem from being a problem of social organisation (the subject of complex science) to being an ad hoc problem of how to determine and control aggregate values. At the time of Keynes this was a betrayal of the agenda of the real classical economists – unless you see his work as a

policy document – and in this respect, the DSGE models are true to Keynes's tradition.

Conclusion

Colander's paper makes the case for a separation of theory from policy. In this respect, he follows Robbins even though he does not make the demand that theory should also be value free. It is based on his justified (and well documented) frustration with the over formalistic drive which left economic theory like the DSGE divorced from intuition and real life experience. He has also highlights and deplored the sociology of the disciplines which allows for such developments. For this, he should be congratulated.

Colander has obviously thought a lot about these questions and developed his own vision of how things should progress. These include both a division of labour between theory and policy and, a division of labour between "structural simplifications" and "replicative processes." In the paper in this volume he attributes a similar way of thinking to Keynes. While I have not been persuaded that he is right about Keynes, the vision is still relevant. Here, however, I felt that I have to differ. While the integration of economics into the social sciences may indeed make Colander's vision the right one, I feel that there is still a lot that can be learnt if we followed the more classical tradition where economics was a social theory yet it was not a form of a "replicative process." The dire state of DSGE should not be a reason to separate theory from policy as it should be a good reason to expect better economic theory.

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Keynes, Wicksell and Active Monetary Policy

Arie Arnon

Abstract The acceptance of monetary policy into mainstream economic thinking was a difficult and slow process. Henry Thornton had already advocated monetary policy in 1802 in his monumental *Paper Credit*. However, it was not until the publication of Walter Bagehot's *Lombard Street* in 1873 that a theory of monetary policy was reintroduced into mainstream thinking, and even then it was in a restricted, defensive form. The profession had to wait another 25 years for a full-fledged theory justifying active monetary policy of the kind that Thornton provided, this time in Knut Wicksell's *Interest and Prices* (1898).

This paper offers a brief survey of the slow rise of a theory of monetary policy in the nineteenth century. The paper then elaborates on Wicksell's innovative thinking on monetary policy, with a focus on Wicksell's influence on the work of Keynes. Keynes' early contributions to monetary theory and policy were embedded in the Cambridge monetary tradition and adopted monetary policy as an effective tool in fighting business cycles. In *A Treatise on Money* in 1930, Keynes presented a comprehensive analysis of that approach; he was well aware of his debt to Wicksell. The changes in Keynes' views that led to the *General Theory* 6 years later modified his attitude to monetary policy and to Wicksell as well. The paper re-evaluates both the changes in Keynes' thinking on monetary policy between 1930 and 1936 and how these changes are reflected in Keynes' changing attitude to Wicksell.

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Introduction

Since the mid-eighteenth century, monetary theory has focused on the ideal structure for the monetary system, and the latter's role in supporting the real economy. For the present paper, the most important factor in the development of monetary theory is the attitude of scholars, whether explicit or implicit, towards interventions in the monetary system and in the financial sector in general. Such interventions contradicted, of course, the common invisible hand approach to money and banking typical of David Hume and Adam Smith. Analyzing the few departures from Hume and Smith in this field may help to identify early thinking about the alternative to the invisible hand – thinking that we came to know in the twentieth century as monetary policy or central banking. Surprisingly for many economists, there was no accepted theory of central banking as late as Walter Bagehot (1896), and even his was not a fully developed theory of central banking. Such a theory had to wait for Knut Wicksell's path breaking 1898 text *Interest and Prices*.[1]

When discussing monetary policy, one has to distinguish between passive and active forms of intervention: the passive form refers to those interventions in the monetary system done when a specific institution – the monetary authority, known later as the central bank – acts to rescue the system. The active form relates to actions aimed at improving the performance of the real economy via monetary instruments. The former is by definition responsive intervention; in many cases the response was to a crisis in the banking system that was usually associated with the central bank's attempt to address the demands of other institutions and individuals. Active intervention refers to those actions initiated by the monetary authority when the authority is not under the pressure of an emergency or a crisis. The two forms of intervention are not necessarily exclusive. Bagehot is associated with passive intervention while Wicksell and, many years before him, Henry Thornton, are associated with the latter (see Laidler 2004; Arnon 2009, 2010).

John Maynard Keynes' intellectual relationship with Knut Wicksell is the focus of the present paper. Wicksell does not explicitly appear in Keynes' early contributions, but in Keynes' 1930 *A Treatise on Money*, he expresses his admiration for Wicksell. However, Wicksell receives a much less positive assessment in Keynes' celebrated 1936 *General Theory* and afterwards. This paper looks for the explanation for this apparent change of heart.

The second section, "The Slow Coming of Monetary Policy: From Thornton to Bagehot" briefly review the well-known canonical approach of classical monetary theory before Bagehot that in its essence avoids the concept of monetary policy. Section three, "Wicksell's Monetary Theory", presents Wicksell's theories concerning the Lender of Last Resort, passive monetary policy and active monetary policy. Section four, "Keynes and Wicksell: Up to *A Treatise on Money* and in the

[1]In the context of the very slow and difficult rise of a theory of monetary policy during the nineteenth century, it is important to remember that such a theory was already offered in 1802, long before Bagehot and Wicksell, in Henry Thornton's *Paper Credit*.

General Theory" will discuss Wicksell's influence on Keynes' early monetary thought, and Keynes' changed attitude towards Wicksell in his mature thought, as represented by *General Theory*. Section five will summarize by speculating on possible explanations for the different interpretations of the Wicksell–Keynes link.

The Slow Coming of Monetary Policy: From Thornton to Bagehot

Henry Thornton's seminal book *An Enquiry into the Nature and Effects of the Paper Credit of Great Britain* (Thornton 1802), written after a monetary crisis shuttered the British financial system in 1797, explicitly rejects Hume's and Smith's approaches to money and banking. Thornton does not accept the self-regulating approach to banking that lies behind the thinking of Hume and Smith; neither the specie-price-flow mechanism assumed by the former nor the competitive banking views held by the latter are accepted by Thornton as appropriate banking mechanisms. According to Thornton, these approaches were clearly unworkable under the inconvertible system that was introduced in 1797 and continued to 1821 and was known as the Restriction period. However, his monetary theory went further than just demanding a quick return to convertibility as a cure (see Arnon 2009). Thornton believed that any monetary system, convertible or inconvertible, is not self-regulating and calls for discretion. The outcome of any competitive banking process could not be trusted, even under convertibility, and a body that represents the best interests of the public should lead the way in directing the system and determining the relevant aggregates.

According to Thornton, discretion by a monetary authority should go beyond just acting as a Lender of Last Resort – a body that advances credit to banks in distress – and extend to implementing active monetary policy. Moreover, Thornton understood the importance of banking beyond its impact on the exchange process and the determination of the price level. He understood the critical role of the monetary system in intermediation and its impact on the real economy. This led Thornton to radical conclusions on the scope of monetary policy.

Thornton advocates acting according to the prevailing economic circumstances rather than leaving the monetary aggregates to the determination of market forces, the demands of borrowers, or a rule. It is the Bank directors' responsibility to assess the economic conditions and decide what course to take. Thornton defines, again, in very modern terms, the conflicting targets of monetary policy.[2] These innovative – some

[2]In an oft-quoted statement, Thornton makes it clear that he has (monetary) policy in mind while talking about the "true policy of the directors of an institution circumstanced like that of the Bank of England" (259). He emphasizes the "principle of restriction" but also the need to control the monetary system:

"To limit the total amount of paper issued, and to resort for this purpose, whenever the temptation to borrow is strong, to some effectual principle of restriction; in no case, however,

would say early Wicksellian – ideas were not accepted by either his contemporaries or, more generally, later nineteenth-century scholars. Thornton did contribute to shaping the British monetary orthodoxy in the 1870s but only indirectly, through his impact on the Banking School; by this time he himself had already sunk into anonymity.

As we noted above, the belief that a competitive banking system would provide the environment for prosperity was supported by Hume (1752) and Smith (1776) but was put to the test again and again in the coming decades. The instability in the economy, including the recurring incidences of financial as well as more general, real, crises had been blamed, at least in part, on the monetary system. The most dramatic incidence, in February 1797, resulted in the Restriction, but crises occurred before that year (1783, 1793) and after (1825, 1836, 1847/1848, 1857/1858).

Walter Bagehot reacted to these crises and in particular to that of 1866. Since its publication in 1873, Bagehot's *Lombard Street* has been perceived as presenting the position of the British monetary orthodoxy (to use Fetter's (1965) famous title) on banking and monetary policy. The full title of Bagehot's influential text – *Lombard Street: A Description of the Money Market* – suggests that it was not planned as a theoretical treatise providing an analytical perspective, but rather was written from a practical, policy-oriented perspective. Bagehot's emphasis on the practical should not lead us to underestimate the depth and clarity of his ideas; neither should the fact that the book's influence had much to do with the author's unique role in British public life. The description of past and present money markets helps the reader appreciate what, to Bagehot's mind, is the single most important fact, one that is emphasized throughout the book: the centralization of the market so that it has a pivot around which the activities are executed. Specifically, the various institutions dealing with other people's money – those institutions that take in deposits on the one hand and advance funds on the other – rely on the center in London. They had become accustomed to keeping their reserves in London for the times when an outstanding demand would arise; it is to London that they could apply for relief. It was a unique structure, not designed with any theory in mind and, in Bagehot's view, did not exist in any other country.

Bagehot carefully describes the circumstances out of which the English financial system grew. In London, the City came to function as the center; within it, the Bank of England was the center's center of gravity: the reserves of the financial system, unlike any other institution in the world and without precedent in England as well, were now concentrated in the Deposit Department of that old and respected institution. This unprecedented power was dangerously concentrated in the hands

materially to diminish the sum in circulation, but to let it vibrate only within certain limits; to afford a slow and cautious extension of it, as the general trade of the kingdom enlarges itself; to allow of some special, though temporary, increase in the event of any extraordinary alarm or difficulty, as the best means of preventing a great demand at home for guineas; and to lean to the side of diminution, in the case of gold going abroad, and of the general exchanges continuing long unfavourable" (259).

of an institution that had never been intended to hold it and whose governance denied it even had such power; hence the Bank never really accepted or even understood its unique role. Based on this foundation, *Lombard Street* and Bagehot's formulation of central banking policy, known as the Bagehot Principle or Rule, gave the final shape to what has since been described as the British monetary orthodoxy. The Bagehot Principle was the most advanced concept concerning central banking and monetary policy of its time, certainly since Thornton, and was closely associated with the Bank's role as a Lender of Last Resort. Strangely enough, especially for a person so well acquainted with the history of monetary theory, one can find almost no reference in *Lombard Street* to the history of the concept; neither Thornton nor any other political economist who wrote about discretionary policy is credited with discussing it.[3] However, contrary to some later interpretations, Bagehot's Principle was not intended as an active, full-fledged monetary policy. It had a very restricted aim: it was intended to prevent a possible dangerous collapse of the financial system. The profession had to wait for Wicksell's breakthrough 25 years later for an active monetary policy framework.

Wicksell's Monetary Theory

The Swedish economist Knut Wicksell (1851–1926) wrote in German and Swedish and is, at least in this regard, an exception to our mainly British story. His career was not what one would expect from someone who is perceived today as an outstanding and influential academic. Throughout his life he was something of an outsider in his contemporary academic world. He got a chair in economics in Lund only at the age of 50. Indeed, his name was first known in Sweden not as an economist but as a radical pamphleteer. He taught himself economics first in Sweden and then during visits to London and Europe, where he focused on reading both classical and modern, post-1870s economics.[4] As we shall see below, Wicksell's analysis should be read and understood against the background of the British debates, the British institutions and the British monetary orthodoxy.

Wicksell's most famous monetary writings appeared in English only many years after their first publication, though his name was probably already known by that time to English readers through Hayek's work. Wicksell's attention turned to monetary questions in the 1890s. *Geldzins und Guterpreise*, his treatise on monetary theory was first published in 1898, but translated into English as *Interest and Prices* only in 1936. *Lectures on Political Economy* [Lectures] was his next major work on monetary theory and was written with an eye to the history of monetary

[3]Thornton is not mentioned at all by Bagehot, which is another indication of Thornton's disappearance from the literature by the 1870s. Joplin is not mentioned either (see O'Brien 2003).

[4]On Wicksell's life see the brief introduction to the translation of *Interest and Prices* by Ohlin (1936), as well as Gardlund (1958) and Uhr (1960), who are the two authoritative biographers of Wicksell; the first is a translation of the original 1956 Swedish biography.

thinking. It was produced in Lund, where he lectured from 1901 until his retirement.[5] *Money*, the second volume of *Lectures*, is more relevant to our subject than the first volume; it was published in 1906, and a revised 1915 edition was translated into English in 1935. One short paper, "The Influence of the Rate of Interest on Prices", originally appeared in English in 1907 in the *Economic Journal* after it was presented before the Economic Section of the British Association in 1906.[6]

The 1898 Geldzins und Guterpreise

In the preface to *Interest and Prices: A Study of the Causes Regulating the Value of Money*, Wicksell states that the book originally aimed at an "examination of the case for and against the Quantity Theory". Many secondary commentaries emphasize this issue although Wicksell's "reflections" caused him "to give up this simple plan":

> "I already had my suspicions – which were strengthened by a more thorough study, particularly of the writings of Tooke and his followers – that, as an alternative to the Quantity Theory, there is no complete and coherent theory of money. If the Quantity Theory is false – or to the extent that it is false – there is so far available only one false theory of money, and no true theory" (xxiii).

According to Wicksell, the criticisms of the Quantity Theory by Tooke and his followers did not result in a theory, a "connected whole", but remained at the level of negative "aphorisms". Ricardo, on the other hand, who presented the Quantity Theory in its classical form, left it "open to too many objections". Thus, it is the debate between Ricardo and the closely-associated Currency School on the one hand and Tooke and the Banking School on the other that attracted Wicksell's attention and led in due course to his innovative analysis. That analysis, as we shall

[5]Wicksell's approval process in joining Lund became a public issue between liberals and conservatives in Sweden. Typically for a principled persona, Wicksell almost lost the appointment for refusing to sign his final application with the traditional "your humble servant", choosing instead to sign "respectfully". See Gardlund (1958), pp. 20–24.

[6]More of Wicksell's papers on monetary issues in English were published in 1958 by Erik Lindhal, who edited *Knut Wicksell: Selected Papers on Economic Theory*, and in 1999 by Bo Sandelin, who edited *Knut Wicksell: Selected Essays in Economics* (volume 2). On the monetary aspects of Wicksell's thought see Patinkin (1952, 1965, 1982), Leijonhufvud (1981), Laidler (1991, Chap. 5), Boianovsky and Trautwein (2001) and references therein. Boianovsky and Trautwein's paper is an introductory essay to "An Early Manuscript" they found in the Lund University Library (Wicksell 1889/2001) which was never published before, where they compare that MS to an article published in 1897 (in Swedish) and to *Interest and Prices*. The dating of the MS is based on a note prepared in the 1970's by the librarian in Lund and on their reading of the MS that confirmed the date. If that date is confirmed it will make Wicksell's basic line of monetary analysis appear immediately after he studied the British texts in London in the 1880's, earlier than scholars thought before. The MS however is relatively short and seems more like a research plan or an outline of Wicksell's major monetary ideas at the stage it was written. See more below.

see, represents a coherent connected whole, a theory which provides an alternative to the Quantity Theory that is, at least under certain circumstances, more than just a negative aphorism.

The theory of interest and its relation to the theory of the price level lies at the center of the shortcomings of the two approaches represented by Ricardo and Tooke. Summarizing the core message of his study in the preface to *Interest and Prices*, Wicksell writes:

> "The Quantity Theory is correct in so far as it is true that an increase or relative diminution in the stock of money must always *tend* to raise or lower prices – by its opposite effect in the first place on rates of interest" (xxviii).

In Wicksell's view, the focus of the inquiry should change; while traditionally the analysis addressed the relationship between the quantity of money and prices, he thinks that the analysis should move to explain the link between interest and prices. Wicksell begins with an elaborated distinction, now familiar, between relative prices and monetary prices; the latter define the purchasing power of money or the value of money.

In the exchange of commodities, where relative prices are determined, money plays a "*double* role". The first is its role as a *medium of exchange,* which prevents the inefficiencies in barter where the need for "double coincident of wants" increases transaction costs. The second role, that of a "store of value", is explained by the fact that the exchange of commodities in a monetary (not barter) economy is not instantaneous; in reality, the purchase of a commodity may take some time after the sale has been completed. Wicksell analyzes several mechanisms for the exchange process, from less sophisticated mechanisms where only cash is used, to more developed systems where credit exists.

Wicksell argues that changes in the level of prices originate "*outside* the commodity market proper". One should either look into the specific market of the commodity which serves as money, for example gold, or take other conditions into consideration. In two separate chapters, Chapters 4 and 5, he addresses two, not mutually exclusive alternatives, "one of which is connected to the so-called Cost of Production Theory of Money and the other with the so-called Quantity Theory" (24).

Wicksell's position on the Quantity Theory attracted many commentators, some of whom present him as in favor of the theory and some of whom present him as opposed. His actual position was more complex. The opening statement of the famous Chapter 5, "The Quantity Theory and its opponents", should be read carefully:

> "It is clear that the higher is the price of a commodity the greater the amount of money required for the purpose of its sale and purchase. But the whole function of the available supply of money – so long at any rate as it retains the form of money – is to be exchanged, sooner or later, for commodities. It is now but a small step to recognizing that the total volume of money instruments in existence in an economic system, or rather their volume taken in relation to the quantity of commodities exchanged, is the regulator of commodity prices" (38).

This doctrine is ascribed to Hume but originated earlier, Wicksell tells us. It grew against the mercantilist concept, wherein money had an invariable, intrinsic value. But although "*under given conditions* the Quantity Theory is capable of being correct" it must not "be imagined" to determine the level of prices (38–39). The conclusion that the Quantity Theory determines the level of prices is valid only under the assumption that money does not create a gap between sales and purchases. Wicksell now complicates the analysis by emphasizing the possibility of a time gap between a sale and a purchase. If there is no time gap and the transactions are executed simultaneously, the stability of the value of money, its purchasing power, is insignificant. However, since in reality simultaneity does not always occur, the value of money does affect the exchange process and the prices of commodities. In an oft-quoted paragraph, Wicksell states his position under the circumstances of a time gap between sale and purchase:

> "Now let us suppose that for some reason or other commodity prices rise while the stock of money remains unchanged, or that the stock of money is diminished while prices remain temporarily unchanged. The cash balances will gradually appear to be *too small in relation to the new level of prices* . . . I therefore seek to enlarge my balance. This can only be done – neglecting for the present the possibility of borrowing, etc. – through a *reduction* in my *demand* for goods and services, or through an *increase* in the *supply* of my own commodity . . . or through both together. The same is true of all other owners and consumers of commodities. But in fact nobody will succeed in realising the object at which each is aiming – to increase his cash balance; for the sum of individual cash balances is limited by the amount of the available stock of money, or rather is identical with it. On the other hand, the universal reduction in demand and increase in supply of commodities will necessarily bring about a continuous fall in all prices. This can only cease when prices have fallen to the level at which the cash balances are regarded as *adequate*". (40)

This famous discussion has since been labeled the "cash-balance" or "real-balance" approach, and presents the "strength and weakness" of the Quantity Theory. Wicksell argues that the Quantity Theory is logical in theory, but he lodges several objections as to its validity in reality. The first is that the Quantity Theory is based on an assumption that has "little relation" to practice: that cash balances are held individually, as in a system that may be described as "a pure gold circulation". In reality, however, more and more transactions are done through deposits; in those cases the story, as we shall see, is different. Wicksell's second objection is that the Quantity Theory makes the unrealistic assumption that velocity is constant. A third objection is that it assumes a constant ratio of the use of money "in the sense of coins or notes" in transactions when in reality, "true instruments of credit (ordinary book credit, bills, cheques, etc.)" sometimes substitute for the use of money in exchange, in particular in periods of crisis. A fourth objection is the assumption that one can distinguish between the use of money as money and its usages outside the monetary function, as commodity or in hoards.

The cost of production approach, argues Wicksell, is now (1898) dead except for in the "orthodox Marxist circles". The alternative *"Credit Theory of Money,* which is supposed to originate from Thomas Tooke and provide a scientific antithesis", does not convince Wicksell (43). Tooke's explanation is based on a disturbing

confusion between relative and absolute prices which Wicksell will try to correct throughout his life.

Different Payment Arrangements and Intermediation

Wicksell turns his attention to velocity, which appears in the literature as the equilibrating variable that makes the exchange with the existing supply of money possible. He specifically directs his criticism at J.S. Mill's idea that velocity depends on prices; clearly, at this stage, Wicksell is still analyzing the exchange of commodities and not intermediation. The crux of the matter is whether the velocity is determined by "*independent factors*" or is a "*resultant*, given the quantity of goods exchanged and the available money, [and] of the particular level of commodity prices, themselves determined by *quite different* causes" (54). Wicksell chooses to address this question separately under three payments systems (A) Pure Cash Economy; (B) Simple Credit; and (C) An Organized Credit Economy. Under the first system, the velocity clearly depends on the above factors. The discussion of money holdings under such a system is straight forward: it depends on the exchange technology for the planned transactions, the holdings for unforeseen transactions and some hoardings that leave the circulation of money (56–59).

The second system, that of "simple credit", is less hypothetical in Wicksell's mind since "at no stage of economic progress can the phenomena of credit have been entirely absent" (59). This case raises the possibility that Wicksell will refute the Quantity Theory, although a careful evaluation will show that he does not. The discussion continues to revolve around the exchange process, excluding intermediation, while allowing credit to affect the velocity of money in the exchange of commodities. In fact, Wicksell argues that the impact of credit can be unlimited, so that a very small amount of money can suffice; the reason for this is that "merchandise credit" and "loans"[7] will enable the traders to save on holding money. In theory this process has no restrictions but for the fact that at some point money has to be physically transferred; hence the above physical restriction is associated with the "speed of transport" of money. The simple credit system can make this transport fast but not as fast as the organized, banking credit system. The velocity of money in the simple credit system is elastic and flexible but does not change fully with changes in the money supply; hence "the conclusions of the Quantity Theory to retain the appearance of substantial validity" (62). That validity is not maintained in the third case Wicksell analyzes.

The "organized credit system" makes the impact of credit on money infinite; the two restrictions mentioned before, the access to credit by individuals and the need to

[7]We will return below to the distinction between "merchandise credit" that belongs to the exchange process and "loans" that are part of intermediation.

hold some cash, disappear.[8] The mechanisms that enable this disappearance are "the *transfer of claims* (the use of bills of exchange) and the *centralisation of lending* in monetary institutions". The modern system of finance combines the two methods and together banks, the bourses, etc. progress towards an organized credit economy. Debts that function as mediums of payments, i.e. as money, and money that is not held by individuals but returned to the banks, change the way we do exchange. "There is no real need for any money at all if a payment between two customers can be accomplished by simply transferring the appropriate sum of money *in the books of the bank*" (68). The consequences for the Quantity Theory are dire.[9]

Up to this point the discussion has focused solely on exchange; from now on, beginning with the section dedicated to the organized credit economy, Wicksell analyzes intermediation as well.[10] After a discussion of the various interest rates in an organized credit economy, he returns to his major question: What determines prices? Now the link of the general price level to interest rates takes center stage. Wicksell asks: Can the rate of interest regulated by the banks influence "the exchange value of money and commodity prices?" (75). The context of Wicksell's discussion is obviously the British monetary debates. On the one hand is Ricardo and what Wicksell calls the "Classical Theory" and on the other, the "school of Tooke". According to Wicksell, Ricardo's *Reply to Bosanquet* gives a positive answer to his question; Tooke and Fullarton deny such a possibility; and Mill takes a disappointing, weak middle position. But all of them miss the main point which is at the center of Wicksell's (1898) theory (81–87).

The Price Level, Interest Rates and the Cumulative Phenomena

Wicksell's analysis provides an innovative link between the rate of interest and prices, and associates the market for loans and credit with the determination of absolute prices. Wicksell assumes in this famous theory that "all payments are

[8]"In a developed credit economy both these obstacles are removed, and either actually or *virtually* a higher velocity of circulation is provided – or, more correctly, the velocity of circulation is *capable* of being increased more or less at will" (62-1).

[9]That the abstract organized system has implications for the real system analyzed in 1840s by the Currency School seems clear to Wicksell. When he refer to a bank-note system he concludes: "Notes provide in themselves the basis for a more or less elastic system of credit, and they circulate with a velocity which is more or less variable. It is for this reason that it was never possible for even the older supporters of the Quantity School to provide a satisfactory demonstration of the exact relationship which they held to exist between the price level and the quantity of notes (and coin)" (69–70).

[10]Its first appearance: "We have so far dealt with the interval of time, dependent on nature and technique, which separates purchase from the corresponding sale. But actual long-term credit itself has a part to play. Many people require in their business, either regularly or at certain periods, more capital than they themselves possess, while others possess more capital than they are able or willing to find use for. The resultant lending and borrowing can be supposed to be effected through the intervention of our Bank" (73).

made by means of cheques", as is done in the organized credit economy. When conditions change in the loans market, for example when credit is extended, the market rate of interest will decrease. However, the effect of the market rate of interest on prices depends on another rate which Wicksell denotes the "natural rate of interest". That natural rate is determined outside the financial sphere, by conditions in the production sphere. Wicksell thinks of this rate as determined by the marginal product of capital, or as the real profit rate in production. If in the original situation, before any changes in the loans market, the economy faced a market rate of interest that was equal to the natural rate, and now, due to credit expansion, the market rate has gone down and is below the natural rate, a process of rising prices will begin. Prices will continue to rise as long as the market rate stays below the natural one. Hence, the economy will experience continuous price changes, a process that Wicksell terms "the cumulative process". The cumulative process will only come to an end when the market rate increases, the natural rate decreases, or both rates change so that they are once again uniform.

Wicksell's innovative cumulative process must be understood in the context of the British monetary debate of the first half of the nineteenth century. When explaining the theory, Wicksell repeatedly refers to Ricardo, Tooke and J. S. Mill. It is not, as Tooke and Mill argue, the rate of interest itself, low or high, that impacts prices; it is the interest rate's relation to the natural rate that helps to explain price changes.[11] The cumulative process theory aims at explaining money prices and not relative prices, thus arguments about relative costs and relative values, which Tooke raises, miss the major point that Wicksell makes. Two oft-quoted metaphors elegantly describe the differences between the equilibrium of relative prices and that of money prices: one is that of a pendulum, that returns to a "stable equilibrium". The second is that of a cylinder, that can "rest ... [while] there is no tendency for it to be restored to its original position. It simply remains where it is so long as no opposite forces come into operation to push it back" (100–101).

The mechanism that brings the economy to equilibrium is that of the many competing capitalists and entrepreneurs who function within a developed banking system. Changes in the loan (market) rate of interest, making it different from the natural rate, will push the economy into disequilibrium, increase profits for the entrepreneurs and put in motion rises in prices. This is true, of course, as long as we are assuming that the natural rate has not changed. Thus, what is pushing the economy into disequilibrium is the gap between the two rates, or the "relative rate"; the analytical difficulty is to establish the conditions in which the relative rate will be zero. The natural rate, as explained above, is determined in the real,

[11] "We have seen that a casual and temporary change in the discount rate would not in itself exert any marked influence on prices. To this extent it can be granted that Tooke was quite right in maintaining, in contradiction to Ricardo, that the banks' discount policy is in itself of direct significance in respect only to such matters as international or interregional movements of capital and the postponement of payment of fluctuating liabilities, but that it is of smaller importance in respect to the structure of prices" (92). Wicksell refers to Tooke's arguments repeatedly in the book and is often critical of his positions, although he sometimes approves of them.

productive economy, independent of the market rate, and functions like an anchor. Thus, if there is equilibrium in the economy the market rate should gravitate to the natural rate. The force that is at work is that of changing money prices.[12]

The equilibrating mechanism depends on a mechanism that can work only under traditional money markets, where the supply and the demand are independent of each other and together determine the price level. Wicksell is of course aware that under the conditions of the assumed modern "giro" system ("pure credit"), where the supply of money is accommodating demand (also called an "elastic monetary system"), this equilibrating mechanism will not work. Thus, "banks can raise the general level of prices to any desired height" (111). The opposite scenario, in which banks keep the rate of interest above the natural one and prices fall, is also possible. The rise and fall of prices in these respective cases are restricted by the freedom of the banks to determine their interest rates. In Wicksell's view, international competition and the actual existence of cash reserves are the factors that restrict the banks in determining whether the money rate is below or above the natural rate. In other words, actual conditions are different from those of a pure credit system; under these circumstances the money rate will tend "to coincide with an ever-changing natural rate" (117). In a chapter entitled "Systematic Exposition of the Theory" (Chapter 9), Wicksell elaborates on the causes that determine the two rates and their relation to price changes.

Wicksell on Monetary Policy

Under the title "Practical Proposals for the Stabilisation of the Value of Money" in Chapter 12 of *Interest and Prices*, one will find a discussion that comes closest to policy. The motivation for the discussion seems to be the policy conclusions of bimetallism.

The bimetallists' proposals to bring "order and security" to the international monetary system failed, as did other proposals for "composite standard" by Marshall, Edgeworth and others (178–183). Wicksell's own proposals are directly derived from the theoretical discussion in Chapter 9 of *Interest and Prices*: what can be done to stabilize the price level and to provide order and security is to "exert an indirect influence on the *money rate of interest* and bring it into line with the natural rate, or below it, more rapidly than would otherwise be the case" (188). Stable prices, the objective of such policy, could be reached "more cheaply, and far more securely through the monetary institutions of the various countries".

Wicksell's strategy, international coordination between central banks of their rates of interest, is the only effective method that will "bring the average money rate

[12]"When the money rate of interest is relatively too low all prices rise. The demand for money loans is consequently increased, and as a result of a greater need for cash holdings, the supply is diminished. The consequence is that the rate of interest is soon restored to its normal level, so that it again coincides with the natural rate" (109–110).

into coincidence with the natural rate" worldwide. One does not have to know the natural rate, which is certainly hard to calculate on a world scale, in order to achieve such an objective. It would suffice to look at world prices and determine whether they are rising or falling:

> "The procedure should rather be simply as follows: *So long as prices remain unaltered the banks' rate of interest is to remain unaltered. If prices rise, the rate of interest is to be raised; and if prices fall, the rate of interest is to be lowered; and the rate of interest is henceforth to be maintained at a new level until further movement of prices calls for a further change in one direction or the other*" (189, emphasis in the original).

This policy rule, which we may call "Wicksell's rule", is aimed, like some of the old monetary policy rules which we have encountered, at the general public good; its implementation may contradict the private interests of the banks. The banks may lose profits if, as recommended, they decrease the interest rate while prices are falling; in the opposite case, they may lose customers. "I should like then in all humility", writes Wicksell, "to call attention to the fact that the banks' prime duty is not to earn a great deal of money but to provide the public with a medium of exchange – and to provide this medium in *adequate measure*, to aim at stability of prices. In any case, their obligations to society are enormously more important than their private obligations" (190). Either in sarcasm or as a matter of his social philosophy, Wicksell adds that if the banks could not fulfill their obligations to society as private institutions, the task would be "a worthy activity for the State".

Such a policy proposal calls for cooperation between the banks of the world, or at least between those of the gold standard countries. Wicksell asks whether such cooperation is realistic. What if, for example, a central bank in one specific country fixes its interest rate according to the balance of trade and the exchanges and would not agree to others fixing its rate, since the country would face "efflux of precious metal". The central bank must "retain a free hand to be used *in the last resort*, if not earlier, over bank-rate policy" (191, emphasis in the original). Wicksell's proposed solution to this dilemma comes in the form of another "mechanical metaphor", this time one that he describes as "two degrees of freedom":

> "There is first of all the individual regulation of *relative* rates of interest, which aims at maintaining the rates of exchange, the balance of payments, and the *relative* level of prices, and which, by the nature of the case, must proceed in *opposite* directions in different countries or groups of countries. At the same time, and more important, there can, and should, on occasion come into being a co-operative regulation of the rate of interest, proceeding everywhere *in the same direction* with the object of maintaining the *average* level of prices at a constant height" (192).

Such a bold proposal for international cooperation, based on a theory that systematically links intermediation and exchange, the rate of interest and the level of prices, was rare. Up to this point Wicksell has maintained as an assumption the gold standard, although in the pure credit economy its role was less and less important. However, while assessing the role of gold flows in his discussion of international cooperation on policy, Wicksell suddenly introduces an intriguing reservation that would send us back to the Restriction period. What if, he asks, gold

production increases to the extent that pressure on the banks forces them to reduce the rate of interest, causing prices to rise? "For my part", writes Wicksell, "I regard such an eventuality as no less undesirable than a further fall in prices. . . . [It] would be possible to avoid such a rise of prices only by the *suspension of the free coinage of gold*. This would mark the first step towards the introduction of an ideal standard of value" (193). Such an "international paper standard", he claims, is welcome and is certainly not a cause for "consternation". Wicksell ridicules now the current gold standard system, describing it as a "fairy tale, with its rather senseless and purposeless sending hither and thither of crates of gold, with its digging up of stores of treasure and burying them again in the recesses of the earth" (193). Such a paper system can function if the credit institutions obey the "Wicksell rule" and adopt an interest rate policy that will guarantee both equilibrium in the balance of payments and stable world prices.

Wicksell's second major work on monetary theory, which he wrote a few years after 1898 while already in academic life in Lund and which he revised several times, sheds more light on the historical context of his ideas. There are some differences between *Interest and Prices* and the *Lectures* (*Volume II: Money* is the relevant text), although most scholars agree that the basic theoretical message remains the same.[13] In the *Lectures*, Wicksell proposes a "Positive Solution" to the determination of the interest rate; while he discusses the same basic ideas of 1898 he goes deeper into both the fundamentals and monetary policy. He makes a distinction between "interest on capital" and "interest on money"; the former – the "real rate" – is determined in the production sphere while the latter – the "loan rate" – is the outcome of the loan market. The real and loan rates "more or less coincide"; the real one (natural or normal) serves as an anchor to which the loan rate tends to gravitate. The real rate is determined by savers and investors (see 192–193).

The market rate of interest will settle, sooner or later, at this real rate but the process is not obvious and depends on the specific credit arrangements. Wicksell starts with a simple credit market in which individuals make loans to each other, and then moves to "organized credit" including, especially, banks. Then the "connection between loan interest and interest on capital will become much less simple". The reason for the complexity is due to the fact that unlike individuals who can loan only their available free funds, banks "possess a fund for loans which is always elastic and, on certain assumptions, inexhaustible" (194). As a result, the

[13]See Ohlin (1936) who explains that the "chief reason why Wicksell changed his views so little was undoubtedly that the criticisms which his theory met did not go down to fundamentals" (xii). Ohlin mentions three changes in the 1906 edition, quoting Wicksell, who was aware of them; they relate to (1) the role that saving and investment play in determining the natural and normal rates; (2) the strengthened emphasis on "bridging the gap between price theory and monetary theory" and (3) the actual impact of additional gold production on prices. One important change in the 1915 edition in Ohlin's mind, but not in Wicksell's, concerns the *mutual* effect the two interest rates have on each other (xiii–xvii).

relationship between the natural rate (on capital) and market loan rate will be established "by virtue of the connecting link of price movements" (194).

Wicksell published a paper on his monetary theory in English in the *Economic Journal* in 1907, where a short review of *Interest and Prices* had appeared a few years earlier.[14] But the two pieces did not attract much attention and Wicksell remained anonymous to the English-speaking world for many years. In the 1907 piece, Wicksell outlines what he thinks are the major analytical novelties of his approach and why they have practical consequences for policy. Again, the most difficult and important analytical point relates to the distinction between the rates of interest and profit when banks ("modern forms of credit") exist. Since banks can create as many loans as they wish, not restricted by their own resources like in a private loan transactions system, the "connecting link between interest and profit" poses an analytical difficulty. The equilibrating process will work so that if the bank loan rate of interest is lower than the profit rate, prices will rise, the bank reserves will fall and the banks will be forced to raise their loan rate. After a brief discussion of actual history, Wicksell concludes with his radical, internationally coordinated policy proposal, but admits that this will work only when and if the gold standard stops functioning. In the one note to the article one can locate the point that continued to bother Wicksell in later years: new gold enters the banks in two forms, as additional "lending capital" that can be loaned and as payments for goods by the gold-producing countries. The latter mode may cause higher prices and in some circumstances a rise of interest on loans; the former mode will bring a decline in interest rates. Hence, as the data indicated, both influences are possible and the theory should not be refuted by these facts.

Keynes and Wicksell: Up to *A Treatise on Money* and in the *General Theory*

The question of Wicksell's influence on the theoretical work of Keynes has attracted the attention of many scholars over the years. One obvious similarity between these two great economists is that in addition to their outstanding analytical powers, they both had an urge to address social reality and to shape their societies. Before turning to economics, Wicksell was a devoted Malthusian who preached on population policy; Keynes was involved in many policy issues over the course of his career. They were also both interested in the history of their profession and knew the classics, although their interpretations were not identical and Keynes' was controversial; one can learn a lot about a scholar from how he or she reads the classics. The two certainly had a broader and much longer view than those we usually get today. And, of course, they were both well aware of their respective places in the history of economics.

[14]The paper was read in 1906 before The Economic Section of the British Association.

When he came to know Wicksell's work, Keynes helped to make it accessible to English readers. There is no conclusive evidence as to when Keynes read Wicksell's *Interest and Prices* and the *Lectures;* he did not read them in the original German and both were translated too late for the *Treatise*. Did he come to know these texts before the *Treatise* and the *General Theory*? To the best of my knowledge it is not clear, but Keynes' involvement in the efforts to translate the texts into English suggests a positive answer. As we will see, Keynes clearly held the quality of these texts in high regard.

The depth and timing of Keynes' acquaintance with Wicksell's theory is important since, as is well known, Keynes changed his own theoretical position between 1930 and 1936, just when the texts became available in English. Thus, as I am speaking at a conference dedicated to *Perspectives on Keynesian Economics*, I would like to assess the part Wicksell played, if any, on Keynes' changing theories of monetary policy. I will not attempt a general discussion of the complicated debt Keynes may have owed to Wicksell either directly or via others (see Leijonhufvud (1968, 1981), Laidler (1999)) but will rather address a narrower issue: their views on the "role of monetary policy", to borrow the famous Friedman title, and the debt, if any, of Keynes to Wicksell concerning this specific issue.

As is well known, Keynes also changed his thinking on monetary policy between 1930 and 1936 (Keynes 1930, 1936). In his early writings on monetary policy, Keynes primarily discussed the cycles and specifically prices, and believed – unlike many Cambridgians in the 1920s – that monetary policy could be effective in minimizing volatility. In the *General Theory*, however, he advocated a different line. It seems that Keynes started to appreciate the Wicksellian analysis during the period he spent working on the *Treatise* in the late 1920s. It is likely that he encountered Wicksell when he was reading (and arguing with) Hayek's approach.[15] Keynes quotes Wicksell a few times in the *Treatise* (see below), and it was around this same time that Keynes was directly involved in making Wicksell better known to the English-speaking world by assisting in the efforts to have his works translated.

The essence of the change in Keynes' view on monetary policy is summarized by Laidler in his *Fabricating the Keynesian Revolution*:

> "In both the Tract and the Treatise, Keynes had been optimistic about the power of monetary policy to generate expansion in a depressed economy, far more so than most of his contemporaries, not least, once more, Pigou and Lavington. On that question, too, in 1936 he reverted to what had been the conventional Cambridge view of the 1920s" (Laidler 1999, p. 272).

As Laidler explains, in 1936 in the *General Theory* Keynes introduced a new element to the story that changed it completely. This change had to do with the roles of expectations and instability caused by a fall in the marginal efficiency of capital and a rise in interest rates. Thus, more specifically, it was the difficulty of defining

[15]I owe this point to Mauro Boianovsky's comment at the conference; however, assume no responsibility on his part for any claims made here.

the role of confidence that made monetary policy less effective. Laidler quotes from Keynes (1936) Chapter 22, "Notes on the Trade Cycle":

> "It is the return of confidence, to speak in ordinary language, which is so insusceptible to control in an economy of individualistic capitalism. This is the aspect of the slump which bankers and business men have been right in emphasising, and which the economists who have put their faith in a 'purely monetary' remedy have underestimated" (Keynes 1936, p. 317).

Don Patinkin, who studied Keynes for many years, came to know Wicksell early in his career and as early as 1952 wrote a paper entitled "Wicksell's 'Cumulative Process'" (Patinkin 1952). In his famous *Money, Interest and Prices* (Patinkin 1965), Patinkin devotes an appendix to Wicksell (Note E, pp. 581–597 of the second edition).[16] Patinkin returned to Wicksell again in 1982; in *Anticipations of the General Theory? The Stockholm School*[17] he summarizes his view of Wicksell after the 30 years he's spent studying him:

> "Let me begin at the end: Wicksell was basically a quantity theorist. The problem that troubled him, however, was that the dynamic mechanism of this theory had been spelled out (according to him) only for an economy in which money – which Wicksell defined as gold or other metallic currency – circulated as the main medium of exchange and was held by individuals. . . . But, Wicksell asked, how was such a pressure generated in an economy in which most of the money (so defined) accrued not to individuals, but to banks to be held as a reserve against deposits – in which case there was no real-balance effect?" (Patinkin, 41).

The answer to the question, argues Patinkin, marks the point at which Wicksell's famous cumulative process enters the analysis. The mechanism, which is responsible for price changes, works through its impact on "general demand" relative to supply. This conclusion is, of course, in direct contrast to Say's law as Wicksell explains in the *Lectures Vol. II*, and is also the basis for the argument – an argument that Patinkin rejects – that the Stockholm school could claim precedence to Keynes. Wicksell "extends the quantity theory to an economy with a banking system", argues Patinkin; "[He] explains how an increase in the quantity of money in such an economy too ultimately brings it to a new equilibrium position at a higher price level" (42). Patinkin adds that this seemed to him in both 1952 and 1982 to be Wicksell's "central message". However, in the 1982 text, Patinkin worries that his earlier conclusion that Wicksell was a quantity theorist might have been wrong, since so many Swedish economists (Myrdal, Ohlin) saw Wicksell as "an opponent of the quantity theory" (42). Patinkin's remarks on the possibility that he was misreading Wicksell are worth quoting in full:

[16]In the Preface to the first edition, Patinkin writes: "from the text the reader will likewise see how much of my thinking has been colored by Knut Wicksell's classic *Interest and Prices*" (xix, second edition). The other influences he mentions are Hicks' *Value and Capital;* Keynes' *General Theory;* Samuelson's *Foundations of Economic Analysis* on dynamic stability analysis and Marget's *Theory of Prices* on the historical literature.

[17]This is Chapter 2 of the book with the same title.

> "But may I suggest that though the Swedish economists should presumably know best about Wicksell, they are also likely to be more susceptible than the outsider like myself to the temptation to rewrite Wicksell in their own image – just as many of those who today call themselves Keynesians have attempted to do with the *General Theory* . . ." (42).

In any case, Patinkin concludes, Wicksell and many others before Keynes focused on the price level, while Keynes shifted the focus to output. To be clear, Patinkin thinks that Wicksell was a quantity theorist; in the section on Wicksell I explained why I think that he was less of a quantity theorist than Patinkin argues.[18]

Wicksell and Keynes met once, in 1916; the former was 65 years old and the latter 33 years old. Gardlund (1958) writes about Wicksell's 1916 visit to England where he also met Marshall, adding:

> "The longed-for climax of the visit, however, was the lunch with J. M. Keynes, 'their keenest theorist', who had also had practical experience in the Treasury during the war. Keynes did not in fact have much time to spare for this foreign colleague whose name meant little to him. But Wicksell made the most of his opportunity and succeeded in prolonging the conversation by walking a little way with the younger man after their meal" (294–295).[19]

By the time Keynes wrote the *Treatise* he was clearly more appreciative of Wicksell. In the *Treatise*, discussing the crucial fundamental equations, Keynes writes:

> "Following Wicksell, it will be convenient to call the rate of interest which would cause the second term of second Fundamental Equation to be zero the *natural-rate* of interest which actually prevails in *market-rate* of interest. Thus the natural-rate of interest is the rate at which saving and the value of investment are exactly balanced, so that the price level of output as a whole (II) exactly corresponds to the money-rate of the efficiency-earning of the Factors of Production. Every departure of the market-rate from the natural-rate, on the other hand, to set up a disturbance of the price-level by causing the second term of the second Fundamental Equation to depart from zero.
>
> We have, therefore, something with which the ordinary Quantity Equation does not furnish us, namely, a simple and direct explanation why a rise in the Bank-rate trends, in so far as it modifies the effective rates of interest, to depress price-levels. To a more complete explanation of the theory of Bank-rate we shall return in Chapters 13 and 37" (Keynes 1930, Vol I, 154–155).

[18]The correspondence between Keynes and Ohlin that was published in 1979 in a supplementary volume to the collected works, corrects the "misleading impression" that the Swedish economists had no impact on Keynes. Patinkin adds that the correspondence reveals Ohlin's view that "Keynes was the moving spirit" in the translation of Wicksell's *Interest and Prices* in 1935.

[19]Gradlund adds: "After this meeting he wrote his wife:

'Then to-day I met Keynes and lunched with him at his club. We had a very interesting conversation. On some points he was not very well informed; for one thing he had no very clear idea of how to go about arranging a rational standard of value; thought it would be relatively easy, for instance on the lines suggested by Irving Fisher; he was very surprised when I began to criticize Fisher, but admitted straight away that my objections were valid.

On the other hand he has a good mind and, as I said, I gained much from our conversation; only wished it could have been longer; but he had to go as soon as he finished lunch and had our coffee – however I walked with him to his barber's'" (295).

When Keynes develops these ideas in Chapter 13, entitled "The 'Modus Operandi' of Bank-Rate", he opens by being a historian of the doctrine: "Before, however, we embark on a detailed development of these ideas, it may be helpful to outline the accepted doctrine as it has been developed historically and as it exists today. It is surprisingly difficult to do this". He rejects the treatments of Marshal, Pigou, Taussing and Irving Fisher and adds that Hawtrey "has a little more to say". But then comes the following:

> "There remains, however, one outstanding attempt at a systematic treatment, namely Knut Wicksell's *Geldzins und Guterpreise*, published in German in 1898, a book which deserves more fame and much more attention than it has received from English speaking economists. In substance and intention Wicksell's theory is closely akin (much more closely than Cassel's version of Wicksell) to the theory of this Treatise, though he was not successful, in my opinion, in linking up his Theory of Bank-rate to the Quantity Equation" (Volume I, 186).

In a reminder to those interested in the history of monetary theory, Keynes continues by pointing out that those discussions started after the crisis 1836/1837 and led to the famous 1844 Bank Act. Ricardo, argues Keynes, did not have a theory of the Bank-rate since the rate of interest during his lifetime was subject to a legal maximum of 5% and the market rate did not depart from that rate. In 1839, however, the rate was actually above the 5% limit. In the 90 years that passed since, Keynes argues, three theories had been offered: the first is associated with Lord Overstone (Loyd, the Currency School leader), with the "usual Quantity Theory of Money", and with Marshall and Hawtrey (187–188). The second is that of the "practical bankers" who use the bank rate as a defensive weapon to protect the system; Bagehot belongs to this tendency. The third tendency is closest to Keynes' own:

> "The third strand of thought is the one which comes nearest to what seems to me to be the essence of the matter. It also winds through many previous discussions, but seldom or never in a clear or distinct fashion. This strand of thought conceives of Bank-rate as influencing in some way the rate of investment, or at least the rate of some kinds of investment, and, perhaps in the case of Wicksell and Cassel, as influencing the rate of investment relatively to that of saving" (Keynes 1930, Volume I, 190).

Let us move to Keynes' views in 1936. Keynes discusses Wicksell at some length in Chapter 17 of the *General Theory*, entitled "The Essential Properties of Interest and Money". In it he admits that he had "overlooked" some important aspects of Wicksell's thought. Let me quote at length:

> "In my *Treatise on Money* I defined what purported to be a unique rate of interest, which I called the *natural rate* of interest – namely, the rate of interest which, in the terminology of my *Treatise,* preserved equality between the rate of saving (as there defined) and the rate of investment. I believed this to be a development and clarification of Wicksell's 'natural rate of interest', which was, according to him, the rate which would preserve the stability of some, not quite clearly specified, price-level.
>
> I had, however, overlooked the fact that in any given society there is, on this definition, a *different* natural rate of interest for each hypothetical level of employment. And, similarly, for every rate of interest there is a level of employment for which that rate is the 'natural'

> rate, in the sense that the system will be in equilibrium with that rate of interest and that level of employment. Thus it was a mistake to speak of *the* natural rate of interest or to suggest that the above definition would yield a unique value for the rate of interest irrespective of the level of employment. I had not then understood that, in certain conditions, the system could be in equilibrium with less than full employment" (Keynes 1936, 242–243).

Keynes had thus overlooked the idea that the natural rate can change and that, as a result one of the consequences should have recognized the case where "the system could be in equilibrium with less than full employment". Did Keynes credit Wicksell with this conclusion? Not directly, but one should read the last section of Chapter 17 carefully, and appreciate the debt he now admits having to Wicksell. Keynes continues the discussion of full employment, saving and investment and equilibrium with reservations that are not directed at Wicksell but at his own 1930 text; one can, however, read these reservations as a departure from his appreciation of Wicksell. In 1936, as we said above, Keynes shifts the focus to determination of employment rather than that of the price level. Thus, he was asking a different question altogether from the one that occupied Wicksell. Here, Wicksell's natural rate does not help.

> "I am now no longer of the opinion that the concept of a 'natural' rate of interest, which previously seemed to me a most promising idea, has anything very useful or significant to contribute to our analysis. It is merely the rate of interest which will preserve the *status quo;* and, in general, we have no predominant interest in the *status quo* as such.
>
> If there is any such rate of interest, which is unique and significant, it must be the rate which we might term the *neutral* rate of interest, namely, the natural rate in the above sense which is consistent with *full* employment, given the other parameters of the system; though this rate might be better described, perhaps, as the *optimum* rate.
>
> The neutral rate of interest can be more strictly defined as the rate of interest which prevails in equilibrium when output and employment are such that the elasticity of employment as a whole is zero" (Keynes 1936, 243).

In a famous and controversial statement summarizing the classical theory of the rate of interest as the theory that guarantees full employment, Keynes mentions policy:

> "The above gives us, once again, the answer to the question as to what tacit assumption is required to make sense of the classical theory of the rate of interest. This theory assumes either that the actual rate of interest is always equal to the neutral rate of interest in the sense in which we have just defined the latter, or alternatively that the actual rate of interest is always equal to the rate of interest which will maintain employment at some specified constant level. If the traditional theory is thus interpreted, there is little or nothing in its practical conclusions to which we need take exception. *The classical theory assumes that the banking authority or natural forces cause the market-rate of interest to satisfy one or other of the above conditions;* and it investigates what laws will govern the application and rewards of the community's productive resources subject to this assumption. With this limitation in force, the volume of output depends solely on the assumed constant level of employment in conjunction with the current equipment and technique; and we are safely ensconced in a Ricardian world" (Keynes 1936, 243–244, my emphasis).

Keynes in 1936 departs from this Ricardian position, although he does not trust monetary policy to correct the market failure that he describes. The tool that was

central to Wicksell and some of the Cambridgians, including the early Keynes, cannot fulfill this task. Thus, Wicksell, who was instrumental to the *Treatise*, was of much less help now.

In a 1937 letter to Myrdal "concerning the doctrines of Wicksell", Keynes refers to Ohlin's paper in the *Economic Journal* of March that year, wherein Ohlin writes:

> "Already Wicksell had stressed that the consumption purchases are governed by that part of individual incomes which people want to consume, whereas investment purchases are not directly governed by the part of income people want to save. The decisions to save and the decisions to invest are taken largely by different individuals, and there is no mechanism which guarantees that the volume of savings and of investment will always be equal. This is the very essence of the Wicksellian approach. Wicksell goes on to investigate what role the rate of interest can play in making them equal, and what happens when they are not made equal".

On which Keynes comments:

> "This took me by surprise and I asked Ohlin to let me have some references where I could see Wicksell's actual wording on these matters. His letter to you presumably arises out of this question. There is not, however, very much connection, I think, between the quotations you have sent and the passage Ohlin gives. Is not Ohlin reading into Wicksell a much later order of ideas?
>
> I do not doubt that Wicksell held the normal old-fashioned view that saving and investment must equal and that he investigated the mechanism by which, as he supposed, the rate of interest made them equal. Whether he also investigated, as Ohlin alleges, what happens when they are not equal I am not aware. But in any case this is diametrically the opposite from my approach ... and the opposite to Ohlin's own view... [we] agreed in supposing that it is not the rate of interest which makes saving and investment equal, since they are equal *ex definitione* whatever the rate of interest" (Keynes *Collected Writings*, Vol. 29, 262).

This may be why, as Lachman wrote many years later, Keynes was "now so fierce on Wicksell and Wicksellians? He spoke with a mixture of sadness and irony" (Young 1987, 52).

Whether Wicksell analyzed the possibility of a gap between savings and investment, whether he presents an early attempt to address "aggregate demand", is beyond the scope of the present paper. But clearly, Keynes' enthusiasm for Wicksell had disappeared by the late 1930s.

I would like to thank my discussant, Warren Young, and the participants in the conference, including Mauro Boianovsky, Bob Dimand, David Laidler and Cristina Marcuzzo for very helpful comments. Of course, only I am responsible for the remaining errors.

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Comments on Arnon "Keynes, Wicksell and Active Monetary Policy"

Warren Young

A caveat before presenting my comments on this paper; to paraphrase Hicks in a letter to Robertson, I defer to the author's knowledge of the early authorities on monetary theory. I will thus limit my comments to the Wicksell–Keynes part of the paper and its relation to "active monetary policy" that is from section "Wicksell's Monetary Theory" onwards. In order to do so, however I have to revert to the two definitions of "active" policy the author presents in the introductory section of his paper. On one page, he defines the "active form of central banking" as "actions aimed at improving the performance of the real economy via monetary instruments". On another page 6, he defines "active" monetary policy as "interventions" that try "to affect the real economy".

Given the above definitions, I can now comment on two issues. The first may affect the way in which we perceive Wicksell's views on "active monetary policy". The second involves Patinkin's interpretation of Wicksell as a "quantity theorist" in the context of his own notion of "canonization".

With regard to the first issue, in volume II of his *Lectures on Political Economy* Wicksell wrote (Kelly reprint, 1978 [1906, trans. pub. 1935], 128–129):

> All practical proposals for the improvement of currency systems actually proceed, though more or less consciously, from the desire to guarantee this stability of value [money]. When it is said that governments or banks should seek to provide enough money of full value, or a monetary system at once sound and flexible, all that is really meant is that the value of money should be protected against violent fluctuations, either downwards in the form of the depreciation of money or upwards in the form of a fall in commodity prices: this includes a demand for the preservation of the stability of value of money in space, i.e. the maintenance of the currency unit of one country at the same level as that of another.
>
> Sometimes, it is true, we hear it said that certain changes in the value of money, especially a gradual decline or progressive rise in commodity prices, might be preferred under certain circumstances to complete stability. Rising prices would act as a stimulus to enterprise and a falling value of money would free debtors from the burden of obligations thoughtlessly incurred. This view is, however, evidently naive.
>
> *It need only be said that if this fall in the value money is a result of our own deliberate policy, or indeed can be anticipated and foreseen, then these supposed beneficial effects will never occur, since the approaching rise in prices will be taken into account in all transactions by reasonably intelligent people* [my emphasis].

What is contemplated is, therefore, unforeseen rises in price. The result of this would seem to be that we should not cross our on arms and wait in order not to frustrate the beneficial workings of nature. But nature does not always guarantee rising prices, falling prices also occur.

At least one observer thinks the above indicates that as early as 1906, Wicksell understood the concept of rational expectations. According to Humphrey (2004, 48)

this was "a concept he employed after World War I to claim that a preannounced and fully anticipated deflation would not affect real activity". While Humphrey was somewhat "cagey" in his attempt to answer the question: "Is Greenspan a Wicksellian?", he did state that "Fed policymakers are Wicksellian only in the broad sense that all modern monetary economists are. They concur with some but not all of Wicksell's views" (2004, 48). A relevant question here, then becomes: "Was Maynard Keynes a Wicksellian"? [As an aside, if the answer is yes, then could he also be considered as the "other English Wicksellian"?, the first being Robertson, as manifest, for example, in *Banking Policy and the Price Level* (1926).]

At this point, however, may I register strong disagreement with Humphrey's assertion regarding "the mutual exclusivity of real and monetary models" in the "real business cycle" context. This goes against what is now known regarding the extension and metamorphosis of Kydland-Prescott and Long-Plosser into their second and third generation variants, and even against the initial extension by Kydland of his own model into the monetary sphere (see Young 2008).

My second comment relates to the question of whether Wicksell was a quantity theorist, and Patinkin's view of this. Interestingly enough, the author cites those passages where Patinkin counterpoints his approach with that of "the Swedish economists" who saw Wicksell as "an opponent of the quantity theory". The author writes that Patinkin "worries" that he may have been "misreading" Wicksell. In view of Patinkin's concept of canonization in relation to IS-LM and Keynesian economics, which I discussed in my paper in this symposium, I would give an alternative explanation of the passages cited from Patinikin, that is to say, the "Swedish economists" may have attempted to "rewrite Wicksell in their own image", to use Patinkin's own terms, parallel those Cambridge (UK) economists who attempted to rewrite "the *General Theory*".

However there is a sting in the tail of Patinkin's approach to Wicksell, for Marget may have influenced his approach to Wicksell. In Volume 1 of *Theory of Prices* (1938), which seems to have influenced Patinkin's views on the history of monetary thought, Marget attempted to make the case for Wicksell as a quantity theorist (see Mehrling, 2001). Whether or not Margett – or Patinkin, for that matter – are correct however, is beyond the scope of this comment. But perhaps a first effort could be made by studying *in detail* the contributions of "Swedish economists" such as Myrdal, who went *beyond* Wicksell on the quantity theory, and according to at least one observer, developed a theory that rejected its postulates using a Wicksellian approach (Tobon 2006).

"The Consequences to the Banks of the Collapse of Money Values", 1931 and 2009

Robert W. Dimand

Abstract John Maynard Keynes's August 1931 essay on "The Consequences to the Banks of the Collapse of Money Values" is as pertinent to the understanding the financial crises in 2009 as when it was originally written. Keynes stressed the importance of debts being contracted in money terms, creating the possibility that a wave of "bankruptcies, defaults, and repudiations" caused by a collapse of the money value of assets – or merely the fear of such events – could "shake the capitalist order to its foundations". This paper examines Keynes's 1931 analysis and its continuing relevance to understanding the fragility of the financial system.

Introduction

On October 16, 1931,[1] just weeks after Britain had reluctantly abandoned the gold exchange standard (to which it had returned in 1925 at the prewar parity with gold and the US dollar, despite Keynes's warnings), John Maynard Keynes sent to his publisher Daniel Macmillan his *Essays in Persuasion*, a collection of "the croakings of 12 years, the croakings of a Cassandra who could never influence the course of

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"A 'sound' banker, alas! is not one who foresees danger and avoids it, but one who, when he is ruined, is ruined in a conventional and orthodox way along with his fellows, so that no one can really blame him . . . The present signs indicate that the bankers of the world are bent on suicide . . . It is necessarily part of the business of a banker to maintain appearances and to profess a conventional respectability which is more than human. Lifelong practices of this kind make them the most romantic and the least realistic of men" – John Maynard Keynes (1931, Essays in Persuasion, Macmillan, London, pp. 176, 178).

[1] Skidelsky (2003, p. 473). Keynes's preface, dated Nov 8, was added shortly before publication.

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events in time. The volume might have been entitled 'Essays in Prophecy and Persuasion,' for the *Prophecy*, unfortunately, has been more successful than the *Persuasion*" (Keynes 1931, p. v). With one exception, the 37 items in the collection were drawn from his earlier books such as *The Economic Consequences of the Peace* or *A Tract on Monetary Reform*, or from his articles in newspapers and weeklies, mostly *The Nation and Athenaeum*, of which he was chairman. The exception was an essay on "The Consequences to the Banks of the Collapse of Money Values" (Keynes 1931, pp. 168–178; Keynes 1971, pp. 150–158), composed in August 1931 and "Not previously published in Great Britain." A year before, observed Keynes in the opening lines of that essay, the world's greatest economic problem had been the unemployment and waste of productive resources resulting from the failure of industry to earn normal profits, but "To-day, in many parts of the world, it is the serious embarrassment of the banks which is the cause of our greatest concern. The shattering German crisis of July 1931, which took the world by more surprise than it should, was in its essence a banking crisis." Writing after the crash of the Credit-Anstalt in Austria with its ripple effects in neighbouring countries[2] (which in September forced Britain to abandon the prewar parity of the pound sterling), but nearly 2 years before successive US banking crises culminated in the "Bank Holiday" of March 1933, Keynes examined causes, consequences, and possible remedies of the fragility of the world's system of financial intermediation. The essay in which he did so has been quoted from time to time (e.g. Minsky 1975, p. 117), but has not figured prominently in the vast literature on Keynes, or even in the literature on financial turmoil of the early 1930s (primarily on the US banking crisis, with a smaller European literature on the Kreditanstalt crash). For decades, the prevailing view among economists, even those deeply concerned with other aspects of Keynes's legacy (but with a few exceptions, notably Charles Kindleberger 1978), considered a repetition of the general breakdown of banking and financial intermediation in the developed countries to be at most a highly unlikely theoretical possibility, not required much attention (see the single sentence devoted to Hyman Minsky's 1964 statement of his theory of financial instability by Basil Moore 1968, p. 116 – which is one sentence more on the topic than competing textbooks would have had). Conducting stress tests on banks, thought experiments about the direct and indirect impact of system-wide fluctuations in national income or prices on major financial institutions, was attempted in the Minsky–Bonen experiments reported by Minsky (1964) (see Minsky 1982, pp. 126, 157), was not much followed up for 45 years (after which it was taken up with both vigour and haste). But in 2009 "The Consequences to the Banks of the Collapse of Money Values" is just as current a topic as it was in 1931, with the difference that the money values whose collapse is imperilling the banks are asset prices rather than the general price level.

[2]See Schubert (1991) on the Credit-Anstalt crisis and Balderston (1994) on the German banking crisis of 1931.

Keynes on Debts and Deflation, December 1930 to June 1931

In December 1930, writing in *The Nation and Athenaeum* on "The Great Slump of 1930," Keynes (1931, pp. 138–139), having warned that, because general wage cuts reduce purchasing power, "neither the restriction of output nor the reduction of wages serves in itself to restore equilibrium," went on to emphasize that "Moreover, even if we were to succeed eventually in re-establishing output at the lower level of money-wages appropriate to (say) the pre-war level of prices, our troubles would not be at an end. For since 1914 an immense burden of bonded debt, both national and international, has been contracted, which is fixed in terms of money. Thus every fall of prices increases the value of the money in which it is fixed. For example, if we were to settle down to the pre-war level of prices, the British National Debt would be nearly 40% greater than it was in 1924 and double what it was in 1920; . . . the obligations of such debtor countries as those of South America and Australia would become insupportable without a reduction of their standard of life for the benefit of their creditors; agriculturalists and householders throughout the world, who have borrowed on mortgage, would find themselves the victims of their creditors. In such a situation it must be doubtful whether the necessary adjustments could be made in time to prevent a series of bankruptcies, defaults, and repudiations which would shake the capitalist order to its foundations." Keynes's year-end reflections on "The Great Slump of 1930" are well-known, but primarily for his image of the world economy having "magneto trouble": there was no need to scrap the whole machine, just to repair the starter – a striking analogy even if Keynes did not drive his own car, but hired a driver. The role of aggregate demand management in a slump is to jumpstart a stalled economy. But notice should also be taken of Keynes's stress in that essay on the importance of debts being contracted in money terms, creating the possibility that a wave of "bankruptcies, defaults, and repudiations" – or merely the fear of such events – could "shake the capitalist order to its foundations."

Minus the word "capitalist" (perhaps unsuited to a Midwestern audience in the USA), the same phrase continued to haunt Keynes's thoughts in June 1931, when he gave three Harris Foundation Lectures on "An Economic Analysis of Unemployment" at the University of Chicago (published in Wright 1931, reprinted in Keynes 1973, pp. 343–367). There Keynes (1973, p. 360) warned that an attempt to reduce money wages in the UK by as much as wholesale prices had fallen "would be to shake the social order to its foundation." The main theme of Keynes's lectures in Chicago reflected the analysis of his *Treatise on Money* (1930), which he had summarized in five days of private evidence to the Macmillan Committee on Finance and Industry: "profits" (net of the normal return on investment) determine investment decisions, so that slumps occur when profits (as defined in the *Treatise*, as the excess of investment over saving, with windfalls not counted as part of income) are negative (see Clarke 1988, Dimand 1988). Wage cuts cannot increase profits and stimulate investment, because wage cuts reduce receipts by as much as costs of production are lowered (in a closed economy, such as the world as a whole).

This analysis, implicitly assuming a marginal propensity to spend equal to one, had the troubling implication that a "thrift campaign" (an increase in saving) would cause a cumulative deflation until all economic activity ceased and everyone starved to death, a result that Don Patinkin's secretary famously rendered as a "coroner solution" (Patinkin 1982, p. 15). Keynes tried out various approaches to the world economic crisis, not always with lasting success: Skidelsky (2003, p. 472) reports Keynes "investing money in 1930 in a process which guaranteed to turn base metal into gold, and speculating that the discovery might solve the world's unemployment problem."[3] But Keynes's Harris Foundation Lectures had something more in them, something that followed from "The Great Slump of 1930:" emphasis on the significance of debt contracts being made in money terms.

The immediate irritant that provoked Keynes's analysis was an address in London by O. M. W. Sprague, the Edmund Cogswell Converse Professor of Banking and Finance at Harvard Business School (and future president, in 1937, of the American Economic Association), who was chief economic adviser to Montagu Norman's Bank of England from 1930 to 1933, and had previously been economic adviser to Benjamin Strong of the Federal Reserve Bank of New York.[4] Stating that Sprague's address had "attracted much attention," Keynes (1973, p. 359) quoted Sprague as urging that "manufactured costs and prices should come down to equilibrium level with agricultural prices rather than that we should try to get agricultural prices up to an equilibrium level with the higher prices of manufactured goods." Keynes announced that "I dissent very strongly from this view and I should like, if I could, to provoke vehement controversy – a real discussion of the problem – in the hope that out of the clash of minds something useful might emerge. Until we have decided whether or not we should wish prices to rise we are drifting without clear intentions in a rudderless world" (Keynes 1973, pp. 359–360).

Keynes was indeed vehement in his controversy with Oliver Sprague. "The notion that you solve the farmers' problem by bringing down manufacturing costs so that their own produce will exchange for the same quantity of manufactured goods as formerly is to mistake the situation altogether, for you would at the same time have greatly increased the farmers' burden of mortgages which was already too high. Or take another case – loans against buildings. If the cost of new building were to fall to a parity with the price of raw materials, what would become of the

[3]Irving Fisher also showed initial enthusiasm for the process, and Reginald McKenna, chairman of the Midland Bank (then the world's largest bank), Macmillan Committee member, and former Chancellor of the Exchequer, invested in it.

[4]For 6 months in 1933, Sprague was financial adviser and executive assistant to the Acting Secretary of the US Treasury, Dean Acheson, and, like Acheson, attempted to persuade President Roosevelt to support the gold standard. The Reichsbank, the Banque de France, and the League of Nations also solicited his advice between the wars. Far from being a straw man (or, in British parlance, an Aunt Sally), Oliver Sprague was a key insider in the making of economic policy, a powerful voice for the gold standard, deflation, and pre-Keynesian orthodoxy (see Sprague 1934, 1938; Herren 1997). He is now best remembered for his National Monetary Commission study, *History of Crises under the National Banking System* (Sprague 1910).

security for existing loans?" protested Keynes (1973, p. 361). "Thus national debts, war debts, obligations between the creditor and debtor nations, farm mortgages, real estate mortgages – all this financial structure would be deranged by the adoption of Dr Sprague's proposal. A widespread bankruptcy, default, and repudiation of bonds would necessarily ensue. Banks would be in jeopardy. I need not continue the catalogue. And what would be the advantage of having caused so much ruin? I do not know. Dr Sprague did not tell us that."

Keynes's remarks in "The Great Slump of 1930" in December 1930 and in the third of his Harris Foundation Lectures in June 1931 on a collapse of money values as a cause of bankruptcy, repudiation, and default reveal the development of a line of thought that the German banking crisis of July 1931 precipitated in August 1931 into his essay on "The Consequences to the Banks of the Collapse of Money Values."

Keynes on Banking Crises, August 1931

In August 1931, Keynes (1931, pp. 168–169) gazed with dismay on the state of the world's banking system: "That the top-heavy position, which ultimately crumbled to the ground, should have been built up at all, was. In my opinion, a sin against the principles of sound banking. One watched its erection with amazement and terror. But the fact which was primarily responsible for bringing it down was a factor for which the individual bankers were not responsible and which very few people foresaw – namely, the enormous change in the value of gold money and consequently in the burden of indebtedness which debtors, in all countries adhering to the gold standard, had contracted to pay in gold." In a remarkable but long overlooked empirical study using a 29-nation data set, Irving Fisher (1935) documented the argument that the fixed exchange rates of the gold standard allowed crisis and depression to be transmitted among gold standard countries but not to other countries (Dimand 2003), a finding much later independently rediscovered (see Temin 1989; Eichengreen 1992). The financial crisis that began in August 2007 differs in several respects from the one that Keynes surveyed in August 1931: there are not now fixed exchange rates between most currencies (but the European monetary union may be viewed as a system of irrevocably fixed exchange rates) and, instead of both asset prices and the general price level falling sharply, the present crisis has been characterized by a collapse of asset prices (undermining the collateral underlying debts) and by fear of a general price deflation, causing a risk in risk premiums, a scramble for liquidity, and the freezing of some credit markets (the auction rate note market in the US, the asset-backed commercial paper market in Canada, and, until governments intervened with massive guarantees, the interbank overnight market). But two key elements of Keynes's essay in August 1931 remain pertinent to the experience of the 2 years from August 2007 to the present. First, financial institutions committed many "a sin against the principles of sound

banking" – as Hyman Minsky (1982, 1986) repeatedly noted, a prolonged period of stability is itself destabilizing because it induces complacency about system risk. Second, although the overconfidence of mortgage lenders, investment banks, and hedge funds exposed them to more risk than they realized, they came to grief not from their own idiosyncratic follies, but from a systemic shock, the bursting of an asset price bubble, when it turned that house prices, to use Irving Fisher's phrase about a comparable situation, had not reached "a permanently high plateau."

"Modest fluctuations in the value of money, such as those which we have frequently experienced in the past, do not vitally concern the banks which have interposed their guarantee between the depositor and the debtor," remarked Keynes (1931, pp. 170–172). "For the banks allow beforehand for some measure of fluctuation in the value both of particular assets and of real assets in general ... Experience has led to the fixing of conventional percentages for the 'margin' as being reasonably safe in all ordinary circumstances ... But consider what happens when the downward change in the money value of assets within a brief period of time *exceeds* the amount of the conventional 'margin' over a large part of the assets against which money has been borrowed. The horrible possibilities to the banks are immediately obvious. Fortunately, this is a very rare, indeed a unique event. For it had never happened in the modern history of the world prior to the year 1931." Large upward changes in the money value of assets, "however disastrous in other ways, did nothing to jeopardize the position of the banks; for it increased the amount of their 'margins.' There was a large downward movement in the slump of 1921, but that was from an exceptionally high level of values which had ruled only for a few months or weeks, so that only a small proportion of the banks' loans were based on such values." In the years following the Wall Street crash of 1929, the collapse of asset prices was followed, first in the US and then in other countries whose currencies were tied to the US dollar through the gold exchange standard, by a decline in wholesale and consumer prices, as the money supply was reduced (without a reduction of the monetary base) by the cash drain from banks and the efforts of banks to reassure depositors by increasing their reserve/deposit ratios (see Friedman and Schwartz 1963 for the classic account of this process). Since 2007, central banks, by massive injections of liquidity into financial markets, and governments, by extending deposit guarantees and by directly providing equity to banks, have limited declines in the general price level, but the decline in the money value of real assets has still shaken the financial system.

"Banks and bankers are by nature blind," lamented Keynes (1931, p. 176). "Some of them have even welcomed the fall of prices towards what, in their innocence, they have deemed the just and 'natural' and inevitable level of pre-war, that is to say, the level of prices to which their minds became accustomed in their formative years. In the United States some of them employ so-called 'economists'[5] who tell us even to-day that our troubles are due to the fact that the prices

[5]Benjamin M. Anderson, Jr., of the Chase National Bank, whose writings have been reprinted by the Liberty Fund, was particularly vocal at the time. More recently, on the morning after the

of some commodities and some services have not fallen enough, regardless of what should be the obvious fact that their cure, if it could be realised, would be a menace to the solvency of their institution. A 'sound' banker, alas! Is not one who foresees danger and avoids it, but one who, when he is ruined, is ruined in a conventional and orthodox way along with his fellows, so that no one can really blame him." Here, Keynes identified the rational basis for herd behaviour among bankers (in recent years, particularly investment bankers): the cost to one's career of being wrong when most others are right far exceeds the cost of being wrong along with one's peers.

As Chairman of the Board of the National Mutual Life Assurance Society from 1921 to 1938 and of the Provincial Insurance Company from 1923 until his death in 1946, and as regular reviewer of the speeches of the bank chairmen to their annual meetings[6] (Keynes 1931, pp. 220–243), Keynes considered himself well acquainted with ways of thought in the financial community of the City of London (Davenport 1975).[7] "The present signs suggest that the bankers of the world are bent on suicide … It is so much the stock-in-trade [of bankers] that their position should not be questioned, that they do not even question it themselves until it is too late. Like the honest citizens they are, they feel a proper indignation at the perils of the wicked world in which they live, – when the perils mature; but they do not foresee them," concluded Keynes (1931, p. 178). "A Bankers' Conspiracy! The idea is absurd! I only wish there were one! So, if they are saved, it will be, I expect, in their own despite."

Debts and Deflation in *the General Theory*

While the fact that wage bargains are typically made in terms of money is omnipresent throughout Keynes's *General Theory of Employment, Interest and Money* (1936), the importance of debts being negotiated in terms in money appears in

bankruptcy of Lehman Brothers, the editorial page of the *Financial Post* announced that the Federal Reserve had finally ended the financial crisis by allowing Lehman Brothers to fail. Within days, as a result of the turmoil afflicting the counter parties of Lehman's trades, and in turn their counter parties, forced the US government to bail out the insurance firm AIG.

[6]In February 1925, as the bank chairmen considered the impending return to gold, Keynes (1931, p. 226) remarked, "Some of our chairmen were like him who, being asked whether he believed that, when he was dead, he would enjoy perfect bliss eternally, replied that of course he did, but would rather not discuss such an unpleasant subject."

[7]Davenport (1975, pp. 228) recalled that Keynes's disdain for conventional bankers "came to the boil … during the 1931 sterling crisis when he inveighed against the bankers' deflationism … Unfortunately, we had a banker on our board [at National Mutual Life Assurance] – Francis Curzon, the brother of the great Marquess Curzon. He was deputy chairman of the Westminster Bank. On one occasion when there was an awkward dispute over investment policy Keynes turned on him and said: 'Really, Curzon, you have all the pomposity of your brother and not a scrap of his intelligence.'"

Chapter 19, "Changes in Money-Wages" (Keynes 1936, pp. 264, 268–269, 271). Chapter 19 is crucial to Keynes's claim for the generality of his theory, because, if its argument is accepted, it demonstrates that Keynes's results (effective demand affects employment and real output) hold even without any rigidity of nominal wages and that expanding aggregate demand through fiscal and monetary policy is preferable to nominal wage cuts as a response to a slump (see Minsky 1975, Tobin 1980, Dimand 1988, 2005). Greater downward flexibility of money wage rates may be destabilizing, making things worse. Keynes (1936, p. 264) warned that "if the fall of wages and prices goes far, the embarrassment of those entrepreneurs who are heavily indebted may soon reach the point of insolvency – with severely adverse effects on investment." He argued that "The method of increasing the quantity of money in terms of wage-units by decreasing the wage-unit increases proportionately the burden of debt; whereas the method of producing the same result by increasing the quantity of money whilst leaving the wage-unit unchanged has the opposite effect. Having regard to the excessive burden of many types of debt, it can only be an inexperienced person who would prefer the former" (1936, pp. 268–269). The concerns and arguments advanced by Keynes in "The Consequences to the Banks of the Collapse of Money Values" were thus carried over by him to *The General Theory*. They do not appear until Chapter 19 because until that point in the book the money wage rate is taken as given (a simplifying assumption that turns out, in Chapter 19, also to be a policy recommendation in the interests of stability). When in Chapter 19 Keynes allowed for changes in money wages and presented an analysis to show that the results of the previous 18 chapters still held if money wages were flexible, the argument about debts, deflation, and the risk of bankruptcy that he had advanced in 1931 reappeared as a key part of his analysis.

Drawing on Chapter 19 of Keynes's *General Theory* and on Irving Fisher's "Debt–Deflation Theory of Great Depressions" (1933), Hyman Minsky (1975, 1982, 1986) and James Tobin (1975, 1980) argued that, notwithstanding the Pigou-Haberler real balance effect, nominal wage cuts caused by high unemployment and an output gap could further reduce real aggregate demand, moving the economy further away from full employment rather than back towards full employment. Don Patinkin (1948, 1965) concluded that A. C. Pigou had won his battle with Keynes on the field of pure equilibrium theory, even if Keynes's analysis was (as Pigou agreed) more relevant for practical policy. A *lower* price level, due to lower nominal wage rates, would increase the real value of outside money (gold and bank reserves, not backed by debt) which would have a wealth effect on consumption (the real balance effect), even if the zero bound on nominal interest rates (the liquidity trap) prevented a larger real money supply from reducing interest rates. (The special case of near-zero nominal values for the target interest rates of central banks, such as the overnight federal funds in the United States, has some practical relevance at the time of writing.) Consequently, according to Patinkin (1948, 1965), the classical adjustment mechanism would automatically restore full employment equilibrium after a negative demand shock, unless money wages were rigid downwards (see Rubin 2002).

But, as in Keynes's Chapter 19, Minsky and Tobin insisted that a *falling* price level is not the same as a *lower* price level. A falling price level reduces the opportunity cost of holding real money balances (so increasing the demand for real money balances and raising the real interest rate) and can create expectations of further price declines. Unanticipated deflation increases the real value of inside debts (owed by one economic agent to another), whose value far exceeds the value of outside money. Such as increase in the real value of inside debt is contractionary through two channels. The first, a transfer of wealth from debtors to creditors (who presumably became creditors because they have lower propensities to spend than debtors), was emphasized by Tobin (1980) and by Mervyn King (1994) in his presidential address to the European Economic Association, for which he chose the topic of debt–deflation. The second, the disruption of the system of financial intermediation by the perceived increase in the risk of insolvency and illiquidity, was stressed by Minsky, by Charles Kindleberger (1978), and by Ben Bernanke (1981, 1983, 1995, 2000). Taken together, these effects of deflation can overwhelm the real balance effect, so that the classical adjustment mechanism can fail to restore full employment after a sufficiently large negative demand shock. The MIT economic historian Kindleberger gave full and generous acknowledgement to Minsky (1975) and to Minsky's essays on the fragility of the financial system, later collected in Minsky (1982). As befitted an MIT graduate student hoping to have his thesis accepted and to make an academic career (Bernanke 1981, 1983 were drawn from his dissertation), Ben Bernanke cited Minsky but emphasized that, unlike Minsky, he believed that agents should be assumed to be rational whenever that assumption could be made consistent with the evidence (Bernanke 1995). Neither Minsky nor Tobin, nor King nor Bernanke, went beyond Keynes (1936) and Fisher (1932, 1933) to note Keynes's discussions of debts and deflation, nor did their comments on Keynes's Chapter 19 refer specifically to the passages in it concerning nominal debts.

Conclusion: The Consequences to the Banks of the Collapse of Money Values, 2009

This line of argument about the failure of the classical adjustment mechanism is relevant today not just because policy rates of interest are near zero in nominal terms or because the collapse of house prices and other asset prices and the possibility of general price deflation raises the risk of a wave of "bankruptcy, repudiation, and default," the increased risk of which causes a destabilizing scramble for liquidity by financial institutions and disrupts the process of financial intermediation. It is also the case that, among the academic students of debt–deflation cited above, Mervyn King is the Governor of the Bank of England and Ben Bernanke is the Chairman of the Board of Governors of the Federal Reserve System, and their previous writings on debt and deflation provide insight into their responses to the current crisis (Koehn 2008).

Milton Friedman and Anna Schwartz (1963) offered, in contrast to Keynes's emphasis on the volatility of private investment in a world subject to fundamental uncertainty about the future, a monetary interpretation of the Great Depression as a "Great Contraction" of the money supply: in an environment without deposit insurance, the Federal Reserve System unwisely permitted increases in currency/deposit and reserve/deposit ratios to shrink the US money supply by a third, thereby reducing aggregate demand. Without dissenting from that analysis, Bernanke (1981, 1983) invoked debt–deflation as outlined by Fisher (1933) to argue for another channel for disaster, in which fear itself was to be feared: with a mountain of debt fixed in money terms, soaring risk premiums and the scramble for liquidity caused the US system of financial intermediation to stop functioning, not just during the "Bank Holiday" of March 1933 but throughout the decade, as banks piled up excess reserves to ward off bank runs. At Milton Friedman's 90th birthday party in November 2002, Bernanke, newly appointed as a member of the Fed's Board of Governors, wittily addressed Friedman and Schwartz on the Fed's responsibility for the Great Depression: "Yes, we did it. We're very sorry. We won't do it again." When the crisis began in August 2007, Bernanke was acutely conscious of the social cost of a breakdown of financial intermediation and that it was now his responsibility not to do "it" again: his paramount concern was to keep the system of financial intermediation functioning, notwithstanding the moral hazard creating by bailing out errant institutions. In contrast, Mervyn King (1994) emphasized the redistribution of wealth between borrowers and lenders during the debt–deflation of the early 1930s, and concluded that their propensities to spend did not differ greatly. Until sandbagged by the Northern Rock collapse, King laid much stress than did Bernanke on the moral hazard of bailouts, and was much less focused on the possibility of a breakdown of financial intermediation. The different lessons that Bernanke and King drew from the US debt–deflation of the early 1930s were reflected in their different policy responses in 2007 – while the intellectual heritage that Otmar Issing brought to the European Central Bank from the Bundesbank (a heritage also reflected in the institutional mandate of the ECB) drew its historical lessons more from the German hyperinflation of the early 1920s (Dimand and Koehn 2008).

In November 2008, Colombian policy fought thousands of rioters demanding the return of money they had lost in a Ponzi scheme promising exceptionally high interest rates. According to McDermott (2008), "Pyramid scams have sparked violence in other developing countries where people have little financial sophistication. The collapse in 1997 of pyramid investment agencies in Albania caused losses of $2 billion and led to about 3,000 deaths."[8] The month after the report from Colombia, Bernard Madoff's Ponzi game unravelled, not in a developing country but also in a country where people had less financial sophistication than they

[8]According to Andrews (1997), "The collapse of the Albanian pyramid schemes in the past 2 weeks, igniting riots in Tirana, the capital, and other cities, was almost identical to calamities for tens of thousands of people duped by similar schemes in Russia, Romania, Bulgaria, Serbia and other places."

thought they had. Financial crises, which afflicted a series of developing countries from Mexico in 1994 to Argentina in 2002, and Ponzi games (not just in the literal sense of Messrs. Ponzi and Madoff, but also the unsustainable US, Irish, British, and Spanish housing bubbles) can no longer be regarded as things that, in the modern era, happen only to developing countries. For advanced industrial countries, a systemic financial crisis was long regarded as a mere historical curiosity, of interest to historians of the 1930s, or, if Minsky insisted, an abstract theoretical possibility, but one of no practical relevance.[9] Now that such a position is untenable, Keynes's "The Consequences to the Banks of the Collapse of Money Values" is as painfully relevant to the interesting times in which we live as to the interesting times in which he wrote. But also, in addition to its parallels with the current situation, the emphasis in "The Consequence to the Banks of the Collapse of Money Values" of the importance of debts being fixed in terms of money (together with the remarks leading to it in "The Great Slump of 1930" and Keynes's Harris Foundation Lectures) shows Keynes developing the analysis of Chapter 19 of his *General Theory*, a chapter that is crucial to his claims that his theory was indeed general, and not just a classical model with an arbitrarily fixed money wage rate. It also shows Keynes discussing the interaction of debts and deflation independently of, and slightly earlier than, the much more extended and detailed analysis of Irving Fisher (1932, 1933), which Fisher first presented in a series of lectures at Yale in the autumn of 1931, apparently without any knowledge of Keynes's essays[10] (Dimand 1995, 2005).

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[9]Some suspicions arose near the end. In March 2007, 4 months before the crisis struck, *The Economist*'s Buttonwood column was presciently entitled "Ponzificating: Is the financial system a confidence trick?" and reported on a research note entitled "Have we arrived at a Minsky moment?" written by George Magnus, a strategist at UBS (Union Bank of Switzerland), which since then has written off more than US$50 billion of losses from toxic assets.

[10]By that late stage in Fisher's career, his extensive reading of other economists had dwindled to looking for support rather than inspiration. It is not even clear that Fisher ever read Keynes's *General Theory*.

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The Consequences to the Banks of the Collapse of Money Values: 1931 and 2009: Some Comments

Elise S. Brezis

I wish to thank Jean-Pascal Benassy for comments and conversations on this matter.

Introduction

This paper by R. Dimand presents some of Keynes' views on the Great Depression. It focuses more specifically on the collapse of the nominal values of debt, money, prices and wages, and their effects on the reduction in output. The second subject this paper takes a stand about is a comparison between the period of the Great Depression and the crisis of 2009.

The Real Effects of Changes in Nominal Variables

In his paper, Robert Dimand stresses that Keynes has focused on the effects of a reduction in nominal variables on real output. He first puts an emphasis on the effects of the debt value on output demand, what is coined the "debt–deflation dynamics." Then, he analyses in a broader way the relationship between nominal and real variables.

The world of Keynes is vast, and one may arbitrary focus on many different aspects of his theories, but I like Dimand's emphasis on these two sub-subjects, because they are certainly the one that should be emphasized when one talks about Keynes and the Great Depression.

The first interesting point about Keynes is that his view goes against what we call the classical model. Indeed, the classical model states that unemployment can be reduced by a reduction in real wages, so that a good therapy against unemployment would be to start reducing nominal wages, which would be transmitted to a reduction in real wages.

R. Dimand stresses in this paper that Keynes was against this view. He clearly points out the danger of nominal values going down. First, prices going down increase the value of debt and, through a wealth effect, reduce demand. Second, lower money wages lead people to have lower income.

The most significant aspect of Keynes' analysis is that a decrease in a nominal value does not necessarily mean that real values will decrease. The dynamics can lead the system to be such that, despite a reduction in wages and debt, we end up with real wages and debt going up.

This brings us to what has been coined "disequilibrium dynamics." Following Keynes analysis, each business cycle can be divided into two main elements. The

first one is the shocks which lead the system to get out of its full-employment equilibrium and to enter a stage of unemployment.

The second and main element of the business cycle is the dynamics of the whole system. The question asked by Keynes is whether the dynamics will lead the system to return to the full-employment equilibrium, or, whether the forces of the system will lead the economy to stay where the first shocks took them to, or even beyond.

While it is clear that the financial crisis was the primary shock of the great depression, it was only the trigger. What makes the Great Depression so different from other business cycles is not the size of the shock or even that there were a series of negative shocks on the side of the financial market as well as the real side. What makes it so different are the dynamics of the system.

As emphasized by Friedman and Schwartz: "An initial mild decline in the money stock from 1929 to 1930 was converted into a sharp decline by a wave of bank failure beginning in late 1930" (Friedman and Schwartz, *A monetary history of the US*). The dynamics did not work as it was supposed to. In Fig. 10.1, I present the regular AS-AD graph. We start from a full-employment equilibrium (point A).

Then, some shocks on the side of the demand (and we can even add on the side of the supply) lead the economy to move to a point of unemployment. The classic theory analysis is that the dynamics of the system are shown by the spiral curve showing how the economy comes back to the full-employment equilibrium.

Theses forces were not in place during the Great Depression. The dynamics of prices, wages and output were not working. This is Keynes' presentation (or at least as developed by Tobin) of the crisis. It is not that the system was unstable. But, after the different shocks, which led the system to enter unemployment, there were no dynamic forces to let it move back to full-employment equilibrium.

> "It is an outstanding characteristic of the economic system in which we live that, whilst it is subject to severe fluctuations in respect of output and employment, it is not violently unstable ... Indeed, it seems capable of remaining in a chronic condition of sub-normal activity for a considerable period without any marked tendency either towards recovery or towards complete collapse." Keynes, *General Theory*. *p. 249*

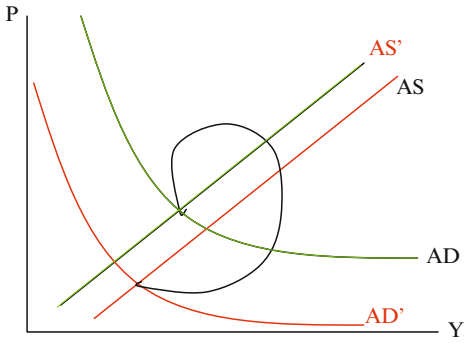

Fig. 10.1 The dynamics of the system

What are the reasons that the system became amorphic and the dynamics did not bring back to full-employment equilibrium? Some scholars have accused the International system to be guilty of the wrong dynamics, as Dimand has mentioned. Indeed, Temin has seen in the Gold Standard the main reason for the system not to return to full-employment equilibrium.

Keynes accused the *animal spirits* of human beings to be the main culpable:

> "There is the instability due to the characteristic of human nature, that a large proportion of our positive activities depend on spontaneous optimism rather than mathematical expectations . . . Our decisions to do something positive, can only be taken as the result of *animal spirits* – a spontaneous urge to action, and not as the outcome of a weighted average of quantitative benefits multiplied by quantitative probabilities." Keynes (1936)

In the terminology of today, the herding which exists in the financial markets is some sort of animal spirit, and was a main element of the crisis of 2009. This brings us to the second point raised by Dimand in his paper: the comparison between the Great Depression and the crisis of 2009.

The Comparison Between the Great Depression and 2009

In his paper, R. Dimand makes a comparison between the Great Depression and 2009. I agree with him that the crisis which started to be felt in 2007 was a financial crisis. I also like the fact that R. Dimand points out that the differences in monetary policy in the US and the UK during the crisis can be due to the difference of points of view of Mervyn King, governor of the Bank of England and Ben Bernanke, head of the Fed, on the causes of the Great Depression, King focusing on redistribution of wealth and Bernanke on the breakdown of financial intermediation.

However, I would like to make a comparison between these two periods following the analysis of the disequilibrium dynamics explained above. It seems evident that the 2009 crisis started with the sub-prime crisis, which, when Lehman Brothers collapsed, led to a main disruption of financial intermediation. There was the possibility of a new Great Depression. Indeed consistent with the model we presented in the previous part, in 2008 like in 1930–1931, there was a continuation of shocks, mostly in the financial market but also in the real economy which could have given place to a strong depression. Not only the first shocks were somehow similar (disruption of financial intermediation), but the herding effects led to the beginning of the disequilibrium dynamics.

However, the main difference between these two periods was the policy of governments. Probably, the fact that Ben Bernanke is a specialist of the Great Depression, led him not to procrastinate and to take very strong measures.

Three factors which went wrong in 1929 went right in 2009. The fiscal policy was correct. Presidents Bush and Obama did not focus on the size of the budget deficit and despite some questions raised on the inflationary dangers of an increase in the deficit, both understood that they have to increase the budget, and the deficit; and they did it.

Second, monetary policy was the correct one. Not only interest rates were reduced rapidly, but the Fed made it clear that big banks and financial firms will not be allowed to go bankrupt. They have learnt the consequence of Lehman Brothers, while in 1931, governments did not know what to do after the collapse of some of the big banks. We can all try to imagine the path of the economy if AIG was going bankrupted as Lehman did.

The third point, and not least, is that the crisis of 2009 occurs at a time of flexible exchange rates, unlike 1929, time of the gold standard. Flexible exchange rates are important at time of crisis. This was maybe an important element of the stability mechanism of this decade. Of course, one could ask about the Euro, whether the EU will suffer from having one currency among countries so different as Greece, Germany and France. It is clear that the single currency does not make life easy for policy makers.

In conclusion, it is clear that even if the financial and housing markets displayed some similar shocks in both periods, the monetary and fiscal policies were very different. Therefore, the Great Depression will still stay a unique phenomenon in the modern period of the Western world. Although 2009 gave us the taste that financial crises are not dead; the collapse did not happen.

Despite this some sort of optimistic conclusion on the crisis of 2009, I believe that the crisis of 2009 has led seeds for a longer and more latent crisis, the solutions of which are not evident. I refer to the problem of income inequality between the financial sector and the rest of the economy.

The crisis of 2009 has made it clear that the new financial intermediations are leading to more harm than to benefits for the economy. As in the phase before the great depression, the economy has developed rapidly and with no doubt the financial market had a part in this growing economy. The same can be said about the period 2003–2008. The financial market and these new financial tools were oiling the economy.

However, when one looks at the inequality in the economy, the working class did not enjoy the fruits of this growth: The financiers kept it all for themselves. While when the market boomed, they enjoyed the fruits thereof, yet on the downturn, the entire economy suffered, but not them. The intervention of governments led to the fact that the financiers did not pay for their mistakes. Not only they did not loose from the crisis, they continue to take huge bonuses.

In conclusion, while the Great Depression led to a restructuration of the wealth, to what Pareto calls: "circulation of elites," the 2009 crisis did not enable a change in the financial markets, due to government intervention. It is clear that letting the economy enter into a huge crisis in order to get structural changes is not a good solution. But, the fact that the financial intermediation sector continues to enjoy the whole benefits of economic growth, is not a good solution either. I would like to remind us that Pareto claimed that if circulation of elites will not come in a natural way, then crisis will make it happen in a costly way through revolts and revolutions. Governments should be aware that the situation nowadays is not a social equilibrium. Crafting good social policy is not less important than continuing with the correct monetary and fiscal policies. Reducing the power of the new financial

intermediation sector is a must. The crisis of 2009 emphasized that the new financial-intermediation firms are leading to more harm than benefit for the whole economy. There is a need for new government regulation in order to reduce the power of the financial elite.

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The Great Depression, the Global Financial Crisis and Old Versus New Keynesian Thinking: What Have We Learned and What Remains To Be Learned?

Alex Cukierman

Abstract Following a brief overview of major developments in macroeconomic thought since the Keynesian revolution in the introduction the paper features three main sections plus a conclusion. The first section presents a detailed comparison of background institutions, policy responses and economic performance during the great depression (GD) and the Global Financial Crisis (GFC). The second section discusses the role of lessons from the GD in shaping fiscal and monetary policy responses to the GFC as well as newer problems that emerged during the second crisis. Among the newer issues are runs between financial institutions, the role of financial derivatives, the opaqueness of securitization, the too big to fail problem, the incompleteness of micro-based risk assessments by financial institutions and a modern reinterpretation of the liquidity trap. The next section contains a systematic comparison of the methodological similarities and differences between old Keynesian economics and its recent New Keynesian Economics reincarnation. The concluding section points out missing elements in both old and new Keynesian methodologies and speculates about the likely path of future macroeconomic research in the aftermath of the GFC.

Introduction

Economics science tends to respond and adapt to major economic crises. Long-ruling paradigms are replaced or modified by new ones following large unanticipated economic events. This feature is particularly in evidence with respect to

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public policy and institutions in the areas of fiscal and monetary policies. Thus, macroeconomics came into being due to wide discrepancies between observed reality during the Great Depression (GD) and the classical view that market forces always propel the economy back to full employment within a reasonable time interval.

Since the official birth of macroeconomics with Keynes's General Theory, macro and monetary economics have undergone a non-negligible number of "revolutions" and "counterrevolutions." The Keynesian challenge to classical economics as encapsulated in Hicks's (1937) IS-LM model was propelled into textbooks and became the ruling paradigm during the 1960s. As a young student in a freshly created macro course in macroeconomics at the Hebrew University in the mid-1960s, I learned that the world can be dichotomized into a (downward sticky prices/wages) excess capacity regime and a full capacity regime. In the first regime, expansionary fiscal and monetary policies raise output and employment without changing the price level, while in the second they only induce price and wage inflation. The empirical work of Phillips (1958) provided a more continuous view of the underlying relation between inflation and economic activity. Two years later Samuelson and Solow (1960) argued that the Phillips curve represents a stable tradeoff between inflation and unemployment. By judicious choice of fiscal and monetary instruments, policymakers could choose an inflation–unemployment combination along this tradeoff.

The inflation of the 1970s dispelled this view, replacing it by Friedman's (1968) and Lucas's (1972, 1973) view that this tradeoff lasts only as long as inflationary expectations have not fully caught up with actual inflation.[1] As those expectations became a prime conceptual mover of actual inflation and employment macro theory needed a paradigm for their formation. Lucas and others proposed the "rational expectations" or model consistent hypothesis according to which expectation formation should be consistent with the actual behavior of the economy up to stochastic disturbances. The view that there is no long-run tradeoff between inflation and unemployment along with the work of Patinkin (1965) resuscitated the neutrality of money paradigm, establishing it as a permanent fixture of macroeconomic thinking.

Being closely associated with the view that prices are flexible, the neutrality of money paradigm led to the construction of macroeconomic frameworks with flexible prices, in which nominal money balances affect only the price level while real variables are affected by factors such as productivity, production functions, tastes, market structure and fiscal policy. A prominent branch of this approach is represented by real business cycle models (RBC) pioneered by Kydland and Prescott (1982) and extended in various directions during 1980s and 1990s. The distinctive feature of these models is that they insist on dynamic optimization at the

[1]Interestingly, the view that the long run Phillips curve is vertical and that money is neutral in the long run took hold in spite of the fact that econometric studies of the time indicated that, although worse than in the short run, the tradeoff between inflation and unemployment does not totally disappear in the long run (Solow 1969; Cukierman 1974).

individual level and use utility functions and production functions as the primitive building blocks of their analysis. Due to the analytic difficulties involved in characterizing their behavior, these models have been largely confined to representative individual frameworks.

During the second part of 1990s and the early twenty-first century, temporarily sticky prices were introduced into the basic core of RBC models by using, *inter alia*, the Calvo (1983) formalism as a proxy for costs of price adjustments. Woodford's (2003) book is a prominent example of this approach. Since they reintroduce sticky prices into an otherwise nominally neutral RBC framework, this class of models is often referred to as New Keynesian. One can view them as a modern, micro-based synthesis between the Keynesian sticky price view and the micro-founded rigor required by RBC methodology. An important advantage of those models is that they feature price-setting firms whose occasional pricing decisions are affected by inflationary expectations, making it possible to meaningfully discuss monetary policy in dynamic micro-based frameworks. Full-scale dynamic stochastic general equilibrium (DSGE) models based on this approach are now routinely estimated and calibrated by the research departments at major central banks.

Although they represent a coherent synthesis of broadly accepted methodological principles with the view that nominal wages and/or prices are sticky, it is likely that the still unfolding global financial crisis (GFC) will challenge and modify the construction and use of New Keynesian models. It is gradually becoming clear that the roots of the GFC reside within the supervision, regulation and modus operandi of the financial sector (Cukierman Forthcoming). From this perspective, an important limitation of New Keynesian frameworks is that they do not provide a sufficiently detailed (and therefore realistic) view of the incentives, the constraints and the behavior of financial intermediaries. More generally it is likely that, somewhat similarly to the GD that led to the reconsideration of the economic doctrines of the time, the GFC will trigger a reevaluation and refocusing of currently accepted views and methodologies within monetary and macroeconomics.

This paper utilizes a broad brush to compare between the GD and the GFC with particular emphasis on what has been learned from the experience of the GD and on what remains to be learned in view of the recent experience with the GFC. The paper highlights differences in the policy responses to the two crises as well as differences in fiscal and monetary policymaking institutions prior to each crisis. Besides the conclusion, the remainder of the paper contains three sections. Section "The Great Depression and the Global Financial Crisis: A Comparison of Background Institutions, Policy Responses and Economic Performance" presents comparative evidence on economic institutions on the eve of the two crises, on the policy responses and on economic performance during the GD and the GFC. Section "What Has Been Learned and What Remains To Be Learned?" focuses on what has been learned since the GD and on what remains to be learned in view of the current crisis. The emergence of the traditional Keynesian view is directly traceable to the experiences of the GD. Much has been learned from this experience as well as from the subsequent stagflation of 1970s and the competing economic

methodologies that evolved in the process. Section "Comparison of New Keynesian Models with Keynesian Frameworks of the Sixties" compares and contrasts the traditional Keynesian models of the 1960s with its novel, New Keynesian, micro-founded incarnation. This is followed by concluding remarks.

The Great Depression and the Global Financial Crisis: A Comparison of Background Institutions, Policy Responses and Economic Performance

Clearly, the global financial crisis (GFC) is the most serious crisis since the Great Depression (GD). There are some striking similarities between the two crises. Both started in the financial sector and gradually spread to the real sector. During both crises many financial institutions were wiped out or had to be bailed out. In both cases the crisis appears to have started with the bursting of a bubble and banking credit drying up. In both cases the lower zero bound on the policy rate became effective. Finally, in both cases the crisis started in the US and subsequently spread to other countries. In spite of those similarities there are important differences between the GFC and the GD in three main areas: Background institutions on the eve of each crisis, policy responses and economic performance during the crisis. This section focuses on those differences.

Comparison of Institutions on the Eve of the GD and of the GFC

There are several important differences in the structure of institutions between the eves of the two crises. First, during the first 3 years of the Great Depression, the US was on the gold standard. During those years, the maintenance of a fixed parity with gold collided with the use of monetary policy to achieve internal balance. The US abandoned the gold standard only after Roosevelt was elected president during the first half of 1933. As a consequence, US monetary policy was effectively "attached to the mast" during the first 3 years of the GD. Some other countries – such as France, that abdicated the gold standard early on – benefited from this self-imposed constraint. No such constraint was present at the eve of the GFC. In fact the dollar depreciated substantially between summer 2007 and summer 2008 in line with the expansionary policy of the Fed reaching a peak of $1.6 to the Euro in July of that year. It appreciated again, reaching a trough of $1.24 to the Euro in November 2008 due to the "flight to safety" triggered by the demise of Lehman Brothers and other investment banks, and continued to fluctuate in a relatively wide range till the writing of this article in July 2009.

Second, and most importantly, deposit insurance did not exist on the eve of the GD but was a long-established fixture of the US banking system on the eve of the

GFC. Deposit insurance became a permanent fixture of US monetary institutions following the massive banking failures of the 1930s. Deposit insurance and the Federal Deposit Insurance Corporation (FDIC) were created as part of the Banking Act of 1933 (also known as the Glass-Steagell Act) which has been in effect ever since. On the eve of the GFC individual deposits were insured by the FDIC up to a ceiling of $100 thousand. With the intensification of the crisis during the second half of 2008 the ceiling was temporarily raised to $250 thousands. Had deposit insurance existed during the 1930–1933 period, many of the banking failures triggered by bank runs and the associated monetary disruptions of the GD would have been averted.[2] Due to deposit insurance there hardly were runs on the banks by depositors during the GFC.[3] However, during the GFC there were runs by financial institutions on other financial institutions. The reasons and consequences of this somewhat different type of runs are discussed in the next section.

Third, there were no bank capital requirements on the eve of and during the GD. By contrast, under the Basel agreements banks were subject to capital requirements on the eve of and during the GFC.

Fourth, the orders of magnitude of the Federal Government on the eve of the two crises and the associated fiscal institutions were completely different. On the eve of the Great Depression, in 1929, the share of federal taxes in Gross Domestic Product (GDP) was less than 4%. On the eve of the GFC, in 2007 or 2008, it was around 18%. Between 1929 and the pre-GFC period the tax collection capacity of the US government expanded dramatically. Indirect taxes were replaced by direct taxes, and universal filing of income-tax returns was instituted at the end of 1930s. Those differences in initial conditions facilitated the quick deployment of fiscal stimulus and bailout packages during the recent crisis.

Although obvious, the last difference cannot be overemphasized. The GD started prior to the Keynesian revolution. As is well known, it triggered this revolution. Since then Keynesian policy prescriptions have been tried, criticized, digested and synthesized into more realistic modes of thinking and of policymaking. Much has been learned about their benefits and limitations. Consequently, today's policy-makers are better informed about the potential salutary effects of expansionary fiscal and monetary policies than Hoover or Roosevelt were when they launched their policies. Largely because of the lessons from the GD, the responses of both fiscal and monetary policies during the GFC have been swifter, stronger and more focused than during 1930s.

[2]In a classic article Diamond and Dybvig (1983) show that deposit insurance neutralizes the incentive of depositors to run on banks.

[3]An isolated exception was the case of Northern Rock in the UK. Following the broadening of deposit insurance by UK authorities, this run too was quickly quelled.

A Comparison of Policy Responses: the GD Versus the GFC

The differences between the responses of fiscal and of monetary policies to the GFC and to the GD are huge in more than one dimension.

Fiscal Policy

Under Hoover, who presided over the first 3 years of the GD, fiscal policy was passive at best. The federal budget had a small surplus during 1930, a small deficit in 1931 and a deficit of 4% of GDP in 1932. The bulk of the increase in 1932 was due to a decrease in tax collections caused by the sharp contraction in income rather than to autonomous fiscal policy. Although the deficit share in GDP increased under Roosevelt, reaching a peak of 5.9% in 1934, it pales in comparison to the magnitude of fiscal expansion during the current crisis. In spite of his image as a practical Keynesian, Roosevelt also displayed concern for fiscal discipline. As a consequence, during the second part of 1930s the budget deficit shrank, leading to a temporary recession in 1938. Table 1 shows the deficit to GDP ratio and nominal GDP during 1930s and the beginning of 1940s. Interestingly, the deficit increased dramatically only in 1942, when the US formally entered WWII and when unemployment was no longer a problem. The deficit to GDP ratio in this year was *almost three times* higher than the highest such ratio during all the years of the GD.

The contrast with the fiscal policy response during the current crisis cannot be overemphasized. Following the demise of Lehman Brothers in September 2008, Congress and the Bush administration committed $700 billion to restore confidence in the US financial system by taking stakes in many US banks and by outright buying of troubled assets. The Emergency Economic Stabilization Act of October 3, 2009 (also known as the Troubled Assets Relief Program or TARP) empowered the Treasury Department to spend this huge amount in order to clean banks and other financial institutions from so-called "toxic assets" and to recapitalize them by taking equity positions in the US banking system. This was soon followed by a $787 billion fiscal stimulus package for the rest of the economy under Barack Obama. This package, known as the American Recovery and Reinvestment Act of 2009

Table 1 The share of deficits and GDP during the great depression

Year	1929	1930	1931	1932	1933	1934	1935
Deficit/GDP (%)		−0.8	0.6	4.0	4.5	5.9	4.0
GDP (in current billions of $)	103	91	77	59	56	66	73
Year	1936	1937	1938	1939	1940	1941	1942
Deficit/GDP (%)	5.5	2.8	0.5	3.8	3.6	4.9	14.8
GDP (in current billions of $)	84	92	86	92	101	127	162

Sources: The deficit GDP ratio is from Budget of the US Government for Fiscal Year 2006, Historical Tables, Office of Management and Budget, Table 1.2. GDP is from the National Income and Product Accounts, Bureau of Economic Analysis, Table 1.1

(ARRA), quickly went through Congress, and was signed into law by Obama on February 17, 2009.

TARP is a bailout package designed to recapitalize the financial system; ARRA is a fiscal stimulus package, one-third of which is for tax relief, another third for aid to the states, the unemployed and health care, and the rest for labor, education, infrastructure spending, water and a green agenda. The sum total of those two packages amounts to $1.487 trillion which (at a current GDP of about 14 and a half trillions) amounts to about 10% of GDP. As of April 2009, almost $600 billion of TARP money has been spent or committed. The US administration estimates that as of May 2009, only 11% of the ARRA money has been given out, creating 150 thousand jobs. The rest of the money is to be spent over the remainder of 2009 and 2010. Although those additional expenditures come on top of a deficit of about 3% of GDP during fiscal 2008, they do not necessarily raise the 2009 deficit by this full amount since disbursement is staggered. Nonetheless, as of May 2009 the Treasury forecasts that the US deficit during fiscal 2009 will be 13% of GDP.[4] Forecasts for fiscal 2010 range between 10.6% and 8.7% of GDP.

Clearly, even the maximum fiscal stimulus under Roosevelt pales in comparison to those figures. Another major difference between the two crises is the swiftness of the fiscal response. It took at least 3 years from the official start of the GD until a sizable fiscal response was worked out. By contrast, the two huge packages mentioned above were conceived, legislated and enacted into laws rather quickly.

Depending on where we place the beginning of the GFC, those responses occurred somewhere between a year and less than half a year from the beginning of the crisis. The longer response is obtained when the beginning of the crisis is located at the official beginning of the recession in the last quarter of 2007. The shorter (and probably more realistic) period places the beginning of the crisis in mid-September 2008 when the depth of the financial crisis became painfully apparent following the downfall of Lehman Brothers.

Monetary Policy

The difference in the responses of monetary policy between the two crises is equally dramatic. Friedman and Schwartz (1963) argue convincingly that during the first 3 years of the Great Depression, the Fed tolerated and even reinforced a substantial shrinkage of the money supply. Instead of pumping liquidity into the financial system in order to prevent the transformation of liquidity shortages into insolvency problems, the Fed often withdrew funds from problematic banks in order to shield its balance sheet from further losses. This policy precipitated thousands of problematic banks into default, accelerated the downfall of others and contributed to further declines in the dwindling supplies of money and of credit.

[4]An additional reason for this large figure is the decrease in tax collections due to the recession. Fiscal year 2009 started on October 1, 2008 and will end on September 30, 2009.

The disappearance of many banks during the GD led to the destruction of "informational capital" produced by banks during the intermediation process. One of the main intermediate outputs of the banking system is the creation of information about the credit-worthiness of different borrowers. This information enables banks to distinguish between adequate and sub-standard credit risks, inducing banks to supply credit to the first group. This process prevents the drying up of credit to this group and maintains the (individually rational) flow of credit to the real economy. The disappearance of many banks during the Great Depression led to the destruction of borrowers' credit ratings, causing serious and protracted declines in the supply of credit by banks. Bernanke (1983b) argues convincingly that this mechanism was partly responsible for the propagation and the persistence of the recession to the real sector during the GD.

By contrast, since August 2007, the Fed gradually eased monetary policy by reducing the discount rate from 5.25% to practically zero during the first quarter of 2009. In parallel the Fed also started buying more risky assets than during normal times. Quantitative easing was stepped up during the last quarter of 2008 through the establishment of a Term Auction Facility (TAF), purchases of government securities and of some categories of Mortgage Backed Securities (MBS). Through these mechanisms the Fed poured huge amounts of liquidity into the financial system. Since August 2008 till the beginning of 2009 the Fed's balance sheet has expanded roughly 2.5 times (*from around $800 billion to over $2 trillion*). Essentially, the Fed has become a major financial intermediary replacing banks and other private intermediaries that have temporary lost their ability to intermediate funds effectively due to the GFC.

Contrary to the GD there were no mass bank failures during the GFC. Such failures were avoided not only due to the existence of deposit insurance but also because the Fed injected large amounts of liquidity and took over insolvent financial institutions or arranged their takeover by other financial institutions. As a consequence the destruction of information about the quality of borrowers and the associated adverse impacts on the flow of credit have been small in comparison to the GD. But, on the flip side, there was widespread destruction of information about the value of financial assets due to opaqueness induced by the complicated structure of MBSs and other Collateralized Debt Obligations (CDO). Finally, monetary policy during the GFC started to respond early on, and this response was quickly intensified as the subprime crisis expanded into a full-blown GFC crisis in September 2008.

Trade Policy

The GD was characterized by beggar-thy-neighbor policies. In mid-1930 the US Congress passed the Smoot–Hawley Tariff Act that raised tariffs on over 20,000 imported goods to record levels. Other countries retaliated by also imposing restrictions on imports and engaged in competitive devaluations. This led to a

serious contraction of international trade. Major trading partners have largely avoided the temptation to engage on this path during the GFC.

At their April 2009 meeting in London the leaders of the G20 group pledged to do whatever is necessary to "*promote global trade and investment and reject protectionism*" (G20 Official Communique 2009). Although it remains to be seen whether that declaration will be followed, the atmosphere of international cooperation that permeated the meeting is a far cry from the trade wars of 1930s.

Comparison of Economic Performance: The GD Versus the GFC

Table 1 shows that nominal GDP was below its 1929 level throughout the entire decade of 1930s. It surpassed this level only at the beginning of 1940s when the US was gearing up toward WWII. At the peak of the GD, in 1932, *nominal* GDP was lower by about 40% than in 1929.[5] By contrast, during 2009, the US economy contracted by a comparatively modest 2.6% and forecasts for 2010, although low, are non-negative.

During the entire decade of 1930s unemployment was persistently above 15%, peaking at 25% in 1933. The highest US unemployment figure during 2009 is a bit above 10%. Although it is the highest such figure over the last 26 years, it is still substantially lower than the unemployment figures during the GD. Unemployment in the US is likely to peak around the ten percent and to subsequently go down during 2010.

The previous observations are consistent with the view that although we have not seen all the ramifications of the GFC yet, this crisis is substantially shorter and milder than the GD. These differences in economic outcomes are, in large part, attributable to differences in policy responses and to differences in economic institutions on the eve of the two crises.

What Has Been Learned and What Remains To Be Learned?

What Has Been Learned from the GD and Applied During the GFC?

The experience of the Great Depression and the related emergence of Keynesian economics demonstrated the potency of expansionary fiscal and monetary policies in the face of credit shocks originating in the financial system. Thirty years after the GD the work of Friedman and Schwartz clarified that letting the money supply

[5]Since the price level went down the real decrease was lower. Romer (2009) puts it at over 25%.

shrink during such times can be extremely disruptive for the financial system as well as for the real economy. The lesson of this momentous policy mistake has been strongly internalized by current policymakers at the Fed. The swift and large monetary policy reaction to the GFC is, no doubt, partly due to Bernanke's academic familiarity with the consequences of this mistake. It probably also had an impact on the structuring of the fiscal bailout package devised under secretary Paulson.

During the peak of the GFC, the Fed and the Treasury were essentially accommodating the desires of both users and suppliers of funds in the private sector in order to reestablish credit flows and prevent the disappearance of crucial financial institutions. Final users of funds need a steady stream of credit, banks require sufficient liquidity to operate effectively and, during times of great uncertainty, providers of funds seek safety first. By stepping in, the Fed and the Treasury accommodated both sides of the capital market and assured the continuation of some intermediation in spite of the panic that has taken hold of the financial industry.

The Treasury and the Fed are in a unique position to provide such systemic insurance, since the first institution is backed by the power to tax and the second by the privilege to print money.[6] When confidence returns to private financial intermediaries, the need for the involvement of governmental institutions will abate. Hopefully, they will then be able to reduce their substantial involvement in financial intermediation in parallel to the orderly return of private financial institutions into this market.

Other lessons from the GD include the virtual disappearance of traditional bank runs after the enactment of deposit insurance and the creation of an anti-protectionist awareness among policymakers. The disappearance of traditional bank runs following the permanent creation of the FDIC in 1933 clearly demonstrates the benefits and long-term viability of deposit insurance.

The statement against protectionism made at the April 2009 official communique of the G20 attests to the fact that major policymakers today are keenly aware of the dangers of trade wars. This is due to institutional memories about the adverse consequences of such actions during the GD. Paralleling this tendency, there is substantially more coordination of monetary policy, mainly through swaps agreements between central banks and related arrangements. Hopefully, the world's major policymaking centers will continue to largely avoid such trade inhibiting policies. The likelihood that this will indeed be the case is greater than during the great depression for two reasons: First is the demonstration effect of the adverse consequences triggered by such policies during the GD. Second, since the current crisis is likely to be milder and shorter than the GD, the temptations to engage in beggar-thy-neighbor policies will be weaker.

[6]Interestingly, the flight to safety at the end of 2008 and the beginning of 2009 enabled the Treasury to borrow funds needed to finance the recapitalization of banks and other activities at low interest rates.

Lessons from the GFC and Open Issues[7]

Runs Among Financial Institutions

Due to deposit insurance and the vigorous involvement of the Fed, there hardly were runs on the banks by depositors during the GFC. But opaqueness about the valuation of certain classes of sophisticated financial assets like MBSs and other CDOs brought interbank credit to a virtual standstill, inducing a serious reduction in the availability of credit to firms and households and a flight to safety by savers and institutional investors.

The inability to assess the value of such assets in conjunction with their huge quantities induced a run-like behavior among financial institutions rather than between depositors and banks. Those institutions stopped advancing funds to each other even for very short periods of time and attempted to repatriate all the liquid assets owed them by other financial institutions. In this context it should be noted that runs by sophisticated financial institutions on each other can materialize much more forcefully and swiftly than traditional bank runs by less financially sophisticated depositors. During the last quarter of 2008 this forced the Fed and the Treasury to inject huge amounts of liquidity and of taxpayers' money to buy questionable financial assets in order to prevent a meltdown of the US (and with it of the world) financial system.

Although it is clear that the root cause of the panic following the demise of Lehman Brothers resided in opaqueness about the valuation of assets, several factors combined to produce this lack of transparency. In a nutshell, opaqueness about the value of assets is due to the rise of an unregulated and unsupervised shadow banking system in conjunction with the creation of sophisticated, but poorly conceived, financial instruments. Clearly, an important general lesson from the GFC is that regulation and supervision of financial institutions have to be restructured in ways that will enhance transparency about the valuation of assets and about the financial positions of intermediaries. Although this general principle is simple enough, the devil is in the details. Those details raise difficult tradeoffs between wide and efficient intermediation on one hand, and transparency on the other, and are beyond the scope of this paper.

Leverage During the GD and the Role of Financial Derivatives During the GFC

Prior to the GD, banks were lending on margin against stock collateral. As a result, when the stock market crashed in October 1929, the value of collateral went down and the banks contracted credit to the real economy. Once the bubble burst, the

[7]This subsection introduces some of the most salient new issues raised by the GFC but does not discuss possible solutions. Those are discussed in greater detail in Cukierman (Forthcoming).

large amounts of leverage built up during 1920s ruined numerous investors and financial firms. This initial adverse shock to the financial system was subsequently transmitted to the real sector through inaction and policy mistakes on the part of policymakers. A similar mechanism of over-leverage operated during the GFC. During the real-estate boom over-optimism led to the buildup of excessive amounts of leverage. Once the real-estate boom turned into a bust, this leverage severely impacted the liquidity and solvency of major financial institutions.

Although there are strong analogies between the broad mechanisms that burst the financial bubbles at the starts of the GD and the GFC, the latter crisis is characterized by financially sophisticated derivative products that did not exist during the GD. One prominent derivative, if only because of its size, is the collateralized debt swap (CDS) invented by Black and Scholes. CDSs are typically bilateral contracts that enable one party to either assume or reduce credit exposure on one or more debt obligations of named issuers (further details appear in Chapter 18 of Sundaresan (2009)). This market – initiated as late as the mid-1990s – grew enormously during the last 10 years, reaching an estimated stock of over $60 trillion in 2008. Prior to the crisis, CDSs written by AIG covered more than $440 billion in bonds, but AIG did not possess the funds required to cover losses of such magnitude in case the contingencies stipulated in the CDS contracts materialized. The collapse of Lehman Brothers triggered such an event, exposing AIG to bankruptcy and forcing the authorities to bail it out. Due to their derivative nature, CDSs are either not regulated or very poorly regulated. This, in conjunction with the fact that they are not traded on organized exchanges, raises important new questions about the pros and cons of such instruments and about their regulation.

The Too-Big-to-Fail Problem

The GFC brought to the forefront the too-big-to-fail problem. This problem was largely non-existent during the GD because, at the time, the central bank and government did not believe they should rescue ailing financial institutions. They simply let thousands of US banks fail.[8] The attitude of contemporary policymakers is completely different. Rescue operations of US investment banks during the GFC, as well as the downfall of Lehman Brothers, clearly demonstrate that the Fed and the US Government will not tolerate failures of systemically important financial institutions.[9] Managers of systemically important institutions expect therefore that, if they fail, the authorities will come to their rescue. They consequently choose portfolios that carry risk levels higher than the socially optimal levels, not only

[8]There was some attempt to recapitalize banks during the GD under Roosevelt. However its size was modest in comparison to the bailout effort, to date, during the GFC.

[9]During 2008 Bear-Stern, AIG, Citibank, Merrill-Lynch, Fannie Mae and Freddie Mac were bailed out in various ways. Although Lehman Brothers was not rescued it is clear that the financial shock waves created by its downfall raised the likelihood that systemically important institutions will, most likely, be bailed out in the future.

because they do not internalize systemic risks, but also because they expect to be bailed out.

Once a crisis develops, it is likely that bailouts of systemically important institutions are preferable to the financial disruptions that would otherwise occur. But this induces *ex ante* opportunistic behavior on the part of large financial institutions – raising the likelihood of crises. Appropriately devised, and tightly enforced, regulation of financial institutions is currently developed with the aim of minimizing this moral hazard problem in a world for which bailouts of large institutions are common practice.

The Incompleteness of Micro Based Risk Assessments by Financial Institutions

The GFC revealed that, in spite of the modern financial wizardry at their disposition, many financial institutions seriously underestimated the risks inherent in some of their investments. Although they often did a reasonable job in estimating micro risks of default on assets, major financial institutions often overlooked systemic risks due to correlations between macro aggregates. In addition, even purely micro based probabilities of default were estimated using data on several recent years rather than data from at least a full cycle. This reinforced the overly optimistic view of risks during the latter part of the real-estate boom.

A Reinterpretation of the Keynesian Liquidity Trap in View of the GFC

When I was first exposed to the (IS-LM type) Keynesian liquidity trap during the mid-1960s, I was told that there is a sufficiently low interest rate (not necessarily zero) at which the demand for money becomes infinite. As a result, any attempt to reduce the interest rate below this rate by monetary expansion is doomed to failure since the public is willing to passively absorb any quantity of money at that interest rate. I recall that this argument always mystified me, since I could not imagine circumstances under which the public would demand such large quantities of money. While watching the unfolding of the GFC I finally understood what I tend to believe now is a realistic (and correct) interpretation of the liquidity trap story.

Two bases for this reinterpretation should be clarified upfront. First, the "infinite" or, more appropriately, "large" demand for money originates mainly in the banking and financial sector rather than with the "public" at large. Second, during the peak of a financial crisis those institutions are willing to passively absorb any amount of liquidity rather than transform it into return yielding assets because of two main reasons. One is the preceding depletion of their liquid assets and capital, and the other is a very substantial increase in the perceived riskiness of lending during the peak of the financial crisis. Microfoundations for such behavior in the presence of learning about the economy during periods of increased uncertainties

are developed in Cukierman (1980) and in Bernanke (1983a). Those observations are, I believe, appropriate for both the GD and the GFC.

The above scenario implies that the transmission of monetary policy to the real economy crucially depends on the behavior of individual financial firms. This behavior depends, in turn, on the state of their liquidity, their capital and on their perceptions about the level of uncertainty in the economy. When times are perceived to be tranquil, injections of central bank money into the economy are swiftly transformed into additional credit and lower rates. But when, as was the case during the peak of the GFC, uncertainties are high, the injected liquidity sits in the balance sheets of banks and has little impact on both the quantity and the price of credit to the real economy.

The reinterpretation above leads to three conclusions: First, the transmission of monetary policy actions to the real economy depends on the reaction of the banking system. Second, this reaction is likely to be quite different during normal than during crisis periods. Last, but not least, a reasonable description of the transmission of monetary policy generally requires sufficiently detailed descriptions of the behavior of banks and other financial intermediaries.

Comparison of New Keynesian Models with Keynesian Frameworks of the Sixties

Introduction[10]

The sixties are generally believed to be the heyday of Keynesian economics with respect to both theory and policy prescriptions. During the last decade, a technically sophisticated body of theory known as "New Keynesian" managed to introduce Keynesian sticky price ideas into dynamic stochastic general equilibrium (DSGE), opening the door for integration of real business cycle (RBC) with monetary theory and policy. Woodford's (2003) impressive monograph lays foundations for this approach. Gali (2008) is a compact graduate textbook introduction to the area.

This new body of literature has been taught to recent generations of PhD students and is quite active research-wise. To many young researchers in the sub-area it is the standard way of doing macro models with sticky nominal variables. My experience has been that many (of the highly clever young researchers) in this active area possess only vague notions about the "old" Keynesian models of 1960s. By the same token, most older researchers who contributed to the development of such models are largely ignorant of the contributions made within the New Keynesian framework. The main objective of this section is to close some of this intergenerational gap by presenting a non-technical but conceptually precise

[10]This section draws on section 2 of Cukierman (2005).

comparison of the main similarities and differences between the new and the old Keynesian frameworks.

Comparison

1. The main factor common to both frameworks is that, at least within some range, prices and/or nominal wages are sticky and economic activity is demand determined. In the old version the microfoundations of this process are not explicitly specified and, at least in its polar version, old Keynesian thinking admits of one of the two following alternative regimes: In one, output adjusts fully to satisfy demand while the price level does not respond at all to demand shocks. In the other, "full employment" regime output does not respond and all the burden of adjustment to demand shocks is borne by the price level. In New Keynesian models (NKM) there are explicit microfoundations based on monopolistic competition, due to product differentiation and sticky prices that are motivated by costs of price adjustments. An early theoretical formulation of such a framework appears in Blanchard and Kiyotaki (1987). Due to the temporary stickiness of prices and the existence of positive profits, it is optimal for firms to accommodate demand shocks by means of higher production within some range. The micro foundations of practically all New Keynesian frameworks originate in the microeconomic model of Dixit and Stiglitz (1977).[11]
2. Contrary to standard old Keynesian models (OKM), prices in NKM are sticky only temporarily.[12] When the appropriate time comes, the individual firm's price is reset at an optimal level that takes into consideration the fact that resetting the price again in the future is costly. As a consequence, current price-setting behavior is a function of current inflationary expectations. This requires the explicit modeling of inflationary expectations which are normally specified as being model consistent expectations. The optimal resetting of prices and the influence of inflationary expectations on this activity are absent in OKM, since firms in this model are price takers rather than price setters.
3. Asymmetries in upward versus downward adjustments of nominal variables is an important element of OKM. In particular, OKM postulate that prices, and particularly nominal wages, are more sticky downward than upward.[13] By contrast, in NKM the degree of nominal stickiness is independent of the direction of pressure for price change.

[11]This statement is also true for open economy versions of New Keynesian frameworks like Obstfeld and Rogoff (1995).

[12]In the old models there is no explicit mention of the length of time over which prices are sticky.

[13]Recent evidence for the US supports the view that nominal wages are particularly sticky downward (Bewley 1999).

4. All firms in the economy normally do not adjust their prices simultaneously. OKM are silent on this issue. An attractive feature of NKM is that they attempt to evaluate the positive and normative consequences of price staggering. For tractability reasons the costs of price adjustment are not modeled explicitly. Following a suggestion by Calvo (1983), it is postulated instead that each firm can reset its price in any given period with a constant probability that is smaller than one. As a consequence, when a firm gets the opportunity to reset its price, it takes into consideration that it might not be given such an opportunity again for a number of periods to come. This formalism is widely used in NKM to evaluate the costs of inflation and to draw conclusions for optimal monetary policy.[14]
5. Like RBC models on which they are anchored, NKM feature explicit dynamic optimization at the level of the individual economic unit. For the most part, OKM of 1960s were static and did not incorporate micro based consequences of those dynamics. In particular, intertemporal substitution of consumption is absent in OKM.
6. The dynamics of inventory accumulations and decumulations play an important role in Keynes's original thinking and in some large-scale econometric models of 1960s. To this point NKM has not incorporated inventories into the analysis.
7. Both old and new Keynesian models largely abstract from supply constraints when dealing with the effects of demand on economic activity. This is usually assumed explicitly in the old models. In the new models this is done by implicitly assuming that the demand shocks are not too large, so that total demand facing a typical firm is smaller than the level of output at which the firm's marginal cost of production becomes larger than its temporarily fixed price. In the absence of this assumption, actual economic activity would have to be specified as the minimum between that level of output and demand.

In summary, the main distinguishing feature of NKM is that they incorporate sticky prices and monopolistic competition into RBC frameworks. In so doing, they build a bridge between neoclassical and Keynesian frameworks in which economic activity is demand determined. But it seems that one could have characterized the body of research with equal justification under the NKM heading as a "new neoclassical synthesis". This equally plausible characterization is reflected in the title of Goodfriend and King's (1997) paper. Titles aside, an attractive byproduct of this synthesis is that it makes it possible to explicitly recognize the effects of inflationary expectations on current price-setting behavior.

[14]Explicit modeling of the consequences of costs of price adjustments for *endogenous* price setting decisions have been extensively studied during the eighties at the *micro* level. A collection of relevant articles appears in Sheshinski and Weiss (1993). Another device occasionally used to model costs of price adjustments in New Keynesian models are quadratic costs of price adjustments (Rotemberg and Woodford 1997). Although convenient analytically, this device is less attractive than its Calvo counterpart for two reasons: First, it does not generate nominal staggering. Second, its implicit assumption – that the costs of price adjustments increase at an increasing rate with the size of the adjustment – appears rather unrealistic.

Further Reflections

The experience with the subprime crisis, which preceded the GFC, revealed the importance of the housing market and of supervision and regulation of the financial system. In particular, it reminded economists that prices in the housing and construction sector are largely determined by the availability of credit and by the relative returns and risks on other assets. Thus a change in prices in the housing sector can occur just because of a shift in portfolio preferences, or because of changes in the availability of credit. Such mechanisms are nonexistent in the NKM but are recognized in more elaborate OKM from 1960s. Examples are Tobin's (1969) general equilibrium approach to monetary theory and Ando and Modigliani's (1969) evaluation of stabilization policies within a large-scale econometric model of that time (the Federal Reserve Board MPS quarterly econometric model).

A large number of supervisory and regulatory failures surfaced as a consequence of the GFC (details appear in (Cukierman Forthcoming)). Both old and new Keynesian models are largely silent about the impact of supervision and regulation of the financial system on the likelihood of financial crises.

Concluding Remarks

What are the main analogies and differences between the GD and the GFC? A nutshell answer is that both crises originated in the financial sector following periods of over-optimism, excess leverage and low interest rates. The GD was propagated and extended to the real sector by inadequate institutions, prolonged policy inaction and subsequent aggregate policy errors. To a large extent those second-round problems have been avoided, at least to date, during the current crisis.

The GFC was allowed to develop due to interactions between the common factors above on one hand, and regulatory forbearance, insufficient financial regulation of the shadow banking system and financial innovations that seriously hampered the ability of market participants to estimate the value of large classes of financial assets, on the other.

The GD led to the Keynesian revolution in economics. It is natural, by analogy, to expect that the GFC will also engender changes in the agenda of economic research. I conclude this article with some speculations about possible changes in the focus of economic research in the aftermath of the current crisis. First, much more attention will be devoted to explicit modeling of the behavior of financial intermediaries. The currently popular New Keynesian framework will either be extended to include a meaningful financial sector or be seconded/replaced by other paradigms. Second, the extreme New Keynesian view that monetary policy operates *only* through the interest-rate channel (developed in Chapter 3 of Woodford (2003)) is likely to be pushed aside by the view that this policy can operate via *both* stocks (quantitative easing) as

well as through the interest rate.[15] Third, future monetary policy procedures are likely to give more emphasis to financial stability in comparison to price stability.[16] In parallel, the role of central banks in supervision and regulation is likely to increase.

Neither old nor new Keynesian models pay much attention to the impacts of financial supervision and regulation as well as of the micro structure of financial markets and contracts on macroeconomic equilibrium. The GFC makes the development of such analysis essential. Preliminary – mostly micro or finance-based – research is starting to emerge. Examples are Hart and Zingales (2009) in the micro area, Duffie and Zhu (2009) in Finance, and Brunnermeier *et al*. (2009) in the more practically oriented regulatory reform arena. The more demanding theoretical macro extensions are likely to follow later. Finally, the wide price gyrations observed in financial markets since summer 2008 are likely to revive the view that bubbles and multiple equilibria driven by *oscillations between fear and greed* may be more common than acknowledged by existing theories. An important challenge in this area is to find ways to evaluate the links between the quality of supervision and regulation on one hand and the likelihood of booms and busts on the other.

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[15]Interestingly, the monetary policy procedures of the ECB and of the Bundesbank before it are based on this view.

[16]Chapter 7 in Cukierman (1992) shows that there is a tradeoff between price stability and financial stability, and discusses determinants of this tradeoff.

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The Great Depression, the Current Crisis and Old Versus New Keynesian Thinking. What Have We Learned and What Remains To Be Learned?: Discussion

Avia Spivak, Ben-Gurion Universeity and the Van Leer Institute

Where I Agree with the Paper

Cukierman correctly characterizes the current crisis (GFC) as less severe than the former crisis (GD). He lists a few reasons behind that phenomenon.

(a) The greatly increased role of the government in the economy. The GFC institutional landscape included features that automatically stabilized the economy, such as the financial safety net to depositors. (Cukierman overlooks the contribution of the social safety net to the unemployed, poor, old and sick in limiting the severity of the crisis.) The increased power of central banks and treasuries in the economy allowed them to react swiftly to the crisis.[17]
(b) Under GFC there was a much better international system as compared with GD.
(c) On the nominal side, the inflation targeting together with flexible exchange rates were successful in keeping international capital markets functioning while allowing substantial exchange rate flexibility.[18] On the real side, countries came to mutual agreement to avoid trade wars and managed to keep the international flow of goods and services unhindered.[19]
(d) Lessons that were learned in GD were applied in GFC, especially the need for active role of the central bank. Luckily, the Fed's chair, Bernanke had studied the failures and mistakes of the Fed in GD, and thus was ready to prevent repetitions of the same mistakes in the GFC.

An important insight of the paper is that FDIC insurance solves run on banks by depositors, but not by banks on other banks or financial institutions. Run on banks by other banks can also have systemic repercussions, as the fall of Lehman Brothers on September 15, 2008 – that sent shock waves through the entire global system – showed. Thus, the theory that big financial institutions are too big to fail was proved and immediately applied to the rescue of AIG. The theory that non-banks financial institution – "shadow banking" need not be regulated because the market will regulate them proved to be false.

[17]See: Goldin–Bordo–White (1998) for a very good summary of the changes due to GD.
[18]See Figures 1 and 2 from: Spivak-Sussman (2009).
[19]See: Golden Fetters by Eichengreen (1996).

Where I Disagree with the Paper

(a) The paper is silent on the political and intellectual background that allowed the phenomenal growth of the shadow banking. Both were united in the person of the then legendary Chairman of the Federal Reserve, Alan Greenspan. In his autobiography (2007) he writes:

> The resulting advance of global financial markets has markedly improved the efficiency with which the world's savings are invested. . . As I saw it, from 1995 forward, the largely unregulated global markets, with some notable exceptions, appeared to be moving smoothly from one state of equilibrium to another. Adam Smith's invisible hand was at work on a global scale. But what does that invisible hand do? . . . Given the trillions of dollars of daily cross-border transactions, few of which are publicly recorded, indeed how can anybody be sure that an unregulated global system will work? Yet it does, day in and day out. Systemic breakdown occur, of course, but they are surprisingly rare. Confidence that the global economy works the way it is supposed to work requires insight into the role of balancing forces. (Those forces regrettably seem more evident to economists than to the lawyers and politicians who do the regulating.). . .
>
> The inevitable mistakes and euphorias of participants in the global marketplace and the inefficiencies spawned by those missteps produce economic imbalances, large and small. Yet even in crisis, economies seem inevitably to right themselves. . ..[crisis] creates opportunities to reap abnormally high profits in the buying or selling of some goods, services and assets. The scramble by market participants to seize those opportunities . . . eliminate both the abnormal profit margins and the inefficiencies that create them. (Greenspan 2007, pp. 367–368).

It is this world view that was behind the abolition of the Banking Act of 1933 (The Glass–Steagal Act) in 1999 which was a signal for the relaxation of regulation of the financial system.[20]

(b) In the same vein, and not less important is the presentation of macro-economics as going through a process of seemingly harmonic evolution, centered around the NKM – New Keynesian Models school, These NKMs represent, according to the view presented – the best of the two worlds: Keynesian models that manage to be based on sound microeconomic foundations. Most economists, however, acknowledge the rift between the Chicago–Minnesota RBC school and the MIT–Harvard–Berkley school, nicknamed by freshwater economics vs. saltwater economics. This is how Nobel laureate Paul Krugman describes the RBC theory:

> By the 1980s, however, even this severely limited acceptance of the idea that recessions are bad things had been rejected by many freshwater economists. Instead, the new leaders of the movement, especially Edward Prescott, who was then at the University of Minnesota (you can see where the freshwater moniker comes from), argued that price fluctuations and changes in demand actually had nothing to do with the business cycle. Rather, the business

[20]Greenspan admitted in a famous Congressional hearing that there was a flaw in his theory. Interestingly, Greenspan recounts in his biography the influence of Ayn Rand, the epitome of libertarianism, on the development of his thinking.

cycle reflects fluctuations in the rate of technological progress, which are amplified by the rational response of workers, who voluntarily work more when the environment is favorable and less when it's unfavorable. Unemployment is a deliberate decision by workers to take time off.

Put baldly like that, this theory sounds foolish – was the Great Depression really the Great Vacation? And to be honest, I think it really is silly. But the basic premise of Prescott's "real business cycle" theory was embedded in ingeniously constructed mathematical models, which were mapped onto real data using sophisticated statistical techniques, and the theory came to dominate the teaching of macroeconomics in many university departments. In 2004, reflecting the theory's influence, Prescott shared a Nobel with Finn Kydland of Carnegie Mellon University.

Krugman goes on to criticize the NKM:

But the self-described New Keynesian economists weren't immune to the charms of rational individuals and perfect markets. They tried to keep their deviations from neoclassical orthodoxy as limited as possible. This meant that there was no room in the prevailing models for such things as bubbles and banking-system collapse. The fact that such things continued to happen in the real world – there was a terrible financial and macroeconomic crisis in much of Asia in 1997–1998 and a depression-level slump in Argentina in 2002 – wasn't reflected in the mainstream of New Keynesian thinking.[21]

The lack of explicit financial system in the NKM due to the rationality of its agents and the smooth functioning of most markets that hence can go wrong is at odds with the original Keynes approach who put much stress on the malfunctioning of the stock market (the famous beauty contest paradigm). Indeed, Cukierman concedes that this part is still missing in the NKM: "The GFC makes the development of such analysis essential" (p. 28).

(c) The rift in academic macroeconomics has implications for policy making in the GFC, and not on a small issue, but on the effect of fiscal policy on economic activity. More technically, by how much will a one percent increase in government expenditures increase the GDP. This magnitude is known as the Keynesian multiplier and the two schools arrive at very different numbers because they use different methodology. Table 2 shows the results:

Romer and Bernstein (2009) use old Keynsian models: "we use multipliers that we feel represent a consensus of a broad range of economists and professional

Table 2 Impact of a permanent increase in government spending by 1% of GDP (federal funds rate set to zero throughout 2009 and 2010)

Percentage increase in real GDP					
	2009Q1	2009Q4	2010Q4	2011Q4	2012Q4
Romer/Bernstein	1.05	1.44	1.57	1.57	1.55
Smets/Wouter	1.03	0.89	0.61	0.44	0.40

Source: Cogan *et al*. (2009)

[21]Krugman (2009), p. 4.

forecasters… They are broadly similar to those implied by the Federal Reserve's FRB/US model and the models of leading private forecasters, such as Macroeconomic Advisers". Smets and Wouters use a NKM model.

The striking difference between the results say it all: the NKM results show only a transitory effect on the output, declining to less than six-tenths after 2 years. If physists and engineers had such disagreement on such basic calculations they could never have landed a man on the moon. (Macroeconomics today looks more like the failed project of landing a probe on Mars, where some teams used the metric system and the other the Anglo-Saxon system of inches and pounds; Mankiw (2006) separates between the role of the Macroeconomist as a scientist that uses very strict methodology with his role as engineer, who can use his intuition and broad knoweldge of the data and the economy to suggest useful analysis of the economy).

Conclusion

Cukierman correctly attributes the success of preventing the GFC from becoming another GD to the practical lessons learned from the GD and the resulting change in the institutional framework.[22]

The influence of the academic development in the macro area is less clear. The Keynesian Revolution was succeeded by a monetarist counter-revolution led at first by Milton Friedman and them by Robert Lucas. According to Lucas and his school, there is very little that the government can do to change the course of the economy, even in times of crisis. It is fortunate that the decision makers were more practical.

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Lucas, Keynes, Animal Spirits, Co-ordination and the Recent Crisis

David Laidler

Abstract This paper begins by examining Robert E. Lucas's views on the relationship of macro-economics to real world economic phenomena, and on Keynes's place in its history, suggesting that these stem from a particular and debatable understanding of how the sub-discipline has evolved. It then considers some implications for today's awkward economic facts of aspects of Keynes' *General Theory,* its speculations about the role of psychology and social conventions in the economic decisions of individual agents recently highlighted by Akerlof and Shiller (*Animal Spirits*. Princeton, N.J.: Princeton University Press, 2009) under the label "animal spirits", as well as its insights into the influence of the monetary system on the coordination of these decisions, along lines later extended by Clower (The Keynesian counter-revolution – A theoretical appraisal. In F. H. Hahn, F. R. P Brechling (Eds.), *The Theory of Interest Rates*. London: Macmillan, 1965) and Leijonhufvud (*On Keynesian economics and the economics of Keynes*, Oxford: Oxford University Press, 1968). It concludes that the questions about co-ordination that Keynes addressed, not to mention some of his answers, are well worth revisiting.

JEL Classifications, B22, B31, E12, E13, E32

Keywords Crisis · Co-ordination · Clearing Markets · Auctioneer · Money · Financial Markets · Animal Spirits · Psychology · Keynes · Lucas

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The Recent Crisis and Macro-economic Ideas

The recent economic crisis had a number of aspects, not least an intellectual one. It was not just that most economists failed to foresee what was coming and that their models required a little readjustment in order to catch up with the evidence. Rather, it was that such re-adjustments were impossible for those who persisted in adhering to the fundamental principles upon which many so-called mainstream models are grounded. That is why the renewed interest in John Maynard Keynes' ideas that the crisis has prompted, not least as epitomized by George Akerlof and Robert Shiller's work on *Animal Spirits* (2009) is so welcome.

What is nowadays called macro-economics is driven by a rather distinctive internal dynamic that differs significantly from the story of unidirectional technical progress within a single paradigm which, in the eyes of so many of its current practitioners, makes the study of its history unnecessary. Though the sub-discipline ceased to be a series of essentially ad hoc responses to current events at some time in the mid-nineteenth century, contemporary happenings have nevertheless continued to exert a systematic influence on its subsequent development. That is because they have and do provide not just policy challenges, but also ongoing empirical tests of the evolving theoretical content of macro-economics.

The development of macro-economics has thus involved a strong element of Popperian "conjecture and refutation", but it has also displayed a tendency to double back on itself from time to time to pick up still useful but temporarily mislaid ideas as starting points for subsequent development. Two factors seem to have played a role here. First, as Harry Johnson (1971) noted, the ability to cope with a currently important policy issue can be an important determinant of a macro-economic doctrine's success. As particular policy problems come and go, therefore, so do ideas that can address them, eventually falling into temporary neglect, not because they have been refuted by empirical happenings but because they have been rendered irrelevant by them for the time being. Thus, the problem of unemployment dominated inter-war discourse, but in the late 1960s the need to cope with inflation gave a strong impetus to what Johnson called "the monetarist counter-revolution", in the process generating renewed interest in the ideas of Henry Thornton (e.g. 1802), Irving Fisher (e.g. 1911) and Knut Wicksell (e.g. 1898) among others. Second, lines of investigation are abandoned sometimes for want of the analytic means needed to carry them further, only to be resurrected later when advances in technique remove these barriers. A striking example of this tendency is to be found in the changing treatment of expectations in macro-economics, first during the 1930s when, at the hands of Keynes (1936) exogenous expectations replaced technically unmanageable ideas about endogenous expectations of various forms, and later from the 1950s onward, as the arts of modeling error learning and rational expectations formation were mastered.[1]

[1]See fn. 11 below for a further discussion of this point.

The interaction over time of macro-economic ideas with the facts that either refute or render them temporarily irrelevant is also a two way affair, as I noted in David Laidler (2003). Forward looking rational behaviour on the part of policy makers and private agents alike must always be based on models – formal or informal – of the economic environment in which they are operating and will be conditioned by those models. Ideas inconsistent with the way the economy actually works nevertheless affect its performance, eventually in ways that produce unexpected results that reveal the inconsistency in question. It is therefore hard to understand economic events, and hence the very empirical basis of macro-economics itself, without also understanding how the ideas that helped generate them evolved. The recent economic crisis is a prime example of a series of events grossly inconsistent with the very macro-economic ideas that helped produce them, but these happenings seem to bear a strong resemblance to those that three quarters of a century ago provoked the "Keynesian Revolution".

Prompted by these considerations, this paper examines some of the relationships between macro-economic ideas and macro-economic crises, both recent and not so recent. It first examines what Robert E. Lucas, who did so much to push the frontier of macro-economics to its current location, has had to say about that frontier's relationship to real world economic phenomena, and also about Keynes' place in the history of macro-economics Then it suggests that Lucas's understanding of how economics has evolved is flawed, and that this accounts for his misleading interpretation of Keynes' place in that history. It also considers some implications for our understanding today's awkward economic facts of taking another look at aspects of Keynes' *General Theory,* its speculations about the role of psychology and social conventions in the economic decisions of individual agents that George Akerlof and Robert Shiller (2009) have recently highlighted, as well as its insights into the influence of the monetary and financial systems on the coordination of these decisions. It concludes that it is these last ideas that are most in need of revival.

Lucas on Macro-economics and Keynes' Place in its History

Talking about "My Keynesian Education" at the 2003 *HOPE* conference on *The IS-LM Model: Its Rise, Fall, and Strange Persistence* (Michel De Vroey and Kevin Hoover 2004) Lucas pointed, almost as an aside, to the problem that "the new theories, the theories embedded in general equilibrium dynamics of the sort that we know how to use pretty well now – there's a residue of things they don't let us think about. They don't let us think about the U.S. experience in the 1930s or about financial crises and their real consequences in Asian and Latin America, they don't let us think very well about Japan in the 1990s" (2004, p. 23)

This remark dates from 2003, when the final "Greenspan boom" was in full swing, and the "residue" of problems to which Lucas referred did indeed seem rather remote from the immediate but apparently well-established economic environment in which it was made, and we should judge its offhand tone in this context.

Viewed with hindsight, though, the remark was ominous, because those same models now, in 2010, do not help us to think about problems that after late 2008 came to dominate the evolution of the entire world economy.[2]

Lucas (2004) also argued that Keynesian economics was just as unhelpful with such issues as the approach that had replaced it, but here it is important to note that the "Keynesian economics" that formed the basis of his education in the early 1960s can be summarized as IS-LM – albeit the rather sophisticated version of it set out in Martin Bailey's (1962) textbook *National Income and the Price Level* – plus large-scale econometric models, of which the then emerging *Brookings Model* was something of a paradigm. This "Keynesian economics" had evolved from *The General Theory* – whether legitimately or not – as a means of coming to grips with, and designing policy to influence, the behaviour of real income and employment, and in the early 1960s it had been extended to deal with inflation by the addition of an analysis of exogenous "cost-push" factors, and/or a simple and apparently permanent inverse relationship between inflation and unemployment – a Phillips curve.

When, at the decade's end, these modifications began to come to empirical grief in the face of failed policy experiments that they themselves had helped inspire, a further patch-up ensued. Endogenously determined, albeit adaptive, inflation expectations were introduced into the Phillips curve, with Lucas himself, in co-operation with the Leonard Rapping being a notable contributor to these early efforts – See Lucas and Rapping (1969). Although this simple modification, inspired by the work of Milton Friedman (1968) and Edmund Phelps (1967), was more empirically helpful than is now commonly believed,[3] its deployment nevertheless came too late to save the day for the macro-economics in which Lucas had been trained, because it did nothing to protect it from deeper theoretical probing along two inter-related lines.

First, the fact that endogenous expectations had been evoked to make macro-economic models work in an inflationary environment made it hard to avoid asking whether simple adaptive formulae were the best that either economic agents or those modeling their behaviour could do. Second, the Phillips curve had started out in A. W. (Bill) Phillips (1958) as an empirical relationship for which its creator had offered scant theoretical justification, but one important strand of the late 1960s

[2]Lucas's talk was not the only item produced in 2003 that should have indicated that something was amiss. Michael's Woodford's magisterial book *Interest and Prices – Foundations of the Theory of Monetary Policy* also appeared that year, and if its apparently thorough index is to be believed, it mentions Japan's experience in the 1990s twice in its 784 pages, financial markets once, and the "lender of last resort" not at all. I take a certain degree of satisfaction in having argued at the time (See Laidler 2006) that Woodford's dynamic general equilibrium approach to policy modelling was well suited to the fair economic climate of a world dominated by successful inflation targeting, but that in rougher weather its narrowness was likely to be a source of trouble, but I had no idea then just how much trouble there was in store. More recently, Axel Leijonhufvud (2009) has issued a harsher verdict on such analysis, terming it "part of the crisis wreckage".

[3]Despite the claims of Lucas and Thomas Sargent (1978) to the contrary. On this, see John Helliwell (2005–2006).

literature, of which Lucas and Rapping (1969) were also pioneers, grounded it in an analysis of the economy's supply side that could be combined with the IS-LM system to form a highly aggregated model of general economic equilibrium. Such a model clearly had to have links to micro-economic general equilibrium analysis, and the exploration of these links fitted naturally into a then already on-going search for the micro-foundations of macro-economics.

As everyone knows, what was soon labeled new-classical economics emerged largely from Lucas's own efforts to come to grips with these two issues. Initially, it was his replacement of adaptive by rational expectations that attracted most of the attention, but his closely related explicit application to traditionally macro questions of micro general equilibrium analysis marked a much more fundamental change in the then dominant approach to macro-economics, because it broke the area's last remaining intellectual links to Keynes' *General Theory*. Moreover, though Lucas's contribution launched a radically new vision of how a market economy functions, he himself thought of it more modestly as involving the exploitation of newly available analytic techniques to deal with age-old problems that the macro-economic theory of the 1960s and the macro-econometrics that went with it had proved unable to address. Viewing the *General Theory* through the prism created by this macro-economics, he saw it as having created an unhelpful detour in the discipline's otherwise orderly history, and interpreted its temporary success as a consequence of the historical situation that had prompted its writing.

In Lucas's view, Keynes had not advanced economics but had merely offered an ad hoc political response to the circumstances of the Great Depression, a response which, seen in relation to what Lucas believed to have been the already long-established internal dynamic of economics, was of no lasting scientific value. In 2004, he made this point as follows:

> "Keynes's real contribution is. . .not Einstein-level theory, new paradigm, all this. . .that's just so much hot air. . . [I]n writing the *General Theory,* Keynes was viewing himself as a spokesman for a discredited profession. . .in a situation where people are ready to throw in the towel on capitalism and liberal democracy and go with fascism or corporatism, protectionism, socialist planning. . . What he hits on is that the government should take on new responsibilities . . .for stabilizing overall spending flows. . .And . . .for everybody in the post-war period – I'm talking about Keynesians and monetarists both – that's the agreed upon view. . .
>
> So I think this was a great political achievement. It gave us a lasting image of what we need economists for. I've been talking about the internal mainstream of economics, that's what we researchers live on, but as a group we have to earn our living by helping people diagnose situations that arise and helping them understand what is going on and what we can do about it. That was Keynes's whole life. He was a political activist from beginning to end". (2004, pp. 23–24)

Now Lucas is hardly the first commentator on the *General Theory* to draw attention to its author's talents as a polemicist, and to be led to dismiss that book's claims to scientific importance in the process. The opening sentences of Arthur C. Pigou's (1936) review of the same book are justly famous, at least among historians of economic thought

> "When, in 1919, he wrote *The Economic Consequences of the Peace,* Mr. Keynes did a good day's work for the world, in helping it back towards sanity. But he did a bad day's work for himself as an economist. For he discovered then, and his sub- conscious mind has not been able to forget since, that the best way to win attention for one's own ideas is to present them in a matrix of sarcastic comment upon other people". (p. 115)

And in 1936, as we all know, Pigou was no more appreciative of the *General Theory* as a substantive contribution to economic theory than Lucas would later be, even drawing a similar unflattering parallel with Einstein.[4]

But Elizabeth Johnson (1978a, b), who was every bit as sensitive as Lucas to Keynes's political agenda, without, however, at the same time dismissing his scientific contributions, argued that we should pay attention to Keynes' skills as a writer if we are fully to appreciate his economics, and nowhere did he make more careful use of these than when he chose the titles of his books. *Economic Consequences* was indeed written to influence current policy. So were *A Tract on Monetary Reform* (1923), *The Means to Prosperity* (1933) and *How to Pay for the War* (1940), not to mention those occasional pieces collected together as *Essays in Persuasion* (1931). Their author's purposes are clearly expressed in the titles of these works. But *A Treatise on Money* (1930) and *The General Theory of Employment, Interest and Money* (1936) signal altogether more serious academic intentions. Pigou missed the point of the *General Theory*, initially perhaps because he was hurt, and justifiably so, by its unfair attacks upon his own work, but, with some acknowledged help from David Champernowne, he would later change his mind about its scientific importance (See Pigou 1938). Lucas's views on these matters, however, have not changed for 3 decades, perhaps for two closely related reasons, namely the logical structure of his own macro-economics, and his understanding of the place of that macro-economics in the history of the subject.

Lucas on Progress in Macro-economics

The development of the expectations augmented Phillips curve in the late 1960s forced increased attention to be paid to the properties of the supply side of macroeconomic models, and one strand in the resulting literature treated the relationship itself as an aggregate supply curve, along which, as Lucas (2004) points out with specific reference to Lucas and Rapping's (1969) version of the analysis, "we have a cleared labor market at every point in time" (p. 26). But the labour market can only be cleared if the demanders of labour both expect to sell what labour produces and are able to do so. Furthermore, the aggregate demand side of the economy to which Lucas and Rapping thought their analysis was complementary was at that time invariably modeled in IS-LM terms and had a postulate about supply side behaviour embedded in it, namely, that what was demanded was

[4]As Don Moggridge has reminded me.

also being produced, for how else could the economy be on an IS-curve along which desired investment equaled desired saving?[5]

The aggregate demand and supply curves of the typical macroeconomic model of around 1970s, that is to say, were not behaviour relationships but equilibrium loci, and to be on either, the economy had to be on both, that is in full equilibrium. However, the then popular large scale quantitative versions of such models that had figured prominently in Lucas's Keynesian education were made up of difference equations that not only tried to deal with out-of-equilibrium adjustment processes, but were also estimated one at a time and then brought together in systems which Lucas disparagingly but with some justification compared to a "church supper" – "a completely crazy way to put together a general equilibrium model of the whole economy. Nobody's thinking about the whole thing". (2004, p. 21)

Lucas seems to have understood from a very early stage, however, and surely correctly, that to bring coherence to these models required more than merely an effort to think self consciously about the "whole thing" when constructing them. He was familiar with Don Patinkin's (1957) search for Walrasian foundations for Keynesian macroeconomics, and had noticed that in the resulting systems, movements between equilibria occurred in real time as prices that adjusted in accordance with what he refers to as the "mechanical auctioneer dynamics that Samuelson introduced". As he pointed out in (2004), because "the rate of change of price in any one market ought to depend on excess demand and supply in all markets in the system" (p. 15) anything could happen in this process.[6] Crucially also, Lucas recognized that this characteristic had a counterpart in those abovementioned difference-equation-based macro-econometric models into which "Keynesian theory . . .[had breathed] some economic life". To match the data they relied on inter-equilibria adjustment processes characterized by parameters whose values were left to be determined freely by those data, and as a result they could explain anything (or almost anything – sometimes there were sign or magnitude restriction) and therefore (almost) nothing As he put it in his Nobel Lecture (Lucas, 1996, p.252), "The dynamics had a kind of patched in quality, fitting the facts, but only in a manner that suggests they could equally well fit any facts" And to complete this unsatisfactory picture, the expectations that implicitly or explicitly entered into determining the behaviour described by the individual equations of these models were routinely unrelated to the outcomes generated by the models themselves.

In (2004), Lucas described the development of macro-economics once these problems were recognized as involving the replacement of this Keynesian economics, in which he had initially been educated, by the Arrow–Debreu model, which

[5]Thus the relationships between aggregate demand and the interest rate that are nowadays widely deployed in monetary policy models should not be referred to as "optimizing IS curves". Rather they are structural behaviour relationships. The apparently simple IS-LM model presents many pitfalls for the unwary who were not brought up on it, as readers of the work of Ingo Barens (e.g. 1999) on its history and logical properties will be particularly aware.

[6]As, according to Lucas, Friedman had already pointed out to Patinkin.

> "shows how you can take what seems to be a static general equilibrium model and talk about markets for contingent claims, talk about any kind of dynamics you'd like, coming right out of the economics. No auctioneer, or the auctioneer worked very quickly … we didn't know this theory existed back in 1960 although it did. But now its potential is getting realized. It has completely succeeded in taking over growth theory, most of public finance, financial economics. Now it's coming in use in macroeconomics with real business cycle theory; certain kinds of monetary variations have been introduced with success …" (p. 23)

This of course is the approach that doesn't let us think about that "residue of things" which includes financial crises and the depression; but it is even more limiting than that. It is also makes it hard for its exponents to double back to the insights of an earlier era in economics for help with this problem because the most important of those insights were into the workings of economies in whose description that phrase deployed by Lucas – "no auctioneer" – is to be taken literally, so that the agents operating within them must themselves set the prices at which they then trade.[7] An economy with "no auctioneer" is thus not the same as one presided over by a "very quick" auctioneer, because when the latter sets the prices of future goods and/or state contingent claims on them alongside those of current goods, he reduces the logic of "any kind of dynamics you like" to that of a static general equilibrium model and precludes the possibility of trade and production happening at non-market-clearing prices, something that is all too likely to happen in his absence.

As some extremely distinguished exponents of the Arrow–Debreu model – for example Frank Hahn (e.g. 1982) – have argued, this quick auctioneer's activities render such an economy crucially different from any that we might encounter in the real world, not least because they eliminate any essential role for money in its processes of price formation or exchange.[8] The market mechanisms embedded in the Arrow Debreu model are thus at best a metaphor for the monetary and financial systems through which exchange among agents both at a moment in time and over time is mediated in the real world. It is because financial crises involve failures of the latter systems that models which analyze only situations in which they are working cannot be of any help in understanding these events.

This does not in and of itself make dynamic general equilibrium models bad economics, because to resort to a metaphor is merely to say that one thing behaves "as if" it were another. Many of us are quite comfortable with empirically testing "as if" statements about how the economy functions, tentatively accepting them if they seem to be useful and otherwise rejecting them. Some of us are even willing to

[7]As Perry Mehrling has reminded me, the use to which modern macroeconomics has put the Arrow–Debreu model is not one that its creators envisaged. Indeed, it was Arrow (Arrow 1959) himself who first seems to have drawn attention to the difficulties posed for anyone trying to understand how real world markets respond to shocks by a model in which, because all agents are price takers, there is no-one to change prices.

[8]Although "certain kinds of monetary variations" – cash in advance, over-lapping generations models, or simply a money stock that responds passively to the demand for money, though Lucas does not itemize these – have been indeed introduced into such analysis, it is not clear by what criteria the success he claims for these exercises should be judged.

deploy particular already well-tested "as if" hypotheses in contexts where they seem likely to work, though we also know that in others they don't. Dynamic general equilibrium modeling in macro-economics, of the type that that began with Lucas's (1972) "money-supply surprise" model, could, and still can, therefore, be defended on an "as if" basis as one potentially useful approach among others, even if limited in its applicability.

Indeed in some places from the early 1990s until recently, such an approach – essentially that codified by Woodford (2003) – provided a very useful framework for monetary policy making, But, as a matter of fact, since the mid-1970s, the market-clearing postulate has often been treated by its exponents, not as a refutable conjecture about how economies might helpfully be modeled in some circumstances, but as an axiom, alongside that of rational maximizing behaviour, upon whose acceptance the admissibility of any model into the ranks of what is then worth testing depends.[9]

One can see why well articulated micro-economic foundations began to take such strong precedence over empirical evidence in macroeconomics in the early 1970s. After all, a permanent inflation-unemployment trade-off had been presented as a well established fact in the preceding decade and had quickly found a central place not just in the text-books, but in the policy debates of the period too, but Phelps and Friedman had challenged its authority with *a priori* micro-economic reasoning before the empirical counter-examples that confirmed their skepticism had been generated. Once inflation began to generate these counter-examples in the early 1970s, however, it became patently obvious that the careful deductive analysis of rational behaviour had been a better guide to assessing propositions about real world economies than had empirical generalizations with no obvious basis in such reasoning.

Even so, the temptation to draw general conclusions about how to proceed with macro-economic modeling from examples such as this has its dangers. The rational maximization postulate is most easily deployed in macro-economics by adopting the "representative agent" simplification to dispose of all the many complications that disparities and interactions among multiple agents can create for the analysis of aggregate behaviour. When such models are extended to multiple-agent formulations, the assumption of continuously clearing markets plays the apparently primarily technical role of enabling the analysis to continue to focus on rational

[9]This author tried, without much success to start a debate about the empirical weaknesses of the money-supply surprise model of the cycle from the late 1970s onwards, drawing attention in particular to that model's inconsistency with one of the best established sets of stylized facts in economics, namely that variations in money growth precede those in output, which in turn precede those in the inflation rate See for example Laidler (1982) Lucas himself does not seem to have acknowledged the model's empirical difficulties, and the impetus they had given to real business cycle theory, until his (1996) Nobel Prize lecture. Recently, in a (2008) lecture offering an interpretation of today's crisis in terms of a framework that harks back to Friedman and Schwartz's (1963) treatment of the Great Depression, he remarks "But we don't have a reliable way to predict how spending changes break down into price effects and production effects". I am grateful to Russell Boyer for drawing my attention to this lecture.

maximizing behaviour, and in particular on the formulation and use of rational expectations to guide it.[10]

But in fact, this technically useful assumption of clearing markets is also of immense substantive significance. To treat it as an axiom rather than an empirical hypothesis is to do nothing less than resolve by assumption, and hence place beyond debate, two of the oldest and most contentious questions in the history of economics, namely whether, and if so how, a decentralized market economy is capable of coordinating the individual consumption and production decisions of those who participate in it. Only the answers "yes" to the first and "as if by agents who respond to the market clearing prices posted by an auctioneer who works very quickly" to the second are admissible. Other responses, such as "sometimes" and "by indirect exchange mediated by monetary and financial systems that are subject to failure from time to time", let alone any more radical suggestions that might boil down to "never", are ruled out.

But, in Lucas's view the deployment of Arrow–Debreu analysis represents nothing more (nor less) than the fulfillment of what was always the goal of a long line of theorists, stretching from David Hume to Patinkin who, wanting ". . . to think in general equilibrium terms . . .resort to disequilibrium dynamics only because the analytic equipment available to them offers no alternative" (1996, p. 253). Or, as he put it in (2004)

> "I see the progressive . . .element in economics as entirely technical: better mathematics, better mathematical formulation, better data, better data-processing methods, better statistical methods, better computational methods, I think of all progress in economic thinking, in the kind of basic core of economic theory as developing entirely as learning how to do what Hume, and Smith and Ricardo wanted to do, only better". (2004, p. 22)

However, as we have seen, no theoretical system based on the assumption of continuously clearing markets – and this is true even of those mislabeled "new Keynesians" models deployed for example by Woodford (2003) in which money-wage and nominal price adjustment is slowed down by arbitrarily introduced overlapping contracts – can deal with some of the critical monetary and financial features of any real world economy.[11] It is not just that such macroeconomic models cannot address the policy issues that the recent convulsions in financial markets created, though they can't, but rather that it is difficult for anyone brought up under their influence even to conceive of such events occurring in the first place. That is why recent economic events created a crisis in macro-economics.

[10]And of course, in empirical work the representative agent assumption permits cross equation constraints to be imposed upon the model's behaviour, whether it is to be tested by estimation or calibration. I am unaware of any arguments, either theoretical or empirical, that support the deployment of such a style of modeling to the exclusion of other less exacting procedures. Of course, there can be no harm in imposing such constraints on a "as if" basis as a prelude to testing their implications, providing one is willing to abandon them if they fail the test in question. What is wrong is to insist on them as a *sine qua non* of sound procedure.

[11]This is indeed to agree with Mark Blaug (2001) that Lucas has misread the intentions of Hume, Smith, David Ricardo et al.

Keynes as an Alleged Scientific Outlier

The extent to which modern economic theory has contributed to the upheavals that began to shake economies in 2007 is a problem for future economic historians and historians of economic thought to worry about. For today's economists, or at least those who take the subject's past seriously, a more immediate question is where in that past we might look for guidance as we try to reconfigure its future.

One answer already widely on offer, not least from Akerlof and Shiller (2009) is the Keynesian Revolution of the late 1930s. Somewhat paradoxically, although these authors have a high opinion of Keynes as an economist, their assessment of the facts of his place in the history of macroeconomics is very similar to Lucas's. They too treat him as an outlier who tried and failed to divert economics from a continuously developing orthodoxy that could be traced back to Adam Smith and the other founders of Classical economics. As they describe that orthodoxy,

According to traditional economics, "free market capitalism will be essentially perfect and stable . . .This line of reasoning goes back to Adam Smith . . .If people rationally pursue their own economic interests in such markets, they will exhaust all mutually beneficial opportunities to produce goods and exchange with one another [and this] results in full employment" (2009, p. 2)

And of course at least one other authority took a similar view of Keynes' place in the history of economic thought even before the publication of the *General Theory*, namely its author. Recall that famous letter to George Bernard Shaw, later quoted on the back cover of the "Papermac" edition of the book in question: " . . .you have to know that I believe myself to be writing a book on economic theory which will largely revolutionise [sic] – not, I suppose at once but in the course of the next 10 years – the way the world thinks about economic problems"; but note also that the *General Theory's* own account of "Classical economics" was castigated by Pigou (1936) as "a macedoine of misrepresentation", and that another reviewer, Frank H. Knight (1937) described Keynes' treatment of it in the following similar, if more even- tempered and informative, terms:

> "The references under this phrase are the sort of caricatures, which are typically set up as straw men for purposes of attack in controversial writing . . .the reader of Mr. Keynes's [sic] book would do well to keep in mind that references to "classical economics" are to be interpreted as relating to economic analysis *at the stage* at which uncertainty and monetary disturbances are assumed absent". (1937, p. 101)

This judgment is surely right, for though Keynes did have interesting and sometimes novel things to say about "uncertainty and monetary disturbances", it is by now a commonplace of the history of macroeconomic thought that he systematically downplayed the fact that so did many of his then academically respectable contemporaries and predecessors. It is worrisome therefore that, at a time when the sub-discipline is having so much difficulty coping with empirical puzzles that seem to be associated with these very phenomena, important theorists

like Akerlof and Shiller, and of course Lucas, remain content to take Keynes' own account of his place in that history at face value.[12]

Some Psychological Elements of Keynes Macro-economics

In *Animal Spirits* Akerlof and Shiller invoke Keynes' name and vocabulary mainly as rhetorical devices in support of the view that macroeconomics should take a little more notice of psychology and rely somewhat less on utility maximizing hypotheses. The ultimate test of their claims must be whether they help us to understand the world we live in better than the alternative, and on this question I am willing to suspend judgment pending the outcome of further empirical investigations. But, though it is certainly true that there is much in the *General Theory* that derives from Keynes' own keen interest in psychology, a point often stressed by Gilles Dostaler – most recently in (2007, Chap. 6) – not all of the precedents for such an approach on display in the *General Theory* are encouraging.

To begin with, Keynes invoked not maximizing behaviour but psychology to get the stable marginal propensity to consume that he needed to generate a stable multiplier, which was both a useful simplification to deploy in his explanation of unemployment as a consequence of deficient private investment and a crucial lynchpin for his policy recommendation that this gap could be filled by public expenditure. He told his readers that though the "propensity to consume" was influenced by both subjective and objective factors, the former, namely "those psychological characteristics of human nature, ... social practices and institutions ...though not unalterable ...are unlikely to undergo a material change over a short period of time, except in abnormal or revolutionary circumstances" so that, ultimately

> "The fundamental psychological law, upon which we are entitled to depend with great confidence both *a priori* from our knowledge of human nature and from the detailed facts of experience, is that men are disposed, as a rule and on average, to increase their consumption as their income increases, but not by as much as the increase in their income". (1936, p. 98)

As Allan Hynes (1998) has documented, however, this building block of *The General Theory,* which Bertil Ohlin had criticized as an over-simplification as early as (1937), began to crack under the weight of empirical evidence in the early 1940s and was ultimately replaced by the utility-maximizing models of Franco Modigliani and Richard Brumberg (1954) and Friedman (1957).

Or to cite another example, Allan Meltzer (1988, p. 146) has drawn attention to Keynes's way of switching between extrapolative and regressive hypotheses about interest-rate expectations as the needs of his case changed. Perhaps the most striking

[12]And, in a widely discussed article published after this paper had been completed and presented, Paul Krugman (2009) seemed to offer a similar interpretation of Keynes' place in the history of economic thought.

example of this occurred between the *Treatise on Money* (1930) and the *General Theory*. In the *Treatise,* when discussing the use of monetary policy to influence the long-term rate of interest in order to offset the effects on investment of swings in "the spirit of enterprise", he argued that the short interest rates that the central bank could undoubtedly influence "affect long rates more than one might expect" because "mob psychology" – at first sight an unreliable foundation for monetary policy – in fact provided the basis for "a homeopathic cure". The real prospects do not suffer such large and quick changes as does "the spirit of enterprise" so "it is not unreasonable to depend on short-period influences for counteracting a violent, and perhaps unreasoning change in sentiment". In the *General Theory,* on the other hand, though Keynes still presented that same long rate of interest as "a highly psychological phenomenon" he quickly qualified this characterization by suggesting that it was "more accurately . . .a highly conventional . . .phenomenon . . ." which would sometimes be impervious to monetary policy because

> "*Any* level of interest which is accepted with sufficient conviction as *likely* to be durable *will* be durable; subject of course, in a changing society to fluctuations for all kinds of reasons round the expected normal. . .it may fluctuate for decades about a level which is chronically too high for full employment . . ." (italics in original)

As to swings in the above-mentioned "spirit of enterprise", which had already appeared, though not under that label, in the work of Frederick Lavington (1922) and Pigou (e.g. 1927) as driven by cumulative and contagious "errors of optimism and pessimism", these would reappear in the *General Theory* as consequences of exogenous changes in "animal spirits" that are there presented as more important determinants of the marginal efficiency of capital than any "weighted average of quantitative benefits multiplied by quantitative probabilities" (p. 161) Now Roger Backhouse and Laidler (2004) among others have followed Jan Kregel (1976) in pointing out that Keynes' treatment of long term expectations about the profitability of investment as exogenous enabled him to develop an essentially comparative static and hence analytically manageable framework to expound the central theme of the *General Theory*, namely that it is changes in expectations *about the future* which largely cause variations *in the present* level of employment, so this was surely a fruitful and productive simplification.[13] But though Keynes had many

[13]Before 1930 there had actually been some non-trivial discussions by Gunnar Myrdal (1927) and Erik Lindahl (1929) of rational forward looking expectations as determinants of current behaviour, which, for want of any means of analyzing these interactions, had given way to ideas about endogenously determined extrapolative expectations. But, as Bjorn Hansson (1982) documents, when embedded in dynamic "model sequences" these too had proved analytically complex and unmanageable and had yielded little in the way of definite results. Others would soon formalize Keynes' essentially static framework into the IS-LM model which was not only technically accessible to the average professional economist of the period, but also yielded clear-cut and above all easily taught results, not least about the effects of shifts of the IS curve on real income, and hence by implication, employment. This was hardly a be-all and end-all as far as modeling the role of expectations in the macro-economy was concerned, but it was nevertheless the best that could be done at the time. Given generally available analytic techniques in the late 1930s, therefore, to make expectations exogenous was actually a progressive step, and it was only in

amusing things to say about the gyrations of the "spirit of enterprise" and its interactions with what he called "speculation" in his Chap. 12 on "The State of Long-term Expectation", the fact remains that in accounting for swings in investment by attributing them to exogenous changes in "animal spirits" (or to exogenous changes in anything else for that matter) he left them entirely unexplained.

Now Keynes was not the first economist to appeal to psychology when lacking an explanation for seemingly important facts: There is, for example, more psychology and less rational maximization at the heart of the *Wealth of Nations* (1776) than either Lucas or Akerlof and Shiller seem willing to concede. Samuel Hollander's discussion at this conference of Adam Smith's defense of the usury laws shows that this rested on a well thought through, though not necessarily correct, psychological analysis that stresses a systematic proclivity on the part of agents to over-estimate their own chances of success in risky undertakings. It is also well known that, lacking any idea of diminishing marginal utility, Smith also invoked an inherent "propensity to truck and barter", to explain the phenomenon of exchange itself.

Nor was Keynes the last to deploy psychology, as Akerlof and Shiller's own example shows. But even given the advances that the latter tell us have been made since 1936 in the course of a further 70 years work on the idea of "animal spirits" (See, p. xi), one wonders whether their optimistic claim that the modern version of this concept provides "easy answers" to the eight undoubtedly important economic questions which their book addresses is justified.[14] Perhaps such answers are just a bit too easy when it is permissible to cast them in terms of behavioural hypotheses that, not being firmly grounded in rational maximization, can be custom-tailored to the relevant facts.

Keynes on Money and Co-ordination

Keynes' deployment of exogenous long-term expectations to permit the analytic simplifications he needed to generate a coherent theory of employment provides a compelling example of how available analytic techniques can constrain the development of economic theory, and the subsequent evolution of the treatment of expectations just as surely illustrate Lucas's contentions about the importance of purely technical advances in permitting theory to move forward. As we have seen, however, Lucas goes beyond this contention to explicitly treat technical advances as in and of themselves defining theoretical progress, and to fail to ask whether this

the third quarter of the century that most economists mastered the methods that permitted more subtle ideas to be explored productively.

[14]These questions, whose specifics need not concern us here, are listed by them on page 6 of their book. Note that, as Peter Howitt has suggested to me, some at least of the differences between Akerlof and Shiller's ideas about "animal spirits" and those of Keynes stem from their habit of using the phrase to characterize any deviation of economic agents' motivation from the rational maximizing norm of neo-classical economics, whereas Keynes used it to refer only to what he elsewhere termed the "spirit of enterprise" among investors.

progress comes at any cost.[15] If he is on the right track here, any doubling back of economic thought to the 1930s, or any other era, is unnecessary, now or at any other time.

Akerlof and Shiller clearly reject this viewpoint, but it is not necessary to share their enthusiasm for psychology in general or "animal spirits" in particular, to agree with their broader judgment. As Backhouse and Laidler (2004) pointed out, much else besides Keynes' deployment of psychological ideas became hidden from view as IS-LM cast its ever deepening shadow over macro-economics in the 1940s and 1950s. This is not to argue that such economics was not legitimately derived from the *General Theory*.[16] But it is to argue that there was much else both in that book, and in the literature in whose context it should be read, that did not find its way into the so-called Keynesian education of Lucas and of most of his contemporaries.

In particular Keynes attributed an utterly central role to money in his new theory of how the economy functioned. Readers of his Preface were told that

> ". . . whilst. . .money enters into the economic scheme in an essential and peculiar manner, technical monetary detail falls into the background A monetary economy, we shall find, is one in which changing views about the future are capable of influencing the quantity of employment and not merely its direction". (p. vii)

Even so, had the *General Theory* said no more than that the key to macro-economic instability was to be found in the workings of the monetary system, this would not have set it apart from a host of other writings of the period. John Stuart Mill's insights into the way an excess supply of output as a whole could arise as a counterpart to an excess demand for money in times of crisis first appeared in a rather obscure paper published in (1844) but written in the late 1820s, but they were also set out clearly in his *Principles* (1848), and were taken up in due course by Alfred Marshall and Mary Marshall (1879). In the inter-war years, much developed, they were on prominent display in Ralph Hawtrey's (e.g. 1919, 1932) work among other places.

Furthermore, as Axel Leijonhufvud (1981) documented so persuasively, Knut Wicksell's (1898) analysis of how an interest rate determined within the monetary system might disrupt capital markets' ability to maintain equilibrium between saving and investment inspired a lively and diverse subsequent literature from which Keynes' own *Treatise on Money* drew much of its inspiration. By 1936, that is to say, the literature of macro-economics was dominated, not by the view that the economy could be analyzed "as if" functioning by barter, but by a bewildering

[15]Krugman (2009) is particularly critical of Lucas on this point, while Lucas (2009) in another article published since this paper was written and presented seems to be unrepentant about his adherence to this view.

[16]A comparison of Chap. 18 of that book – "The General Theory of Employment re-stated" with John Hicks' famous (!937) article will provide ample evidence that it was, while Patinkin (1990) provides a much more elaborate statement of the case for regarding IS-LM as conveying a – he would surely have said the – legitimate account of the book's central message.

variety of efforts, each in its own way unsatisfactory, to articulate just what it was about money that made an economy that used it different in general, and crisis prone in particular.

As I argued in Laidler (1999), there was therefore nothing original about this question when the *General Theory* posed it, nor in any of the concepts Keynes deployed in formulating his particular response. The multiplier was borrowed from Richard Kahn (1931) and Jens Warming (1932), the "marginal efficiency of capital" from Fisher (1907) – where it was called the "rate of return over cost" – while "liquidity preference" – also a new name for an older idea – came from Lavington (1921) by way of Keynes's own earlier *Treatise on Money* and Hicks (1935).

What was new in (1936) was Keynes' uniquely powerful and coherent synthesis of these ideas. The point of liquidity preference theory was that money, whose use as a means of exchange and unit of account made coordinated economic activity feasible in the first place, also could, and did, function as a store of value along-side claims to the income streams generated by capital goods.[17] Thus

> "The psychological time-preferences of an individual require two distinct sets of decisions to carry them out completely. The first is concerned with that aspect of time-preference which . . .determines how much of his income he will consume and how much he will reserve in *some* form of command over future consumption.
>
> But this decision having been made, there is a further decision . . . namely in *what form* he will hold the command over future consumption which he has reserved, whether out of his current income or from previous savings. Does he want to hold it in the form of immediate, liquid command (i.e. in money or its equivalent)? Or is he prepared to part with immediate command for a specified or indefinite period, leaving it to future market conditions to determine on what terms he can, if necessary, convert deferred command over specific goods into immediate command over goods in general?" (p. 166)

In the "as if" model of a barter economy that Keynes misleadingly identified as representing the sum total of classical economics, the rate of interest has only one function, to equilibrate the first of the abovementioned decisions with the investment choices of firms. But, he argued, a monetary economy *is not* an "as if barter" economy precisely because, when money can be held as a store of value, the rate of interest also acquires a crucial role in portfolio allocation decisions, which *may* undermine its capacity simultaneously to coordinate the allocation of resources over time.

Keynes posited the logical possibility of co-ordination failures in a monetary economy, not their logical necessity, however, and he went to considerable trouble to describe the circumstances under which a monetary economy would converge on an equilibrium in which what he called the "neutral" rate of interest – that which

[17]Akerlof and Shiller make much of the potential for money's unit of account role to create "money illusion" and hence to cause departures from rational behaviour. Without wishing to commit myself here either against or for their particular applications of this idea to current macroeconomic issues, let me record my judgment that they overstate role played by this idea, which in any event is mainly due to Irving Fisher (1928) in the *General Theory's* analysis of the importance of money. Perry Mehrling has made the interesting suggestion to me that it is perhaps because this idea is American in origin that it came to play such a prominent role in American versions of "Keynesian economics", of which Akerlof and Shiller provides the latest example.

would equate saving and investment at a full employment level of income – would prevail. However, writing as he was in the mid 1930s (and recall that in his native Britain, mass unemployment had been chronic since 1921) it was nevertheless reasonable for him conclude that at least some of the many factors to which he could point as making this outcome unlikely had in fact prevailed, and to suggest both a diagnosis of the depression, and policy remedies for it.

The General Theory's treatment of these issues will seem to many nowadays both dated and politically naive:[18] It attributes the depression of its times to a chronic lack of investment opportunities that are privately profitable even at a low positive value of the long term rate of interest, this state of affairs being caused in turn by a secular slowdown in technical progress which was expected to persist into the foreseeable future; it then suggests remedies that require some permanent changes in economic and social organization: "a much lower rate of interest than has ruled hitherto", a "state of affairs . . . quite compatible with some measure of individualism, yet it would mean the euthanasia of the rentier" (pp. 375–376), not to mention a "somewhat comprehensive socialization of investment" (p. 378).

We might conjecture that the clearly political content of these recommendations combined with the dramatic flair with which they were advanced as seeming ". . . to a nineteenth-century publicist or to a contemporary American financier to be a terrific encroachment on individualism" (p. 380) were what helped to convince Lucas that their author's theoretical ideas could be of no account for the development of scientific economics. But, as Patinkin (1983) pointed out in the course of his critique of Meltzer's (1981) "different perspective" on the *General Theory,* that book's final Chap. 24, in which these ideas appear, is not a conventional closing summary of a scientific monograph dedicated to expounding a new theory, but merely "Concluding notes on the Social Philosophy towards which the General Theory *might* lead" (italics added).

Keynes himself, that is to say, was well aware that the political appendages of the *General Theory* were not logically implied by its theoretical economic content, but by certain empirical judgments with which he supplemented the book's analytic framework. But claims on behalf of the importance and originality of that theoretical economic content are not, as Lucas would have them, "hot air". That "general theory" to which the book's title alludes is, after all, a systematic and successful effort to show why the logic of a monetary economy cannot in general be reduced to that of an economy in which, to borrow Lucas's terminology, a very quick auctioneer is always at work, and how that economy's distinguishing monetary characteristics render it prone to co-ordination failures.

Furthermore, this logical barrier seems to operate in both directions, because as Peter Howitt has argued in a number of places (e.g. 1997, 2006), macroeconomic systems that rely on the fiction of an auctioneer to support the interlinked

[18]Though Roger Backhouse has rightly warned me that much of this impression stems from viewing the book from the stand-point of today's political and economic orthodoxy, which is of relatively recent origin. It is worth speculating that, when the dust created by the present crisis has settled, a different perspective on some of these matters might emerge.

hypotheses of clearing markets and rational expectations do not seem to be capable of extensions that might encompass co-ordination failures. If therefore, as Lucas would have us do, we treat the differences between these two classes of models as reflecting not the presence of different substantive and testable "as-if" hypotheses about the way in which economies actually function, but simply the technical superiority of one of them, which we are then bound to choose, we not only find ourselves unable to think about co-ordination failures, but we also rule out any efforts to do so as scientifically retrogressive.

The General Theory's Current Relevance

The General Theory of Employment, Interest and Money is not a treatise on the overall anatomy of economic crises. Though it does have things to say about their origins, it is primarily about why they involve unemployment, and what might be done about this, and it is with these same issues that it is most likely to be helpful today. Keynes argued that an understanding of monetary matters was essential to grasping how the level of employment could sometimes be deficient, that this very understanding implied that wage and price flexibility, which operated through its effects on the monetary system, could not and should not be relied on to restore it to a satisfactory level, and also that expansionary monetary policy might sometimes be of limited usefulness for this purpose.

It is convenient to take up the role of price flexibility, or the lack thereof, in the story first of all, not least because its significance for a monetary economy does not rest in any essential way – *pace* Akerlof and Shiller and a host of others – on that psychological phenomenon known as money illusion, or – *pace* the New Keynesian theorists so ably discussed by Alex Cukierman at this conference – on contractually determined money wage rigidities, or for that matter on any other *deus ex machina*. Such phenomena may be empirically significant, but they do not lie at the heart of the reasons why monetary economies are vulnerable to co-ordination failures that generate fluctuations in real variables.

Rather, as only became entirely clear in the 1960s from the work of Robert Clower (1965) and Leijonhufvud (1968), what matters is the more fundamental fact that, though in such an economy variations in the general price level are required to keep the supply and demand for money in equilibrium at full employment, there is no auctioneer present to set this variable's value.[19] Its behaviour is instead the aggregate byproduct of the activities of a host of individual agents, each of whom sets the nominal price of whatever is traded in his or her own market with no regard to that aggregate outcome. These agents can bring as much (or little) rationality and flexibility to these activities as we care to attribute to them, but just so long as they do not always make exactly the right array of decisions to keep the overall price

[19]The work of Clower and Leijonhufvud' on the market-theoretic foundations of Keynesian macro-economics of the late 1960s was eclipsed by the success of Lucas's new-classical approach

level at its market-clearing value, then trading at false prices will take place in some markets, with consequences for subsequent decisions about quantities whose aggregate outcomes may sometimes look like multiplier processes.

This is not to suggest that Keynes regarded a degree of price level stickiness as a drawback in a monetary economy, which would function better in its absence. On the contrary, he clearly believed that the predictability in nominal values that it brought with it helped to make rational decisions possible in the first place, and from the *Tract on Monetary Reform* (1923) to *How to Pay for the War* (1940), he was a firm advocate of price level stability as a policy goal. This was his position in the *General Theory* as well, even in the face of employment problems

> "...the chief result of ...[money wage flexibility] would be to cause a great instability of prices, so violent perhaps as to make business calculations futile in an economic society functioning after the manner of that in which we live. To suppose that a flexible wage policy is a right and proper adjunct of a system ...which on the whole is one of *laissez-faire*, is the opposite of the truth". (p. 269)

But when it came to explaining the possibility of unemployment arising, and warning about limits on the efficacy of monetary cures for this state of affairs, Keynes emphasized not price stickiness *per se* but money's capacity to satisfy liquidity preference, which in turn, of course, derived from money's role in the processes of price formation and exchange that price stability helped support..

According to Keynes, The basic reason that unemployment could arise was that, to use a more modern terminology than he deployed himself, liquidity preference interfered with the rate of interest's ability to keep the allocation of resources over time coordinated in the face of changing investment and saving decisions, thus forcing it to abdicate this task to movements in income and employment; and monetary cures for unemployment were unreliable because the sensitivity of the demand for money to the rate of interest might place limits on the expansionary impact of increases in real balances, even when these were brought about directly by policy-induced increases in the supply of nominal balances, rather than price level variations.

Keynes' claims on behalf of the novelty and importance of these implications of his monetary theory of the rate of interest generated much immediate criticism, and the ensuing debate established a point that is crucially relevant to today's policy debates: namely that, though his explanation of how variations in employment could occur and his speculations about the weakness of monetary stabilizing measures are linked to one another by his insights into the unique nature of a monetary economy, one can accept the former without also assenting to the latter. Dennis Robertson (1936), Jacob Viner (1936) and Hawtrey (1937) each in his own way took this position, and there is a lot to be said for it.

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to the same issues, unfortunately so in my view. For a fuller discussion of its place in the history of macro-economics, see Laidler (2009b). Note that there was also what we might call a "monetarist" branch to this line of thought, See, for example, Karl Brunner and Meltzer (1971) and Laidler (1974).

The key point here is that the interest sensitivity of the demand for money must be *non trivial* for a monetary economy to generate lapses from high employment in the face of shocks to saving and investment, but it needs to be *very high* to support doubts about the effectiveness of monetary stimulus in the face of such lapses. There is much less empirical evidence to support this latter position than the former. That is why Keynes' claims about the importance of liquidity preference as the key to co-ordination failures that can generate unemployment are still worth taking seriously, while his doubts about monetary policy as a cure for them are more questionable. Though he was himself ambivalent about the empirical relevance of that extreme case which Dennis Robertson later called the *liquidity trap,* there is ample textual evidence that he believed liquidity preference usually to be highly interest sensitive and unstable as well, a matter that Friedman (1974) in particular emphasized when he denied that his demand for money function was not so much a restated quantity theory as a development of Keynes' monetary theory.[20]

If one can accept Keynes' view of the monetary roots of unemployment without being obliged to accept his skepticism about the efficacy of monetary remedies for it, this still leaves open questions about the adequacy of the explanations on offer in the *General Theory* for the onset of a crisis like today's. There is much that is attractive here: not least the parallel between Keynes' paradoxical proposition, already explored at length, that the very institution of monetary exchange that makes a decentralized market economy possible also renders it vulnerable to failure, and his equally paradoxical argument that the modern financial markets that do so much to mobilize savings and make them available for investment, also undermine the rational basis on which of the latter activity should be founded.[21] As he put it: "The social object of skilled investment should be to defeat the dark forces of time and ignorance which envelop our future. The actual private object of the most skilled investment today is to 'beat the gun' as the Americans so well express it, to outwit the crowd, and to pass the bad, or depreciating, half crown to the other fellow" (p. 155). And his recognition that "these tendencies are a scarcely avoidable outcome of having successfully organized 'liquid' investment markets" did not soften his judgment that "when the capital development of a country becomes the by-product of the activities of a casino, the job is likely to be ill done" (p. 159)

It is hard to deny that all this had considerable contemporary resonance in 2007–2009, or to fail to recognize that Hyman Minsky's (e.g. 1982) nowadays

[20]Furthermore, many contemporary commentators shared his views that experience in 1932 in the US had demonstrated at least the temporary impotence of monetary measures. Not all of them did, however, – Lauchlin Currie (1934) being notable among the dissenters – while the much later work of Friedman and Anna Schwartz (1963) and of Allan Meltzer (2003) has by now made dissent from Keynes' skepticism on this point something closer to a majority view; not a universal one, though, for Paul Krugman (e.g. 2007) is an important dissenter, but since his case for the liquidity trap's existence, as set out in (e.g. 1999) is a purely deductive one, based on a model rigged to produce an L shaped demand for money function by making this demand depend purely on an arbitrarily imposed cash-in-advance constraint while introducing a bond into the system that is a perfect substitute for money as a store of value, it is hard to know what to make of this dissent.

[21]I am grateful to Harald Hagemann for suggesting that I address these issues.

finally popular analysis of financial markets' tendencies to develop into gigantic Ponzi schemes has some of its roots here. But Keynes' analysis is nevertheless incomplete, as has already been noted above.[22] His insights into the role of financial markets in detaching investment decisions from fundamentals notwithstanding, a spontaneous collapse in animal spirits and hence in the marginal efficiency of capital and in investment expenditure is more an *ex post* rationalization of an economic crisis than an explanation for it, and it also leaves too many facts unaccounted for. That is why, as I have explained at greater length in Laidler (2009a), I find analyses of booms and their collapse into financial crisis along Austrian – See e.g. Friedrich von Hayek (1929), and Roger Garrison (2001) – or Robertsonian – See e.g. Robertson (1928) – lines more satisfactory, inasmuch in particular as these find a place for the incomplete capital expenditure projects that, as an empirical matter, always seem to be left behind by these events; and if forced to refine this choice further, I would opt for Robertson, who avoided Hayek's tendency towards extreme policy pessimism about how to deal with the subsequent slump because he had a better appreciation of the fact that bubbles are primarily sectoral in nature, even though the consequences of their collapse may often be economy-wide, and be susceptible to economy-wide remedies too.

Even so, my skepticism about the adequacy of Keynes' views on monetary policy, and on the causes of crises notwithstanding, I hope that it is clear from the foregoing discussion that his interpretation of falling income and employment as a monetary economy's way of moving saving and investment back towards equilibrium when its mechanisms prevent the rate of interest from doing the trick is as relevant today as it was in 1936, and an idea that modern macroeconomics would do well to revive.

Keynes' Contemporary Significance

Lucas (2004) described Keynes's response to the depression as that of a political activist whose claims to have contributed to economic theory were unsustainable. And yet Lucas was not willing to argue that Keynes' influence was over, because further progress along "the internal mainstream of economics . . .that. . .we researchers live on", whose flow Keynes had temporarily interrupted, had not yet produced an adequate treatment of matters related to monetary instability. "Some people just deny that there are real effects of monetary instability, but I think that it is just a mistake" and in any event, researchers "as a group. . .have to earn our living

[22]Leijonhufvud (2009) notes a different element of incompleteness about Keynes' treatment of these matters in the *General Theory*, namely its relative neglect of what he terms "balance sheet effects" and "leverage dynamics", arguing that there is more that is of relevance in the *Treatise on Money*, and in Minsky's writings too. Michel Lawlor (2006) finds more to be said in favour of the *General Theory's* treatment of financial markets as having evolved naturally and constructively from Keynes' earlier writings on these matters.

by helping people diagnose situations that arise and helping them understand what is going on and what we can do about it"

While waiting for the above-mentioned "internal mainstream" to produce relevant results, therefore, Lucas seems willing, when discussing current policy as he does in a recent lecture (2009), to offer analysis in the spirit he attributes to Keynes, and also to Friedman, whose approach he doesn't find too different (see 2004, p. 24).[23] But, as I argued at the outset of this paper, the mainstream of macro-economics does not follow a straight line characterized by purely technical advances. It is much more wayward, and sometimes encounters empirical obstacles sufficiently strong to divert its course and even turn it temporarily back on itself in search of theoretical ideas that have been prematurely mislaid in the face of changing policy problems or inadequacies of available analytic techniques.

Today's economic crisis is just such an obstacle, and my main purpose in this paper has been to argue that it calls for a reconsideration of Keynes's insights into the mechanics of inter-temporal co-ordination in a monetary economy and their proneness to occasional failure. More generally, because I have also argued that this exercise will not even be attempted by exponents of a macro-economics that insists on beginning everything from the assumption that markets clear continuously, I have also implied that it is not just Keynes' specific answer that nowadays needs reconsideration, but perhaps even more so, the basic question that he shared with his contemporaries: namely, what is it about a monetary economy that sometimes causes its coordination mechanisms to break down? Perhaps Lucas's recent ventures into "helping people diagnose . . .and understand what is going on" in today's economy will lead him, not to mention the many other researchers he has influenced, to restore this question to its rightful place at the centre of macro-economic theory.

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[23]Roger Backhouse has correctly pointed out to me that many other earlier commentators – e.g. Johnson (1971), Patinkin (1974) - have noted affinities between the approaches taken by Keynes and Friedman to economics, but when Lucas does so, this is nevertheless of more than routine significance, for in doing so, he implicitly differentiates his own approach from Friedman's, thus casting doubt on the appropriateness of James Tobin's (1981) characterization of New-classical economics as "Monetarism Mark 2"

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Comments on David Laidler's Paper

I welcome this opportunity to comment on the work of David Laidler, who has contributed so much to the development of both monetary theory and the history of economics. Although I did not comment on the paper at the meeting where it was presented in its original version, or on the revised version presented at this symposium, I think that the issues he raises are worth careful consideration.

I will comment on the paper as one who still considers himself to be a "hagiographer", as Frank Hahn called me over 2 decades ago, albeit, now one of documents pertaining to the development of post-1960 economic thought for the most part. In this context let me first turn to the theme of Laidler's interpretation of the treatment of Keynes at the hands of Robert Lucas and then to its variations. I start at the end of Laidler's paper for it is there that he states, what is in my view, its central message, as Don Patinkin would have put it.

Laidler maintains that there is a possibility of what Klug, among others, (Klug, 2006, ed. Young and Bordo) called "recurrence" in the application of "theoretical ideas" in economics, and goes on in his conclusion to stress the need "for a reconsideration of Keynes' insights into the mechanics of inter-temporal coordination in a monetary economy and their proneness to occasional failure", proceeding even further to assert that the "basic question" that Lucas and others "he has influenced" should address is: "what is it about a monetary economy that sometimes causes its coordination mechanism to break down ?".

In my view, however, this is not necessarily the task, or should it be, of New Classical or "Equilibrium Business Cycle" economists, whose economic *weltanschuung* – based upon the competitive market equilibrium approach – *simply cannot co-exist with coordination failure*, as is recognized by Laidler himself in the paper. Rather, I think Laidler should have also addressed his critique at the protagonists of the *other* school of *new mainstream macroeconomics* that is to say, the venue of New Keynesian economists. There he would find confirmation of "recurrence" in the form of imperfect competition and their belief in wage and price stickiness, and their efforts to reconcile these elements with, as Laidler puts it, a "rational maximizing behavior and in particular on the formulation and use of rational expectations to guide it".

What then of the New Keynesian Macroeconomics approach to coordination failure; is it simply the result of market imperfections or stickiness, and not, as Laidler would have it, "the occasional failure" of "inter-temporal co-ordination in a *monetary* [my emphasis] economy"?. Whether or not they have – to borrow Joan Robinson's term – bastardized Keynes's *General Theory* message regarding the possibility of *this* type *of coordination failure* cannot be discussed in detail here; however, in my view, it as an issue worth consideration by Laidler and others. Moreover, is the existence of widely utilized *New Keynesian* ***DSGE*** *models* such as those, for example, of Christiano et al (2005), Blanchard and Gali (2007) and Smets

and Wouters (2007) simply "coincidence", or a manifestation of the "progress" New Keynesian macroeconomics has made ***away*** from Laidler's interpretation of Keynes's *General Theory* approach to coordination failure?

I now turn to Laidler's discussion of Lucas as a critic of Keynes's role in the history of economic doctrines, especially regarding the role of the *General Theory* and Keynes's "economics of depression". Simply put, as Laidler knows well, having written the masterpiece entitled *Fabricating the Keynesian Revolution* (1999), Lucas was not the *first* to question the universal or general relevance Keynes's *General Theory* or its focus on the "economics of depression". Indeed, what is, in fact Hicks's 1937 *critique* of Keynes's *General Theory* approach, among others, surpasses, in my view, the intensity of Lucas's critique. Moreover, as Laidler also knows well, Hicks liked Keynes's *Treastise*, but not his *General Theory*, because he thought the latter to be "static", whereas the former, in Hicks's view, was "dynamic" in nature. Given the emphasis placed by Lucas and those he influenced on the development of, as Laidler puts it, "dynamic general equilibrium modeling in macroeconomics", it should not surprise us that Lucas sees *General Theory*, given its *static* nature, not as the theoretical breakthrough Keynes thought it was; and, given its focus on the economics of depression, as a polemical tome, rather than a "*general*" theory per-se, ***parallel to the views of Hicks***.

My final comment deals with Laidler's assertion that Luca's statement regarding real business cycle theory that has been "augmented" by "monetary variations" is, as Laidler puts it, "the approach that doesn't let us think about. . .financial crises and the depression". I would refer the reader of Laidler's most interesting paper to consult, among other things, the volume entitled *Great Depressions of the 20th Century* (2002), edited by Tim Kehoe and Ed Prescott , to ascertain how far the augmented real business cycle approach has actually evolved (on this see Young, forthcoming).

A Response to Warren Young

David Laidler

I am grateful to Warren Young for his constructive comments, which pay me the considerable compliment of carrying the discussion to which my own paper sought to contribute a step or two further forward. Rather than devote the small space available here to disagreeing with him explicitly about anything, let me rather clarify my own position on two of the issues he raises, and permit readers to make up their own minds about where to take matters next.

In some contexts, it is indeed important to distinguish between the New Classical and New Keynesian strands in modern macroeconomics. (How much I dislike both labels, though, because they have so little to do with the historical antecedents that their creators claim for themselves!). I have no doubt, moreover, that there have been areas in which the latter has proved extremely useful. It is hard to imagine successful inflation targeting without the type of model that Woodford (2003) codifies, sticky wages and all, and it is worth recalling that a number of successful inflation targeting countries – Canada, Israel and Australia, for example – largely managed to avoid the excesses that led up to the recent crisis while basing much of their policy thinking on this kind of apparatus.

But, as I argued in Laidler (2007), which in some respects is complementary to my discussion of these particular questions in the current paper, there are a number policy problems with which this kind of model is not much help: – e.g. the role of fiscal deficits in prompting seigniorage-raising monetary expansions, the choices that face some countries between maintaining monetary autonomy or joining a wider monetary union, and, of course, the design of policies to avoid the onset of asset-market crises and to respond to them when they occur. These deficiencies, I persist in believing, arise because the models in question provide inadequate room for the analysis of monetary and financial systems. This weakness in turn stems, or so it seems to me, from their starting with the assumption of a fully coordinated economic system. Money and finance are not "frictions" that change the behaviour of an otherwise well coordinated economy and therefore need to be introduced as elaborations of some more basic model. They are the socio-economic institutions through which coordination is, or sometimes is not, achieved in the first place, and they need to be there from the very beginning of the analytic story. Keynes showed us one coherent way of doing this, which is why his work remains theoretically interesting and important. To re-iterate this hardly original theme in today's context was the main point of my paper.

Real business cycle theory, to which Young also alludes in his comment, is a type of modern equilibrium modeling, but it also provides an example of macroeconomics' tendency to double back on itself with fruitful results. As Charles Goodhart (1992) pointed out quite some time ago, its central idea of cyclical fluctuations as equilibrium responses to productivity shocks looks a lot like Dennis

Robertson's (1926) story about the "appropriate fluctuations" in output that follow technical innovations and ought to be encouraged rather than ironed out by policy. Furthermore, as I noted towards the end the current paper, and as I have elaborated at much greater length in Laidler (2003), there is much to be said for Robertson's account of the roots of economic crises, including the one we have recently encountered, as lying in shocks to technology.

If asked to identify the crucial innovations this time around, I think I would locate them in the technology of the financial system – e.g. in innovations in the securitization of household debt and the invention of the credit-default swap. Thus it was probably the market rate of interest that took the shock, rather than the natural rate, which would have responded to an innovation to the technology of production, and I don't think that modern real business cycle theory in its current state of development has room for this possibility, though I'm open to correction on this point. Nor, so long as it confines itself to the clearing-markets assumption, will it be able to come to grips with the financial-sector-driven "inappropriate" fluctuations that Robertson believed to be all too often superimposed upon an "appropriate" base, and to be capable of causing much mischief.

All this being said, however, New Keynesian macroeconomics and real business cycle theory are both works in progress, and who knows where they might end up. If my paper, and this brief exchange with Young, nudge them further in the direction of helping us address our post-2007 problems, no-one will be more pleased than me.

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