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REVIEW ARTICLE

Keynes, Chicago and Friedman

A review essay

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Abstract *This paper is a review essay of Leeson, R. (Ed.),* Keynes, Chicago and Friedman *(2 volumes), Pickering and Chatto, London, 2003. These volumes contain a comprehensive collection of previously published papers, and also some interesting new materials, relating to the controversy about the accuracy of Milton Friedman's depiction of the "oral tradition" in monetary economics at the University of Chicago in the 1930s and 1940s. As such, the work is a notable addition to the scholarly literature. The broader issue raised by this collection is the precise relationship between Friedman's "monetarism" and the so-called "Keynesian economics" of the neoclassical synthesis, and specifically, whether there was any real difference between them.*

Introduction

The publication of these two volumes represents an impressive feat of scholarship on the part of the editor, Robert Leeson. He has collected a comprehensive set of published articles and notes, and also some interesting previously unpublished materials, relating to the controversy over Friedman (1956, cited in Leeson, 2003)[1] depiction of the "oral tradition" in monetary economics at the University of Chicago in the 1930s and 1940s. Each of the volumes is subdivided into two parts, with headings such as the "The Initial Controversy", "The Debate Widens", and so on, and Leeson contributes an introductory essay to each of the resulting four parts. In addition, two of his own published articles (Leeson, 1998, 2000, cited in Leeson, 2003) are reprinted, and the final essay in volume 2, "Towards a resolution of the dispute", is also a previously unpublished piece by the editor. One of the original protagonists, Milton Friedman, contributes a foreword with his own retrospective view of the debate.

Friedman's claims about the nature of the Chicago tradition were made in the article "The quantity theory of money: a restatement", which was an introduction to an influential volume of essays by prospective Chicago doctoral candidates at the time. This was intended to be, and so it turned out, one of the "opening shots" (Friedman, 2003, Vol. I, p. ix, cited in Leeson, 2003) of the "Monetarist counter-revolution" against the previous "Keynesian revolution" (Johnson, 1971,

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cited in Leeson, 2003). We should note, however, that one of the main problems running through the entire debate is disagreement as to what both the latter, in particular, but also the former actually were. As will be seen, the safest course throughout the discussion is to think of "Keynesianism" as something like the consensus position on macroeconomics in the American economics profession *circa* 1956, rather than (either) anything that Keynes actually wrote, or that self-styled "Post Keynesians" would advocate. On the other hand, "monetarism" should be seen as specifically the theory and policy recommendations associated with Friedman. Otherwise, the battle-lines become very blurred indeed, with quantity theorists favouring budget deficits, New Dealers advocating monetarism, Chicago school economists opposing monetarism, and so on.

The initial controversy

In any event, to a reader with no particular interest in goings on at the University of Chicago, Friedman's initial discussion of the oral tradition (in just four paragraphs in the original essay) would probably still seem fairly innocuous. It reads as just an attempt to provide some kind of imprimatur for his own theory in terms of a lineage running from his respected teachers and mentors. These were Henry Simons and Lloyd Mints, in particular, but also Frank Knight and Jacob Viner. The suggestion was that Chicago had been one of the "few" academic centres in which discussion of the quantity theory had been kept alive in the 1930s and 1940s. There was also a clear implication that this tradition had indeed resembled the version presented by Friedman, for example, in recasting the quantity theory as "in the first instance a theory of the demand for money" (Friedman, 1956, Vol. I, p. 34, cited in Leeson, 2003). If unremarkable to outsiders, these claims were also apparently not of much interest even to those connected with Chicago, for well over a decade. But then, in Friedman words again (Friedman, 1956, Vol. I, p. ix, cited in Leeson, 2003) "as if a long-delay time fuse had gone off", there were attacks on Friedman's position by two prominent economists with well-established Chicago connections, Patinkin (1969, cited in Leeson, 2003) and Johnson (1971, cited in Leeson, 2003). Of these, Patinkin's critique was the more focused and directed and Johnson's the more vitriolic, going so far as to accuse Friedman of "scholarly chicanery" (Johnson, 1971, Vol. I, p. 179, cited in Leeson, 2003).

Patinkin had been a graduate student at Chicago in the 1940s, and basing his argument on his old lecture notes, the published record and his own recollections, disputed the account given by Friedman. According to Patinkin (1969, Vol. I, pp. 91-2, cited in Leeson, 2003), the quantity theory discussed in Chicago was basically a version of Fisher's $MV = PT$, and did not focus on the demand for money or treat velocity as a "stable function of a small number of variables" (to use the usual paraphrase of Friedman). On the contrary, unpredictable changes in velocity were seen as a major problem, which (presumably in some "short run" with sticky nominal wages) could cause serious fluctuations in real output and employment. The remedy would be a monetary policy that induces changes in *M* to offset changes in *V*. Significantly, in terms of monetary policy "rules" this would imply price level stabilization rather than the sort of money growth rule later favoured by Friedman. The required monetary policy could in principle be conducted by open market operations, but in some circumstances (such as, precisely, those of the 1930s) "money-financed" budget deficits might be required. This last point can then, in some lights, make the policy recommendations of the Chicago school seem very close to "Keynesian economics", at least as this is generally understood by the public. The unkindest cut of all, perhaps, was Patinkin's (1969, Vol. I, pp. 101-2, cited in Leeson, 2003) suggestion that Friedman's demand for money theory was itself simply a reformulation of the Keynesian theory of liquidity preference, and that Friedman had actually provided a "restatement of Keynesian economics" rather than the quantity theory. Patinkin's views themselves did not go unchallenged, of course, and Friedman, 1975 made his one reply in another context a few years later. There was also an exchange between Michael Parkin (1986, cited in Leeson, 2003) and Patinkin (1986, cited in Leeson, 2003), with an even longer time-delay than the original. However, these points from Patinkin's initial article set the parameters for the subsequent discussion.

From Leeson's point of view the most interesting question about the initial controversy is why it took Patinkin and Johnson so long to respond to Friedman's claims. His answer, essentially, was the degree of influence and prestige that Friedman's monetarism had achieved by 1969-1971 as compared to 1956. In the late 1960s/early 1970s monetarism was at the height of its academic influence following Friedman's (1968) presidential address to the American Economic Association, and was to enter the policy mainstream over the next decade and a half. This may be seen as creating an awkward situation for other prominent economists such as Patinkin and Johnson. In the public mind Chicago economics had become synonymous with Friedman's position, and both therefore ran the risk that they would be seen simply as Friedman's disciples, even where their own views were different. Patinkin, for example, was committed to the version of Keynesianism that had emerged from the neoclassical synthesis, and had written his major book (Patinkin, 1956) on this topic. This, in Leeson's view, explains why the challenges to Friedman came when they did, and why the protagonists seemingly felt so strongly about the issues. By this time, Friedman could no longer be ignored, and it seemed urgent from the point of view of the critics to use whatever possible methods to diminish his influence.

The debate widens the uniqueness of the Chicago tradition

The next phase of the debate began with a paper by Thomas M. Humphrey (1971, cited in Leeson, 2003), which looked at the contributions to the development of the quantity theory of money by several "non-Chicagoan" economists in the USA in the 1930s and 1940s. Particularly prominent among these were the work of Clark Warburton, an economist with the Federal Deposit Insurance Corporation, and Lauchlin Currie, who in the early 1930s was a junior faculty member at Harvard. These writers seemed to hold a view of monetary theory, and on the role of errors in monetary policy in either causing or prolonging the Great Depression, which closely anticipated those later associated with Friedman. The case of Currie introduces another dimension also, as was pointed out by his biographer Roger Sandilands (1990). Later in his career, Currie was a prominent "new dealer" in the Roosevelt administration in Washington, and hence, on the face of it, at the opposite end of the ideological spectrum to Friedman. This, therefore, raises the whole question of the relationship of monetarism *per se* to political ideology.

In any event, Humphrey's paper set the debate off on to a new track, that of the "uniqueness" of the Chicago tradition, as Leeson puts it in the sub-heading to the collection of papers in the first part of volume 2. One of the most active participants in this phase of the debate was George Tavlas. This is illustrated by the fact that as many as ten of his single or co-authored contributions over a period from 1977-1999 are reprinted here, (numerically) more even than Patinkin. Notably also, Tavlas was willing to engage several times in polemics with other contributors. There are, for example, sometimes-heated exchanges between Tavlas (1976, 1979, cited in Leeson, 2003) and Davis (1979, cited in Leeson, 2003), Tavlas (1976, 1981, cited in Leeson, 2003) and Cate (1981, cited in Leeson, 2003) and Tavlas (1997, 1998a, 1998b, 1999, cited in Leeson, 2003) and Laidler (1998, cited in Leeson, 2003, 1999). One issue that recurs in these debates is the criterion for distinguishing a quantity theorist from either a traditional "underconsumptionist" or a Keynesian, in circumstances when (as was the case with many economists in the 1930s) all sides were willing to advocate expansionary fiscal policy as a way out of depression. The difference in this context is that the quantity theorist supposedly advocates fiscal policy simply as a means to monetary policy. It is a method of increasing the money supply, as an alternative to open-market operations or other financial techniques. Underconsumptionists or Keynesians, however, at least on the definitions common in this literature, are unconcerned about where the money will come from to pay for new government expenditure. They are willing to advocate bond-financed deficits, or even (in the case of later "textbook" Keynesians) a "balanced-budget" multiplier. As an illustration, one of the issues in dispute between Laidler and Tavlas was whether Paul Douglas, who was a Chicago economist (later a US Senator) but well to the "left" of Friedman's mentors in terms of political orientation, should be counted as an underconsumptionist or quantity theorist in these terms.

As to the uniqueness of the Chicago tradition, Laidler (1993, 1998, cited in Leeson, 2003) focuses in particular on an alternative Harvard connection

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involving Allyn Young, who was a professor there until 1927, the English economist Ralph Hawtrey, a visiting professor in 1928/1929, and Currie who, as mentioned, was a junior faculty member until 1934. According to Laidler, the views of this school anticipated Friedman's later position in most respects (such as a monetary explanation of cyclical fluctuations), except for the modern monetarist emphasis on "rules rather than discretion" in monetary policy. More recently, Laidler and Sandilands (2002, cited in Leeson, 2003) seem to have clinched this argument by publishing for the first time a hitherto almost unknown 1932 memorandum on monetary policy containing all these features, and co-authored by Currie, P.T. Ellsworth, and Harry Dexter White, all associated with Harvard at the time. There also exists another memorandum along the same lines, that signed by a number of prominent economists after the 1932 Harris Foundation conference, which has been in the public domain for a longer period of time. This just seems to confirm the point. Although the conference took place in Chicago[2] at least half of the signatories were not from that academic institution. Friedman (1975) had once cited this memorandum as a prime example of what he meant by the Chicago tradition, but the participation of non-Chicagoans, and the similarity of some of the policy proposals to the contemporaneous Harvard memorandum, seem to lead to the opposite conclusion.

A resolution of the dispute?

The obvious question to be asked after more than three decades of debate (that is since 1969) is whether by now the dispute over the nature of the Chicago oral tradition is any closer to being resolved. Leeson thinks that several points have indeed been definitely cleared up, and the last section of volume 2, and particularly the final paper by Leeson himself, is devoted to such a summing up. Meanwhile, in the foreword Friedman (2003, cited in Leeson, 2003) states that his opinion remains that the substantive ideas in his 1956 restatement were of primary importance and that the question of their origin is secondary. This is surely the stance that would be taken by any original thinker, and one imagines that Keynes himself would have agreed in principle with this. Nonetheless, although Friedman remains convinced that he was inspired by a Chicago tradition, he concedes that with hindsight he would probably have worded the opening paragraphs of his re-statement somewhat differently had he been aware of the material in these volumes, and, in particular, would not have insisted on the uniqueness of the Chicago tradition.

The final paper by Leeson provides another twist on the long saga as it shows that the graduate monetary theory course given by Mints at Chicago in the 1930s actually did include a good deal of material on Keynes. This was not, obviously, the Keynes of the later *General Theory* (Keynes, 1936), which was a highly unpopular book in Chicago, but of the earlier and apparently more acceptable *Treatise on Money* (Keynes, 1930). Friedman's own lecture notes from 1932 (which neither he nor anyone else had thought to look up at the time of the Patinkin controversy) bear out this early Keynesian connection. The relevance of this is that much of the detail of Keynes's theory of the demand for money had already appeared in the *Treatise*, as Keynes (1936, p. 195) himself pointed out. So, ironically, not only was the topic of the demand for money definitely part of the Chicago teaching on money as Friedman would have experienced it, this teaching was also "Keynesian" in the literal sense of deriving from Keynes himself. Friedman (1975, Vol. I, p. 148), on the other hand, had already responded to Patinkin about this line of argument by saying that "Keynes was a quantity theorist long before he was a Keynesian".

As an aside to the above, one direction of inquiry that might have been explored in more detail in this literature, and hence in *Keynes, Friedman and Chicago*, is the fascinating question of when Friedman himself became a "monetarist". In Friedman and Friedman (1998, p 113), Friedman states that he was "cured" (of his early Keynesian tendencies) "shortly after the end of [WWII]". However, on the face of it this timing seems to leave too long a gap between the "cure" and the "re-statement" of the mid-1950s.

Friedman and Keynes and the history of economic thought

These two volumes are certainly a valuable contribution to the scholarly literature, and refute Friedman's view (cited by Leeson, 2002, p. 514) that the debate has produced a lot of "wasted paper". They provide a wealth of information about the development of mainstream monetary economics and macroeconomics in the USA in the twentieth century. There does remain one puzzling feature of the entire discussion, however, which deserves further comment. The problem is that if in the end we are to interpret both Keynes and Friedman as Patinkin and others would have suggested, and putting aside the more personal arguments and animosities that surface in these pages, it would be difficult to understand why there was any dispute about macroeconomics at all in the mid-twentieth century. One of the contributors Laidler (1993, Vol. II, p. 140, cited in Leeson, 2003), also the author of a later book entitled *Fabricating the Keynesian Revolution* (Laidler, 1999), hits this particular nail on the head, discussing the reasons why "the "monetarism" that emanated from Chicago in the mid-1950s … turned out … to have … a good deal more than a(n) … approach to the demand for money in common with the American "Keynesian" orthodoxy to which it was opposed". In a different context, Leijonhufvud (1981, p. 184) made a similar point in the following way, "someone whose macrobeliefs consist of neoclassical growth, variable velocity monetarism and unemployment caused by lags in wage adjustment should not fight Milton Friedman but join him". This is definitely the impression that emerges from many of the interpretations appearing in *Keynes, Chicago and Friedman.* The larger question must be asked, however, whether this does justice either to the ambitions or the achievements of both Friedman and Keynes?

As a possible answer to the above, recall that in his 1971 polemic Johnson invoked Friedman's own favourite "as if" methodology as a vehicle for textual interpretation. Applied to this debate, this may yield more intuitive insight than Patinkin's (1982, p. 17) alternative "regression line" approach. For example, Smithin (1994, 2003) has argued that a useful way to think about Friedman's restatement is "as if" the purpose was not directly to challenge Keynes or the Keynesians, or to propagate any particular Chicago tradition, but actually to confront head on the various criticisms that could be made of the "atrophied and rigid caricature" of the quantity theory (Friedman, 1956, Vol. I, p. 33, cited in Leeson, 2003), with which most economists would then have associated the term. Regardless of whether this was a legitimate inference from the work of Keynes, or that the work of more sophisticated contributors in the 1930s and 1940s was thereby slighted, this was still the majority view, which would need to be challenged if any version of the quantity theory was to be rehabilitated at this stage.

The caricature version assumes that velocity is essentially a constant for the purposes of monetary analysis, that real income is determined independently of any monetary influence (money is neutral), and that (in a simple closed-economy version of the model), the nominal money supply is determined by the *fiat* of the monetary authorities. The quantity theory then becomes a very simple theory of price level determination, and a change in the money supply leads to a proportionate change in the price level in the same direction. Evidently, this simplistic version of the quantity theory is open to attack, and outright dismissal, on three separate grounds corresponding to the original assumptions made. First, velocity may not be a constant but a variable, and some or all of any change in the money supply may be absorbed by a change in velocity. Second, real income may itself be affected by money so that money supply changes may be reflected in real output rather than prices. Third, the money supply may be an endogenous rather than an exogenous variable (even in the closed economy case), with causality running from nominal income to money. So it is suggested that Friedman's primary task in attempting to breathe new life into the quantity theory may be best understood in terms of an effort to meet each of these potential objections head on, generating a "more subtle and relevant" version (Friedman, 1956, p. 33, cited in Leeson, 2003), which would be immune to such criticisms.

Restating the quantity theory as a theory of the demand for money, particularly as "a stable function of a small number of variables", implies that on the one hand the quantity theorist escapes from the restrictive assumption of constant velocity, but, on the other, velocity remains determinate and will respond in predictable way to changes in the variables affecting money demand. Whether or not the formal structure of such a money demand equation can be found in earlier contributions by Keynes or Hicks or elsewhere, to assert its stability is a very different proposition than that of those "Keynesians" (or for that matter Simons *et al.*), who held that velocity is either unstable or purely passive.

Friedman's position on the effect of money on real income, meanwhile, was essentially the same as that of the earliest writers on the quantity theory such as Hume (1752), centuries before. The impact of money on output is conceded for the "short-run", owing to temporary nominal rigidities or misperceptions, but not for the "long-run". In the first instance, the quantity theory is a theory of nominal or money income (Friedman, 1975, Vol. I, p. 147), with the details of the division between prices and output to be filled in later. Even if the length of the short-run is left a bit vague, however, the basic idea remains that money is "neutral" in the long-run, and "non-neutral" only in the short-run. In this way, monetarism was able to retain the basic proposition about the long-run relationship between money supply growth and inflation, while at the same time providing a coherent explanation for major business cycle fluctuations, which (as Johnson said) could be presented as a viable alternative to the orthodoxy of the day. The blame for both types of economic problem could be laid firmly at the door of the central bank.

As for the exogeneity of money, the difficulty here for the quantity theorist is that in a modern credit economy the largest part of the "money supply" consists of the liabilities of various financial institutions, rather than physical commodities or fiat note issues. This gives rise to two problems. First, that of defining the money supply. Which liabilities of which institutions should be included? Second, the liabilities side of the balance sheets of financial institutions will indeed change endogenously whenever changes are made on the asset side, and loans are extended and retired. The well-known theory of the "money multiplier", as expounded (e.g.) in Friedman (1956, cited in Leeson, 2003), is the monetarist response to this. The argument is that even in a fractional reserve banking system the process of credit creation is nonetheless constrained by the need for commercial banks to hold reserves of base money to satisfy the intermittent demands of their depositors for "cash" payments. The monetary base consists of the nominal liabilities of the central bank, and the required reserves/deposits ratio is supposedly determined either by legislation or prudent banking practice. Hence, control of base money by the central bank will be translated into control of the statistically defined money supply.

This last point may perhaps be the least convincing of the monetarist responses, particularly in an environment of rapid financial innovation. Having said this, however, on it own terms the outstanding characteristic of Friedman's effort was the extent of its success in its self-imposed theoretical task. In the sense of both providing an internally consistent theory, and achieving notable academic and political influence, Friedman did succeed in rehabilitating what might reasonably be called "classical" theory of money (Friedman, 1975; Humphrey, 1999). Such a statement, of course, does not necessarily imply that this theory is correct or that it leads to desirable policy outcomes. The situation with Keynes, however, is more complicated. In his case, it may be that the only way to reconcile the competing interpretations is to treat his contribution "as if" the intention was to provide an alternative theory of money, but that ultimately he did not succeed in doing so in a decisively convincing manner. Keynes obviously was a success, as was Friedman, in the sense of attracting attention, dramatically refocusing the policy debate, and inspiring legions of followers, but the reference here is rather to the failure to permanently overturn classical theory.

What, in fact, would be the requirements for a genuine alternative theory of money and the macroeconomy? First and foremost, it may be argued, there would need to be a re-examination of the basic "sociology of money" and its fundamental role in capitalism. Evidently, if the starting point of theory is that economic activity is ultimately only a question of the barter exchange of goods and services for other goods and services that must be the endpoint also. By definition, money will be a "veil". Keynes did, in fact, make an attempt to provide an alternative view of money, particularly in Book 1 of the *Treatise*, which owes a good deal to Knapp's (1924) "chartalism". There are also draft chapters of the *General Theory*, in which Keynes tries to make clear the distinction between a money-using "entrepreneur economy" and a barter-orientated "co-operative economy" (Asimakopoulos, 1988; Dillard, 1988). However, the decision finally to leave this discussion out of the *General Theory* apparently had negative consequences. Certainly it seems to allow these fundamental issues simply to be ignored in the various "neoclassical" interpretations. Ironically, the neglect of any serious consideration of the specific role of money as a social institution may well have been damaging for monetarism also. Friedman (1956, Vol. I, p. 34, cited in Leeson, 2003) was explicit in treating money demand as just an example of the "usual theory of consumer choice". However, rejection of any deeper inquiry of the role of money in the socio-economic system, which may have seemed a fruitful simplification at the time, caused problems later on. It was easy for the monetarists to be outflanked on the right by "real business cycle" theorists and others, who believe that after all money "does not matter", and only a barter economy is worthy of serious consideration.

A second important requirement for an "alternative" monetary theory would be consistently to treat the money supply as an endogenous rather than an exogenous variable, regardless of the nature of the exchange rate regime[3]. Again, Keynes did seem to take this position in the *Treatise*, but, as is well known, allowed "technical monetary detail" to fall "into the background" in the *General Theory* (Keynes, 1936, p. vii). In the later book he reverted to the idea of a "given" money supply, and once again, this seems to be a mistake if the objective was to challenge orthodoxy.

Closely connected with this, a third "must" for a heterodox theory is to provide a monetary theory of the (real) rate of interest, rather than a "natural rate" theory, in which the interest rate is assimilated to a variety of non-monetary concepts such as time preference or the marginal physical productivity of capital. In this respect, the *General Theory* rather than the *Treatise* was the more innovative book, as the latter still relied explicitly on a Wicksellian natural rate concept. The new approach taken in the *General Theory* also relates specifically to the role played by "liquidity preference" in that book. It was supposed to provide an alternative theory of the rate of interest, rather than simply a repetition of the theory of the demand for money *per se*. Interestingly enough, of all the contributors to *Keynes, Chicago and Friedman* only Friedman himself (e.g. 1975) really stresses this point. The problem with Keynes's attempt at innovation, of course, is that if a liquidity preference theory of interest rates is combined with the previously mentioned assumption of a fixed money supply, is it easily undermined by an appeal to "real balance effects" and the like, as Patinkin (1948) himself was quick to see. To avoid this defect, some other monetary theory of interest rates (such as that in Post Keynesian horizontalism) would have to be *combined* with a recognition of the endogeneity of the money supply[4]. As it was, Keynes's interest rate theory was bound to fail in the eyes of orthodox theorists.

Further, not only a heterodox theory, but any empirically accurate theory, should recognise that in practice the monetary policy instrument has usually been a short-term interest rate of some kind, not a quantity of money *per se*. The monetarist school was in favour of directly controlling the rate of growth of some statistically defined measure of the money supply, or of the base itself, consistent with the theory in which changes in *"M"* had a direct causal impact. However, in reality, changes in a policy-related interest rate (e.g. the federal funds rate in the contemporary USA) have usually been the method by which monetary policy is conducted, and there has always been a problem in reconciling theory with practice in this respect. As for Keynes, he seemed quite clear on this point in the *Treatise*, stating that "it is broadly true to say that the governor of the whole system is the rate of discount" (Keynes, 1930, Vol. II, p. 189), and even in the *General Theory* there is at least one explicit statement (Keynes, 1936, p. 191) along these lines. However, for the most part in the *General Theory* monetary policy seems to consist of direct changes in *"M"* just as in later monetarism.

A final requirement for a monetary theory to be different from the classical approach from Hume down to Friedman, must be the position taken on whether monetary changes can permanently affect real economic variables such as output and employment. If the argument is simply that money can be non-neutral in the "short-run" owing to rigidities or imperfections, then, as we have seen, this is not new at all, but has been part of orthodox economic thinking for several centuries (even if it is necessary to reinforce this point periodically). However, to assert that there can be permanent effects either of monetary policy or demand management would indeed be something "revolutionary" from the orthodox point of view. In the case of Keynes, again the textual evidence can only be described as ambiguous. He certainly wrote "as if" he intended to provide a theory of permanent unemployment equilibrium, but did he actually succeed in doing so? Patinkin's view, as made clear in several of the papers reprinted in Leeson's collection (Leeson, 1975, Vol. II, p. 355, cited in Leeson, 2003), is that what is actually in the text of the *General Theory* is an analysis of "unemployment disequilibrium". Viner (1936, cited in Leeson, 2003) had already picked up on this point in his well-known review of Keynes. According to what he wrote later (Viner, 1963, Vol. II, p. 417, cited in Leeson, 2003) "I interpreted Keynes's theory as in fact, *whatever its intent*, a theory only of the short-run determinants in changes in employment ... " (emphasis added). It may be difficult to refute that view in terms of the analysis actually provided, but this does not mean that Keynes was not *attempting* to do more, or that the book was not inspiring to others to work in that direction.

So in the end, we have perhaps inevitably a confused picture in which Keynes had large ambitions and, taking both of his major books together, provided a good deal of innovatory material that went much of the way towards achieving them. Nonetheless, he left himself vulnerable to critics on several major points. The "as if" methodology can explain both why it is possible for neoclassical interpreters of Keynes (as with many of the contributors to Leeson's collection) to read his work in they way they do, but at the same time why a "fundamentalist" Keynesian like Joan Robinson was able continually to make statements such as: "the Keynesian revolution still remains to be made" (Robinson, 1975, p. 131), and "there were times when we had trouble in getting Maynard to see ... the point of his revolution ... " (Robinson, 1975, p. 125), and so forth. This does seem to be true, regardless of the differing judgements that can be made as to Robinson's own success in completing the revolution.

Concluding remarks

The best thing about Leeson's collection is simply that it provokes thought about all the issues discussed above. In the end, the "long-view" put forward elsewhere by Humphrey (1999) seems to be a reasonable judgement. This is that what Friedman was trying to do was to rehabilitate and restate the "classical" monetary theory, in the line from Hume, Thornton and Ricardo to Wicksell and Fisher, whereas Keynes, in his major work, was attempting to make a contribution to the "mercantilist" tradition involving (in its monetary aspects) such figures as Law, Steuart, Tooke and Attwood (although Keynes would certainly have resisted the label of "inflationist", usually applied to this group)[5].

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It is, therefore, hard to call Friedman a Keynesian on this basis, whatever the academic influences running from Keynes to Chicago at one stage, or from Harvard to Chicago at another. Another basic difference is that Friedman's theoretical effort was broadly successful in its own terms, whereas Keynes, by the strictest standards, fell short. This is also why is it possible for some to claim that there is no difference. Humphrey (and surely Friedman) might argue that this state of affairs arises because the "classical" theory is essentially correct, the "mercantilist" view is basically flawed, and that it is necessary to painfully relearn these truths in each generation. Another point of view, however, might be that the longevity of the classical theory is attributable simply to its intellectual clarity and astringency and hence perennial appeal to a certain type of social thinker, not to mention its undoubted importance in the "performative" role of preserving the value of accumulated financial capital[6]. Meanwhile no-one as yet, *including Keynes*, has been able to present an alternative vision of the functioning of capitalism in a sufficiently convincing form to overcome the combined strength of the forces of intellectual conservatism and "vested interest" (Keynes, 1936, p. 383). Finally, where does all this leave the contributions of the late Don Patinkin, the late Harry Johnson, and other mainstream critics of Friedman? It is difficult to resist the conclusion that in logic they should have not have "fought Milton Friedman, but joined him".

Notes

[1] In cases where a work cited does not appear in the bibliography below, the reference is to one of the contributions reprinted in the volumes under review. Page references are to volumes I and II of *Keynes, Chicago and Friedman,* and the date is the original date of publication.

[2] Incidentally, the corresponding conference in the series in the preceding year (1931) was the only occasion on which Keynes actually visited the University of Chicago.

[3] For a small open economy with fixed exchange rates, there is no difficulty for monetarism in recognising that the monetary base will be "endogenous" is the sense of responding to balance of payments developments. For example, the "price-specie-flow" mechanism was a always a prominent part of the classical theory of the gold standard. Domestically, causality still runs from money to prices. An "alternative" view, however, would hold that the money supply is in principle an endogenous variable, i.e. even in closed economy.

[4] That is, both are necessary conditions, but neither is a sufficient condition.

[5] According to Humphrey (1999, p. 4) Keynes was an "ex-classical" who "defected to the opposite side" in 1936. Moreover, (Humphrey, 1999, p. 26), "(h)e returned to the classical fold shortly before his death in 1946".

[6] One of the reasons that Knight opposed Keynes, apparently, was that he was perceived as "essentially taking the side of the man-in-the-street against the effort of the economic thinker and analyst . . .", whose task ". . .is to dispel the short-sighted . . . prejudices of the former". As quoted by Patinkin (1979, Vol. II, p. 385, cited in Leeson, 2003).

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