The 1930s Chicago Tradition in Monetary Economics *

by

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To begin at the beginning: Yes, there was a “Chicago Tradition” in monetary economics in the 1930s. Only one author, Harry Johnson (1971), has ever denied this, and he was wrong. By all credible accounts, this tradition had a strong oral element, but it also left a substantial written record, and it has been a controversial topic since Milton Friedman drew attention to it in (1956). The nature of the tradition’s content, its uniqueness and intellectual homogeneity, and the extent to which its specific ideas lived on in Chicago’s contribution to the 1950s-60s “monetarist counter-revolution”, have all been debated.

To state my own position on these matters: In the 1930s, the central message of Chicago monetary economics was that, provided, among other things, the monetary system was governed by appropriate rules, the organisation of economic life on a largely Laissez Faire basis was viable. This idea, whose plausibility was then under serious pressure from economic events, was also propounded by Chicago monetarism from the 1950s onwards, by which time the Keynesian consensus in macroeconomic analysis had added considerable intellectual weight to skepticism about it.

The Chicago message was mainly a positive one about how a liberal economy might actually work. No doubt most of its protagonists also had an ideological preference for Laissez Faire, but this was secondary. If an economic order is prone to fail because of a fundamental flaw in its monetary mechanisms, there is no point in promoting it, no matter what its other supposed virtues might be. From the 1930s until the 1960s, the majority of economists believed that the market economy suffered from such a flaw, which could only be overcome by essentially permanent governmental activism. Chicago provided an alternative point of view.

Nevertheless, the uniqueness to 1930s Chicago of some of its monetary doctrines, the degree of consensus about them that existed within the department, as well as the smoothness of their subsequent transmission to 1950s Chicago, should not be exaggerated. The history here is very untidy.

**Friedman on the Chicago Tradition**

Friedman’s (1956) initial claims on behalf of 1930s Chicago may be summarised as follows: he argued, first, that although the quantity theory of money as expounded in many places had indeed become moribund by this time, leaving them open to an easy intellectual take-over in the face of the evidence generated by the Great Depression, and the theoretical arguments of Keynes’s (1936) General Theory, a much more resilient version of this doctrine had been developed at Chicago, particularly by Frank Knight, Lloyd Mints, Henry Simons, and Jacob Viner. This had promoted an optimistic and constructive message about prospects for the market economy, even in the face of the Great Depression, which
immunised those exposed to it against what came to be called Keynesianism; he also suggested that he and his colleagues were building upon foundations provided by this tradition as they constructed their alternative to what by the 1950s had become a new macroeconomic orthodoxy.

In 1956 Friedman also claimed that the 1930s Chicago version of the quantity theory had treated this doctrine as a theory of the demand for money, a newer and more sophisticated version of which lay at the heart of his own work-then-in-progress. But subsequent (much delayed) criticism from Patinkin (1969) cast considerable doubt on this specific claim. The true ancestor of Friedman’s new demand for money theory, Patinkin suggested, was none other than Keynes’s (1930, 1936) theory of liquidity preference, a point which Friedman himself (1968) had already partially conceded in response to Harry Johnson (1962). Arguably, however, this aspect of Patinkin’s critique paid insufficient attention to the roots of Friedman’s work (not to mention Keynes’s) in the so-called “Cambridge” version of the traditional quantity theory as developed by Alfred Marshall (1871) and Pigou (1913), among others (see Laidler 1980).

Friedman’s (1974) response to Patinkin conceded that the quantity theoretic basis of the Chicago tradition had been the theory’s traditional MV = PT (or perhaps PY) version, rather than any theory of the demand for money, and simultaneously opened up an altogether broader set of claims about its substance. He now argued that, in the 1930s, Chicago economists had (a) attributed the 1929-33 contraction to an incompetent policy response on the part of the Federal Reserve to what could have been no more than an ordinary cyclical downturn, (b) prescribed vigorous monetary expansion, supported by and perhaps integrated with fiscal expansion, as a response to the immediate problem, and (c) to ensure that there would be no repeat performances, advanced a comprehensive plan for the overhaul of the U.S. monetary system. This plan involved a version of what we would now call “narrow banking” and included “100 percent money” – the phrase is due to Irving Fisher 1935 - with commercial banks being required to hold at all times Federal Reserve monetary liabilities equal to the total of their chequable deposits. Crucially, monetary policy was also to be rule-constrained rather than discretionary. (see J. Ronnie Phillips (1995) on the details of the evolution and nature of this plan)

It was these views, Friedman still argued in (1974), which had prevented the Keynesianism which would soon spread so rapidly elsewhere from taking hold at Chicago because they provided a coherent and comprehensive alternative to it. Moreover, he pointed out, they had also anticipated many aspects of his own then-recent work, not least the interpretation of the Depression set out in Friedman and Schwartz (1963).
Questions Prompted by Friedman’s Account

There can be no denying that the ideas sketched out above were indeed present at Chicago in the 1930s. The written element of the local tradition leaves no doubt about this, as other contributors to this conference will no doubt document in some detail - but see, e.g. Viner (1932, 1933), Simons (1934, 1936), Simons et al. (1933), Wright (ed.) (1932, especially Appendix 1).

More debatable however, and much debated too, are questions about how exclusively these ideas were confined to Chicago, and about the extent to which economists located there were united in their support of all of them. My own answers to these questions, which I shall now summarise, have been presented at greater length elsewhere – see, e.g Laidler (1993, 1999, 2010), and Laidler and Sandilands (2002, 2010) - and have generated controversy. George Tavlas (1997, 1998) and James Ahiakpor (2010) are among those who have registered serious and coherently argued disagreement with some of them.

To begin with, Viner (1932, 1933) certainly gave early and thorough expositions of the case for attributing what Friedman and Schwartz (1963) would call “the Great Contraction” to the policy incompetence of the Fed. And even though he argued for keeping the gold standard in place, he also made a case for vigorous monetary and fiscal expansion as a means of combatting it. There is no reason to question the subjective originality of these papers, and they do indeed, Viner’s lingering commitment to the gold standard notwithstanding, form a core component of an authentic Chicago tradition, just as Friedman claimed in 1972.

However, some non-Chicagoans expressed similar views at about the same time – for evidence see e.g. Humphrey (1971), Tavlas (1976), Steindl (1995) – notably the Harvard instructor Lauchlin Currie, writing under the acknowledged influence of Ralph Hawtrey, whose assistant he had been when the latter visited Harvard in 1928-29. Currie submitted a Ph.D thesis in 1931, one chapter of which, with some revisions, would appear in the April 1934 JPE – then edited by Viner - under the title “The failure of monetary policy to prevent the depression of 1929-32”. (Two chapters of this thesis are published in Sandilands ed. 2004). And it is worth noting that Hawtrey himself expressed views similarly critical of the Fed in his (1932) Art of Central Banking.

There is no compelling evidence that Currie’s work had a direct influence on Chicago thinking in 1932, though circumstantial evidence, about whose significance there can be reasonable disagreement, does point in this direction. Nevertheless, he was certainly working along the same lines as Viner at about the same time, and by 1935 when Henry Simons reviewed his (1934a) book The Supply and Control of Money in the United States – not a rewrite of his thesis, but a largely new piece – Currie’s work was already well known and admired at Chicago, and its similarity to local opinion was fully recognised.
By 1934, Currie had also independently developed a scheme for 100 percent money, in broad outlines similar to that embodied in the 1933 Simons et. al. “Chicago Plan”, a fact to which his former student Albert G. Hart apparently alerted him. His paper, written at the Treasury in 1934, appears as an appendix to the (1968) reprint of his 1934 book. Notably different to Simons et. al., however, Currie presented this scheme as a means of enhancing the efficiency of discretionary monetary policy, not as a basis for the enforcement of a monetary policy rule. His aim in designing it was not to provide a sound monetary basis for a return to Laissez Faire, but to strengthen the Fed’s policy making powers once it had been reformed along more centralised lines, an endeavour in which he was soon to play an important part as a senior member of its staff. (See, Sandilands 1990, Phillips 1995)

Even so, there was neither novelty nor distinctiveness in Chicago’s espousal of rule-based monetary policy in the 1930s. Irving Fisher for one had been tirelessly promoting legislation to bind the Fed to the pursuit of price-level stability since 1919, on one occasion, in 1928, mustering enough academic and political support for the idea to get the (Representative James) Strong Bill before the House of Representatives, where, however, it failed. And Fisher sporadically continued this advocacy throughout the 1930s, not least when, in (1935), he followed Chicago’s lead in arguing for 100 percent money. It was only with the appearance of Simons (1936a) that Chicago caught up with Fisher on the matter of a price level rule for monetary policy. In Simons et al. (1933) and Simons (1934) a rule of some sort governing the behaviour of the money supply had been the favoured option.

In a nutshell, then, Chicago economists certainly stood apart from much of the economics profession in their monetary views before 1936. Policy inertia was the order of the day among adherents of the then new Austrian business cycle theory (whom Friedman misidentified as a species of quantity theorists), and of the old Banking School views associated, ironically enough, with the earlier Chicago monetary tradition of James Laurence Laughlin – see e.g. Gottfried von Haberler and Henry Parker Willis in Wright (ed.) (1932). But as we have already seen, the Chicago group nevertheless found themselves in rather visible company located elsewhere on some important matters. Nor were they always unanimous in their views. Once more, others contributions to this conference will no doubt supply details, so suffice it here simply to mention two notable cases where unanimity was lacking.

Let us begin again with Viner. Despite his views on the Fed’s responsibility for the Depression, and his support for monetary and fiscal expansion as immediately necessary counter-measures, he was not a supporter of the 1933 Chicago plan for longer run reform (see Phillips 1995). His resistance here was
apparently friendly, but it was firm and carefully considered. It provides an early example of what turned out to be a life-long skepticism on his part about the feasibility of subjecting monetary policy to pre-ordained rules. Viner’s profound and encyclopaedic knowledge of the history of monetary thought - not least in this instance that generated by 1844 Bank Charter Act - made him extremely sensitive to issues associated with what we would now call the Lucas critique when it came to such matters.

Paul Douglas, not usually regarded as a core member of the Chicago monetary group – though see Tavlas (1977) - did however support the 1933 Chicago plan, and, as Phillips (1995) notes, as late as (1939) he was still promoting 100 percent money in the company of Fisher, Frank Graham and Earl Hamilton among others. But Douglas was not so much a quantity theorist as an under-consumptionist in the style of Foster and Catchings. By (1933), having been convinced of the futility of open-market operations by experience with them in 1932, he was already endorsing Keynes’s ideas about the income multiplier, as expounded in the Means to Prosperity. And this is not to mention his substantial monograph, The Coming of a New Party (1932), written in support of a rather comprehensive program of economic planning as a long term solution to the economic ills of the United States: the very antithesis of any kind of program for Laisser Faire.

Quite how steadfast Frank Knight and Lloyd Mints were in supporting all aspects of the Chicago monetary tradition in the earlier1930s is hard to judge definitively, since they left no individual published record of their views on its specifics at this time, but other evidence – Mints’ lectures as recounted by Patinkin (and others too), for example, and Knight’s well documented (Stigler 1988) feud with Douglas about Simons’ future in the department - suggest that they stood closer to Simons, and on more matters, than did Viner.

All in all then, even though not everything about the Chicago tradition of the earlier 1930s was unique to that department, and though not all of its creators supported all of its components, the phrase does characterise a distinctive local outlook on the causes of the Depression and appropriate policy responses to it, centered on a conventional, but nevertheless deftly and optimistically deployed, version of the quantity theory of money. And - something that did distinguish Chicago from other places - this outlook, especially as expounded by Simons, was explicitly centered on a belief that, with an appropriate policy regime in place, it would be possible to the United States economy to emerge from its troubles without any need to replace its traditional political commitment to Democracy and Laissez Faire with one of the many dirigiste alternatives then proliferating in the intellectual and political market places.
After 1936
Chicago’s association of the quantity theory with the case for *Laissez Faire* in the early 1930s marked an important way-station on that theory’s long and tortuous journey from the left to the right of the political spectrum (Laidler 2004). In the then not so recent past, it had been deployed by a long line of monetary reformers - some extremely able and responsible, other less so – in their efforts to get the gold standard replaced with some other more “progressive” arrangement. Even though there is a strong American populist element to Simons’ *Positive Program for Laissez Faire*, the latter was well to the right of anything emanating from the *New Deal* or, after 1936, the *General Theory*.

Furthermore, Chicago’s intellectual uniqueness in continuing to focus on the quantity theory became more strongly defined as the decade progressed. Harvard, for example, had dispensed with the services of Currie and his young associates by 1934 – Currie and Harry White having been recruited to Washington by Jacob Viner - leaving the intellectual field there open to the authors of the barely coherent *Economics of the Recovery Program* (Brown et al. 1934), and preparing the ground for this department’s easy takeover only two or three years later by Keynes’s ideas, newly imported from Cambridge by incoming graduate students. Chicago, on the other hand generated no fewer than three critical reviews of the *General Theory*. Viner (1936) was constructively skeptical in the manner of Robertson (1936) and Hawtrey (1937); Simons (1936b) was brief and scornful; and Knight (1937), who would soon successfully oppose the award of an honorary degree to Keynes as the University marked its fiftieth anniversary, was comprehensively and patronisingly hostile.

Clearly, then, what would soon be called “the new economics” did not have an easy entrée at Chicago, let alone an open-armed welcome. But it did make inroads nevertheless, in a story whose details would take me far beyond the bounds of my assigned topic. Let the complexity of this episode be illustrated here by the single fact that Don Patinkin, whose lecture notes are an important source of evidence on just what the Chicago quantity theory tradition looked like to a student, also submitted in 1947 a Ph.D. thesis supervised by Oscar Lange, which is still recognised as a seminal study of what we would now call the micro- foundations of that “new economics”.

If Stigler (1988) exaggerated a little when he suggested that the study of monetary economics at Chicago was “moribund” when Friedman took up his appointment there in 1946 – after all, Mints’ important *History of Banking Theory* had been published only the year before – it was certainly not what it had once been. Nor was Friedman’s arrival a simple case of the return of a former student
who had continued to nurture what he had learned at Chicago while in exile, and who would now revive this old tradition in its department of origin.

To begin with, by his own confession, Friedman had been a supporter on the New Deal in the late 1930s, although he was already a long way from this position in 1946 by which time his then recently published “Methods of forecasting inflation” (1943) had explicitly rejected Keynesian multiplier analysis for this task. But this study had also rejected the quantity theory of money in favour of an approach, based on the “inflationary gap” concept. He had found this idea in the 1941 UK budget which, though he was unaware of it at the time, followed a blueprint provided by Keynes’s (1940) How to Pay for the War. Clearly, in 1946, Friedman had some intellectual tidying up to do before becoming the leader of the “new Chicago” that would begin to emerge a few years later.

And, though new Chicago doctrines would indeed bear some strong resemblances to the old, particularly in their interpretation of the Great Depression and in their commitment to the viability of a market economy underpinned by rule-based monetary policy, they were also supported by a strong scientific preference for empiricism over a priori theorising that seems to have had no local roots. Rather it reflected the influence on Friedman of the NBER and Columbia (and perhaps of a later encounter with Karl Popper).

But these matters are also beyond my assigned subject and to explore them further would be to encroach upon territory allotted to other participants in this conference.

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