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## Chicago Monetary Traditions

by

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**Abstract:** This paper, prepared for the forthcoming *Elgar Companion to the Chicago School of Economics* (Ross Emmett and Malcolm Rutherford eds.) describes monetary economics as it existed in four eras at the University of Chicago. It begins with analysis, based on the Real Bills Doctrine, developed by J. Laurence Laughlin, the first chairman of the Chicago Economics Department, and such students as Henry Parker Willis, which had an important influence on the original Federal Reserve Act, and then successively discusses: the inter-war “Chicago tradition” of such economists as Jacob Viner and Henry Simons, but also encompassing Paul Douglas’s under-consumptionism; Milton Friedman’s “Monetarism”; and New-classical economics as pioneered by Robert E. Lucas Jr. The paper’s basic theme is that, although Chicago monetary thought has never been quite as unique and single-minded as some of its admirers have claimed from time to time, it did maintain a certain distinctiveness during its first three eras that has now disappeared.

In popular understanding, the University of Chicago monetary tradition is inextricably linked to the ideas of Milton Friedman, which extend well beyond that area to encompass a general belief in the efficacy of market mechanisms as regulators of economic life, and an equally general skepticism about the desirability of government intervention therein (Reder 1987). Chicago “monetarism” is seen as having challenged (more or less successfully, depending on the commentator) a previously dominant “Keynesian” consensus on these matters. Though Chicago monetary economics can, and should, be judged on its theoretical and empirical merits, I shall also pay some attention to its broader political connections in this essay, because such considerations are important in locating its position in the broader history of American economic thought..

### *The First Chicago Tradition*

The history of Chicago monetary economics long antedates the outbreak of the monetarist controversy of the 1950s. The first chairman of the Chicago economics department, James Laurence Laughlin, was one of America’s most influential monetary economists between the 1880s and the foundation of the Federal Reserve System in 1913. (Friedman 1987, Dimand 2002). Along with his student Henry Parker Willis, eventually a professor at Columbia Business School, he continued to expound his ideas in the debates about monetary policy of the 1920s and 1930s, though. another important student, Wesley Clair Mitchell, had by then largely broken free of his influence.

Laughlin was conservative in both his politics and his economics throughout his long life, as his writings (eg. 1885, 1903, 1933) attest. He was a staunch defender of the gold standard during the debates about bimetallism, and his close association with the Republican Party, forged during that debate, prevented him playing a direct role in the creation of the Federal Reserve system, though he exerted considerable indirect influence on that process, both through the force of his ideas, and through Willis who played a direct role as counsel to Congressman Carter Glass’s Banking sub-committee. Here, Laughlin and Willis represented what Robert Dimand has termed the “Lasalle Street Tradition” which sought to prevent any American Central Bank being dominated either by the Federal Government or private interests centred on New York. The

division of the system into twelve districts, each served by what was initially supposed to be its own central bank, reflected this viewpoint..

But Laughlin's conservatism does not link him to any later Chicago monetary tradition, because, in the monetary area, and in the first of several strokes of irony that mark that tradition's development, he was a leading opponent of the Quantity Theory of Money. Long after marginalist ideas permeated micro-economics Laughlin remained a devotee of the classical cost-of-production theory of value, and of the related idea that gold was the "natural" standard of value. These ideas led him to treat price level variations under the gold standard as originating in changes in the cost of production of individual goods. They also led him to oppose proposals for countering the deflation that marked the period 1873-1896 by monetary expansion based on re-introducing silver into the monetary system, and hence restoring bimetallism to the US monetary system.

For Laughlin, the latter ideas, which were supported by the progressive wing of the Democratic Party, and notably by William Jennings Bryan, were ill-conceived and dangerous. (Laidler 2001) There was indeed a strong inflationist element among the bimetallists, but Laughlin rejected not just that, but the quantity theory of money upon which even responsible bimetallists, such as Francis Walker, (ironically) of MIT, based their case. He and his fellow defenders of gold monometallism were so successful in discrediting the quantity theory in popular thought that Irving Fisher's 1911 *Purchasing Power of Money* was written explicitly to re-instate it as a scientifically respectable doctrine, and is best read as a rebuttal of Laughlin's ideas.

When it came to banking, Laughlin expounded a version of what Lloyd Mints (1945), a contributor to the inter-war Chicago tradition to be discussed below, would later call the "real bills doctrine". Laughlin took over from the mid-19th century British "Banking School" the view that the banking system should grant short term credit in sufficient amounts to meet the "needs of trade", which were themselves conditioned by real factors. This goal would be best accomplished by a banking system configured to provide an "elastic currency". Neither Laughlin (nor the Banking School before him) made a careful and consistent distinction between money

and credit, but in practice their prescription was that a banking system should confine itself to providing short term loans to finance inventories and goods in process in whatever amounts were demanded, and they were sometimes unfortunately vague about the role of the rate of interest in conditioning that demand. Such behaviour, they believed, would make the banking system a passive agent in an economy that could otherwise be trusted to run smoothly, and prevent it from contributing to monetary instability.

These views, which were also held by Willis, had a considerable influence both on the design of the Federal Reserve system, and on its policies in the 1920s and early 1930s, when, it is now widely agreed, the Fed's passivity permitted an ordinary cyclical downturn to get out of hand and develop into what Friedman and Schwartz (1963) would call the Great Contraction. Laughlin and Willis were prominent among those who took a fatalistic attitude towards the economy's collapse in the early 1930s, defended the Fed's passive stance, and opposed any policies actively designed to promote recovery. By that time, however, such ideas were no longer represented among active members of the Chicago department.

#### *The Inter-war Chicago Tradition*

No proposition is more closely associated with Friedman's monetary economics than that the Federal Reserve system was largely responsible for the severity of the Great Contraction, and Lloyd Mints had developed a version of this argument as early as (1948). The contrast between early Chicago monetary economics as represented by Laughlin, and Friedman's version could, therefore, hardly be starker. Friedman argued (1956, 1974), that even in the early 1930s, Chicago was distinct in being already the home of a body of ideas, built around the quantity theory of money, that took an optimistic view of what monetary policy could have achieved in fighting the Contraction. And, he claimed, the strength of the intellectual tradition in question had rendered Chicago essentially immune to the influence of Keynes's ideas after 1936, and hence had laid the intellectual basis for his own work of the 1950s and 1960s.

Now in the economic thought of the inter-war United States, the quantity theory of money is most closely associated with Irving Fisher, whose academic home was (another irony) Yale, not Chicago; but to understand just what is right and what is wrong with Friedman's

account of the Chicago monetary tradition during the Great Contraction, it must first be appreciated that the debates of that time were considerably more than a simple matter of “quantity theory versus real-bills doctrine” with Fisher and his followers on one side and Laughlin and his heirs on the other. In the 1920s, Fisher was beginning to display some of that intellectual rigidity that would in due course earn him the reputation of a monetary crank, and he spent a good deal of time campaigning, first for a monetary policy based upon his “compensated dollar” scheme, and then, less specifically, for the imposition by law of a price-level stability mandate on the Federal Reserve system.

Fisher’s extremism left open a considerable middle ground in the debate between him and the Real Bills camp that was occupied by a variety of proponents of what was then called “credit control”, the deployment by the Fed. of discretionary monetary policy to help stabilize cyclical fluctuations (Mehrling, 1998). These middle ground ideas were heavily influenced by the British economists Ralph Hawtrey (eg. 1919) and the Keynes of the (1923) *Tract on Monetary Reform*. Prominent, though by no means alone, among their exponents was Allyn Young, who had been Frank H. Knight’s Ph.D. supervisor at Cornell. Young was at Harvard in the 1920s, and, prior to his departure for the London School of Economics in 1927, an occasional advisor to Governor Benjamin Strong of the Federal Reserve Bank of New York. He was, however, evidently well known and admired at Chicago, and not just by Knight. The influenza which was to cause his premature death in 1929 was in fact contracted during a visit to the University to discuss and decline an offer of the chairmanship of the economics department (Blitch 1996).

A good part of the Chicago monetary tradition of the interwar years is properly located in the abovementioned middle ground. Before the onset of the Contraction it offered cautious support for the deployment of discretionary monetary policy, and more urgent support thereafter. Charles Oliver Hardy, a lecturer at Chicago between 1918 and 1922, and later closely associated with the Brookings Institution, and Jacob Viner, at Chicago from 1919 until 1946, come to mind here as cautious proponents of monetary stabilisation policies. But the heretical under-consumptionism of William Truffant Foster and Wadill Catchings was also vigorously represented in the Chicago department by Paul Douglas, better remembered nowadays for his

work in labour economics and his later accomplishments as a US Senator, who had joined the Department in 1920, and would remain an active member until his entry into politics in the late 1930s. At the other end of the political spectrum, Henry Simons who joined the Department in 1927, combined the quantity theory with a rule-based approach to monetary policy, similar in many essentials to Fisher's, and with a broader commitment to laissez-faire, while mention should also be made of Aaron Director and Lloyd Mints, who came to share his views.

Director, later Friedman's brother-in-law, initially joined the Department to work with Douglas, and in 1931 produced with him a rather conventional survey volume on *The Problem of Unemployment* (in which Douglas's under-consumptionism was temporarily replaced by monetary analysis that owed a good deal Keynes's *Tract*). Soon afterwards, however, Director became, along with his sister Rose Director and Simons, a leading member of the conservative group that gathered around Frank Knight. At this time, relations between the Knight and Douglas were so bad that they communicated only in writing, and Douglas, having failed to prevent Simons obtaining tenure, was rewarded by Director's banishment to the Chicago Law School. Mints wrote nothing of interest before 1945 but nevertheless taught a brand of monetary economics throughout the 1930s and into the early 1950s that seems to have been heavily influenced by the ideas of Simons and owed essentially nothing to the Keynes of the *General Theory* (See Mints 1946, 1948).

Evidently, then, monetary economics at Chicago was anything but scientifically or politically homogeneous during the Great Contraction, but the economic problems of the time meant that economists with very different understandings of how the economy functioned, and of very different political persuasions too, could sometimes agree about policy, and so it was at Chicago for a while. In the early 1930s it was natural for an under-consumptionist of Douglas's stripe to support with renewed vigour the continuous fiscal and monetary expansion that he had been recommending even during the boom of the 1920s. And it was equally natural for a quantity theorist such as Viner (1932, 1933) to deplore the influence of the real bills doctrine on Fed. Policy and to join Simons and others among his colleagues in supporting immediate monetary expansion as a palliative for the contraction, though Viner was troubled that such policies might require the US to abandon the gold standard, of which he was a supporter.

As banking problems in the United States became more and more acute in 1932-33, it was also natural to look for ways of ensuring that monetary expansion could in fact be engineered. and, it is not surprising that “fiscal inflationism”, the use of money financed budget deficits to bring about monetary expansion, should find support from a group that had Douglas at one theoretical and political extreme and Simons at the other, and included Viner for a while too. Nor was it surprising that a proposal for backing demand deposits with a 100 per cent reserve requirement in order to give the authorities firm control over the behaviour of the money supply, and to prevent any future collapses of the market for bank credit generating monetary contractions, - the central feature of A. G. Hart (1935) would term the “Chicago Plan for Banking Reform” - would also emerge from and find support among this diverse group.

Friedman’s story, recently revived by Tavlas (1998), of a unique “Chicago Tradition”, based on the quantity theory, that advocated tackling the Depression by fiscal and monetary expansion long before the ideas of Keynes began to sweep the United States from a beach-head established at Harvard in 1936, is nevertheless over-simple. To begin with, even the policy proposals emanating from Chicago itself were not uniquely related to the quantity theory of money. Douglas’s version of them was, as we have seen, based not on the quantity theory, but on an under-consumptionist theory. By 1933, furthermore, he was also beginning to cite Keynes’ multiplier analysis as set out in *The Means to Prosperity* (1933) in support of his policy recommendations. More generally, monetary explanations of the contraction, proposals for monetary expansion in general, and fiscal inflationism and 100 per cent reserve requirements in particular were neither unique to Chicago economists, nor originated among them (See, inter alia. Reeve (1943), Humphrey (1971), Phillips (1995), and Laidler, (1999, Chs. 8 and 9). Such ideas are to be found independently and more or less fully articulated in the writings of a number of non-Chicagoans in the early 1930s, in some cases before they appear in Chicago sources.

This is not to deny that such ideas were discussed and promoted at Chicago. Over and above the contributions made by members of the Chicago department of economics, two conferences held there in 1931 and 1932 under the auspices of the Harris Foundation, also attest to this. (Wright (ed.) 1931, 1932) At the first of these, Keynes himself spoke, and following his recently published *Treatise on Money*, proposed a monetary cure for the gathering down-turn,



based on driving down the long rate of interest. According to Davis (1971) he disappointed his audience, Chicagoans and others alike, by his timid attitude towards expansionary fiscal policy. From the second conference, there emerged a nowadays well known petition to President Hoover advocating vigorous monetary and fiscal expansion, signed by 12 members of the Chicago Department (though not by Douglas), but also by 12 non-Chicagoans, among whom were Irving Fisher, Alvin Hansen (Minnesota), Carl Snyder (Federal Reserve Bank of New York) and John H. Williams (Harvard)

The picture that emerges from all this is not of Chicago as a unique institution where ideas that had difficulty maintaining more than a foothold elsewhere were developed and promoted during the 1930s, but rather as a department that was one important and vigorous participant in a much more widespread tradition in monetary economics, many (but not all) of whose adherents deployed the quantity theory of money in a critique of economic policies based on the real bills doctrine, and in proposals to supplant those policies with vigorous monetary and fiscal expansion.

To the extent that Chicago made a unique contribution to this tradition, it was the approach to economic policy that we associate with the writings of Henry Simons, but to which Aaron Director almost surely significantly contributed as well, not to mention Lloyd Mints. Most advocates of a monetary explanation of the Great contraction, fiscal inflationism and one hundred percent money combined these ideas with support for the more general policy activism of the New Deal - Lauchlin Currie, whom Viner took with him to Washington from Harvard in 1934, is a case in point - but these Chicago economists linked them instead with Fisher's case for rule guided monetary policy and made the resulting package a central component of what Simons in (1934) termed *A Positive Program for Laissez-faire*. Within the context of the Chicago Department, it is natural to contrast this program with Paul Douglas's democratic socialist agenda, as set out in his (1932) *The Coming of a New Party*, but, including as it did proposals for vigorous anti-trust policies, and serious income re-distribution, Simon's program was (another irony) in some ways closer to the progressive populism against which Laughlin had fought in the 1890s, than to the conservative agenda associated with Chicago from the 1950s onwards.

The indisputable continuity between the specifically monetary component of Simons' program and Friedman's later work is not, however, the result of Chicago's being free of Keynesian economics in the interim. Chicago reviewers of the *General Theory*, who included Viner, Knight and Simons were critical of the book, the latter two being downright hostile, and Knight does seem to have managed to prevent Chicago awarding Keynes an honorary degree in 1941. Beginning in the 1940s, Chicago was nevertheless home to such Keynesians such as Oscar Lange, Jacob Marshak, and Lloyd Metzler, not to mention the staff of the Cowles Commission, and to graduate students such as Don Patinkin whose work provided a key foundation of what came to be called the *Neoclassical Synthesis* (On all this see, Patinkin 1981). This Keynesian influence would not be dominant for long, but that too was not because of the resilience of any local tradition. After Simons untimely death in 1946, only Lloyd Mints remained at Chicago to teach the monetary ideas of the mid-1930s, and it is notable, that, among the accomplishments with which George Stigler (1988) credits Friedman after his arrival at Chicago in 1946, is the revival there of "the study of monetary economics, which had become moribund".(p.151)

#### *Friedman and the Monetarist Tradition*

Friedman had been a graduate student at Chicago for a while in the 1930s, but the links between his work and that of Simons are not straightforward. His Ph.D was from Columbia, and his empiricism, not to mention his frequently displayed preference for the *NBER* techniques of Arthur Burns and Wesley Clair Mitchell over the more conventional econometric methods of the Cowles Commission, also display a Columbia influence. Furthermore, Friedman's early macroeconomic work was conventionally Keynesian (in the tradition of the inflationary gap analysis of Keynes 1940) and paid little explicit attention to the role of money.

Before the early 1950s, Friedman was, in any event, mainly visible as a statistician and micro-theorist with strong mathematical skills. It was his (1953) essay "On the Methodology of Positive Economics", which defended empirical relevance of the theory of the profit-maximising firm, and particularly its perfectly competitive version, on the basis of its capacity to make useful predictions (an approach influenced by contacts made with Karl Popper through the Mont Pelerin Society - see Friedman and Friedman, 1999) and his (1956) "The Quantity Theory of Money - a Restatement" that established him as a critic of the then dominant economic

orthodoxy that American economics had inherited from the late 1930s. This orthodoxy viewed the economy as dominated by large firms, whose administered prices had more to do with the exercise of monopoly power than the efficient allocation of resources, and hence fit objects for regulation, and it put its faith in fiscal rather than monetary tools for activist macro-stabilisation,

Friedman “restated” the quantity theory as a theory of the demand for money that bore a strong resemblance to Keynes’s (1936) version of liquidity preference theory, albeit without the “liquidity trap”, which was then a prominent feature of textbook expositions of the latter, but that hardly made his work “Keynesian”. Keynes’s theory had roots of its own in the work of Marshall, Pigou and Lavington, who had developed a version of the quantity theory based on the interaction of the stock supply and demand for money (in contrast to Fisher’s more traditional money times velocity flow approach), and this approach had also influenced the middle ground of American monetary economics discussed earlier, where much earlier Chicago work had been located

Even though, before Friedman, the quantity theory had been understood to deal with the influence of the quantity of money on prices and not just the demand for money, it is a necessary condition for such influence to be systematic and predictable that the demand for money be a stable function of but a few arguments, and this was the central proposition of his essay. Furthermore, empirical studies of the influence of money on inflation played a central role in the work of his colleagues and students (in addition to Friedman 1956, see also, for example, Meiselman (ed.) 1970), so, overall, Friedman was surely right to claim a place for his work in a quantity theory based tradition..

The claim that a particular macroeconomic relationship was both simple and empirically stable, was not new in 1956. Keynes had made it on behalf of the consumption function twenty years earlier, and by the 1950s, a stable marginal propensity to save out of current income, and hence a stable multiplier, was seen as the *sine qua non* for successful fiscal policy. Friedman’s 1957 *Theory of the Consumption Function*, (for which he received his Nobel Prize) showed that the, by then numerous and well known, anomalies in observed income consumption relationships could be resolved by postulating that consumption (of non-durable goods) was a stable function

of *permanent* income, so that saving would be an unstable sum of the fraction of permanent income saved and transitory fluctuations around it. Thus, Friedman's work of the 1950s replaced a supposedly empirically stable savings function with a stable demand for money function, thereby undermining conventional policy wisdom which emphasised the efficacy of fiscal rather than monetary tools. And to this he added a keen appreciation of the possibility that policy in general, but monetary policy in particular, worked with long and variable time lags, that made its use for active stabilisation purposes problematic.

By (1959), then, Friedman was suggesting that monetary policy be used to create a background of macroeconomic stability by being tied down to a constant rate of growth for the money supply, and suggesting that, in such a regime, there was no place for stabilisation by fiscal means. Small wonder that he recognised the relationship between his views and those espoused by Henry Simons two decades earlier, (though Simons (1936) had favoured a price stability rule) and small wonder too that, with the links between his policy proposals and a conservative political agenda being so evident, his work was greeted with skepticism and sometime outright ridicule in more orthodox circles.

But, it is important to note, Friedman's policy conclusions followed from well specified economic theory. And he soon extended the latter by showing (1968) that then widely held beliefs, that inflation and unemployment varied inversely with each other along a Phillips curve that might be exploited by policy makers, rested on the hidden postulate that economic agents suffered from permanent money illusion. Friedman also developed a considerable body of empirical evidence, not least that presented in Friedman and Schwartz's (1963) *Monetary History of the United States*, that argued for an essentially monetary explanation of the cycle in general and the Great Contraction of 1929-33 in particular. In the latter case, his arguments followed lines that a number of middle ground American economists of the early 1930s had adopted at the time. Politically unpalatable or not, then, the economics profession in due course had to take Friedman's views seriously. Because they involved economics that had political implications, rather than economics distorted by preconceived ideology, they invited theoretical and empirical criticism, and ultimately received it, in the course of the so-called "monetarist controversy".

The style of monetary thought that Friedman had established at Chicago by about 1970 had antecedents, of course. In addition to Simons, his analysis of money in the Great Contraction had important predecessors such as Clark Warburton (See 1966) and Lauchlin Currie (1934) (whom he did not acknowledge till much later). And Friedman had ideas in common with economists working elsewhere too. Karl Brunner and Allan Meltzer, first at UCLA, and later at Ohio State, Rochester and Carnegie-Mellon Universities respectively, had begun to publish the first of their own significant contributions (surveyed retrospectively in 1993) to the development of “monetarism” in the early 1960s; Franco Modigliani’s life cycle model of consumption was close to Friedman’s; A. W. Phillips had analysed the problems raised by lags in the effect of policy; while Edmund Phelps’ debunking of the idea of a long-run inflation unemployment trade-off had even appeared a little earlier than Friedman’s.

Moreover, Friedman was anything but a lone wolf in his own university. His theoretical and policy ideas were buttressed by a series of empirical studies, mainly carried out by students, many of whom would in due course go on to make considerable reputations of their own. It was also at about this time, specifically in 1960, that the formidable Harry Johnson was appointed to Chicago as a “Keynesian” counterweight to Friedman, and undertook the task of ensuring that students were exposed to the whole literature of monetary economics, and not just Chicago’s contribution to it.

But, when all is said and done, the Chicago monetary tradition of the late 1960s was more homogeneous, more distinctive, and more the product of the ideas and energy of one man than anything that had been seen in the inter-war years. And yet at that time it was also a tradition in search of an important policy problem (Johnson 1971). The quantity theory was primarily a theory of price level behaviour. Friedman and his associates had demonstrated its relevance to important inflationary episodes of the past, and to Latin America too, where Arnold Harberger’s (1963) paper on inflation in Chile made an important contribution to the “monetarist” side of the highly ideologically charged “monetarist-structuralist” debate that was then in progress. And even if monetary factors had played a major role in US monetary history, not least the 1930s, the relevance of all this to current US and European experience was not quite clear, not least

because, no matter what theory said, the latter economies did at that time seem to be characterised by an inflation-unemployment trade-off.

All this changed in the 1970s as the inflation which, with hindsight can be seen to have begun developing from the early 1960s onwards, took hold, of not just the United States but the whole Western World. The Chicago monetary tradition not only had its problem, but it was simultaneously presented with extra empirical support as the inflation-unemployment trade-off vanished. At this time too, it went through an further important theoretical extension, as the idea of a stable demand for money function was used by Harry Johnson, Robert Mundell (appointed to Chicago in 1964) and their students, as the foundation of what came to be known as the “monetary approach” to balance of payments and exchange rate analysis. (See, for example, Frenkel and Johnson (eds.) 1976)

By the end of the 1970s, “monetarism” had also found strong political supporters, notably among what would become the Reagan Administration in the United States and the Thatcher Government in the United Kingdom, though here, it should be noted that the first efforts to bring inflation down by way of monetary contraction started, albeit tentatively, in both countries before the accession of either to office. But it was under them that a monetary cure for inflation was vigorously applied, alongside a more general shift to laissez-faire economic policies. As a result, the political association between the Chicago monetary tradition and conservative politics, which had already received an unfortunate boost in Chile where, under the Pinochet regime, such policies had already become associated with military dictatorship, became firmly cemented in public perceptions.

### *After Monetarism*

There is not space here to discuss why the quantity theory of money, which had been associated with progressive politics when the University of Chicago was founded, had migrated to the right by the late 1970s. (See Laidler 2001). It is, however, important to the story of monetary economics at that University to note that problems with definition and measurement of money, long the Achilles heel of that doctrine, reasserted their significance with a vengeance at that time. Financial innovations, some but not all of which were themselves responses to inflation,

began to undermine the empirical stability of the demand for money function. On the policy front, this contributed to the difficulties that central banks encountered in ensuring a smooth end to inflation, and within academic monetary economics it contributed to an intellectual vacuum which was soon to be filled, not least at the University of Chicago, by New-classical economics.

Tobin's (1981) characterisation of it as "Monetarism Mark 2," notwithstanding, the latter doctrine's connection to Chicago was not as strong as that of Monetarism Mark 1. Three of its leading proponents in its early days, Robert E. Lucas Jr. Thomas Sargent and Robert J. Barro, held faculty positions at Chicago, but the latter two, both Harvard Ph.Ds did not settle there. And while Lucas and Neil Wallace were Chicago graduates, neither had written Ph.D. theses on monetary topics, while Lucas's earliest work on rational expectations and the cycle was done at Carnegie Mellon University, where he was certainly influenced by Allan Meltzer.

It is too early to assess the place of New classical economics in the history of monetary economics. In its earliest form, which had the Lucas (eg. 1972) Sargent and Wallace (eg. 1976) "money supply surprise" model of the cycle as its centrepiece, it had seemed to have a great deal in common with Friedman's work: hence the "Monetarism Mark 2" label. It attributed economic fluctuations to monetary shocks, denied the existence of a long-run inflation unemployment trade-off, and in ruling out as a theoretical, rather than merely empirical, matter, the effectiveness of discretionary stabilization policy, it affirmed its conservative political links. But crucially, the model's logical structure required that prices move simultaneously with, or even ahead of quantities after a monetary shock, and this was quite contrary to one of the most basic stylised facts of the cycle that Friedman had long stressed.

In the face of the money supply surprise model's inevitable empirical failure, New-classical economists clung not to their model's monetarist characteristics, but to the market-clearing and rational expectations postulates, the two features that they had added to Friedman's analysis, and took them in a number of directions - into real business cycle theory, monetary models based on Samuelson's over-lapping generations model of money, models of the origins of money, and even into endogenous growth theory. And at the same time, so called "New-Keynesian economics", which emphasises the role of market failures and nominal stickiness in

the monetary economy began to look a lot like the “old monetarist economics” of Friedman and Brunner and Meltzer. In short, though old debates in monetary economics carry on in new forms, and economists based at Chicago still contribute to them, it is hard indeed to identify a distinctively “Chicago Tradition” within the literature they are generating.

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