

2013

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Citation of this paper:

Laidler, David. "2013-5 Reassessing the Thesis of the Monetary History." Economic Policy Research Institute. EPRI Working Papers, 2013-5. London, ON: Department of Economics, University of Western Ontario (2013).

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Working Paper # 2013-5

October 2013

(Updated February, 2014 and October, 2014)



***Economic Policy Research Institute
EPRI Working Paper Series***

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Reassessing the Thesis of the *Monetary History**

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Abstract: The economic crisis that began in 2007 and still lingers has invited comparison with the Great Depression of the 1930s. It has also generated renewed interest in Milton Friedman and Anna Schwartz's explanation of the latter as mainly the consequence of the Fed's failure as a lender of last resort at its onset, and the ineptitude of its policies thereafter. This explanation is reassessed in the light of events since 2007, and it is argued that, in some important, but not all respects, its plausibility emerges enhanced, even though policy debates in recent years have paid more attention to interest rates and credit markets than to Friedman and Schwartz's key variable, the quantity of money

JEL Codes: B22, E32, E51, E58, N2

Key Words: Great Depression, Great Contraction, Great Recession, Keynesianism, Monetarism, Lender of Last Resort, Money, High-powered money, Monetary base, Currency, Bank reserves, Quantitative easing, Open-market operations.

*This paper is based on speaking notes prepared for a conference entitled *Retrospectives on the Great Depression*, held at Princeton University on Feb. 15-16, 2013. The influence of conversations and correspondence with Michael Belongia, James Boughton, Michael Bordo, Tim Congdon, David Glasner, Bob Hetzel, Jeff Hummel, Douglas Irwin, Jim MacGee, Allan Meltzer, Edward Nelson, Russ Roberts, Roger Sandilands, Scott Sumner and Lawrence White on its subject matter is gratefully acknowledged, as are subsequent extremely helpful written comments from Peter Howitt, Douglas Irwin, Allan Meltzer, Edward Nelson and Roger Sandilands on earlier drafts. Nevertheless, the views expressed herein are entirely the author's responsibility.

On November 28th 2008, barely two months after the collapse of Lehman Brothers, Paul Krugman told his *New York Times* readers that

"A central theme of Keynes's *General Theory* was the impotence of monetary policy in depression-type conditions. But Milton Friedman and Anna Schwartz in their magisterial monetary history of the United States [sic] claimed that the Fed could have prevented the Great Depression. . . .

[T]he Depression could have been prevented if the Fed had done more – if it had expanded the monetary base faster and done more to rescue banks in trouble.

And then he continued as follows

So here we are, facing a new crisis reminiscent of the 1930s. And this time the Fed has been spectacularly aggressive about expanding the monetary base: And guess what – it doesn't seem to be working

I think the thesis of the *Monetary History* has just taken a hit" (Krugman, 2008b)

Given that the monetarist model underlying this book and its thesis about the Depression held (and still does) that monetary policy works with long and variable time lags, Krugman was surely a little quick off the mark with this comment, but he was certainly to the politico-economic point in several dimensions.¹

By the time he was writing, a major financial crisis had been developing for more than a year, economic activity in many countries, not least in the U.S. itself, was already beginning to contract sharply, and the Fed had begun to react vigorously (along with the Treasury and Congress). Krugman was by no means the only one already beginning to see resemblances here to the onset of the Great Depression that had begun in the US with the contraction of 1929-33. Nor was he the only one to notice that the lessons economists thought they had learned from this earlier disaster were available to be deployed, and their validity thus put to a new test, as what would soon be labeled the "Great Recession" evolved. More than competing ideas about the proper conduct of counter-cyclical economic policy were at stake here, moreover. Profound and ideologically loaded questions about the capacity of the market economy to function smoothly without the benefit of constant attention from government, and hence about the appropriate political framework that should underpin macro-economic policy, were also getting renewed attention in 2008.

Keynesianism, Monetarism and the *Monetary History*

For two decades following World War 2, The Depression, and the Keynesian Revolution in economic thought that accompanied it, were widely held to have established the obvious truth of some rather specific answers to the above-mentioned questions, namely:

¹ By the end of 2008 Krugman had already been involved for over a year in a debate about Milton Friedman's contributions to economics, including, but my no means confined to, the *Monetary History*, with Edward Nelson and Anna Schwartz. This debate was precipitated by Krugman's not always accurate obituary of Friedman in the February 15 2007 issue of the *New York Review of Books*, and culminated in an exchange in the May 2008 issue of the *Journal of Monetary Economics* - See Nelson and Schwartz (2008a & b) and Krugman (2007, 2008a). Thus Krugman's views as quoted here had not been formulated on the spur of the moment. As Doug Irwin has stressed to me, Krugman has subsequently attacked Friedman's work on occasions too numerous to cite in detail. His most recent comment (Krugman August 8, 2013) bearing the title "Milton Friedman, Unperson" concludes that ". . .Friedman has vanished from the policy scene – so much so that I suspect that a few decades from now, historians of economic thought will regard him as little more than a footnote" Readers of this essay may form their own judgement.

that a largely unregulated economy had come catastrophically adrift in the early 1930s for reasons inherent in its very nature; that then available policy tools had proved powerless to stabilize it; and that a rather pervasive role for the state in economic life was necessary. Specific policy responses to this way of thinking, whose smooth arrival was both delayed and heavily conditioned by the war, differed considerably among countries, of course. In the Kennedy-Johnson era, the US was very different from, say, Sweden, or even the UK. Viewed from today's vantage point, however, macroeconomic policy almost everywhere in the 1960s did indeed seem to be conforming to essentially the same activist principles, which stressed the primacy of output and employment over price level goals and fiscal over monetary tools.

As the decade wore on, such policies began to generate inflationary problems, and a new economic doctrine – “Monetarism” – that claimed to have solutions to them began to emerge. Crucial to this essay, this doctrine also suggested that “Keynesian” policy orthodoxy was based upon pessimistic misconceptions about the self-regulating capacity of the market economy, not to mention a misunderstanding of what the evidence generated in the 1930s implied about this capacity. One of its defining texts, *A Monetary History of the United States 1867-1960* (Milton Friedman and Anna Schwartz, 1963a) – more specifically chapter 7 of this book, “The Great Contraction” – argued that the Depression had not resulted from an inherent flaw in the market economy, but was the consequence of inept monetary policy, which undermined this economy's own capacity to deal with what began in the late summer of 1929 as a routine cyclical downturn. This thesis supported Monetarism's more general insistence on “the inherent stability of the private sector” (see Thomas Mayer, 1978, p. 2), an empirical hypothesis that was in due course turned into an un-debatable axiom by the New-classical macroeconomic orthodoxy that succeeded Monetarism. The *Monetary History's* interpretation of the Depression thus played a pivotal role, albeit at one remove, in bringing about those shifts in economic policy towards reliance on un-(or de-)regulated markets that marked the three decades that began in the 1980s.

There is rich irony here. Robert Lucas long remained an admirer of the *Monetary History* (see 1994); but he would also note in (2004) that New-classical economics itself evolved during those same years along lines that left it unable to analyze episodes such as the Depression, thus implying that the interpretation of this event that the book had offered could no longer provide intellectual support for what by then had become the mainstream macroeconomics that he himself had done so much to create and promote. The Dynamic Stochastic General Equilibrium (DSGE) analysis that dominated the area when the Great Recession began would prove at best inadequate in the face of the intellectual challenges it presented, and it was small wonder that, even as early as 2008, so acute a political economist as Krugman would conclude that the *Monetary History's* by then old-fashioned thesis about the Depression and the alternative Keynesian interpretation of this episode were up for the rematch that is still in progress in 2013.

The *Monetary History's* importance was widely recognized from the outset. It drew no fewer than six major review articles upon publication, and, as Michael Bordo (1989), as well as Frank Steindl (1995), would later stress, it had staying power too. Three more review articles in the August 1994 *JME* marked its thirtieth anniversary; while its account of the causes of the Great Depression, just one episode in the century of monetary history it covered, having received a major boost from the publication of Karl

Brunner (ed.) (1981), would later serve as a starting point for, among others, Ben Bernanke (1983), Christina and David Romer (1989), Allan Meltzer (2003, Chs. 3 and 4), and Robert Hetzel (2008, Ch. 3, 2012, Chs. 3-5). Most recently, the fiftieth anniversary of the *Monetary History's* publication has been celebrated by a special session at the January 2013 meetings of the ASSA. where the papers did the book the honour of building upon, rather than simply reiterating, its themes – See Bordo and Rockoff (2013), Romer and Romer (2013) and Mitchener and Richardson (2013).²

To be sure, subsequent monetarist work on the Depression differed in some details from the story that the *Monetary History* had told. For example, Anna Schwartz (1981) writing alone in Brunner (ed.1981) was more definite in attributing the initial sharp downturn of 1929-30 to monetary impulses than Friedman and Schwartz had been. Or again, Meltzer (2003) and, following him, Hetzel (2008), placed more emphasis than had Friedman and Schwartz on faulty economic analysis within the Fed. - specifically the Riefler-Burgess view of the monetary policy process, and a neglect of the Fisher distinction between real and nominal interest rates - in explaining the intellectual sources of its policy errors. And I suspect that careful study would also show that, under the influence of Charles Kindleberger (1973) and Barry Eichengreen (1992), later accounts have usually paid more attention to the Depression's international dimension, and rightly so to the extent that this episode was far from being a uniquely American event.³ But these modifications have left largely intact the basic thesis of the *Monetary History* about the performance of the US economy itself in the '30s: namely, that a collapse of the US money supply between 1929 and 1933, precipitated by inept Fed policy, was the principal immediate cause of the extraordinarily severe economic contraction that the country experienced in those years, from which its subsequent recovery was so painfully drawn-out.

It is true that, recently, a younger generation of macro-theorists, writing in the New-classical tradition, have been inclined to take the *Monetary History's* story less seriously. For example Harold Cole and Lee Ohanian (2007) accorded monetary impulses a somewhat naïve and even perfunctory treatment in an otherwise extremely technically sophisticated investigation of the 1930s. Here the main goal was to assess the explanatory power of real business cycle theory for those years, and hence the likelihood that the economy's behavior represented an equilibrium response to productivity shocks and the

² Some of the *Monetary History's* durability and capacity to withstand close scrutiny surely stems from the fact that it was only one, albeit the most important, product of a research program that had begun in the late 1940s, and did not really come to an end until the publication of Friedman and Schwartz's *Monetary Trends* . . in (1982). Centered on the NBER, but also involving Friedman's "Money Workshop" at Chicago, this program's meticulously checked and edited output included, in addition to other books - two of which Cagan (1965) and Morrison (1966) - will be discussed below, technical papers and journal articles sometimes later incorporated in those books, not to mention unpublished Ph.D dissertations, also sometimes later published as books. In this highly collaborative enterprise, ideas and data sets moved freely among contributors, and as we shall see, publication dates give little guidance to scholarly priorities.

³ David Glasner (2013) argues that the *Monetary History's* relative neglect of the botched attempts to restore the international gold standard after World War I, which set much of the scene for the Great Depression's international dimension in the '30s, is an extremely serious flaw. But the book's focus was after all the United States, and, never having given up the gold standard in the first place, that country's main contributions to international monetary instability in the 1920s stemmed from the fact that, with very few exceptions, its monetary policy was kept insulated from foreign influences and aimed at domestic goals.

like in the face of institutional rigidities, not least in the labour market. But this approach to analyzing the Depression, which Christiano, Motto and Rostagno (2004) extended to incorporate the effects of a money-demand shock that policy had failed to offset, hence bringing it into somewhat closer contact with the thesis of the *Monetary History*, is very much a work in progress. The *Monetary History* is, in short, still respected and influential, and questions about whether events since 2007 have indeed inflicted serious damage on its interpretation of the Depression require serious attention.

The *Monetary History*'s Thesis about the Great Contraction

This interpretation boils down to the proposition that the collapse on the US economy between 1929 and 1933 was a disaster that the Fed could have prevented, and, as already noted, a particular monetarist view of the nature of economic fluctuations in general underlies it. This in turn may be summarized in three propositions. First, the main influence on the course of the business cycle is the rate of growth of the quantity of money in circulation; when this rate of growth slows down, it usually, but with a significant time lag, brings on a downturn, and when it speeds up, an upturn. Second, the amplitude of cyclical fluctuations in nominal national income's growth rate tends to vary with the magnitude of preceding changes in money growth, with variations in its real components on the whole preceding those in rates of change of prices.⁴ Third, the transmission mechanism involved here (which was described in some detail, not in the *Monetary History* itself, but in a stand-alone paper on "Money and Business Cycles", Friedman and Schwartz 1963b) is not unidirectional; rather it has the potential to generate feed-backs from the course of economic activity to the subsequent behavior of money growth, in a recursive process with considerable capacity to amplify business fluctuations. The *Monetary History* deployed this framework in a systematic account of the progress of the US economy between 1867 and 1960, and analyzed the Great Contraction as a particular application of it in extreme circumstances rather than as a unique event, a fact that the later publication of its chapter 7 as a stand-alone volume (Friedman and Schwartz 1965, 2nd ed. 2008) has sometimes tended to obscure.

In this particular instance, the economy began to turn down in the late summer of 1929, and though the initial contraction, helped along by the financial uncertainty that followed the stock market crash of October 1929, was steep, it only began to turn into a collapse later in 1930. In that year, the rate of money growth continued to fall and became significantly and increasingly negative. Then, at the end of the year, what would prove to be the first of a series of banking crises began, and the destabilization of the monetary system gathered further momentum, only reaching its climax in the full-scale banking panic of 1933. According to Friedman and Schwartz, the cumulative collapse of 1929-33 could have been avoided by prompt and determined lender of last resort actions on the part of the Fed in 1930, and failing this, mitigated or even reversed by vigorous enough action at any later time.

In their interpretation of events, the money supply itself, whose rate of growth is the active driver of economic activity, was not of course treated as a variable directly controlled by the Fed. Rather it was linked to the variable that was (or could be), the monetary base, (aka. the stock of high-powered money), by way of a "money multiplier".

⁴ Note that the *Monetary History*'s analysis uses NBER chronology to date cyclical turning points rather than the behaviour of any single simple indicator such as nominal national income.

The multiplier's value in turn was seen to be the outcome of choices made both within the banking system about the ratio of the reserves of high powered money that were held against deposit liabilities to those liabilities, and among the non-bank public about the balance that they would maintain between currency and bank deposits in their overall money holdings.

One formulation of this relationship, where M (money) = C (currency held by the non-bank public + D (bank deposits), H (high powered, or base, money) = $C + R$ (reserves), and r and c respectively are the two above mentioned ratios, may be written as follows:

$$M = (1 + c)/(r + c)H$$

In the first instance this equation is simply an accounting identity, but if the ratios, c and r , are indeed the outcome of systematic and predictable choices made by the relevant agents, it becomes a structural relationship linking the variable that matters for economic activity, the money stock, to the variable under the Fed's control, the stock of high-powered money.⁵ A crucial element in the *Monetary History's* thesis about the Great Contraction was, then, that these ratios, though anything but constant, did indeed remain the outcome of systematic choices made within the private sector throughout the episode, so that, to use Friedman's own words (in a 1986 letter to Frank Steindl and quoted by the latter twice, Steindl (1995) p. 58 & p. 75 fn.) "The Federal Reserve could, at all times have controlled the stock of money."⁶ It needed only to vary H , the stock of high-powered, or base, money, to offset the effects of changes in c and r .

In fact, in 1928-29 in the run-up to the depression, the Fed had kept the stock of high powered money essentially steady but then reduced it (or allowed it to fall) by close to 5 per cent in the twelve months following October 1929, the month of the stock market crash. It is true that, thereafter, high powered money slowly but steadily increased, but, according to the *Monetary History*, this gave only the appearance of actively expansionary policy. Increases in the public's holdings of currency more than absorbed the increase in question so that bank reserves actually declined, and over the October 1929 - October 1933 period as a whole, the (broad, M_2) money stock, the variable that mattered, contracted by more than 30 per cent. To put matters in terms of the money multiplier, this was a decline overwhelmingly driven by increases in the currency deposit and reserve deposit ratios.⁷

⁵ This is the version of the multiplier that this author always used when teaching this material. Cagan (1965) actually works with a slightly different formulation, in which the critical ratio describing the non-bank public's behaviour is that of currency held to total money holding, rather than deposits. Friedman and Schwartz (1963a, pp. 794-796, and Table B 1) mainly work with a version of this equation whose explicit parameters are the inverses of what we here call c and r , and they also explicitly discuss versions relevant to pure specie and pure fiduciary standards, and a mixed system in which the monetary authority's liabilities are related to its own specie reserve ratio. Nothing of substance hinges on these distinctions, but readers of this paper who also consult its sources need to be aware of them.

⁶ As Ed. Nelson, has pointed out to me, the relevant passage in this letter essentially paraphrases a longer expression of the same views that appears on p.693 of the *Monetary History* and in turn is quoted at length by Friedman in (1967). Evidently Friedman meant what he said here!

⁷ The relevant raw data are to be found in Friedman and Schwartz (1963a) Appendix A: Basic Tables, and Appendix B. Proximate Determinants of Money.

If these shifts are interpreted as the outcome of voluntary portfolio choices made by the relevant agents in the face of growing uncertainty about the banking system's viability, then the conclusion that they could and should have been offset by much larger increases in the stock of high powered money than in fact occurred follows. More specifically, had the Fed acted as an aggressive lender of last resort in 1930, as it should have done, then high powered money would have grown vigorously rather than contracted, the banking panics that began late in that year would not have happened, and the further shifts in r and c that these engendered would never have got under way. But given that this opportunity was missed, open market operations after 1930 could still have halted subsequent contraction of the money supply had the Fed pursued them with sufficient purpose. Even as late as mid-1932, when such operations were indeed undertaken for a few months, they showed signs of working according to Friedman and Schwartz, but were abandoned prematurely. Their opinion on these matters was shared by at least one contemporary commentator, namely Lauchlin Currie, the author of a (1934b) article entitled "The failure of monetary policy to prevent the Great Depression of 1929-32", who observed in another publication of that year,

"Much of the current belief in the powerlessness of the reserve banks appears to arise from a complete misreading of the monetary history of 1929-32. It is generally held that the reserve administration strove energetically to bring about more expansion throughout the depression but that contraction continued despite its efforts. Actually the reserve administration's policy was one of almost complete passivity and quiescence" (Lauchlin Currie, 1934a, p. 147)

On this reading of events, then, to quote Friedman and Schwartz's later and much quoted verdict: "The contraction [was] in fact a tragic testimonial to the importance of monetary forces" (1963a, p. 300).

Evidently Krugman disagrees, and we should not dismiss out of hand his belief (shared with many others) that the increases in the currency deposit and reserve deposit ratios that happened during the Great Contraction were simply the observed consequences of currency and reserves being allowed to pile up in portfolios, and that any further policy-induced increases in the stock of high powered money would have been passively absorbed there, with no effect on the money supply. There is, after all, support for this view as well in the writings of some notable contemporary observers. Paul Douglas (1933) for one argued that the 1932 experiment with open market operations had been futile: "The expectation was that the banks would be so choked with cash that they would have to increase their loans. . . . That did not happen, because in a period of depression businessmen are afraid to borrow and banks are afraid to lend" (1933, p. 10); while to John Maynard Keynes (1936), this same episode presented the only evidence for the possible operation of an actual "liquidity trap" to which he was willing to give even grudging credence. (see 1936, pp. 207-208)⁸

⁸ This phrase, which was coined not by Keynes but by Dennis Robertson (1940), is used indiscriminately nowadays to refer to a variety of phenomena that might prevent expansion of the monetary base, and/or the supply of money itself, affecting aggregate demand. For discussions of these issues, which are by no means merely semantic, see Ingo Barenz (2012) and Roger Sandilands (2010)

Evidence from Other Episodes

The thesis which troubled Krugman in 2008 involves not only what actually did happen during the Great Contraction, but also beliefs about what would have happened had the Fed's policy been different. To assess its plausibility, then, the credibility of this counterfactual element needs to be examined, and this rests on a much wider range of experience than that of 1929-33, or even 1929-39. As Bordo and Rockoff (2013) have recently stressed: "One set of natural experiments, however suggestive, cannot prove that money matters; the proof comes from the weight of all the evidence. That evidence includes some experiments that fell within the period 1929-39, but many more that fell outside it." (p. 7)

Hugh Rockoff (2010) has stressed that the *Monetary History's* longer story rests on a "statistical underpinning" published in Phillip Cagan's (1965) study of *Determinants and Effects of Changes in the Stock of Money 1875-1960*, which had been available to Friedman and Schwartz long before it saw print, as Friedman (1965) himself acknowledged in his Foreword to this book. The *Monetary History's* account of 1929-33 derives a good deal of its plausibility from this underpinning, particularly its finding that, viewed in historical perspective, there was nothing unusual in the behavior of either the currency deposit or the reserve deposit ratio during those years. To use Cagan's own words, "All sudden large increases in the currency-money ratio during peacetime have reflected banking panics, stemming from expectations that banks might suspend payments" (p.139) and "The growing distress of banks in the three years preceding the 1933 holiday had the same effect [as had actual financial panics in earlier cycles] of drastically increasing the demand for currency" (p. 265). As to the reserve deposit ratio "The usable reserve ratio in the 1929-33 contraction did not rise to unusually high levels, and from 1933 to 36 it went no higher than after panics or in depressions before 1914. . . [U]sable reserves were . . . low in the early 1930s, partly in response to the supposed security provided by the Federal Reserve Banks" and though they rose during this episode, "in the historical perspective . . . the large ascent of usable reserves from 1929 to 1933 does not seem extraordinary for such a period" (p. 211)⁹ In short, viewed in the context of previously observed regularities, increases in the currency-deposit and reserve ratios during the Great Contraction brought no surprises, and hence, like similar behavior in other episodes, could reasonably be interpreted as the outcome of systematic portfolio choices made by the relevant agents. And if this was indeed so, then the effects of these increases on the money supply could have been offset or even reversed by a sufficiently large expansion of the stock of high powered money. This observation, though striking, does not settle matters definitively, and even evidence drawn from the later years of the Depression -1934-39 - though much of it points in the same direction, is also perhaps not quite decisive, as we shall now see.

After the bank holiday of 1933 and the revaluation of gold that soon followed, the stock of high powered money in the US began to grow rapidly, the currency deposit ratio

⁹ The reserve ratio of course had a component whose value was determined by reserve requirements and the composition of the stock of deposits against which these had to be held, and a component, held at the discretion of the banks, in excess of this. Excess reserves, minus the amount of them borrowed from the Fed, are usually referred to as free reserves, while Cagan's "usable" reserves variable, the denominator of the fraction "r" above, is equal to total minus required reserves plus till cash held by the banks. Nothing in the current discussion hinges on these niceties, but it helps to have them in mind when consulting its sources.

at first fell and then stabilized in the wake of the introduction of deposit insurance, but the reserve deposit ratio continued to increase. Money growth nevertheless did become systematically positive, and the economy began a significant recovery, though it failed to return to full employment before the next downturn in 1937. One possible but discomfiting explanation of these last two facts is implicit in a well-known simile, acknowledged by Cagan: namely that “The failure of monetary expansion after 1933 to achieve full employment by 1937 has been likened to the futile gesture of pushing on a limp string” (Cagan, p. 287). Or, to put it more directly and specifically, that the faster deposit growth needed to bring about a full recovery was impossible because of a lack of willing business borrowers among the banks’ customers. This explanation of events after 1933 invites speculation that a similar tendency might also have been at work earlier, as Paul Douglas among many others had suggested at the time, and as of course Krugman suggested in (2008). In short, the steady growth of the reserve ratio between 1933 and 1940 not only raises uncomfortable questions about those years, but also challenges the credibility of the *Monetary History*’s thesis about the Great Contraction of 1929-33.

Being well aware of this, Cagan took pains to address the issue along lines that Friedman and Schwartz also followed. To state his conclusion at the outset, he argued that the correct analogy for “the failure of monetary policy to achieve full employment by 1937” was not the “futile gesture of pushing on a limp string; but . . . pushing on a taut coil spring, which compresses – but not indefinitely” (p. 268) Cagan thus contended, himself following a then unpublished (1962) study of the issue by George Morrison (but see Morrison 1966, Ch. 4), that the slow but steady growth of the ratio of usable reserves to deposits from 1933 to 1940 reflected not a passive build-up of idle cash within the banking system, but systematic, albeit lagged, choices driven by “continuing apprehension instilled by the 1933 panic, and by the business contraction from May 1937 to June 1938” (Cagan 1965, p. 200).¹⁰ Cagan also singled out a significant detail in the relevant data to support this explanation: namely, that the build-up of useable reserves was less pronounced among smaller country banks whose businesses were mainly retail, and whose customers were protected by deposit insurance after 1934, and hence less prone to make sudden large withdrawals of funds, and more pronounced among larger Reserve City institutions more heavily engaged in inter-bank business. As Cagan remarked, citing Morrison, “Banks that owe large amounts to other banks may adopt special rules for safe operation, especially in a period like the five years following 1933.” (p. 200).

Cagan, again echoing Morrison, also drew explicit attention to the response of Federal Reserve member banks to the three-step doubling of reserve requirements in 1936-37, a precautionary measure by a Fed that feared that the large build-up in usable reserves over the preceding three years had put its capacity to control monetary conditions at risk should there be a sudden and potentially inflationary increase in bank lending. If those useable reserves had in fact been sitting idly in banks which had no well-calculated desire to hold them, then re-designating them as required should have had no further consequences. But in fact, as Cagan’s data show, the usable reserve ratio of member banks, having fallen from 14.6 in 1936, to 4.4 in 1937 as a result of this re-designation, promptly rose to 19.0 in 1938. This time pattern not only suggests that

¹⁰ Morrison’s study of *Liquidity Preferences of Commercial Banks* in turn built upon pioneering analyses by A.J Brown (1938) and R. M. Goodwin (1941)

useable reserves were indeed being held for consciously precautionary purposes in 1936 but also that this motivation was strengthened by the downturn of 1937-38. Clearly, and as Friedman himself explicitly noted in the letter to Frank Steindl cited earlier, this interpretation of the behaviour of reserves in 1936-38 supports the contention that, at all times during the Great Depression, including 1929-33, the Fed could indeed have controlled the money supply. Furthermore, it carries weight independently of how one views a further and more debatable element in the *Monetary History's* interpretation of those years: namely, that the contraction in the money supply that followed the doubling of reserve requirements (and the simultaneous efforts on the part of the Treasury to sterilize gold inflows on which Douglas Irwin has recently refocused attention) was itself responsible for the sharp recession of 1937-38.

The trouble with this last contention is that it stresses just one of the two available and plausible explanations of the contraction in question. By 1936, Lauchlin Currie, who never recanted his 1934 interpretation of the period 1929-1932 cited earlier, occupied a senior position within the Fed., and having come to believe that the build-up of usable reserves after 1933 meant that the authorities were in danger of losing control of the money supply, was one of the architects of the abovementioned doubling of reserve requirements.¹¹ Even though the money supply did indeed contract in its wake, with help from sterilization measures, Currie, like many others would nevertheless attribute the down-turn of 1937-38, not to monetary impulses, but to a more or less simultaneous and inadvertent tightening of fiscal policy, mainly brought about by the introduction of the Social Security program and the end of the payment of Veterans' Bonuses, and this remains the standard "Keynesian" interpretation. Though Meltzer's (2003, pp. 493-394) exemplary presentation of the relative merits of these competing views about this episode comes down on the monetarist side, it remain one about which reasonable observers can and still do disagree.

Furthermore, there is also surely room for a degree of skepticism about whether banks were still building up their reserves in the late 1930s in response to shocks they had experienced in the panic of 1933. This hypothesis certainly merits the serious attention that Cagan and Friedman and Schwartz accorded it, but the length of the time lags on which it relies, which derive from Morrison's work, strain credulity a little, especially given the risks he took in allowing his data to participate in choosing that length. The pitfalls inherent in the econometric deployment of free parameters are much more apparent today than they were in the 1960s. And finally, rapid though it was by historical standards, the failure of the 1934-37 expansion that preceded this downturn to restore full employment needs to be squared with the monetarist view of a reliably self-stabilizing economic system recovering from earlier monetary policy errors. Earlier exponents of that view invoked, not implausibly, the adverse effects of other policy measures associated with the New Deal on the efficiency with which markets, particularly the labour market, worked, and on the accuracy of the data they generated. Michael Darby

¹¹ On Currie's role in this episode, and his views on it see Roger Sandilands, (1990) pp. 87-92. Sandilands tells me, however, in private correspondence (October 2013), that Currie later expressed doubts about whether he would have supported such measures had he been aware of the scale of the inadvertent tightening of fiscal policy that was about to occur. It is worth noting that Currie's initial judgement on this issue was formed before the work of Brown (1938) and Goodwin (1941), who was heavily influenced by Currie (1934), had formulated banks' attitudes to holding reserves in excess of requirements as a portfolio choice conditioned by perceived risks.

(1976) can here serve as an example and reminder of this earlier work, whose contentions are now, as has been noted, receiving renewed attention in real business cycle based literature on the Depression.

In short, when it comes to providing support for the *Monetary History's* thesis about the causes of 1929-33 Great Contraction, the evidence of 1934-39, like that drawn from cycles before 1914, though extremely suggestive, cannot be definitively convincing.¹² The simple fact is that historical parallels are never perfect enough for their deployment to carry the weight of replicative experiments, so a counterfactual hypothesis about a particular historical event can at best be established as more or less plausible by reference to other episodes. Even before 2008, then, there was always room for reasonable doubt and debate about this thesis.

The *Monetary History's* Thesis in light of the Great Recession

The Great Recession has had much in common with the Great Depression. The years preceding both events saw asset market booms - in the stock market in the earlier case and the housing market in the later; both booms were supported by innovations in financial markets – income trusts and the like in the 1920s, and securitized mortgages, credit default swaps and what have you in the 2000s; and both booms eventually prompted monetary tightening that brought them to abrupt ends, precipitating real downturns that were strikingly similar in both pattern and magnitude during their first year. This last similarity marked not only the US, but other economies linked to it as well - as Barry Eichengreen and Kevin O'Rourke (see e.g 2010) would quickly document. And finally, the Fed's actions in both instances were initially hesitant, even inconsistent, thus creating much confusion.

Even so, the similarities end here, because in 2008, the Fed abruptly seemed to remember the famous confession and promise that Chairman Bernanke had made to Milton Friedman on his 90th birthday: namely, that it had indeed caused the Great Depression and would not do it again. The failure of Lehman Brothers in September 2008 was therefore not only the first, but also the last, major institutional collapse it permitted, rather than the first of a sequence as the 1930 closing of the Bank of United States had been. The Fed's subsequent collaboration with the Treasury in the rescue of AIG, an insurance company and a major seller of Credit Default Swaps, was unprecedented, but among other things it very likely prevented a repeat performance in Europe of the kind of crisis associated with the 1931 failure of Credit Anstalt in the wake of the withdrawal of US lending to Europe.¹³ Moreover, the Fed also operated in close co-operation with the

¹² Even so, it should be noted that Tim Congdon (See e.g. 2010) has long contended that, quite apart from its amount, how high powered money is injected into the system can matter for the effectiveness of such policy. In circumstances where injecting it into the banking system encounters a "narrow liquidity trap" in his vocabulary – "a limp string" in Cagan's - and has no effect on bank lending and hence the money supply, direct dealing with the non-bank public can still increase the latter variable. In this case the money multiplier's value is zero when high powered money is injected into the banking system and unity when the authorities deal with the non-bank public. Congdon has told me in private correspondence that Friedman never accepted this contention despite a lengthy correspondence between them on the issue, but it does appear to be valid, and hence blunts the apparently crucial significance of the behaviour of the banks' reserve ratio in the debate about how much room for manoeuvre the Fed had after 1933, or indeed has in any situation where a "narrow liquidity trap" might exist.

¹³ Europe nevertheless failed to take full advantage of the opportunity thus created to repair its fragile banking system, with consequences that are still too evident to require further comment here. To follow this

Treasury as the Federal Government became a major shareholder in large banks and mortgage providers, not to mention automobile producers. Then, having cut its policy interest rate essentially to zero, the Fed followed up its extensive and highly innovative lender of last resort actions by entering financial markets on a massive scale in two programs, eventually to be followed by a third open-ended one that began in 2012, of “quantitative easing” - simply a new name for open market operations of the type that it had been so hesitant to deploy in 1930-33, despite the urging of such highly visible contemporary observers as Keynes (1930, 1931) and Hawtrey (1932).

Overall, as a result of these measures, and contrary to Krugman’s (2008) musings to the contrary, a major component the *Monetary History*’s thesis about the role of money in the Great Depression has stood up to the experience of the last few years rather well. It is a simple fact that, in the wake of the Fed’s aggressive response to events after the fall of 2008, the money supply, whether measured narrowly or broadly, did not collapse as it did after 1929, nor did the price level. And the extremely sharp fall in output and employment that marked the contraction’s first year stabilized and even began to reverse itself thereafter. On the other hand, though the expansion of high powered money was enormous, with this aggregate more than doubling in the year immediately following the collapse of Lehman Brothers, money growth itself as measured by the M2 aggregate remained sluggish, as did the more transactions oriented MZM aggregate after an initial burst of expansion. Some commentators, most recently, for example, Peter Ireland (2014) have plausibly suggested that this probably has had a lot to do with the Fed’s policy of paying interest on reserves, a practice that has no 1930s precedent. Be that as it may, According to some commentators – e.g. Hetzel (2012) – slow money growth offers a straightforward and not implausible monetarist explanation of the economy’s subsequent, and until recently, miserable performance, and could have been remedied by even more vigorous and single minded action of the Fed’s part.

Even so, though the currency deposit ratio remained relatively stable – surely the effect of deposit insurance, first introduced in 1934, and actually extended and strengthened in 2009 - the huge rise in the reserve deposit ratio combined with sluggish money growth for a while began to lend new credibility to that old “pushing on a limp string” argument about the effectiveness of monetary policy in depressed times, the new practice of paying interest on reserves notwithstanding. But only for a while, because money growth did begin to gather steam in 2011, and has continued at a modestly reasonable pace ever since, making Cagan’s alternative analogy - “pushing on a taut coil spring” - more and more plausible as each month has passed. The open ended nature of the QE3 program introduced in the fall of 2012, after the spring in question had already begun to uncoil be it noted, will in due course give us a firmer test of this conjecture, which is essentially similar to that expressed by Hawtrey in (1932): "There must ultimately be a limit to the amount of money that the sellers [of long term securities to the central bank] will hold idle" (Ralph Hawtrey 1932, p. 17). If this test is indeed passed, then the *Monetary History*’s thesis that the Fed could have prevented the Great

tragic story further, even in outline, would take the current paper too far away from its main topic to be practicable. See however, Hetzel (2013)

Depression if only it had tried hard and long enough, will emerge from recent experience not weakened, as Krugman suggested it would be, but strengthened.¹⁴

Money, Credit and Interest Rates in Today's Macroeconomics

But more than the right outcome to this test will be required. The economic theory underlying this thesis gives a strategic role to the rate of money growth in driving economic fluctuations and therefore in the conduct of monetary policy as well. Continuing interest in the *Monetary History* itself notwithstanding, this element in its analysis had fallen into neglect long before 2007 and remained so afterwards. Though the Fed's rapid reduction of policy interest rates to their practical lower bound and its vigorous expansion of its balance sheet after 2008 may indeed have first prevented a collapse of money growth, and then promoted its renewal, along exactly the lines that an attentive reader of Friedman and Schwartz on the onset of the Great Contraction might have hoped for, this is not how the policy in question was either promoted or usually discussed.

As Jeffrey Hummel (2012) has documented, much stress was laid by the Fed on the need to restore and/or maintain credit markets in working order, but very little if any on supporting the money supply. Clearly, and hardly surprisingly, as Hummel also emphasizes, the immediate inspiration in academic work for this stems not from the *Monetary History*, but from papers by Chairman Bernanke himself on the Great Depression, and, more generally, on the place of credit markets in the transmission of monetary impulses. (See, e.g., Bernanke 1983, and Bernanke and Gertler 1995) Much more surprising, however, was the reaction to Fed policies of several prominent monetarists, who might have been expected to take their lead from the *Monetary History*. Rather than welcome those policies as doing essentially the right thing, albeit for reasons not quite central to the monetarist story, these commentators, notably Allan Meltzer (2009) and for a while at the onset of the crisis Thomas Humphrey (2010) and the late Anna Schwartz (2009) herself, were among the Fed's most strident critics, warning of impending inflation even while money growth remained extremely subdued and the real economy stagnant.

The simple fact is that neglect of the money supply in recent discourse about the Great Recession on all sides has been, if not total, then nevertheless so pervasive that Edward Nelson (2013) was surely right to characterize this all important variable as "an example of a dog that did not bark" (p. 25) as the episode unfolded. I have discussed the reasons for this error, in much need of correction and which lie in the history of macroeconomics in the wake of the so-called New-classical revolution, in Laidler (2013), and they are too complex to discuss here. Suffice it to recall only that, by the late 1990s, the dominant theory of monetary policy had come to focus on the central bank's control over interest rates rather than monetary - or indeed credit - aggregates of any description, and was supported by equilibrium macroeconomic models whose account of the transmission mechanism completely by-passed the institutional complications presented by the monetary and financial system. Given that this approach nevertheless made the control of inflation the centerpiece of monetary policy, and denied the existence of a

¹⁴ Japan's recent determined redeployment of an open ended program of quantitative easing will provide a further test here. Its earlier experiment, that began in 2001 and ended in 2003 was surely too short-lived to provide an adequate test, particularly if the "coiled spring" analogy is taken seriously

long-run inflation unemployment tradeoff, it is easy to see not only how it came to be called “monetarism without money”, but also how it was attractive to some of those who had previously stressed the importance of money in the face of the Keynesian orthodoxy of the 1960s. For example, Anna Schwartz’s preface to the new (2008) edition of the stand-alone *Great Contraction*, (presumably conceived and written before the nature and extent of the recent crisis could have become apparent) gives the impression that she was reasonably comfortable with this new doctrine’s emphasis on interest rates. And her later (e.g. 2009) criticisms of the Fed’s policy follow John Taylor (see 2011 for a systematic statement of his often repeated critique) in focusing on the alleged error of keeping interest rates too low for too long in the wake of the earlier “dot com” bubble, and actually fail to note that money growth gave no signals of undue monetary ease during this period.

For his own part, Chairman Bernanke answered such criticisms by arguing that interest rate settings were indeed appropriate in the light of available evidence on inflation at the time, not by arguing the relevance of money growth to such decisions. And subsequently, when explaining the aim of “quantitative easing” to a sometimes skeptical public, he and his colleagues invariably emphasized the importance of putting downward pressure on long term interest rates, not upward pressure on money growth. In this respect, they argued not along the lines followed by Hawtrey (1932) and Currie (1934a) and revived by the *Monetary History*, but rather those taken by the Keynes of the *Treatise on Money* (1930), and the (1931) Chicago Lectures, thus opening themselves to criticism from exponents of Keynes’s very different later views on these matters, based on various versions of the “liquidity trap” doctrine. Krugman for example, has not opposed Fed policies since 2008. He has treated them as a futile and unimportant distraction, and in particular as no substitute for fiscal stimulus.

The Fed as Lender of Last Resort in the Great Recession

Some of the Fed’s monetarist critics, on the other hand, characterized those same policies as downright destructive, particularly their lender of last resort component. Their bill of particulars included the following: that the Fed had exceeded its responsibilities in this role by rescuing insolvent investment banks, mortgage lenders, insurance companies and the like instead of limiting itself to providing liquidity to otherwise solvent commercial banks; that by co-operating with the Treasury in such activities, it had surrendered its policy-making independence, and, as has already been mentioned, that the massive increase in the size of its balance sheet that resulted from these actions, not to mention subsequent quantitative easing, carried with it a serious inflationary threat.

Now there can be no doubt that the Fed did indeed grossly violate those text-book principles for lender of last resort activities that are usually attributed to Walter Bagehot (1873), namely that these should be limited to lending freely at a “high” interest rate to all solvent borrowers. Thomas Humphrey (2010, 2012) documents this case thoroughly and accurately. But, as I argued in Laidler (2003, 2004), behind such specific rules as these, there has always lain a more general, though less precisely formulated, principle, which has informed the theory of central banking since Henry Thornton (1802): namely that the central bank, being the entity whose liabilities are used among the institutions making up the monetary and financial system to meet what Hyman Minsky (1982) usefully labeled their survival constraints, has a public obligation to do whatever is

necessary to keep that system functioning. Bagehot devised his rules for a system much simpler than that of 2008, where the Bank of England's over-riding task was the preservation of the gold convertibility of the currency, and in which the principles of limited liability had not yet been generally adopted by banks, and they were no doubt well suited to it.¹⁵ But whether they would also have served the Fed well in dealing with the situation that confronted it in the much more complex financial system that existed at the onset of the current crisis is an altogether different matter.

To begin with, in Bagehot's scheme of things, the main purpose of that "high" interest rate was to forestall convertibility problems by generating a gold inflow when market uncertainty was provoking an outflow. He was well aware of the fact that, in times of financial crisis, what was known as "the internal drain" – the tendency of domestic depositors to withdraw funds from the banks – and the "external drain" – an adverse balance of payments, needed opposite remedies, the one expansionary and the other contractionary. With the US on a well-established flexible exchange rate regime in 2008, only the internal drain mattered and there was no role for a high interest rate to play, so a very low rate was an appropriate part of its response to internal matters. Furthermore, the line between liquidity and solvency has a very different significance when banks are limited liability companies and owners no longer have an obligation to assume their losses to the bitter end as so many of them did in Britain in the 1870s. In Bagehot's time recapitalization of most banks would in this sense be automatic, up to the point of owners' personal bankruptcy, and the central bank did not have to worry about it in the way it would have to in 2008. More generally, as Nelson (2013) has noted,

"In a modern context, a broad conception of the financial institutions that require support in an emergency is consistent even from a viewpoint, such as Friedman's, that sees maintenance of commercial bank deposits as paramount. Because of counterparty relationships, contraction of a major part of the financial system is likely to have repercussions for commercial banks' balance sheet positions and thereby aggregates like M2" (p.24)

From a viewpoint such as Bernanke's – and incidentally Bagehot's as well – in which maintaining those counter-party relationships in working order is also valuable in its own right, this argument is even more compelling.¹⁶ In particular it makes it very difficult to fault the Fed's own broad interpretation of its responsibilities after the collapse of Lehman Brothers, which led it, in co-operation with the Treasury, to become not just the lender, but, in Perry Mehrling's (2011) felicitous phrase, "the dealer of last resort" to the US, and even in some measure, the international, financial system.

And there are also ample historical precedents for close co-operation between the central bank and political authorities in times of crisis. To refer only to the British experience with which Bagehot was familiar, in 1797, the Bank of England required government authority to suspend gold convertibility; while in 1847, 1857 and 1866, only

¹⁵ The evolution of the liability regimes of British banks in the 19th century is a complicated business. It will suffice to note here that in 1869 a majority of banks (89 out of 136) operated under unlimited liability arrangements. A general shift to limited liability took place after 1879. For this and many other details see Graeme Acheson, Charles Hickson and John Turner (2010),

¹⁶ It was Henry Thornton (1802), not Bagehot, who stressed support of the money supply (broadly defined) as a major goal of lender of last resort activities. The "money market" that *Lombard Street* describes is the market for short term credit, including interbank credit, not that for currency plus deposits. See Laidler (2003)

the government was in a position to relax the reserve requirements against the note issue which were creating so many unnecessary difficulties for the Bank as it coped with the crises of those years. And in 1890, Viscount Goschen, the Chancellor of the Exchequer was an important player in the Bank's successful efforts to organize the rescue of Baring Brothers – an investment bank, incidentally, not a commercial bank – as a means of averting a financial system collapse.¹⁷

As to all those warnings about the inflationary threat implicit in the growth of the Fed's balance sheet during 2009 and after, it is surely a key element, not only of the *Monetary History's* specific thesis about the Great Contraction, but also of monetarist doctrine in general, that the critical monetary aggregate is some appropriate measure of the money supply and not the monetary base. It is hard to believe that some monetarist critics of recent Fed policy, not least that institution's historian, Allan Meltzer forgot this, but apparently they did, if only for a while.¹⁸ This is not to deny that, as the US economy continues its recovery, tensions generated by the “coil spring” of useable reserves still sitting within the monetary system might generate problems, but this is a very different proposition from the suggestion that injecting those reserves in the first place, thus preventing a collapse of the money stock, was an error. To the extent that the experience of 1936-39 yields lessons about how matters are likely to evolve from now on, moreover, it warns more clearly of the dangers inherent in premature monetary (and perhaps fiscal) restraint, than of any chances of imminent inflation.

Conclusions

All in all, then, and contrary to Krugman's (2008) speculations, the *Monetary History's* thesis about the importance of monetary policy in general, and the behavior of the quantity of money in particular, during the Great Contraction has emerged stronger rather than weaker from recent US experience. In 2007-08, the Fed was initially slow and even inconsistent in coming to grips with a developing crisis, but when finally faced with the paralysis of financial markets that followed the collapse of Lehman Brothers, its reactions were swift, vigorous and innovative. Though output continued to contract sharply for a year, neither the money supply nor the price level collapsed and a slow but painful recovery began. In short, the story of 2007-2010 began like that of 1929-33, but then evolved along the lines of the *Monetary History's* counterfactual speculations about what could have happened in this earlier episode had monetary policy been different.

And the *Monetary History's* account of 1934-39, always a somewhat less plausible part of its story about the Great Depression as a whole, also looks a bit stronger in the light of recent experience, particularly since 2011. Both episodes saw a continued build up of useable reserves in the banking system, and widespread suspicions first that monetary policy was “pushing on a limp spring”, second that inflationary dangers were building up for some unspecified future date, and third that when these materialized, the

¹⁷ See Leslie Presnell (1968). Incidentally, there was a strong similarity between the Bank of England's handling of this crisis, which cemented its reputation as a reliable lender of last resort, and the Fed's of the near collapse of Long Term Capital Management in 1998. I do not recall the Fed being criticised for coming to the aid of a hedge fund at that time.

¹⁸ Thus, in (2012) Humphrey does not repeat the warnings of imminent inflation that he gave in (2010), while in (2013) Meltzer pays considerable attention to money growth, as opposed to base growth, and his qualms about the latter are explicitly linked to inflationary difficulties that this might cause in a less than immediate future.

Fed would already have lost control of events. In 1936-7, the Fed reacted to such fears by doubling reserve requirements, and if the subsequent economic downturn cannot definitively be blamed on these actions, the behavior of the banks in promptly restoring useable reserve ratios to their former levels (and even beyond) nevertheless suggests that their reserves were being held with a purpose, not simply being idly hoarded. This time around, the Fed exercised more patience, and the economy's recovery eventually began to gather pace in 2012 in wake of a revival of bank-lending and money growth. The Morrison-Cagan-*Monetary History* story of a lagged response of the demand for reserves to financial market panic after 1933 surely looks more plausible, not to mention generally applicable, in the light of this experience, whatever qualms may remain about the length of the lags needed to square it with the facts in both episodes.

Even so, there has surely not been enough here to convert Paul Krugman to Monetarism. The Fed's efforts after 2008 perhaps worked as the *Monetary History's* thesis implied they would, but they did so in concert with considerable fiscal stimulus. Traditional Keynesianism, which lamented the absence of the latter in the early years of the Depression, can surely find support from this fact.¹⁹ As with the causes of the downturn of 1937-38, those of the current recovery are going to be very difficult to allocate between monetary and fiscal measures. And when evidence from Europe is brought to bear on the current rematch between Monetarist and Keynesian views of macroeconomic policy, it will be hard for even the most dedicated monetarist to deny a special role to fiscal restraint (if that is a strong enough word) in causing the collapse of all those economies along the Euro-zone's Southern and Western fringe, even if sluggish money growth can plausibly be allocated a good measure of blame for the stagnation of the area as a whole. (See Hetzel 2013)

To the extent that it concerns anti-depression policies, then, the Monetarist-Keynesian rematch shows little sign of coming to a quick – let alone decisive – end. And this conclusion is only strengthened if we consider how much uncertainty also surrounds the origins of the initial downturns of 1929-30 and 2008-09 in the US. Both came largely unexpectedly on the heels of a long period of macroeconomic stability, and were marked by considerable asset market turbulence. Keynesianism has always concentrated on explaining what determines the level of unemployment in a depressed economy and what to do about it. It has usually been content to attribute the initial downturn to an exogenous collapse of “animal spirits” among investors, but this is a *deus ex machina* whose role in the theory is to get the more interesting analysis going, not a testable explanation of anything. Monetarism has always had more that is testable to say about what precipitates cyclical downturns, attributing them to contractions in the rate of money growth coming at the end of booms characterized by rising money growth and inflationary pressures. It has also been able to point to the narrative of the *Monetary History* as a prime source of supporting evidence here.

But as it happens, the specific downturns of 1929-30 and 2008-09 do not fit the monetarist template that well. Both were preceded by a degree of price level stability that gave little (the Recession) or no (the Depression) warning of serious problems to come, nor in either case did money growth display the kind of clear-cut turnaround that one might expect to see in advance of such sharp and disruptive contractions. In the case of

¹⁹ Though the emerging fact that recovery has continued into 2013 despite significant fiscal tightening surely suggests that monetary impulses have been dominant here.

1929-30, as the *Monetary History* suggested, a monetarist interpretation can just about be supported by sufficiently careful scrutiny of earlier money growth data, provided that the subsequent downturn's severity is treated as an outlier. But no discernible slow-down in money growth foreshadowed the downturn that began in the US in 2008. In Laidler (2011), I argued that the onsets of both the Great Depression and the Great Recession are perhaps better explained by Austrian or Robertsonian ideas about the effects of increases in policy determined interest rates on already distorted and fragile markets for both financial and real assets.²⁰ Note, though that the main purpose of this earlier paper was to argue that the analytic lines routinely drawn in today's macroeconomics between the market on the one hand and the monetary and financial systems on the other are misleading, and that the "market economy" and the "monetary economy" are in fact one and the same thing. Thus, the foregoing conjecture about the origins of these downturns poses a challenge to monetarist beliefs about the inherent stability of such an economic system, regardless of what we might make of the role played by monetary policy in the events that followed.²¹

Be that as it may, though the old debates about the causes of the Great Depression and the lessons to be drawn not only about the conduct of policy, but more generally about the nature of market economies, may have been revived recently, they are no nearer to being settled than they ever were. However, given the neglect into which these debates had fallen among so many economists from the 1990s onwards, despite the continuing respect accorded to the *Monetary History*, the very fact that they are back on the discipline's current agenda is surely itself a sign of progress, and of the enduring importance of this great book.

²⁰ And note that just as exponents of such views gave advanced warnings before the crisis of 1929, so did they before 2007— notably researchers associated with the BIS such as Borio and White (e.g.2003)

²¹ I am particularly grateful to Peter Howitt for discussion of this issue.

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