Mark Blaug on the Quantity Theory: A Skirmish on the Border between Science and Ideology in the History of Economic Thought*

by

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Mark Blaug brought his usual standards of historical awareness and respect for empirical content to bear when he wrote about the Quantity Theory of Money, but he hesitated to probe too deeply into the political and ideological elements of its history, perhaps leading him to underestimate their importance for the theory's evolution.

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My Encounters with Mark

I first met Mark Blaug in the early 1960s in the UK, though I'm not sure exactly when or where. No matter: at that time, Mark was already a well established scholar of whose work I had been aware even as an undergraduate. He was thus something of an authority figure in my intellectual landscape, and also, as I look back, he became an example as well, though I can't claim to have been immediately conscious of this. Mark was passionate about the history of economics, not as a separate field of study, but as an integral part of the discipline. So am I, at least as far as monetary economics is concerned. I have never dared to range as widely Mark did, but I must have acquired this way of looking at things from somewhere, and in addition to our personal interactions, I was certainly a regular user in my teaching of various editions of *Economic Theory in Retrospect* (Blaug 1962) which it permeates.[1]

In the '60s and '70s, when I was in the UK, I saw Mark often, and after moving to Canada, still regularly though less frequently, because for some years we went to many of the same conferences on both sides of the Atlantic. I always looked forward to our encounters, though sometimes with just a little trepidation. Mark was habitually direct and you always knew where he stood. One of my later memories of him is from the 2002 HES meeting at Davis. As a discussant I defended Robert Leeson's suggestion – see Leeson (2003) - that his recent discovery in Milton Friedman's notes that the *Treatise on Money* (Keynes, 1930) had figured prominently in the monetary theory course the latter had taken from Lloyd Mints in 1932-3 provided some support for an innocent explanation for Friedman's (1956) attribution of a very Keynesian theory of the demand for money to a Chicago oral tradition. Mark was in the room, and the expression on his face that this provoked took me back for just for a moment to some of my earliest encounters with him, when I had learned so much, sometimes the hard way. But this expression quickly gave way to a resigned smile. Even Mark mellowed as he got older! [2]

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[1] All quotations from this book appearing here are from the 4th edition (1985)

[2] I still think Leeson had a point with regard to Friedman's personal credibility, which had been questioned by Harry Johnson (1971) who suggested that he had "fabricated" a spurious Chicago tradition. Friedman's first (1956) reference to that tradition was more a rhetorical flourish than a carefully considered account of history, and he would hardly have been the first to treat a particular feature of a department at the time he was actually a student there as indicating more about the longer run intellectual climate of the place than it really did. And Friedman was not a monetary specialist when a student. That the *Treatise* played a prominent role in Mints' course that year is, in any case, confirmed by Rose Friedman in Friedman and Friedman (1998, p. 38). This point, however, has very little to do with broader questions about whether the Chicago tradition in question was either as unique or as homogeneous as Friedman would have had it.

Mark's directness meant that his praise could be taken seriously, and I still get great pleasure from knowing that he liked my (1991b) paper "The quantity theory is always and everywhere controversial – why?" Not only did he tell me so, but he also paid me the compliment of making it a starting point for a paper of his own on a closely related set of questions about that theory (Blaug 1995). I should have taken up this conversation at the time and tried to carry it further, but I was fully occupied with other projects, and I let the opportunity slip. But better late than never: my tribute here to Mark is a discussion of some issues raised by his paper.

In what follows, I shall argue that though "Why is the quantity theory the oldest surviving theory in economics?" illustrates the strengths of Mark's general approach to the history of thought, it also leaves open the question about the quantity theory's history that had particularly concerned me, and indeed still does. Specifically, I shall suggest: that this paper's insightful analysis of the reasons for the quantity theory's longevity fails to deal fully with the matter of its perpetually controversial nature; that this characteristic reflects the fact that Mark's customary unwillingness as a historian of economic thought to stray too far beyond the boundaries the "positive economics" into ideological territory is very much on display in this study; and finally that the application of such standards to the quantity theory's history is unduly limiting because they inhibit discussion of the ideological environment which has so often provided a vital context for scientific debates about this theory, conditioning their progress and sometimes their outcomes as well.

Mark's Approach as Applied to the Quantity Theory

Mark's approach to the History of Economic Thought is summed up in the very title of his great textbook: *Economic Theory in Retrospect.* It was indeed a book about economic theory, not economic policy or economic philosophy; this is one reason why my students, who were almost always B.A. honours majors or M.A candidates enrolled in otherwise very technical programs, liked it, I think; and it was retrospective because Mark knew that if we are to understand today's version of any theory, a knowledge of its past is at least helpful and more likely essential. He offered his book as "a critical study" too, where "Criticism implies standards of judgment, and my standards are those of modern economic theory" (p. 1).

Even so, Mark was always careful not to project contemporary economic ideas onto the past along with contemporary standards. He knew that his "innocent sentence" about the latter obscured a multitude of difficulties. These arose, first because the standards themselves were the

product of history, and were prone to change over time, but second because "The development of economic thought has not taken the form of a linear progression towards present truths. While it has progressed, many have been the detours imposed by exigencies of time and place".(p.7) A full understanding of those detours required that the exigencies in question be acknowledged, but sometimes these lay beyond the boundaries that today's standards laid down for the subject, and Mark was hesitant to cross those boundaries: "If, in the chapters that follow, there is little about Zeitgeist, social milieu, economic institutions and philosophical currents, it is not because these things are unimportant, but because they fall outside of the scope of our enquiry" (p. 7). My students liked this too, but here I was, and still am, less comfortable.

When it came to monetary economics, as with the rest of contemporary economics, Mark believed that ". . . as it is now conceived [it] is . . .what it is because of the entire trajectory of received economic doctrines; . (1995, p.1); and the quantity theory of money provided him with the ideal context in which to demonstrate the advantages of studying the history of an economic theory as a means of achieving a fuller understanding of today's version of it. There were also lessons here that were relevant beyond the boundaries of monetary economics: ". . .this oldest of economic theories is also one of the most misunderstood economic doctrines . . . we can learn a great deal about what are called "theories" in economics by studying the history of the quantity theory. (p, 28)

Mark was generous in suggesting that his (1995) essay was addressing the same questions as my earlier piece, because in fact it went much deeper in one important direction. It asked, as I had not, why the theory had lasted so long in the first place, what that longevity itself might have to tell us about its current standing, and what all this revealed about the nature of economic theories more generally. I had tended to take the theory's longevity for granted, and hence had little to say about these questions.

But on another matter, the reasons for the quantity theory's habitually being controversial, Mark said little explicitly. To be sure, he was clearly skeptical about the monetarism to which the quantity theory was so central and which was still prominent in the intellectual landscape when he was writing his essay. Also, his respect for the theory itself was mixed with a good deal of exasperation, not only about the company it was then keeping, but also about the intellectual quality of many of its earlier manifestations as well. Even so, he paid little attention to such qualms in arguing his way to his principal conclusion: "The point is, and perhaps this is my main point, that if we believe that the quantity theory of money is true, it is not because

we find the theory underlying it so plausible and precisely expressed that we feel compelled to assent to it. It is facts and not analytic rigor that make the quantity theory good economics. I venture to assert that this is so with most if not all economic theories" (1995. p. 44) In this assessment, we have a succinct expression and application of the standards that Mark had discussed explicitly in *Economic Theory in Retrospect,* and which always guided his work on the History of Economic Thought.

The Quantity Theory as Positive Economics

To a modern reader the lack of stress on analytic rigor as opposed to empirical content here might seem to sit uneasily with Mark's abovementioned claims to be applying the standards of modern economic theory in judging the economics of the past. It needs to be recalled, therefore, that Mark's standards were those that ruled four decades or more ago. The "positive economics" of that era and the standards that went with it have by now been widely superseded among the discipline's "mainstream" by an insistence on analytic precision and rigor as ends in themselves.[3]

If these later standards are brought to bear on Mark's work, including his study of the quantity theory, it could be made to look muddled, but Mark himself would have had a strong defense against such a verdict. His well known skepticism about modern formal analysis already marked passages of his (1995) paper and would play a more prominent role in some of his later writings – e.g. (1997, 2001) – and it did not stem from a failure on his part to value analytic precision in the expression of economic ideas, let alone of an unconsidered refusal to move with the discipline's times. Rather it reflected a deeply held and well thought through belief that the pursuit of analytic precision for its own sake was leading economics away from, rather than towards, greater empirical relevance, and that the times were therefore moving in the wrong direction.

Thus Mark's conclusion that the quantity theory's longevity stemmed from it being good empirical economics was quite consistent with the scathing denunciation of the imprecision with which it had routinely been expressed over the centuries that also appears in his (1995) paper. "At the end of our story, we are struck by the failure of just about every quantity theorist to provide any rigorous statement of the theory. Wicksell and Fisher are the best of them . . . [but] an almost indescribable analytic sloppiness characterized some 200 years of development in monetary theory." (p. 43)

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[3] A discussion of the precise nature of Mark's positivism could easily occupy a much longer study than this one in its own right. I hope it suffices here to note that it was closer to that of Richard Lipsey (1963) than Milton Friedman (1956)

The requirement that it should provide clear instructions about how to bridge the divide between the logical time inherent in its formal specification and the real time in which the data it was intended to explain were generated, was very important to Mark when he assessed the acceptability of any economic theory of any vintage. Here he found the record of the quantity theory particularly deficient. As he showed at considerable length, the theory had been understood for centuries to yield two predictions: first that, in the long run, the price level would move in proportion to the quantity of money, and leave real variables unaffected – the "notorious proportionality theorem" (p. 20) as he branded this idea of neutral money - and second that, in the short run, changes in the quantity of money would have systematic real effects. But these predictions raised two further questions: how long was that short run likely to last? And if the quantity of money was continually growing fast enough to keep prices always rising, would non-neutralities also persist?

The only attempt to answer the first of these questions that Mark could find in 200 years of literature was by Friedman, who put the duration of significant non-neutralities in the wake of monetary shocks at anything between 3 to 10 years (see 1995, p.42). But Friedman's answer came accompanied by his natural unemployment rate hypothesis, and this made Mark uncomfortable. First of all, this hypothesis contradicted David Hume's apparently affirmative answer to the second of Mark's two questions about non-neutralities. And second, Friedman's vagueness about the time interval over which his natural-rate hypothesis was supposed to hold made it difficult to bring empirical evidence to bear on settling this difference between him and Hume, perhaps to the point of putting the very scientific status of this hypothesis in doubt.

In Mark's view, then, the advent of Friedman's version of the doctrine of money's super-neutrality had not necessarily been a step in the right direction as far as the quantity theory's empirical content and hence scientific value were concerned. He would return to this matter later (See Blaug, 2001) when he criticized Robert E. Lucas (1996) for reading the New-classical view of the quantity theory's place in modern general equilibrium theorizing about macroeconomics into the past, as a by-product of his overall claim that progress in these matters since Hume had lain solely in applying successively refined analytic techniques to the same old substantive issues: "It does not seem to occur to Lucas that this is *not* how the quantity theory of money was interpreted by Hume or anyone else in this golden age before the rational expectations revolution of the 1970s" (2001, p. 155) As Mark had remarked in (1995), "The object of the quantity theory

from its very outset in Hume's formulation was to demonstrate that the absolute size of the quantity of money was of no real significance in an economy" (p. 29), not to expound the claims to super-neutrality that lay at the heart of Lucas's contributions, because for Hume, the rate of change of that price level did matter for real variables.

It was surely Mark's resistance to theoretical formalism that made him unwilling to attribute more precision to the quantity theory, considered over the sweep of its history, than was embodied in a prediction that "In any monetary regime, any dramatic and unexpected increase in the quantity of money will in due course raise prices, although not necessarily in the same proportion – that is all the quantity theory of money amounts to", and to add immediately and approvingly, "Nevertheless, painting with a broad brush, the quantity theory is supported by an overwhelming body of empirical evidence" (p.43)

The Quantity Theory's Controversial Nature

Mark thus explained the longevity of the quantity theory by applying his own customary standards for judging any economic theory to this task. He boiled down its essentials to an empirical prediction that had barely changed over the years, compared that prediction to evidence, and found the theory broadly validated. But in so doing, and as I have already noted, he didn't directly address that other question which had concerned me in (1991b), the reason for the perpetually and often acutely controversial nature of this theory during its long life.

Mark did of course have a good deal to say about some of the specific empirical debates that had dogged the quantity theory over the years, particularly those repeated discussions of "reverse causation" and money's endogeneity more generally, matters that I had also taken up and are still with us today. And, as I had also done, Mark drew attention to controversies about the stability of the demand for money function, which look rather different today than they did even in 1995.[4] But, beyond pointing out that acceptance of the quantity theory's usefulness presupposes a significant degree of faith in the capacity of market mechanisms to function spontaneously in keeping the real economy working, Mark did not extend his discussion of the controversies in question to encompass their implications for deeper differences of opinion about this capacity. Presumably this was because he was observing a self-imposed prohibition

[4] Problems with institutional change within the monetary sector were already becoming apparent in the mid-1980s, as Geoffrey Wood (1995) discussed in some detail. But see below pp. 11-12 for further discussion.

on straying too deeply into matters of "Zeitgeist, social milieu, economic institutions and philosophical currents," because it is indeed hard to stay out of such territory when questions about the stability and efficiency of the market economy are followed up too assiduously.

This reticence didn't matter much for many aspects of Mark's analysis of the evolution (or lack thereof) of the internal logic of the quantity theory of money and what empirical evidence had to say about its practical relevance. But monetary economics, like economics in general, is concerned with the workings of institutions that enable societies to function, and it has a great deal to say that is relevant to the political choices that have to be made among them. The evolution of the quantity theory's relationship to these broader issues is thus also part of its history. A very special set of beliefs about the overall socio-economic order is required to make one comfortable with a body of doctrine that reduces monetary theory to a means of making predictions about the relationship between the quantity of money and the price level; not all of these beliefs of are matters of positive social science; and many of them are very controversial indeed. An explicit discussion of such beliefs has to be part of any assessment of the quantity theory's standing at any time within the broader corpus of monetary theory, indeed of economic theory more generally, and the study of their evolution over time, as this has interacted with the evolution of monetary theory, is therefore integral to an understanding of the quantity theory's history.

The Theory's Golden Age

This was a point I was trying, rather timidly, to get at in (1991b) in discussing the reasons for the quantity theory's perpetually controversial nature, and Mark's failure to follow me into this territory underlies an important difference between our treatments of at least one important episode in the quantity theory's history. Specifically, in the title of my (1991a) book, I labeled the period running roughly from 1870 until the outbreak of World War 1 *The Golden Age of the Quantity Theory.* Mark commented as follows on this choice: "Was this the *Golden Age of the Quantity Theory?* No, say some, because the golden age was also the era *par excellence* of the international gold standard . . . " (p. 55) whose basic mechanisms, he correctly argued, rendered the quantity theory operationally irrelevant.

Though Mark didn't explicitly include himself among the abovementioned "some", he didn't exclude himself either, and his ambivalence here is closely related to two other features of his treatment of the quantity theory's development during this period: first his suggestion that

"so far as the quantity theory is concerned, the Marginal Revolution of the 1870s might just as well never have happened" (p. 34) and, second, his failure to discuss explicitly the highly politicized bimetallic controversy that began in the same decade – though he did of course cite some of the literature that was its by-product.

Now in terms of Mark's own critical standards, and the limits they imposed, his judgments on these two salient events of this period are understandable and even defensible. To be more specific, in a critical comment on Mark's paper, Don Patinkin (1995) correctly pointed out that the modern agenda associated with the integration of monetary and value theory to which he himself had contributed so much had in fact begun with those two pioneers of marginalism, Walras and Marshall. But Mark's point had been that this agenda had in fact made little progress relevant to the way in which the quantity theory itself was presented until the 1920s. And on the policy front, he had noted that the practical consequence of the bimetallic controversy had after all been to leave the gold standard, one of whose essential roles was to ensure that " nominal stock of money in small, open economies . . . was adjusted to the level of prices via the balance of payments, so that the quantity theory was simply irrelevant" (p. 35) more deeply entrenched than it ever had been. On both of these matters, he had been right.

However, if little change took place in the quantity theory's logical and empirical properties as a piece of economic theory, or in its relationship to the actual conduct of monetary policy between 1870 and the outbreak of World War 1, its place within the body of monetary theory, and more specifically within the theory, as opposed to the practice, of monetary policy as well, nevertheless changed dramatically. Quite simply, the marginal revolution rid mainstream economic theory of the cost of production theory of value, and hence monetary theory of the idea that, under a commodity standard, the price level was determined exogenously to the monetary system by the cost of production of the monetary metals. Thus an explanation of the price level which, as Mark's own essay documents so well, had often been the quantity theory's competitor during the preceding century was removed from the scene, leaving it as the only one remaining.

As a consequence, though it took a while for this change to be digested, it became untenable to present the gold standard as the embodiment of a naturally ordained monetary order and to defend it as such in political debates. Even for its advocates, this monetary regime now had to be treated as an arrangement to be chosen or rejected on its merits in a competition with others, a competition whose rules were for while defined

solely by the quantity theory because no other source of such rules was available within economic theory. As shifts in the Zeitgeist etc. have gone in the history of western civilization, the demise of the idea that the gold standard represented the monetary component of a naturally ordained economic system may not have been of the first order of magnitude, but surely it qualifies as such an event. It is hard to imagine either the debates about rules and discretion in monetary economics that dominated American discourse in the 1920s and 1930s and culminated in Simon's (1936) classic work on these issues, or the extraordinarily rich literature on the role of monetary policy that was so prominent in the early years of the Great Depression developing as they did, had the cost of production theory of the value of metallic money not lost its scientific respectability and been replaced by the quantity theory between the 1870s and 1914[5].

It was these factors that persuaded me to label this period a golden age for the quantity theory, even though it was indeed, as Mark pointed out, the heyday of the gold standard in the economy itself. There was an intended touch of irony in my title as well, which many commentators, perhaps including Mark I'm afraid, missed, but more important than this, they also missed my substantive point about the dramatic change in the quantity theory's intellectual standing within the theory, as opposed to practice, of monetary policy to which this title was intended to draw attention.

The Monetarist Episode

To put it (almost) in Mark's own words, the quantity theory depended upon ". . . three propositions: (a) the exogeneity of the money supply; (b) the stability of the demand-for-money function; and (c) the real determin[ation] of the level of output or transactions" (p. 41).[6] Each one of these is surely empirically testable on its own merits, at least in principle, and it is indeed this property, as Mark stressed, that makes the quantity theory a theory. But, as I have noted above, discussions of his third proposition (c) can quickly stray beyond the boundaries of positive economics into ideological territory, and this propensity can make dispassionately convincing tests of the quantity theory hard to generate. Throughout the quantity theory's history, that is to

[5] Of course the quantity theory did not retain its monopoly on intellectual respectability within monetary theory for very long. As Leijonhufvud (1981) demonstrated, Knut Wisckell's efforts – eg 1898 – to adapt it to the circumstances of economies dominated by banking systems soon led to a body of theory, largely Austrian and Swedish, within which the central question ceased to be the influence of the quantity of money on prices, and became instead, the influence of the rate of interest on savings and investment. And by the mid-1940s, it was Keynesian economics that provided the main intellectual foundations for the Bretton Woods system.

[6] Mark writes "real determinants".

say, there has often been more at stake in the interpretation of relevant empirical evidence than the question of what determines of the price level.

This was true during the golden age just discussed, when the quantity theory became so deeply embroiled in populist politics that Irving Fisher felt impelled to write *The Purchasing Power of Money* to restore it to respectability in sound money circles. And it was also true in other episodes in its long history, not least during the monetarist episode towards whose end Mark and I actually wrote our papers.[7]

To be more specific here, it was no accident that Joan Robinson was one of the earliest critics of monetarism, taking on the then new and rising doctrine in *The Journal of Money, Credit and Banking,* one of its own intellectual bastions (See Robinson 1970). She was, after all, an exponent of a political theory of inflation based on ideas about struggles over income shares between social classes, and hence also a leader of the post-Keynesian revival of assaults on what I have argued above are the inevitably ideologically charged propositions about the inherent stability of the real economy summarized in Mark's proposition (c). So when, in the above-mentioned paper, she accused quantity theorists of reading the direction of causation in their equation in the wrong direction, she also imbued debate about his point (a) with political overtones that still reverberate. Nor, a decade later, was it entirely co-incidental that political attacks on the "monetarist" policy agenda of that well know composite politician Ronald Thatcher should provide the context for Hendry and Ericsson's (1983), in and of itself purely technical, assault on Friedman and Schwartz's (1981) handling of the empirical evidence bearing not only upon proposition (a) but also upon proposition (b) about the stability of demand for money, and so on.[8]

The Ideological Connection

In short, I persist in believing that it is the ideological resonance of the quantity theory that has kept it controversial for so long, and perhaps also contributed to its longevity. If I am right here, then Mark's explanation of this longevity as the result of the quantity theory being good empirical economics is incomplete. His approach can of course be defended by arguing that the logical properties of any economic theory, the quantity

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[7] And between the bimetallic controversy, when it was the major tool of those attacking gold-standard orthodoxy, and the monetarist controversy, the quantity theory's political affiliations moved from left to right, as I argued in Laidler (2004)

[8] The fact that a version of this same paper appeared eight years later in a much more accessible academic source than the original – See Hendry and Ericsson (1991) - has tended to distract attention from its original political context.

theory included, and its empirical content too, exist independently of its ideological connections and can therefore be assessed in isolation from them. But this argument is at its most convincing when the evidence used to test a theory is also generated independently of those ideological connections, and this has not always been true of the quantity theory.

Such independence might well usually exist for some economic theories, but it is not always there in the case of monetary theories.[9] The data available to test these are the product of the monetary policy regimes within which the policy experiments that have generated them were implemented, and of the reactions of economic agents which were themselves conditioned by expectations about how those experiments would work out. All of these elements are in one way or another dependent on the particular beliefs, some of which are ideologically loaded, about the workings of the monetary system current at a particular time and place; and this fact in turn implies that the empirical messages conveyed by any particular experience about the validity of any particular monetary theory, the quantity theory included, depend not just how that theory is formulated today, but on how or even whether that same theory was formulated and how it fitted in to the prevailing political environment when the evidence being brought to bear on it today was generated.

I didn't argue this point in 1991, but I began to do so later (e.g. Laidler 2003) and if there is something to it, then it has implications about why, but also how, we should study our subject's history. These implications do not contradict the principles that Mark deployed in discussing the quantity theory in 1995, but perhaps they do require them to be extended. As he argued, it certainly helps to know where a theory has come from if we are to understand today's version of it, and, *pace* my fellow monetary economists in their devotion to formal rigor, I remain in complete accord with him that the ultimate test of a theory's value lies in its ability to explain empirical evidence. But in interpreting such tests, it also helps to know not just where the theory under test has come from, but also what if anything some earlier version of it had to do with generating the evidence being deployed. Since ideological elements often condition the monetary policy environment, and also the specific actions of policy makers and private sector agents alike, those awkward questions of "Zeitgeist, social milieu, economic institutions

[9] Thus, the arguments presented here can probably be applied also to other monetary and macroeconomic doctrines: e.g., and among others, Keynesian economics or New classical economics, but to explore these issues further, particularly with respect to the way in which the arguments in question might or might not need adapting to other particular cases would take us far beyond the bounds of this paper. I am nevertheless indebted to Richard Lipsey and Roger Sandilands for raising these issues, and arguing, correctly, that they should at least be noted here explicitly.

and philosophical currents" that Mark was inclined to set to one side, can have a critical role to play in assessing the significance of particular data sets for a theory, and hence in influencing the future course of its history.

A Recent Example

Let me end with a single assertive illustration of what I have in mind here, drawn from recent experience. Though the effects of institutional change on the nature of the demand for money function that had become so evident by the time Mark was writing certainly played a role in causing central banks to retreat from money growth targeting, it was not these effects alone that caused the quantity theory's virtually complete disappearance from respectable professional discourse even as Mark was writing. At least as important was the failure of Milton Friedman's (e.g. 1984) very public quantity-theory based predictions of an imminent resurgence of serious inflation in the United States, an apparently straightforward consequences of – to use Mark's phrase - the "dramatic and unexpected increase in the quantity of money" that had followed the Volcker disinflation.

Would this failed prediction have been as decisive for the quantity theory's standing in academic economics had it not come from someone so closely and publically associated with other aspects of Ronald Thatcher's policy agenda? And has the resulting decline in its reputation not had consequences for the subsequent history of monetary theory as a body of seemingly positive doctrine? And did that subsequent history not in turn influence economic events, not least the recent crisis and the reactions of policy makers and economists alike to it?[10]

I would have loved to discuss these questions with Mark. That look he gave me at Davis in 2002 suggests he would have had a few instructive things to say about them and with his usual directness too. I shall miss him.

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[10] As Richard Lipsey has reminded me, a striking feature of recent debates in the US has been the frequency with which economists with market-oriented political sympathies have cited the growth rate of the Federal Reserve system's monetary liabilities as threatening the imminent onset of inflation, despite the fact the growth of the money supply itself, on any traditional measure relevant to the quantity theory, has been at best modest. It is difficult not to see this as an indicator of just how deeply into neglect quantity theoretic reasoning sank after the early 1980s, even among some of its former exponents . See, e.g. Allan Meltzer (2009)

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