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**by**

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**Eliminating History from Economic Thought**[1]

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Abstract: Formal analysis, in which maximizing agents use today's "true" model of the economy to form expectation upon which they then base their behaviour, trivializes the role of the future in economic life and ignores the possibility that the past's models, which helped generate the data against which today's models are tested, differed from the latter. Such analysis denies that economics has a relevant history, and its current dominance explains the decline of the history of economic thought within the discipline.

JEL Classifications A10, B10

Keywords, Time, Progress, Empirical Evidence, True Model, Rational Expectations

[1] After-dinner speech, delivered in mid-Bosphorus on May 21st 2011, in acceptance of honorary membership in the European Society for the History of Economic Thought at the Society's annual conference in Istanbul. Helpful discussions with Harald Hagemann and Hans-Michael Trautwein are gratefully acknowledged, but I am entirely responsible for any boredom or other adverse reactions generated by what follows.

*Fears for the Future of the History of Economic Thought*

This honorary membership brings me great pleasure. Some of my colleagues think of me as a monetary economist who has sought refuge in the history of economic thought (HET hereafter) as his ideas have become unfashionable, and perhaps they have a point, but that isn't the way that I think of myself. HET constituted a large and compulsory segment of my undergraduate education at the LSE, and along with monetary economics it was one of my two fields of specialization as a Ph.D student at Chicago. Moreover, my first paper in the field, on Thomas Tooke, though not published until 1972, was actually written in 1965-66, within 18 months of the completion of my degree, and I had no qualms about including it in my (1975) collection of essays on then current issues involving money and inflation. In short, I learned long ago that its own history is an integral part of monetary economics, and I have never drawn a sharp distinction between the two areas.

But my pleasure tonight is tempered by the apprehension that many of us feel about HET's future. Though matters are not so far gone in Europe as in North America, its serious study as a branch of economics seems to be in decline everywhere. So tonight I want to sketch out a monetary economist's suggestions about why the academic mainstream - please forgive this label, but I don't have a better one - has recently wandered so far off course in its treatment of HET, but also about why the inevitability of the end of history within economics might just be an intellectual illusion created by a certain way of doing the subject that is itself perhaps on its way to becoming part of that history..

This topic is well suited to a long and boring lecture, but our President, Harald Hagemann, has warned me that, if I talk for longer than fifteen minutes, my honorary membership will be revoked. Since I really do value it, I shall take full advantage of the conventions of a brief after-dinner talk to be assertive and to avoid all intellectual nuances.

*The Myth of Orderly Progress in Economics*

It is not news that today's mainstream believes economics to be a science which makes orderly progress, that old ideas which are still useful are in the current body of knowledge, and that those which are not there have disappeared because they are not useful. Paul Romer's rather condescending view, expressed to Snowden and Vane in 1999, that "ancestor worship as a research strategy [is] probably. . .unproductive. But as a consumption activity it…can be fun" is widely held, and though it might leave HET an honourable place in general undergraduate education, or even in the graduate history of science curriculum - which has probably been just as well for the area's survival in recent years - it excludes it quite firmly from any serious economics program.

This view has always been around, of course, but it has taken hold with increasing strength over the last four decades. It should not have done so, because over this same period its falsity was quite evident. Now as ever, it is closer to the very complicated truth to describe economics as a subject in which new ideas do sometimes turn up, but in which old ones also regularly reappear in various disguises and for various reasons, seldom if ever disappear entirely or achieve complete dominance, and usually, though not always, are sharpened up a little along the way.

Consider a few examples: the quantity theory of money seems to have begun its modern journey in 16th century Salamanca, and after much coming and going to have arrived in Chicago in the 1950s and 60s on its way to a prominent position in monetary policy in the 1970s and 80s, before sinking into today's (probably temporary) obscurity; the two interest rate approach to discretionary monetary policy, with which the quantity theory sometimes crossed paths, began its wanderings in 18th century Stockholm and Regency London, passed through Sweden again in the 1890s, only to vanish in the 1930s and then re-emerge at the turn of the millennium in the research departments of all those inflation targeting central banks; and, more broadly, the reputation of the "market" as a way of organizing economic life has regularly risen and fallen over two and a half centuries, and has continued to do so since the early 1970s.

Economics makes much use of arguments whose logic is either sound or not, and it sometimes develops potentially refutable empirical predictions as well. These attributes surely give it some claim to be classified as a science; but a science which makes, and always has made, orderly and unidirectional progress? In which almost forgotten ideas from the past never have nor ever will take on renewed relevance? And in which there never was nor ever will be anything to be gained from some familiarity with its own history? Please! As the old English saying puts it: "Pull the other one, it's got bells on"

But over the last thirty or forty years, this is what more and more economists have come to believe, and with growing confidence too, making HET's position within the discipline increasingly precarious. Surely, the rise and prevalence of these beliefs in the face of so much contrary empirical evidence needs explanation, and because this is a historical phenomenon within economics, those of us who still take HET seriously had better try to find one. Here, I confess that I am getting a bit out of my depth – when I first inserted this phrase into my notes, I had no idea that I would be speaking in mid-Bosphorus! - but since I have a sympathetic audience, at least for about another ten minutes, let me offer a few conjectures.

*The Future and the Past in Present Day Economics*

I suggest that the very approach to economic theory which has been increasingly widely taught, particularly in the monetary area, since the early 1970s – rational expectations modeling and all that, itself one of those successful new sets of ideas that do indeed turn up from time to time - presents a serious barrier to observing, let alone, comprehending, the facts presented by the subject's own history, and that this is because it has turned that particular view of the discipline's scientific nature already mentioned into an axiom upon which economics itself is founded.

In his recent book, *The Age of Fracture* Daniel T. Rodgers (2011) discusses the development of American social, political and economic ideas since the early 1970s, and notes that this has been marked by the widespread and persistent creation of *wrinkles in time* into which matters of history as well as intellectual and institutional evolution tend to disappear. I have no qualifications that would allow me to judge the overall validity of Rodgers' analysis, but it certainly seems to fit recent developments in American economics even better than he thinks.

That theories based on the idea that agents form and act upon rational expectations derived from their knowledge of a true model of the economy fold the economy's future into its present is a phenomenon that has already attracted a good deal of critical attention. We've all long known that the assumption of perfect foresight totally eliminates the distinction between present and future, and that the rational expectations hypothesis gets as close as one can to the same outcome while still permitting the construction of explicitly dynamic models that have the potential to mimic what happens in the real world as real time passes there. We've also known for some time that this hypothesis, and the clearing markets assumption that goes with it, excuses its exponents of any need to pay attention to what Keynes called the "dark forces of time and ignorance". Whether this should be regarded as a boon or not is a matter of considerable and still ongoing controversy, though let me remark in passing that I for one have felt increasing qualms about it lately.

But mainstream economics does strange things to the relationship of the present to the past as well, and these are less widely noted. At first sight, the past seems to matter quite a lot to the forward looking agents who inhabit today's models. It has left behind endowments of productive resources to which they can apply inherited technology, as well bequeathing an institutional framework defining property rights and the terms on which they can be exchanged, not to mention knowledge of the "as if" true model that is the basis of the expectations that inform their choices. Had these starting conditions come by way of a big bang at the instant we encountered these agents, however, everything would be the same, because the processes by which they came to exist in the past are irrelevant to the way in which agents deploy them in the present, which is, as I have noted already, completely forward looking.

Furthermore, and crucially, mainstream economic theory became explicitly self-referential with the rise of the rational expectations hypothesis. Today's archetypical model teaches that the economy behaves the way it does because, among other things, the agents inhabiting it use that very same model to devise their strategies. But this incorporation of the true model of the economy into its own structure has transformed the hypothesis that economics is a science that makes unidirectional progress from just one among several ideas about how the subject develops into the only one tenable by exponents of modern theory, and hence has ensured the simultaneous spread among them of the view that HET cannot be integral to the discipline.

The past may be the only source of data against which economic hypotheses can be tested or calibrated, but data never speak entirely for themselves. They need to be interpreted though a theory. When the only theory deemed suitable for this purpose embodies itself as part of its own structure, even on an "as if" basis, then that structure is inevitably projected onto the past, and other perspectives on the historical record are obscured The "as if" element here does perhaps leave a little room for HET, but only for an ultra-Whig version of it that focuses on the increasing mathematical sophistication with which economists have analyzed the same old questions and answers about economic life that their theory insists have always informed agents' behaviour. To adapt Bob Lucas's deservedly well known (1980) remark - "to ask why the monetary theorists of the 1940s did not make use of the contingent claim view of equilibrium is. . .like asking why Hannibal did not use tanks against the Romans instead of elephants" - it is as if the history of warfare has to be confined to tracing the slow but steady technical evolution of the battle-tank from the war-elephant. This might be an interesting story, but whether it is enough to occupy the whole history syllabus even of a military academy is another matter.

*The Self-referential Nature of Economics*

I persist in thinking that the key to getting mainstream economics to begin paying attention again to the intellectual diversity and ambiguity that is at the heart of HET lies, first of all, in embracing wholeheartedly its insights that economic ideas are self referential and that they do indeed affect economic behaviour, but, second, in insisting that when the actual outcome of the application of some particular set of ideas deviates from what they have led their exponents to expect, this dissonance be taken seriously and followed up.

As I have often argued before, the real world is inhabited not by representative agents but by diverse ones who practice the division of labour, and those who create the economic ideas that inform economic activity are called economists. HET is the history of their activities, and it teaches us that there never has been a single economic theory which has also been the undisputed common property of all agents, let alone a theory that was also agreed to be clearly true - let's not get diverted so late in the evening into discussing what this last phrase might mean! HET tells stories about the continuous interaction and evolution of competing and often contradictory theories that have not only influenced behavior, but have also been influenced by its consequences, as events have forced agents to rethink old ideas and conjecture new ones.

Today's mainstream monetary economics, with its reliance on clearing markets and rational expectations, has surely earned a permanent place in the subject's history, but as an important part of the story of its ongoing evolution, not as an end-point whose achievement has rendered what went before it irrelevant to understanding what is now happening. The idea that political history came to an end at some time during the Reagan administration – another example that Rodgers cites of time-wrinkling, by the way – did not last long in the face of the evidence generated in the years that followed. Perhaps the economic crisis that began in asset markets in the summer of 2007, and whose consequences continue to reverberate, is now forcing a similar reconsideration of the ideas that blinded so many to its approach, and perhaps also helped to create its preconditions.

If such reconsideration does take hold, then HET's decline over the last three or four decades will itself turn out to have been yet another example, albeit on a large scale, of the way in which economic ideas almost disappear from time to time, only to resurface again as people begin to find a new use for them. Let us at least hope so, and, given tonight's occasion, demonstrate our optimism by raising another glass of wine to the prospect as well.

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

by

David Laidler

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.

JEL classification: B1, B2, B31

Keywords: money, Quantity theory, value theory, positive economics. empiricism, formalism, bimetallism, gold standard, monetarism.

*Paper prepared for a volume of essays honouring the late Mark Blaug

*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.[2]

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 an alleged 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! [3]

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

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

[3] I still think Leeson had a point. Friedman's first (1956) reference to that Chicago tradition was more of rhetorical flourish than a carefully considered proposition about history, and he would hardly have been the first student to treat a particular feature of his department in a particular year as indicating more about the overall longer run tone 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 confirmed by Rose Friedman in Friedman and Friedman (1998, p. 38)

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 the same topic (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 how his (1995) paper on the quantity theory both illustrates the strengths of his general approach to the History of Thought, but also poses a particular puzzle in his handling of one episode that was of particular concern to me at the time, and which perhaps reveals a certain limitation to that approach.

*Mark's Approach*

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 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 a bit 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)

Though Mark was generous in suggesting that his (1995) essay was addressing the same questions as mine – why has the quantity theory always been controversial? – in fact it went deeper in one direction, asking 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 neglected the issues this fact raised. But on another matter, the reasons for its habitually being controversial, Mark said little explicitly; this even though he was skeptical about the monetarism to which it was so central and which was still prominent in the intellectual landscape when he was writing his essay, and despite the fact that his respect for the quantity theory itself was mixed with a good deal of exasperation about not only the company it was then keeping, but also about the intellectual quality of many of its earlier manifestations as well.

Even so, these latter qualms did not prevent him concluding: "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 of the standards that Mark had discussed explicitly in *Economic Theory in Retrospect,* and always guided his work on the History of Economic Thought; but here we should recall that though he referred to those standards as "today's", the "today" in question is now three decades or so into the past. They were dominated by a style of "positive economics" that stressed empirical content, and have by now been widely superseded among the discipline's "mainstream" by an insistence on analytic precision and rigor as ends in themselves.[4] Mark was no intellectual Luddite, and he certainly believed that analytic precision in the statement of a theory had a lot to be said for it, but only when this quality made the task of bringing facts to bear in assessing a theory easier rather than harder. If it did not, then he was willing to tolerate a good deal more in the way of logical looseness to achieve empirical relevance than would pass muster in 2012, not least among monetary economists.

*The Importance of Empirical Content*

If these later standards were brought to bear on Mark's work then, 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 a 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)

Prominent among Mark's requirements of economic theories of any vintage was that they provide clear instructions about how to bridge the divide between the logical time inherent in their formal specifications and the real time in which the data they were intended to explain were generated, and here he found the record of the quantity theory particularly deficient. As he showed at considerable length, the theory had been understood for

[4] 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)

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, would non-neutralities persist for ever?

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 p.42). But Friedman's answer came accompanied by his natural unemployment rate hypothesis, and this made Mark uncomfortable, not only because this hypothesis contradicted David Hume's apparently affirmative answer to the second of Mark's two questions about non-neutralities, but also because vagueness about the time interval over which it was supposed to hold made it difficult to bring empirical evidence to bear on settling this difference between Hume and Friedman, perhaps to the point of putting the scientific status of the natural rate 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 relevance 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 New-classical views 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.

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 controversial nature of that theory during its long life. He did of course have a good deal to say about some of the empirical controversies that had dogged the quantity theory over the years, particularly those repeated debates about "reverse causation" and money's endogeneity more generally, matters that I had also discussed and are still with us today. And, as I had also done me, 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.[5]

But Mark stopped short of extending his discussion of these controversies to their implications for deeper differences of opinion about the capacity of market mechanisms to function spontaneously in keeping the real economy working, even though, as did point out explicitly, acceptance of the quantity theory's usefulness presupposes a significant degree of faith in this capacity. Presumably he did not venture further here in observance of a self imposed prohibition on straying too deeply into matters of "Zeitgeist, social milieu, economic institutions and philosophical currents," because it is indeed hard to stay out of this territory when questions about the stability and efficiency of the market economy are raised.

This reticence didn't matter much for most 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

[5] 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.

a great deal to say that is relevant to the political choices among them that have to be made. 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 version of monetary theory that reduces it to a means of making predictions about the relationship between the quantity of money and the prices level; not all of these beliefs of are matters of positive social science; and many of them are very controversial indeed. But an explicit analysis of such beliefs has to be part of any assessment of the quantity theory's standing within the broader corpus of monetary theory, let alone economic theory more generally, and to any understanding of how this has evolved.

*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 did affect our treatments of at least one important episode in the quantity theory's history. Specifically, in choosing a title for my (1991a) book, I had labeled the period running roughly from 1870 until the outbreak of World War 1 *The Golden Age of the Quantity Theory*, and 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 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 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, though Don Patinkin (1995), was right to point out that the modern agenda associated with the integration of monetary and value theory to which he himself had contributed so much had after all begun with Walras and Marshall, Mark too was right to note, relying heavily on Patinkin's own earlier work, that this agenda had in fact made little progress that was relevant to the way in which the quantity theory itself was presented until the 1920s. And on the policy front, 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.

However, if little change took place in the quantity theory's 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, 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 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 been the quantity theory's long-standing competitor for over a century was removed from the scene, and it was left as the only one remaining.

As a consequence, though it took a while for this change to be digested, it became impossible 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, it now had to be treated as a monetary regime 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. As shifts in the Zeitgeist etc. have gone in the history of western civilization, the demise of the gold standard as a component of a naturally ordained economic order 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[6].

[6] 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

These factors explain why this period, which was indeed, as Mark pointed out, the heyday of the gold standard in the economy itself, nevertheless seemed to me to merit being labeled a golden age for the quantity theory. There was, of course an intended touch of irony in my title as well, which many commentators, including Mark I'm afraid, missed, but more important than this, they also missed the dramatic change in the quantity theory's intellectual standing to which this title was intended to draw attention.

*The Ideological Element in the History of Monetary Economics*

I have already noted that, to put it now (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).[7] Each one of these is surely empirically testable on its own merits, at least in principle, and it is indeed this property that makes the quantity theory a theory. But, as I have also noted, discussions of his third proposition (c) can quickly stray beyond the boundaries of positive economics into ideological territory and dispassionately convincing tests of it can be hard to generate. This is why throughout the quantity theory's history there has always been more at stake in the interpretation of empirical evidence than the determination of the price level.

This was at least as true of the monetarist episode during which Mark and I actually wrote our papers as of 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 or indeed as of many others in its long history. It was no accident that in (1970) Joan Robinson, 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) should also accuse quantity theorists of reading the direction of causation in their equation in the wrong direction, thus imbuing debate about his point (a) with political overtones as

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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.

[7] Mark writes "real determinants".

well. Nor was it entirely co-incidental that political attacks on the "monetarist" policy agenda of that well know composite politician Ronald Thatcher should provide the ideological context for Hendry and Ericsson's (1983) assault on Friedman and Schwartz's (1981) handling of the empirical evidence bearing upon proposition (b) about the stability of demand for money, and so on.[8]

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 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 connections, and this is not true of the quantity theory.

Such a condition might well be met for some economic theories, but not monetary theories: the evidence available to test them is the product of the monetary policy regimes within which the policy experiments that have generated them were implemented, and of the reactions to those experiments of economic agents which were themselves conditioned by their expectations about how those experiments would work out. All of these elements are in one way or another dependent on the particular beliefs about the workings of the monetary system that are 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 when the evidence being brought to bear on it today was generated.

I didn't argue this point in 1991, but more recently I have (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

[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.

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

### *A Recent Example*

Let me end with a single 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, 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 this ideological loaded 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 alike and economists to it? 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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