The Role of the History of Economic Thought in Modern Macroeconomics*

by

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Abstract: Most "leading" economics departments no longer teach the History of Economic Thought. Prominent macroeconomists nevertheless frequently deploy inaccurate accounts of the earlier development of ideas as rhetorical devices. These same economists have, however, also taught us that an understanding of how the economy functions helps condition the behaviour of maximising agents. The History of Economic thought documents the evolution of that understanding, so it is hard to see how economic history, which is the source of all the time series data which form the empirical basis of macroeconomics, can be interpreted without its help. Some implications of this insight are discussed and illustrated.

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I intend it as a compliment to say that I do not regard Charles Goodhart as a modern macroeconomist. From the very beginning of his career, with his 1962 Ph. D. thesis on the activities of the New York money market at the beginning of the last century, (Goodhart 1968) until now, with his work on the Wicksellian nature of modern monetary systems (Goodhart 2002) Charles' economics has been informed by a deep understanding of the history of monetary institutions and policy, and of the economic ideas that have underlain that history. It might appear paradoxical that this very characteristic of his intellectual equipment, which to many would seem to have suited him only for a life in the ivory tower, has also made him an important and effective policy advisor. But, as I hope to show, an approach of the type that Charles has always taken to economics is as productive and practical as it is unfashionable

The Status of the History of Thought within Economics

The title of a recent article by Mark Blaug (2001) succinctly and accurately describes the current status of the History of Economic Thought among economists: "No History of Ideas Please, We're Economists". One of the most distinguished of the younger generation of macroeconomists, Paul Romer, expresses the attitude that prompted Blaug's title in the following way: "I guess I would describe ancestor worship as a research strategy as probably an unproductive one (*laughter).* But as a consumption activity it is something that can be fun." (Snowden and Vane 1999, p. 304). He explained his attitude as follows:

> ". . . it is very hard to tell, quite frankly, when you go back and read economics that is stated in purely verbal terms. There is always the danger that you read between the lines and say, oh, they had it exactly right - here is this mathematical model which shows what they were thinking. But that is usually based on a charitable reading and one that ignores some of the ambiguities and confusions. I wrote a paper like that at one time interpreting Allyn Young's paper . . ." (p. 304)

Romer takes it for granted that anyone can read older literature without help or training, and that the only productive role for such an activity is as a source of ideas for mathematical formulation. Both presumptions present serious pitfalls, as some of Romer's own work (1991), illustrates.[1] In discussing the historical background to modern endogenous growth theory, he begins by telling his readers that

> "Adam Smith put two propositions at the center of economic theory. The first was that competition allocates the preexisting stock of productive inputs in a way that is wealth maximising. The second was that an endogenous process of accumulation and investment was the fundamental force that led to increases in the stock of these inputs, and, therefore, to growth in income and wealth. " (p. 83)

Now these two propositions, and their reconciliation, are certainly central to Romer's own work but it is unlikely that Adam Smith would have summarised the message of the *Wealth of Nations* in quite this way.

Older texts were not written for us, but for their authors' contemporaries. Words and phrases change their meaning over time, knowledge that we now take for granted, and in whose light we might be tempted to interpret works, has not always been current, while commonly held ideas that once provided a context for the discussion of economic issues have got lost, and so on. Reading older texts is tricky even for the specialist, let alone the economist whose primary interest lies elsewhere. That is why it was once thought essential to train all economists in this skill. Furthermore, though modern economists sometimes revisit old themes unaware, to suggest that there is no point in studying the subject's history unless it provides insights for further theoretical development is to adopt an extremely narrow point of view.

In this paper I shall consider two other uses to which the history of thought is more

[1] This may be the paper to which Romer refers in the preceding quotation. Certainly, it contains a discussion of Allyn A. Young's 1928 paper on "Increasing returns and economic progress" which anticipates many of the themes of modern endogenous growth theory.

frequently put. I shall discuss the rhetorical use that certain modern monetary economists make of the older literature when they expound their own ideas, and then I shall take up the part played by the history of economic thought in interpreting the experience which forms the empirical basis of our subject. I shall conclude that mainstream economists fail to appreciate the importance of the History of Economic Thought as a sub-discipline in its own right, and that this has a debilitating effect on economics as a whole.

The History of Thought as a Rhetorical Weapon

The History of Economic Thought has received more than passing attention from those who have developed the most influential strand in modern macroeconomics, namely New classical theory. Robert Lucas, Thomas Sargent and their associates have claimed to be re-instating an approach to economics whose pedigree runs back to the 18th century, and whose dominance in the subject was only temporarily interrupted by the "Keynesian Revolution". It is worth quoting Lucas and Sargent (1978) at some length on the approach to economics that Keynes was alleged to have subverted, and on the nature of the alternative he offered.

> "Before the 1930s, economists did not recognise a need for a special branch of economics, with its own special postulates, designed to explain the business cycle. Keynes founded that subdiscipline, called "macroeconomics," because he thought explaining the characteristics of business cycles was impossible within the discipline imposed by its insistence on adherence to the two postulates (a) that markets clear and (b) that agents act in their own self-interest. The outstanding facts that seemed impossible to reconcile with these two postulates were the length and severity of business depressions and the large scale unemployment they entailed. A related observation was that measures of aggregate demand and prices were positively correlated with measures of real output and employment, in apparent contradiction to the classical result that changes in a purely nominal magnitude like the general price level were pure unit changes which should not alter real behaviour.

> After freeing himself of the straightjacket (or discipline) imposed by the classical postulates, Keynes described a model in which rules of thumb, such as the consumption function and liquidity preference schedule, took the place of decision functions that a classical economist would insist be derived from the theory of choice. And rather than require that wages and prices be determined by the postulates that markets clear - which for the labor market seemed patently contradicted by the severity of business depressions - Keynes took as an unexamined postulate that money wages are sticky, meaning that they are set at a level or by a process that could be taken as uninfluenced by the macroeconomic forces he proposed to analyse" (Lucas and Sargent 1978, pp. 304-305)

As a description of the approach to cycle theory that Lucas and Sargent were trying to establish in 1978, and of its contrast to what, in the textbooks of the 1970s, was passing for "Keynesian economics", and making due allowance for the polemical nature of the paper in which it appeared, this passage is fair enough. But it purports to be, not a summary of a new research agenda, but an account of the history of an important branch of economic thought. Judged as such, and to put it kindly, it leaves much to be desired: the business cycle, let alone the credit cycle, was a mainstay in the literature of economics from the early 1860s onwards; those who wrote about it took it for granted that markets didn't clear; some, who forged what Axel Leijonhufvud (1981) called "the Wicksell connection" located the problem in the capital market, and others, notably Marshall and Pigou, argued that wage stickiness, the reasons for which were extensively examined, both played a part in market failure, and underlay the fact that prices and output moved together; economists who insisted on decision functions derived from the theory of choice as a basis for cycle theory were few and far between before 1936; in the *General Theory,* which was not about the cycle, Keynes explicitly denied that his theoretical results depended upon wage stickiness; etc.[2]

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[2] I have discussed all of these matters extensively in Laidler (1991, 1999), but in no case do I claim to have been the first commentator to have pointed out the often stark contradictions between the actual history of particular ideas and modern myths about them. Michael Woodford

Nor is this passage an isolated example of the misrepresentation of the history of economics by modern monetary economists. Lucas himself (1996) returned to the discussion of the history of classical and neo-classical monetary economics in his Nobel Lecture along lines related to those which he and Sargent had followed in (1978). Blaug (2001) accurately tells us that he treats of David Hume and his successors (all the way down to Patinkin) as "want(ing) to think in general equilibrium terms in which people are conceived as maximising over time" but "resorting to loose equilibrium dynamics because the analytic equipment available to them offered no alternative" and comments as follows: "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". (All quotations from Blaug, p. 155)

Another example of the rthetorical use of the History of Thought in modern macroeconomics is Sargent and Wallace's (1982) claim that, in proposing the "overlapping generations" model as a potentially fruitful basis for a general monetary theory, they were also reviving and throwing new light on 18th and 19th century debates about the quantity theory and the real bills doctrine. They told their readers that the real bills doctrine "asserts that unrestricted intermediation either by private banks or by a central bank has beneficial economic effects and should be promoted by public policy" (p.1212), a proposition that their own theoretical results supported. They thereby appropriated a well established label for the quite different hypothesis that *if bank money is always issued by way of discount of good quality short-term commercial paper issued to finance inventories and goods in process, variations in its quantity can impart neither inflationary nor deflationary impulses to the economy*. Since this latter hypothesis has been associated with the policies of the Reichsbank during the Weimar hyper-inflation and the Federal Reserve system during the great contraction of 1929-33, among other episodes, the semantic confusion thus introduced has not been a minor matter for anyone trying to teach a little history to students who are simultaneously being "well trained" in modern monetary theory.

(1999) provides a balanced and carefully nuanced account of the evolution of macroeconomics in the 20th century, written from the viewpoint of a modern practitioner.

Now none of this is intended as covert criticism of the *substance* of modern macroeconomics. Any body of ideas must stand or fall, not by the status of its intellectual antecedents, but by the conformity or lack thereof of its predictions to empirical evidence, and by its logical coherence. But the application of such tests, and the interpretation of their results, do not occur in an intellectual vacuum. Theories are not judged to be good or bad in isolation in economics, but better or worse in relation to other theories. If already existing literature is misrepresented by the exponents of a particular body of new ideas, and their readers accept their misrepresentations as accurate, then that body of ideas gets more than a head start in the race to become established. Thus, though (*pace* Arjo Klamer and Deidre McCloskey) modern economics is more than a series of "good conversations" the effectiveness of the rhetoric that is deployed in support of particular points of view does effect the way in which questions of substance are settled.[3]

There is nothing new in macroeconomics about the self-conscious use of rhetorical devices to promote an intellectual cause. The *ad hominem* element in Harry Johnson's (1971) Ely Lecture on "The Keynesian Revolution and the Monetarist Counter-Revolution" tended, at the time of its publication, to distract attention from its subtle and rather general discussion of the way in which debating tactics had helped to shape the development of macroeconomics in the previous thirty five years or so. And though the deployment of ideas, purportedly drawn from the history of economic thought, to lend a little authority to the case for a contemporary piece of doctrine was by no means the only weapon in the rhetorical armoury that Johnson described there, he drew attention to the way in which competing and dubious versions of the history of ideas had been deployed in the debates he was discussing. As he noted

> "The Keynesian Revolution derived a large part of its intellectual appeal from the deliberate caricaturing and denigration of honest and humble scholars, whose only real crime was that they happened to exist and stand in the way of the success of the

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[3] To avoid misunderstandings, let me explicitly note that I don't think Blaug (2001) would disagree about this.

revolution. The counter-revolution had to endow these scholars, or at least their intellectual successors, with a wisdom vastly superior to what their opponents had credited them with. *Obiter dicta* and an oral tradition are at least a semi-legitimate scholarly means to this polemical end" (pp. 198-9)

With its final sentence removed, this passage could very well stand as a comment on the two paragraphs from Lucas and Sargent (1978) quoted above, but the counter-revolution to which Johnson referred was Friedman's monetarist counter-revolution, the *obiter dicta* were those of "the great neo-classical quantity theorists" which had been "combed. . .. for any bits of evidence that showed recognition . . . of the fact that a decision to hold money involves a choice between holding money and holding wealth in other forms, and is conditioned by the rates of return on other assets" (p.198), while the "oral tradition" was one associated with the University of Chicago "that was alleged to have preserved understanding of the fundamental truth among a band of the initiated through the dark years of the Keynesian despotism" (p. 198).[4]

It is nevertheless worth recalling that Johnson offered not just criticisms of Friedman's views on the history of thought in 1971, but of the substantive content of his monetarism too. In Johnson's view, Friedman had failed to come to grips with how the effects of monetary shocks divided themselves up between output (and/or employment) and prices, and had relied too much on the methodology of positive economics and too little on general equilibrium theory. Both of these points carried considerable weight, and it is surely no accident that they were directly met

[4]Friedman's (1956, 1974) claims about the "Chicago tradition" have been much debated over the years, and some of them, particularly those made in 1956 about the allegedly unique role played in that tradition by a stable demand for money function, have not withstood careful examination. But Friedman's (1974) argument, that the Chicago Department was home to a lively group of economists who discussed the great contraction in monetary terms and did advocate expansionary monetary and fiscal policies to combat it long before Keynes wrote the *General Theory,* was better grounded. Even though subsequent research has shown this version of the tradition to be neither as homogeneous, nor as uniquely associated with Chicago, as Friedman claimed, Johnson's characterisation of it as the product of "scholarly chicanery" was at the very least uncharitable and overdone.

by the very features that distinguish New classical economics from monetarism: namely, its embedding of the rational expectations hypothesis in a Walrasian framework to produce a short-run aggregate supply curve, along which output, and therefore employment too, respond to surprise price level movements.[5] In highlighting these features, however, New classical economics rendered the Chicago quantity theory tradition of the 1930s, which lay in the mainstream of what Joseph Reeve (1943) termed "fiscal inflationism" as a cure for the depression, irrelevant as an ancestry from which it could draw academic respectability. This perhaps explains why Lucas and Sargent laid claim to a very different and altogether broader set of intellectual antecedents for their ideas.

Rational Expectations and True Models

I did not single out Lucas, Sargent and Wallace for particular attention in the preceding section of this paper without a constructive reason. It was they, above all, who showed the rest of us how fertile John Muth's idea of "rational expectations" could be in simultaneously bringing extra rigour to the logic of macroeconomic theory and empirical novelty to its predictions.

As Sargent and Wallace (1973, p. 328) explained it, this hypothesis held that expectations about any variable's behaviour would "depend, in a proper way, on the same things that economic theory says actually determine that variable". Agents were assumed to have the same level of understanding of any model's behaviour as the economist constructing it. The "money-supply surprise" class of models of cyclical fluctuations that was so popular in the 1970s, and in whose support Lucas and Sargent (1978) deployed the brief history of macroeconomic thought quoted earlier, postulated that agents all believed in the quantity theory of money, had information about the time series processes underlying the behaviour of the money supply,

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[5]James Tobin (1981) referred to the money-supply surprise model at the core of early New classical economics as "monetarism mark 2", and inasmuch as it represented a response to Johnson's challenge to Friedman, this classification is defensible. My own preference, however, has always been to regard its reliance on Walrasian general equilibrium analysis as setting it apart from any earlier macroeconomic doctrines.

formed their expectations accordingly, and then acted on them when interpreting and responding to changes in the nominal prices ruling in the constantly clearing markets in which they were active.

When these models were tested empirically, they were found to be severely wanting.[6] The work that undermined them typically used post-Second-World-War data, usually drawn from the US. During the period over which those data were generated, however, to the extent that there was any dominant theory of price level behaviour within the economics profession, let alone among the public at large, it had nothing to do with the quantity theory of money. Rather it was an eclectic amalgam of "cost-push inflation" ideas, overlaid, from the early 1960s onwards with the idea of an inflation-unemployment trade-off that might be susceptible to "demand-pull" forces; and among the latter forces monetary policy was but one of many. Thus, the economic ideas of the agents whose behaviour underlay the data that the exponents of money supply surprise models were trying to explain were anything but homogenous and bore no resemblance at all to the economic ideas attributed to them in those models.

The point here is quite general. Modern macroeconomics, like all of the sub-discipline's earlier incarnations, builds models which its exponents hope are "true", or at least not proven to be false too quickly. Unlike earlier approaches, however, and this is its great advantage over them in the eyes of its exponents, it postulates that the agents who inhabit those models understand the economy in which they are operating as well as the economist who creates them,

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[6]Beyond doubt the main reason for the failure of money supply surprise models was their assumption that markets cleared instantaneously, with prices set "as if" by a Walrasian auctioneer, which implied that, in aggregate data, the price level would move in response to money supply changes, and that the level of output would respond to the portion of the price level change which had not been anticipated. But it is, and, in the early 1970s, it already was, one of the best known stylised facts in the history of macroeconomics that quantity changes precede those in the price level. The first generation of money supply surprise models simply could not cope with this fact, and that is why those who work with their modern successors have contrived to bring various kinds of price stickiness assumptions back into the picture.

and condition their behaviour upon such knowledge. If we treat the history of economic thought with any degree of seriousness, however, three truths become apparent, and one conjecture becomes plausible: first, in the real world, it is economists, not agents in general, who specialise in creating models of the economy; second, at no time in the past have economists ever provided a single model with an undisputed claim to embody the truth; third, the models that they have provided have always evolved continuously in the face of logical and empirical criticism; and, finally, it seems unlikely that the future is going to differ from the past in any of these respects.

If economic agents do use economic theory to condition their behaviour, therefore, it is possible that, in the same economy at the same time, different agents will react differently to exactly the same information and incentives, and that, as time passes, those reactions are likely to change as the state of economic understanding changes. We should not, therefore, expect that what seems to be the "true model" of the economy for any particular time and place should necessarily be able to explain the behaviour of the economy at any others. Nor, if a model that has been successful in the face of one set of data fails when confronted with a second, should we conclude that its first application was necessarily faulty. If we take the principles used to model the creation and use of expectations that underlie modern macroeconomic theory seriously, that is to say, we seem to be pushed hard in the direction of concluding that empirical economics needs to be informed by a heavy dose of narrative history dealing with ideas, events and their interaction.

Economic Ideas and Economic Policy

Now economic agents in general do show a capacity to learn from experience, and to adapt their behaviour accordingly, but they do not act "as if" they know as much about the economy as the specialists who model it.[7] However, there is one group for whom a literal version of the rational

[7] Boschen and Grossman's (1981) finding that money supply movements already apparent in published United States data seemed to have subsequent consequences for real economic variables, but that those which were hidden by initial measurement and reporting errors did not,

expectations hypothesis is more plausible: namely, policy-makers. They routinely have access to professional advice on specific issues, though the fact that there are usually differences of opinion among economists at any time, implies that they have to choose which advice to take. But their actions can, nevertheless, often be clearly related to specific economic ideas.

The extent to which ideas in general and economic ideas in particular influence economic life and economic policy has long been contentious. On the one hand we have Keynes's well known, and extremely comforting for economists (among others), testimonial:

> ". . .the ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed the world is ruled by little else. Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back" (1969, p. 383)

On the other hand, we have George Stigler's less flattering view that, in a world where policy, like everything else, is driven by the pursuit of self-interest, the support of intellectuals ". . .is available to the highest bidder, just as other resources in our society are allocated" (1982, p. 32), a view that he elaborated as follows:

> "That intellectuals should believe that intellectuals are important in determining the course of history is not difficult to understand. The position is less easy for even an intellectual economist to understand, since it sets one class of labourers aside and attributes special motives to them. On the traditional economic theory of occupational

is decisively inconsistent with this view; Jonung and Laidler (1986) found that inflation expectations as revealed by Swedish survey data, though unbiased relative to the true variable, could nevertheless have been systematically improved had agents made use of information, such as, for example the lagged unemployment rate, that was already published and readily available to them.

choice, intellectuals distribute themselves among occupations and among artistic, ethical, cultural, and political positions in such numbers as to maximise their incomes, where incomes include amenities such as prestige and apparent influence." (1982, p. 34)

The old Meade-Tinbergen approach to policy analysis enjoined the policy-maker to assign weights to various policy goals, and then deploy policy tools so as to maximise the resulting social utility function, subject to the constraints implied by a model of the economy, and it may be reformulated as a positive explanation of how policy makers actually behave. In this guise, it points us to a middle ground between Keynes and Stigler. Economic ideas are used by policy-makers, even when they are self-interested, not just in defence of their policies, but in their design as well, and economics has the task of providing those ideas.

As the economic ideas available to policy makers change, then, so perhaps will their conduct, and Keynes can be right about the importance of ideas, even if the services of those who generate them are, as Stigler suggests, for sale to the highest bidder. When there is a conflict between competing ideas about how the economy works, moreover, the conduct of policy will depend upon the choice of ideas made by those in charge of it. If economic policy is important to the way in which economies behave over time, then, anyone wishing to understand economic data must pay attention to the nature and evolution of the ideas that have underlain policy. In fact, economists often do just that, as I shall now illustrate with reference to work on the Great Contraction of the early 1930s and the Great Inflation of the 1970s.

Two Illustrations: the Great Contraction and the Great Inflation

The Monetary History of the United States (Friedman and Schwartz 1963), and particularly its chapters on the interwar years, is arguably the most influential book written on a macroeconomic topic in the second half of the twentieth century. Anachronistic in approach and technique even at its time of publication, it nevertheless broke the influence of the so-called "Keynesian consensus" about how market economies functioned in general, and had functioned in particular during the 1930s, and it reoriented economists' attention towards the power of monetary forces in a way that still marks the discipline.

It was not, however, Friedman and Schwartz's time series charts of the behaviour of the money supply and related variables that convinced their readers. Rather it was their detailed examination of the evolution of Federal Reserve policy in the 1920s and 1930s, of the way it was affected by the particular personalities of, and interactions among, those who were making it, and of the extent to which it eflected the climate of opinion in which they operated. It was narrative history, rather than state-of-the-art theory and econometrics, that so profoundly changed the views of economists in the 1960s and '70s: strong testimony to the importance then attached to the analysis of the interaction of economic ideas with events as a means of bringing empirical evidence to bear on the assessment of economic theories.

In outlining the causes of the contraction, Friedman and Schwartz gave pride of place to ". . .a sequence of more or less accidental events and the running conflict for power within the [Federal Reserve] system". In their view, the death of Benjamin Strong, Governor of the New York Bank, in 1928, and a partly consequent shift of power over open market operations in late 1929 from "a 5-man committee dominated by the New York Bank" to a "committee of the 12 Federal Reserve Bank governors" were of critical importance. This latter shift decisively reduced the influence of the New York Bank, where, "[d]espite the general climate of opinion, the technical personnel . . . were consistently in favor of the policies which seem to us in retrospect the ones that should have been followed" and "stacked the cards heavily in favor of a policy of inaction and drift". Crucially, Friedman and Schwartz concluded that the abovementioned climate of opinion was "certainly a necessary condition" for policy to evolve as it did. (all quotations pp. 593-4), and they described it as one which

> ". . . in the main regarded recessions and depressions as curative episodes, necessary in order to purge the body economic of the aftereffects of its earlier excesses. The prevailing opinion also confused money with credit; confused the elasticity of one component of the

money stock with the elasticity of the total stock; regarded it as desirable that the stock of money should respond to the "needs of trade," rising in expansions and falling in contractions; and attached much greater importance to the maintenance of the gold standard and the stability of the exchanges than to the maintenance of internal stability. Most of these attitudes characterized the public at large and not merely the financial community or the Reserve system in particular" (p. 692)

It is worth emphasising that Friedman and Schwartz did not claim that these erroneous economic ideas were universally held, nor that their existence was a sufficient condition for the depression to occur. But they did argue that their prevalence among those who had the upper hand in debates among policy makers had a decisive influence on what was actually done and not done. Furthermore, the contrast that Friedman and Schwartz drew between those ideas, and their own explanation of events based upon a version of the quantity theory of money, was one of the key elements that made their claims about the importance of monetary factors in precipitating the contraction so convincing.[8]

The publication of the *Monetary History* coincided with the beginning of another great monetary upheaval: namely, the inflation that started with the Kennedy administration's pledge to "get America moving" and slowly gathered speed in the later 1960s as the Vietnam War and President Johnson's War on Poverty were financed by money creation. This inflation finally came fully into view after the Nixon administration, encountering the constraints imposed upon even United States monetary policy by the Bretton Woods System, abandoned it, along with the

[8]I do not mean to imply that I regard Friedman and Schwartz as having had the last word on these matters. For example, Barry Eichengreen's (1992) work on the role of the gold standard in the international transmission of monetary disturbances in the inter-war years is an important addition to theirs, and shifts the emphasis away from purely domestic United States considerations in what is surely a helpful and constructive way. On the question of the economic thought of the period, it is now widely agreed that they paid insufficient attention to the views of such economists as Allyn Young and Laughlin Currie. See Laidler and Sandilands (2002, forthcoming) on this, and Laidler (1993), which also gives extensive references to the earlier literature dealing with it.

last vestiges of the gold standard. This complicated episode has recently begun to attract the attention of macroeconomists. Bradford De Long (1997) and Thomas Mayer (1999) have recently analysed it along lines similar to those earlier followed by Friedman and Schwartz, and they also reach similar conclusions about the role of economic ideas: namely that the dominant climate of economic opinion did not cause the inflation, but did provide a necessary condition for it to begin and in due course accelerate. Mayer summed up this episode as follows:

> "It would be nice if one could tell the story of the Great Inflation along the lines of a Victorian melodrama by identifying a single villain, preferably someone at whom the audience enjoys hissing. But such a story does not fit the facts. there were several villains, and the biggest one turns out to be then [sic] prevailing views of economists, and not malicious political interference with the central bank, or cartel imposed supply shocks. We have met the enemy and he is (or rather was) us." (Mayer 1999, p.117)
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Patrick Minford (2000) has suggested that this conclusion is quite consonant with Thomas Sargent's (1999) recent econometric work on monetary policy during the great inflation. Sargent, however, states his view of the role of economic thought in the episode as follows:

> "In 1960, Paul Samuelson and Robert Solow found a Phillips curve in the U.S. time series for inflation and unemployment. They taught that the Phillips curve was exploitable and urged raising inflation to reduce unemployment. Within a decade, Samuelson and Solow's recommendation was endorsed by many macroeconomists and implemented by policy makers." (Sargent 1999, pp. 2 -3)

The trouble with this story is that Samuelson and Solow (1960) did not urge raising inflation to reduce unemployment, and that policy makers had begun to follow no such recommendation by 1970. As De Long and Mayer document in some detail, their views were altogether more complicated and confused than Sargent would have it.[9]

To begin with, as the then definitive Bronfenbrenner and Holtzman (1963) survey of inflation theory demonstrates, the idea that inflation was largely a "cost push" phenomenon on which monetary policy could exert little or no influence was widely held in the 1960s and early 1970s. De Long (p.262) points out that among its adherents was Arthur Burns who succeeded William McChesney Martin as Chairman of the Board of Governors of the Federal Reserve System at the end of 1969. This view of inflation was complemented by the ideas that monetary policy's main influence was on output and employment, and that inflation was best tackled directly by way of wage-price "guideposts", or even by outright controls. Its popularity meant that the attribution of rising inflation in the second half of the 1960s to monetary expansion associated with war (in Vietnam and on Poverty) finance was very much a minority viewpoint at the time. The same view underpinned the Nixon wage-price control programme of 1971-74, which was accompanied by a fiscal and monetary policy maybe "looser . . .than otherwise would have been put in place", and which ended with inflation rising rapidly to double digits when controls were lifted.

De Long remarks, with respect to the 1971-74 episode, "Perhaps the policies adopted truly were prudent and optimal given the consensus understanding of the structure of the economy held by both public- and private-sector decision makers. But this consensus understanding was flawed" (p. 267); and, as he might have remarked but didn't, those policies had absolutely nothing to do with deliberately inducing higher inflation in order to reduce the

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[9]Sargent defends his interpretation of Samuelson and Solow's views on the basis of "reading p.192 of their paper". He acknowledges that qualifications to his interpretation appear on p. 193, but suggests that readers consult Chapter 10 of his book before "thinking that I treat Samuelson and Solow unfairly" (p. 2, fn. 3). My own suggestion would be that readers also consult pp.192-3 of Samuelson and Solow's paper, or Abba Lerner's (1960) original discussion of it, which gently chides them for not recommending the purchase of less unemployment with more inflation.

unemployment rate.[10]

Not that unemployment was unimportant in the policy calculus during the Great Inflation; on the contrary, De Long, and Mayer too, though the latter less forcefully, make a good case that the 1930s cast a long shadow over policy in the 1960s and ‘70s. But, as De Long and Mayer also show, it was not until the Nixon wage-price control programme failed, and inflation reached double digits that these concerns ceased to manifest themselves mainly in a search for policies that would “shift the Phillips curve” to the left, and began to give rise to an explicitly expressed reluctance to use monetary policy to reduce inflation because of the perceived unemployment costs of doing so.[11]

This later development too has roots in the evolution of economic understanding. Samuelson and Solow did indeed present the Phillips curve as a policy menu in 1960, but that was only one ingredient to what Phelps (1972) would later call “The Cost-Benefit Approach to Monetary Planning” that became influential for a while in the 1970s. The idea of maximising a social utility function in “Okun gaps” and “Harberger triangles” subject to a Phillips curve constraint had to be developed (Reuber 1962, 1964), popularised (Johnson 1968, Tobin 1972) and dynamized (Phelps 1967, 1972) before that could happen. And John Taylor’s (1997) suggestion, made in the course of a commentary of De Long’s paper, that the arrival shortly thereafter of the rational expectations idea was “influential in changing views both about the costs of reducing inflation and the costs of inflation itself” (p. 279) towards the end of the decade is surely plausible.

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[10] See De Long 1997, p. 266-7, and Mayer p.119 on these matters. In this context it is worth noting that Sargent does not mention this crucial episode, even to wonder whether it might have distorted the behaviour of the data on which his econometrics were based.

[11] A recent study by Orphanides (2001) estimates forward-looking Federal Reserve policy reaction functions for the 1970s and 1980s, and finds that a larger sensitivity to unemployment, rather than a smaller sensitivity to inflation forecasts, is the main factor distinguishing the earlier period.

Now the Great Depression and the Great Inflation are by no means the only episodes in monetary history that have been, or will be subject to study along the lines described above. Historians of economic thought and economic historians have long been teaching their students about the first two great monetary debates of the 19th century - the Bullionist and Banking-School - Currency-School, controversies, and their treatment has usually dealt, not only with how versions of the quantity theory of money and the real bills doctrine were deployed in the intellectual arena, but also how those competing doctrines influenced and were influenced by the actual conduct of policy. The later controversy about Bimetallism is susceptible to similar treatment, with the declining influence among economists of the classical cost-of-production theory of value in the face of marginal utility and supply and demand analysis, forming a vital background to the evolution of the views of "practical men" about metallic and managed currencies. And, in due course, as the creation of the Euro passes into history, the economic debates that led up to and accompanied this event will also be studied as a necessary guide to understanding the evidence about the workings of the monetary system that it will generate.

Concluding Comment

Historians of economic thought have routinely taken an approach to their work that is broadly consistent with what modern macroeconomic theory has to say about the role of ideas in conditioning economic behaviour. Perhaps they should take as much pleasure from the knowledge that this is so as did M. Jourdain from the discovery that he had been speaking prose all along, and find therein ample compensation for the discomfort that they feel when they see their subject being used, or misused, as a rhetorical device to justify the claims of particular economic ideas to professional attention. There is, after all, as we have seen, nothing new about this latter practice.

Even so, despite its continuing and widespread use for both persuasive and substantive purposes, there are grounds for disquiet about the current status of the history of thought in modern economics. The fact is that Lucas and Sargent's rhetorical deployment of historical arguments, discussed above, attracted far less attention and generated far less debate than did Friedman's earlier excursion into the area. This is not surprising, for Friedman's main critics were Don Patinkin (1969) and Johnson (1971), each of them a major figure in monetary theory in his own right, but each of them, by virtue of his training and understanding of the nature of the sub-discipline, well steeped in its history, and convinced of the importance of that history for understanding contemporary issues. Nowadays "ancestor worship" is regarded by leading figures of the discipline as no more than an entertaining consumption good, so there is no professional gain to be had from critically examining any claims that might be made about the historical legitimacy of contemporary doctrines. Nor is there much point in graduate students taking courses in the area, and in departments offering them.

There is surely something amiss about the History of Economic Thought being used as a rhetorical weapon, in support of the intellectual credentials of their own work, by the same leading figures who denigrate the field's importance. The skills that would enable their intended audience to assess such arguments on their merits are, increasingly, not being taught. All this, when the substantive theoretical ideas that are defended by these means themselves imply that a knowledge of the economics of the past is critical to disciplining the empirical application and further development of those very ideas. In neglecting the History of Economic Thought, and discouraging its study, modern macroeconomic theorists are turning their backs on a tool of considerable potential, while simultaneously creating an intellectual monopoly for themselves in its rhetorical deployment. A quiet life may be the best of monopoly profits, but it will not be conducive to the longer term health of economics if they are allowed to enjoy such a life for much longer.

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