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by

**David Laidler**

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## **History of Economic Thought's Place in Macroeconomics Revisited\***

**by**

**David Laidler**

**Abstract:** The History of Economics Society was founded at a time when the History of Economic Thought was being expelled from the Economics post-graduate curriculum in many universities, and was one of the key institutions around which the sub-discipline successfully re-organised itself and continued to develop. Laidler (2003) argued that Economics itself, especially Macroeconomics, was suffering serious damage from this expulsion. It has continued to do so since.

**Key Words:** History of Economic Thought, Macroeconomics, New Classical Economics, Empiricism, Deductivism.

**JEL Classifications:** B2, B22, B41, E10.

\*This brief paper has been written for a special issue of the *Journal of the History of Economic Thought* to be published in December 2024, marking the 50<sup>th</sup> anniversary of the founding of the History of Economics Society. Mauro Boianovsky, Peter Howitt, Susan Howson, Lars Jonung and Roy Weintraub have commented helpfully on earlier drafts, but only the author is responsible for its contents.

## I

I attended those now famous conferences at Sussex in 1968 and Nottingham in 1969 which preceded the formal founding of both the Society for the History of Economics (SHOE) and the History of Economics Society (HES). In 1969, I also helped to found the UK Money Study Group at its first conference in Hove, while in 1970 I was at Karl Brunner's first Konstanz Seminar. In both of my fields, these conferences were followed by many, many, more. At that time new specialist journals were also appearing, beginning with the *Journal of Money, Credit and Banking (JMCB)* in 1969, where I had a paper in the second issue, and *History of Political Economy (HOPE)*, also in 1969, to which I was not an early contributor. Ironically, given Lionel Robbins' still notorious attack on this proposed journal at Sussex, to which I shall return below, my first completed research paper in the area - on Thomas Tooke (Laidler 1972) - was already committed to his forthcoming Festschrift, and I had nothing else with which to try my luck.

In hindsight, it might look as if my two fields were proceeding along similar tracks in those years, driven by the same forces. To an extent that's right. I belong to that small Depression-and-WW2 cohort which, upon graduation, met the first of the baby-boomer masses demanding scholarly services. Between the start of my B.Sc. (econ) in 1956 and the completion of my Ph.D. in 1964, Sputnik had been launched, closely followed by the National Science Foundation, and the UK government had followed Lionel Robbin's advice (better in this instance) to expand the University Sector, if not quite in ways he had intended. Lots of academic jobs with reasonable teaching obligations and ready access to research funds had therefore materialised in English-speaking universities. This was especially so in the sciences, and Economics, having managed to get itself so classified, was expanding rapidly and becoming more professionalised and internally specialised as it did so.

But Craufurd Goodwin's ambitions for HET were not quite the same as Karl Brunner's for Macroeconomics when they prepared the first issues of their journals. The *JMCB*'s foundation was yet another step in an already confident campaign, on the part of those whom Axel Leijonhufvud (1973) would soon dub "Macros", to achieve equal status within Economics with the "Micros". But a significant goal motivating *HOPE*'s founding, and a little later, those of SHOE and the HES, was the stabilisation and refocussing of a field that, even to a beginner like me, seemed to be slipping down the discipline's pecking order. Some clues that I had noticed by 1968: HET occupied a central and required place in my undergraduate curriculum in Economics, Analytic and Descriptive at LSE in 1956-9, but when I arrived at Essex in 1966, I found that the started-from-scratch-in-1963 honours degree course had room for just six lectures a year in the area, not to be examined and only to be given if there was someone around (such as me) wanting to do so; at Chicago, where some exposure to HET was still mandatory in the Ph.D. program, I chose to over-fulfill this requirement by taking a Field Examination in the area, but I was the only student to do so in 1962; and when, as an Assistant Professor at UC Berkeley in 1965, I confessed to a senior visiting colleague (Michael Farrell from Cambridge, I think) that I was working on Tooke's views on monetary policy, his response was to ask me how I thought the resulting paper would contribute to my candidacy for tenure in that department.

## II

As an LSE undergraduate, I also studied Scientific Method with Karl Popper, and the majority of those who actually taught me Economics, those who took the classes and seminars rather than delivering the big

lectures (and were later my colleagues in 1961-2), were also leading members of the Methodology, Measurement and Testing (M2T) seminar. I was encouraged from the outset, then, not only to learn how to do Economics, but also to think systematically about how it was done, and how it might be done better. The Chicago department, on the other hand, insisted that its graduate students first learn how to do Economics according to already established local practices before worrying themselves about such questions. There is a lot to be said for this “boot camp” method of instruction, and it worked wonders for my future in Macroeconomics. But it cannot work for HET, where the interpretation and evaluation of the work of others, rather than its imitation, is of the essence. So, when debates within the sub-discipline about whether it should break free of Economics and find a place in the History of Science, or intellectual history more generally, developed in the ‘80s and ‘90s, I was able to follow them, more or less. But, for a long time, I found the prospect of writing about how to write about economics too daunting to attempt on my own account. Being pre-occupied throughout the ‘90s with trying to unravel the story of the Keynesian Revolution (See Laidler 1999), I didn’t pay close attention to them. When I eventually strayed into this territory, inspired by Mark Blaug’s example (cf. Blaug, 2001) things did not go well.

In 2001, I presented a paper entitled “The Place of the History of Economic Thought in Modern Macroeconomics” (Laidler, 2003) at a Bank of England conference in honour of my friend Charles Goodhart, an economist with the same out of fashion historical leanings as mine. I didn’t expect it to go down well with many of the Macros who attended that event, and I was not disappointed. But I was taken completely by surprise by the paper’s even more hostile reception when I presented it at the 2002 HES Conference in Davis. My discussant there was Roy Weintraub, a colleague with whom I had, as I still do, excellent personal rapport. But, I was completely unaware at the time that Roy had recently finished editing *The Future of the History of Economics* (Weintraub (ed.) 2002). Most readers will be familiar with this still important volume and his introduction to it. So, though Roy has no recollection of our encounter on that day, they will have no problem accepting my version of his reaction to the very idea that HET could have a place in any kind of economics, no matter what its prefix. His approach to my paper was respectful and courteous, but in the manner of a matador’s to a bull, and it was duly and humanely dispatched. I was left perplexed, not just immediately, but for a long time afterwards.

### III

When, in January 1968 at Sussex, Lionel Robbins threatened to forbid his students to submit their work to the proposed specialist journal which would become *HOPE*, it was because he believed that the separation of HET from Economics that its launch might encourage would have dreadful consequences for the discipline. Lionel hated provincialism of any sort, but he believed what he called “provincialism in time” to be its very worst manifestation. In his view, Economics was Economics, no matter when it had been written, and the only relevant question was whether it was worth reading. He offered destructive advice in 1968, to be sure, but from the best of motives. What he had missed, perhaps because by then he had been out of day-to-day touch with academic Economics for too long, was that the decisive forces driving the separation he feared were not coming from the historians. If HET had not begun to strengthen its own separate identity when it did, economists would still have pushed it out of the way in the decades that followed. So it’s just as well that the sub-discipline looked after itself.

The expulsion of HET from economics was the by-product of a particular episode in that perpetual debate about what sort of science Economics is. An answer to this question whose popularity was already

growing in the '60s (not unrelated to the discipline's standing with the NSF, and to the considerable boost this standing received in 1968 when The Bank of Sweden established a Nobel Memorial Prize in Economic *Science* rather than plain Economics or even Political Economy), was that Economics was just like the natural sciences. Such sciences were said to make steady progress over time, with old ideas that were still good ideas being retained in an accepted body of knowledge that was continuously being purged of bad ideas as new potentially good ideas replaced them. It followed that, intrinsically interesting though HET might be, knowledge of it was not essential to the training of economists. Ever more mathematics was undeniably becoming necessary at this time, something had to make room for it in the post-graduate curriculum, and HET was the obvious candidate.

These developments interacted with the swing away from the empiricism of Friedman (1953) and the M2T group and back towards Robbinsian *a priori* deductivism (irony unavoidable, cf. Robbins 1932) which followed the launch of the so-called New Classical Revolution by Robert E. Lucas Jr. (1972). In its new and more extreme version, this approach – formulate clear assumptions about (alleged) “fundamentals” (tastes, endowments and technology), “rules of the game” (clearing markets and rational expectations), and then carry on maximising - became central to the Macros' campaign for parity of status with the Micros. Walrasian general equilibrium theory was proclaimed to be the only “proper” micro-foundation for the sub-discipline. Thus, among other things, monetary exchange was tacitly declared irrelevant to the functioning of the market economy, an awkward implication already well understood before the 1970s (cf. Frank Hahn 1965), but nevertheless widely ignored. (cf. Laidler 2021).

The proposition that old ideas not included in the currently accepted body of economic knowledge are also bad ideas is open to testing against historical evidence. But the New Classical revolutionaries, true to their methodology, preferred to deduce a view about what the history of their sub-discipline must have been from its current state. In their story, with the important exception of a Keynesian detour of more political than scientific interest, it had been dedicated since the mid-18<sup>th</sup> century to the postulates that “markets clear and . . . agents act in their own self-interest” (Lucas and Thomas Sargent, 1978, p. 304) and progress made since then had been mainly technical (cf. also Lucas 1996, 2004). These ultra-Whiggish propositions overlooked the possibility that, as macroeconomic engines of analysis had become more and more refined, the area over which they were capable of searching for concrete truth might also have become dangerously smaller. But, occasionally illustrated with suitably selected examples of alleged early anticipations of current doctrines (for specific references, see Laidler 2003), they were assiduously propagated in the course of teaching contemporary Macroeconomics to a student body no longer being encouraged (to put it kindly) to acquire the systematic knowledge of the discipline's earlier efforts which they would need to question them.

By 2001 I had come to believe that these ideas were not just bad for Economics but a threat to the economy as well, because they were becoming the basis of policy. Analysis, soon to be codified by Michael Woodford (2003), that studied only situations where markets are cleared, and treated the quantity of money as a passively endogenous variable with no role to play in market processes, might suffice to guide monetary policy during the tranquil times of the “Great Moderation” but seemed unlikely to be adequate should macroeconomic conditions again become turbulent. Thus, the message I intended to convey in my paper for Charles' festschrift was that Macroeconomics badly needed help from HET, not that HET should put its head back into a noose from which it had only recently freed itself. But, Roy's

reaction suggests that I didn't make this distinction clearly enough. I think that the Macros at the London conference got the message though. That's why they also found it so disagreeable.

Even so, because HET's status within Macroeconomics, indeed within Economics in general, has only changed for the worse in the last two decades, and because policies based on still-currently mainstream macro-models did indeed help to create both the Great Recession of 2007-10, and the inflation of 2020-date (c.f. Laidler 2021), let me now briefly restate the case against the New Classical version of HET, as seen from the perspective of Macroeconomics. As in (2003), I make no claims about the extent to which the arguments that follow can be applied to micro areas, of whose current state and prior history I have insufficient knowledge to formulate a properly informed opinion.

#### IV

To begin at the beginning: the fact that a large part of Economics as a whole involves arguments that are supposed to follow the laws of logic and yield falsifiable predictions about the world around us does indeed seem to this former pupil of Popper to qualify it as "science". But this does not make it a science like the natural sciences. In particular, a notable feature of the phenomena Economics studies, briefly but clearly described by Kenneth Boulding (1965, pp.8-9), is that agents make use of their own knowledge of how the economy functions in formulating the plans that guide their behaviour, and that they can and do revise this knowledge in the light of experience, potentially revising their subsequent behaviour as a consequence. In modern Macroeconomics, an anaemically simple version of this idea is embodied in the Rational Expectations Hypothesis and its associated Lucas Critique, which envisage agents basing their plans on knowledge of the same *but true and therefore unchanging* model of the economy that the economist is deploying to analyse their behaviour. The observation that economic models are in fact always open to revision in the light of experience, no matter who is applying them, explains the fact obvious to any student of HET, that ideas about what constitutes a "true model" of the economy have been in a constant state of evolution and subject to continuous disagreement ever since economic thought began. And it also yields the implication, awkward for modern macroeconomists to contemplate for long, that today's models are at best empirically relevant only to data generated since they were formulated. Empirical investigations going further back than this need to deploy older ideas as well as older data. This is one important reason why Macroeconomics needs HET.

HET's story of the evolution of both economic ideas and real world economics is not the New Classical one of unrelenting progress, then. Nor, however, is it a tale of random and independent processes. In the past, when the economy's performance has turned out to have had unexpected and apparently inexplicable features, established economic ideas have usually been called into question and revised, and the latter element in the evolution of economic ideas has thus followed a Popperian process of conjecture and refutation. But this is not the whole story. Because new conjectures can influence future outcomes of the processes being studied, ideas evolve by a more complicated and recursive interaction with experience than the straight application of Popper's ideas – at least as I understood them as an undergraduate – would lead us to expect. This pattern of development explains the important fact that useful ideas which nevertheless do not cover all possibilities can get lost when real world problems that they can't address appear. This is just as well, because HET is littered with examples of mislaid ideas being rediscovered and/or recreated from scratch. Consider what Christopher Dow (1964) referred to as the "nine times ninety" lives lived by the quantity theory of money since Copernicus first suggested it as an explanation



of price level behaviour five centuries ago, or on a lesser scale, the comings and goings of the Fisher effect, or of Wicksell's cumulative process, and so on. Another important reason why Macroeconomics needs HET, which, for example permeates Leijonhufvud's (1968, 1981) work, is that it stores old ideas that can be doubled back to if and when it turns out that they have been discarded prematurely.

Keynes' *General Theory*, far from being an irrelevant historical aberration, as the New Classicals would have it, fits perfectly into the above pattern. Its component ideas were all in the pre-existing literature, though they sometimes carried different labels, and the extent of Keynes's own previous knowledge of some of them is open to debate. Consider: effective demand (Ralph Hawtrey 1913); fundamental uncertainty (Frank Knight (1921), but see also Keynes 1921); the marginal efficiency of capital (Irving Fisher, 1907, 1930); animal spirits (Frederick Lavington (1922) and Arthur Pigou 1927); liquidity preference (Lavington, 1921); and the income multiplier (Jens Warming 1932). But in (1936), Keynes developed these components and linked them together into a new overarching framework that enabled him to explain, as none of those predecessors had been able to, what determined the level of employment. This framework, nowadays known as IS-LM, simplified by James Meade (1937) and given a geometric representation (as LL-SI) by J. R. Hicks (1937) was in due course used (surreptitiously before 1974) by Milton Friedman to launch Monetarism. Macroeconomics thus underwent a crucial change in (1936), but if *The General Theory* provided a basis for just about everything that came after it, it did so at least as much by extending ideas that had gone before it as by overthrowing them. This is why that phrase "The Keynesian Revolution" continues to make me uncomfortable.

V

Obviously, this reading of the sub-discipline's history flatly contradicts the New Classical version which first intruded on the teaching of Macroeconomics five decades ago. That the Macros were unable to see the merits of my case in 2001 because of the strength of their own commitment to a particularly flattering view of the scientific nature of their sub-discipline was not surprising. However, in 2002, I was puzzled, and indeed dismayed, by the lack of concern within HET about the damage that its expulsion from Economics was already inflicting on the discipline. However, some things do indeed come to those who wait. What was current Economics twenty years ago is now firmly in the subject's past, including a great deal more evidence on the consequences of the expulsion of HET not just for macroeconomic analysis but for macroeconomic policy too. Today's historians are already evaluating this evidence, and perhaps their work will eventually lead to a re-assessment of an expulsion that most Macros still believe to have been a good idea. If these things come to pass, the next fifty years of Macroeconomics will surely be more productive than the last and the future of HET, and hence the HES, will also be bright. Optimism is surely permissible at a birthday celebration.

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