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HISTORY OF ECONOMIC THOUGHT'S PLACE IN MACROECONOMICS REVISITED

BY

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I. MY OWN BEGINNINGS IN HET

I attended the now famous conferences at Sussex in 1968 and Nottingham in 1969 that preceded the later founding of both the History of Economic Thought Society in the UK and the History of Economics Society (HES) in North America. 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 fields, these conferences were followed by many, many more. At that time new specialist journals were also appearing. The *Journal of Money, Credit and Banking* (*JMCB*), where I had a paper in the second issue, started life in 1969. So did *History of Political Economy* (*HOPE*), but there I had nothing to submit. Ironically, given Lionel Robbins's still notorious attack on this proposed journal at Sussex, to which I shall return below, my first completed research paper in history of economic thought (HET)—on Thomas Tooke (Laidler 1972)—was already committed to his forthcoming Festschrift.

In hindsight, it might seem that 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 the small Depression and WW II cohort that, upon graduation, met the first of the baby-boomer masses demanding scholarly services. Between the start of my BSc (econ) at the London School of Economics (LSE) in 1956 and the completion of my PhD at Chicago in 1964, Sputnik's launch had inspired a more than doubling of the National Science Foundation's budget; and the UK government had followed Lionel Robbins's advice (better in this instance) to expand the university sector. Lots of academic jobs with reasonable teaching obligations and ready access to research funds had therefore materialized 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 professionalized and internally specialized as it did so. But the ambitions that lay behind the launches of the *JMCB* and *HOPE* were not the same. The former was yet another step

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in an already confident campaign on the part of those whom Axel Leijonhufvud ([1973] 1981) 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, that of the HES, was the stabilization and refocusing of a field that was already slipping down the discipline's pecking order.

By 1968, I had already encountered some ominous signs. HET had occupied a central and required place in my undergraduate curriculum in economics, analytic and descriptive, at LSE in 1956 to 1959, but when I arrived at Essex in 1966, the started-from-scratch in 1963 honours degree course offered just six final-year lectures in the area, not to be examined and only to be given if there was someone around (such as me) wanting to deliver them.[1] At Chicago, there had been a minimum one-HET-course requirement in the PhD program (usually satisfied by taking George Stigler's course), but I had been the only student in my cohort to prepare instead for a much more demanding comprehensive field examination in the area. And when, as an assistant professor at University of California Berkeley in 1965, I had 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 had been to ask me how I thought the resulting paper would contribute to my candidacy for tenure in that department.

Had anyone asked me in 1969 how I ordered my two fields, I would certainly have placed macroeconomics (or "Money" as they still called it on the Midway) first. Even so, though I wrote little in the field in the '70s, I remained seriously engaged with HET. In 1971, under the watchful eye of Bob Black, I organized a conference at Manchester to commemorate the centenary of William Stanley Jevons (1871) (see Collison-Black 1972); and, after arriving at the University of Western Ontario (UWO) in 1975, I took over the local organization of an already planned conference on the writing of John Maynard Keynes's *General Theory* (see Patinkin and Leith 1978). I also revived UWO's undergraduate honors course in the area. Last but not least, Don Patinkin, a regular visitor to UWO in the mid-'70s, gave me regular and priceless guidance about why and how I should continue to work on the history of macroeconomics. By the early '80s, then, I had finally read enough of it to begin writing regularly on earlier economics, taking an approach that, under Don's influence, drew no sharp lines between economics and its history: this just as the "Breaking Away" question was beginning to be discussed by historians.[2]

II. THE "BREAKING AWAY" QUESTION

As an LSE undergraduate, I had studied scientific method with Karl Popper, and the majority of those who actually taught me economics, those who took the classes and

[1] The final year LSE course for economics specialists, who numbered only about a dozen, was formidable. Bernard Corry delivered a year of weekly lectures covering "Moses to Marshall," as he described their scope, and, with Kurt Klappholz, conducted a weekly seminar that focused in detail on Smith, Ricardo, and Marshall. In addition, Lionel Robbins held an informal Friday afternoon seminar that often turned into a conversation between him and my fellow undergraduate Sam Hollander about some historical topic.

[2] Robbins's Friday afternoon seminars, where current economics and its history were thoroughly mixed, were perhaps an earlier influence.

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seminars rather than delivering the big lectures (and were later my colleagues in 1961–62), were also leading members of the "Methodology, Measurement and Testing" (M2T) seminar. I had been 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, had 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 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 those discussions about whether the subdiscipline should break free of economics and find its place in the history of science developed in the '80s and '90s, I was reasonably well equipped to follow them.[3] Indeed, thanks to my regular contact with the University of Toronto–York University History of Thought group, I was exposed to Margaret Schabas's views on these matters even before they saw print (Schabas 1992).[4] 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. Besides, while these debates were going on, I was busy working on the evolution of monetary economics. When, inspired by Mark Blaug (2001), I eventually did take a public position on the relationship between economics and its history, things did not go well.

In 2001, I prepared a paper entitled "The Place of the History of Economic Thought in Modern Macroeconomics" (Laidler 2003) for a Bank of England conference honoring my friend Charles Goodhart, an economist with the same out-of-fashion historical leanings as myself. 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 unprepared for its even more hostile reception at the 2002 HES Conference in Davis.[5] My discussant there was Roy Weintraub, a colleague with whom I had, as I still do, excellent personal rapport. But I was completely unaware that Roy had recently finished editing *The Future of the History of Economics* (Weintraub 2002). Most readers will be familiar with this still important volume and his introduction to it.[6] So, though Roy has no recollection of our encounter on that day, they will easily accept 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

[3] The underlying issues were already apparent at Sussex and Nottingham. But in 1968–69 I still had a lot to learn. For example, I was simultaneously unable to deny Bob Coats's insistence that the sociology of the economics profession—a new topic to me then—was important for understanding the discipline's history but privately baffled about what such work could contribute to an economics syllabus.

[4] At various times this group included Avi Cohn, Bob Dimand (like me, an out-of-towner) Allan Hynes, Sam Hollander, Sue Howson, Don Moggridge, and Margaret Schabas. For a long time its workshops provided my main opportunity for regular face-to-face contact with other historians, though I was also a sporadic attendee at meetings of the HES, the UK Society, and on two occasions the European Society for the History of Economic Thought. The travel expense issues mentioned by Weintraub (this issue) combined with the pressure of other obligations—e.g., to the Canadian Economics Association—made regular annual attendance at these meetings impractical. Toronto also gave me the only opportunity in my career to teach a full-year graduate course in HET during Sam Hollander's sabbatical in 1993–94.

[5] An early version of the paper was presented at the Toronto workshop, but I don't recall how the group reacted. Margaret Schabas would have been in British Columbia by then.

[6] See Weintraub (this issue) for his own current memories of this work.

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was duly and humanely dispatched. I was left perplexed, not just immediately but for a long time afterwards.

III. THE EXPULSION OF HET

When, in 1968, Robbins had denounced the proposed specialist journal that would become *HOPE,* it was because he believed that the separation of HET from economics that its launch might encourage would have destructive consequences. Lionel hated provincialism of any sort, and he believed "provincialism in time" to be its very worst manifestation. To him, economics was economics, no matter when it had been written, and the only relevant question was whether it was worth reading. He offered bad advice at Sussex but from the best of motives. Perhaps because he had been out of day-to-day touch with academic economics for too long, he underestimated the strength of the forces driving the separation he feared that originated from this quarter. If HET had not begun to strengthen its own separate identity when it did, economists would still have pushed it out of the way. So, as I was only too aware by 2001, the subdiscipline had been prudent to look 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 National Science Foundation, 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, because anything worthwhile from the past was still in the current curriculum. As ever more mathematics was undeniably becoming necessary at this time, something had to make room for it in post-graduate teaching, and HET was the obvious candidate.

Whether or not old ideas not included in the accepted body of economic knowledge are also of no current interest is, of course, an empirical question, and HET is the source of the evidence needed to test it. But a swing away from empiricism was part and parcel of New Classical macroeconomics, whose deductivist approach to the conduct of analysis—formulate clear assumptions about (alleged) "fundamentals" (tastes, endowments, and technology) and "rules of the game" (clearing markets and rational expectations), and then carry on maximizing—became central to the Macros' campaign for parity of status with the Micros after the publication of Robert Lucas (1972). Equilibrium modeling based on explicit microeconomic foundations was proclaimed to be the only "proper" basis for the subdiscipline, and in due course, as Pedro Duarte (2012) has recounted, this view came to dominate its mainstream.

In keeping with this methodology, macroeconomists adopted a story about the evolution of their science deduced from its current state rather than from the historical record. In that story, with the important exception of a Keynesian detour of political but not scientific interest, economics had been dedicated since the mid-eighteenth century to the postulates that "markets clear and … agents act in their own self-interest" (Lucas and Sargent [1978] 1984, p. 304), and progress since then had been mainly technical (Lucas 1996, 2004). This view of course ignored the possibility that strict adherence to these postulates might be rendering the area over which macroeconomic engines of analysis were capable of searching for concrete truth dangerously narrow, and that other approaches (such as Keynes's) that contemplated the possibility of markets not always clearing might sometimes have empirically interesting things to say. But, often illustrated with suitably selected examples of alleged early anticipations of New Classical doctrine, this ultra-Whiggish version of HET was assiduously propagated to a student body no longer encouraged (to put it kindly) to acquire any knowledge of the discipline's earlier efforts that might cause them to question it.[7]

IV. WHY MACROECONOMICS NEEDS ITS OWN HISTOR*Y*

By 2001 I had concluded that the dominance in macroeconomics of equilibrium modeling supported by pseudo-history was not just bad for the subdiscipline but a threat to the economy as well, because its ideas were becoming a basis for macro-policy. Analysis, soon to be codified by Michael Woodford (2003), that studied only situations where markets are cleared, ignored the financial system's role in coordinating activity, 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 intended message of my Goodhart Festschrift paper was that students badly needed enough systematic exposure to HET to enable them to assess for themselves the claims of mainstream macroeconomics to be the final product of two centuries of continuous intellectual progress. I did not also mean to suggest that HET could deliver such support only if it put its head back into an intellectual noose from which it was still wriggling free. But Roy Weintraub's reaction suggests that I didn't make the latter point clearly enough. So, because HET's status within macroeconomics, indeed within economics in general, has changed only 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 to 2010, and the post-2020 inflation (cf. Laidler 2021), let me now briefly restate the case I presented in Laidler (2003).[8]

To begin at the beginning: the fact that economics 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 student 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

[7] Specific examples of modern macroeconomists' misrepresentations of earlier doctrines were cited in Laidler (2003); many more have accumulated since then.
[8] As in (2003), I make no claims about the extent to which the arguments that follow can be extended to micro areas, of whose current state I have insufficient knowledge to formulate a properly informed opinion.

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phenomena that economics studies, described by Kenneth Boulding (1966, pp. 8–9), is that agents make use of their own knowledge of how the economy functions in formulating the plans that guide their behavior. They constantly revise this knowledge in the light of experience, also modifying their subsequent behavior, in an ongoing recursive process that may or may not always make progress. In modern macroeconomics, an anemically simple version of Boulding's idea is embodied in the Rational Expectations Hypothesis and its associated Lucas Critique, which envisage agents basing their plans on knowledge of the same *true and therefore unchanging* model of the economy that the economist analyzing their behavior also uses.

The historical record confirms, however, that economic models are and always have been open to revision in the light of experience, no matter who is applying them, and that ideas about what constitutes a "true model" of the economy have been subject to continuous disagreement and constant evolution ever since economic thought began. Awkwardly for modern macroeconomics, these indisputable facts imply that, strictly speaking, today's models are at best empirically relevant only to data generated since they were formulated. Empirical investigations going further back need to deploy older ideas as well as older data. If it is to take empirical evidence seriously, then, macroeconomics cannot do without its own history.

In the past, moreover, useful ideas that did not cover all possibilities sometimes fell into neglect when problems that they did not address became centers of attention. They disappeared not because evidence refuted them but because they had become seemingly irrelevant if only temporarily. HET is littered with examples of such mislaid ideas being rediscovered and/or recreated from scratch when circumstances have changed yet again.[9] 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 behavior five centuries ago, or, on a lesser scale, the comings and goings of the Fisher effect, or of the Thornton-Wicksell cumulative process, and so on. The idea that the evolution of economics has been one of unidirectional progress is inconsistent with these and many other observations, and they point to yet another reason why HET is indispensable to macroeconomics: it stores memories of once useful ideas that might become useful again. To cite but one example, Leijonhufvud's highly original theorizing (1968, [1976] 1981) was profoundly influenced by his habit of doubling back to find and then refurbish old ideas that turned out to have been discarded prematurely.

V. THE EXAMPLE OF THE "KEYNESIAN REVOLUTION"

Seen in this light, Keynes's *General Theory*, far from creating an irrelevant detour along micro-founded macroeconomics' path of progress as the New Classical history of the subject claims, or a unique revolution against some mythical "classical" orthodoxy as,

[9] Sometimes ideas also disappeared for lack of techniques available to develop them further. Many of the insights of the Stockholm School about endogenous expectations discussed in the 1930s suffered this fate. See Laidler (1999, pp. 57–62).

following Keynes himself, the post-Keynesian story would have it, turns out to have brought about changes in macroeconomics, albeit striking ones, in the usual way.

To begin with, the book's component ideas were all in the pre-existing literature.[10] 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 by 1936, persistent mass unemployment had replaced cyclical fluctuations in the price level as the central feature of macroeconomic life, and it needed an explanation. Keynes took the above components, refining some of them in the process, and linked them together into a new overarching framework that showed, as nothing that had gone before it had done, what determined the level of that unemployment. This framework, formalized and simplified by James Meade (1937), given a geometric representation (as LL-SI) by John R. Hicks (1937) and eventually known as IS-LM, would in due course come to dominate mainstream macroeconomics. Furthermore, a special case of it (one that its early exponents had barely considered) was in due course used (surreptitiously before 1970 (cf. Friedman 1974) by Milton Friedman to launch monetarism and to present an alternative interpretation to Keynes's of causes of the unemployment of the 1930s, as well as to explain the inflation of the post-World War II years.[11] In the latter application it bore a remarkable resemblance, as Friedman himself would stress, to the at that time apparently dead and buried quantity theory of money. And after Friedman, came Lucas (1972).

In short, macroeconomics certainly 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 and clarifying ideas that had gone before it as by overthrowing them, and the doctrine that it embodied would in its turn and in due course be extended along unforeseen paths as the subdiscipline continued to evolve.[12]

VI. THE EFFECTS OF MACROECONOMICS' NEGLECT OF HET—A RESEARCH TOPIC FOR HISTORIANS

Obviously, this reading of the history of macroeconomics flatly contradicts the New Classical version that first intruded on the subdiscipline's teaching five decades ago and now dominates it. 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 subdiscipline was not surprising. In 2002, I was, however, puzzled, and indeed dismayed, by the lack of concern within HET about the damage that its expulsion from economics was already inflicting on the latter discipline. I could (and can) see nothing in the powerful case for relocating research and teaching in HET within

[10] However, they sometimes carried different labels, and the extent of Keynes's own previous knowledge of some of them is open to debate.

[11] The special case was of course that in which the Keynesian liquidity preference function is relatively interest-inelastic and stable over time.

[12] So, to be explicit, I am still unrepentant about the deliberate ambiguity embodied in the title of Laidler (1999).

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the history of science to invalidate the proposition that its own history is also integral to macroeconomics and needs to be studied by its practitioners. After all, we take it for granted that research and teaching in mathematics should be located in dedicated departments, but we still require our students to learn and apply it when they study economics.

However, some things do indeed come to those who wait. What was current economics twenty years ago, and hence of no concern to those historians who wanted to relocate HET in the history of science, is now firmly in the subject's past. Some of those same historians, and their students, are now deeply engaged in evaluating this material. The question of what influence the expulsion of HET from the economics curriculum has had in enabling DSGE analysis to achieve its current dominance of macroeconomics is thus one they must address. If they find an important role here, as I believe they will, they will also find it hard to avoid the conclusion that HET was, and still is, integral to economics after all, even if it has also established itself within another discipline in the interim. Should this come to pass, then the future of HET, and hence the HES, will be bright in both of their intellectual homes, and even the prospects for macroeconomics might improve. Such optimism is surely permissible at a birthday celebration.

COMPETING INTERESTS

The author declares no competing interests exist.

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