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No History of Ideas, Please, We're Economists

Mark Blaug

It is no secret that the study of the history of economic thought is held in low esteem by mainstream economists and sometimes openly disparaged as a type of antiquarianism. There is nothing new in this. Practically every commentator on the role of history of economic thought in modern economics in the last 30 years has lamented the steady decline of interest in the area since the end of World War II and its virtual disappearance from university curricula, not just at the graduate but sometimes even at the undergraduate level.¹ The trend is more pronounced in the United States than in Europe but it is manifest just about everywhere.²

However, along with fewer and fewer university courses in history of economic thought, there appear to be more and more scholars attending scholarly meetings in history of economic thought and publishing articles about the history of economic thought. History of thought journals are burgeoning, and their quality seems to be high and steadily improving. In addition to the premier *History of Political Economy* founded in 1969 and the old *History of Economics Review* founded in 1973, there is *Research in the History of Economic Thought and Methodology* appearing annually since 1983, the *Journal of the History of Economic Thought* dating from 1990, the *European Journal of the History of Economic Thought* and *History of Economic Ideas*

¹ See Blaug (1991) and Cardoso (1995, p. 198) for an almost complete listing of relevant papers.

² A survey by Cardoso (1995, p. 202) of 300 teachers of history of economic thought courses in 25 countries found that such courses are predominantly compulsory in graduate programs but predominantly optional in undergraduate courses, an anomalous result which contradicts widely reported experiences in the United States and the United Kingdom.

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both dating from 1993. When the U.S. History of Economics Society was set up in 1973, it had just over 200 members and its first annual conference in 1974 was attended by only 50 members; in 1999, the membership totalled over 600 and 300 scholars participated in the annual meeting, presenting some 150 papers over three days. In addition, there are now three active societies for history of economic thought in Europe, publishing an annual newsletter and meeting once a year in the United Kingdom and in two separate venues on the continent of Europe. Similar societies function in Japan and Australia. Schabas (1992) conjectured that there were something like 500 to 600 active historians of economics worldwide and probably 1000 more who either teach the subject or dabble in some history of economic thought research; to bring those numbers up to date they should be raised by about 50 percent.

How can we account for those two opposite tendencies: the decline of history of economic thought in the classroom, accompanied by a rise in papers, seminars, and specialized journals in the area?

Down and Up with the History of Economic Thought

Let us begin with the decline of interest in history of economic thought among true-blue economists. This is so easy to explain that any attempt to do so tends to suffer from the methodological sin of overdetermination.

There is, first of all, the philosophical overhang of positivism. Alfred Whitehead (1929, p. 162) once said: "A science which hesitates to forget its founders is lost"—and that says it all! Jean Baptiste Say expressed the same idea more succinctly: "The more perfect the science, the shorter its history" (quoted by Barber, 1997, p. 93). The hard sciences do not much bother with their own histories—a statement less true than it used to be³—and if economics is a real science, neither should economists.

A second explanation is milder and a typical example of an economics-of-economics argument: In an ideal world there is nothing wrong with courses in history of economic thought as a form of entertainment for students and a respite from the ardors of studying mathematics and statistics, but the ultimate scarce resource is time and history of economic thought simply cannot earn its keep in the trade-off between different courses. As Paul Samuelson (1988, p. 52) put it: "Graduate students need at least 4 hours a night of sleep; that is a universal law, so something had to give in the economics curriculum." Besides, history of economic

³ History of science is sometimes taught in a separate department from the teaching of physics, chemistry and biology (I wish I had solid numbers to offer but I have been unable to find any quantitative study of this phenomenon). Some historians of economic thought have become so depressed by their colleagues' denigration of the subject that they have argued for breaking away from economics and forming alliances with historians of science (Schabas, 1992). This proposal has been loudly rejected by most historians of economic thought.

thought is not vocationally useful. Who has ever heard of an employer outside academia being impressed by the fact that the applicant had completed a course in history of economic thought?

But if these arguments are valid, how do we account for the growing participation in history of economic thought conferences and the steady multiplication of history of economic thought journals?

One all-too-easy explanation is that in a world of growing population, rising participation rates in higher education, and rising employment of university teachers, all numbers are bound to increase, including the number of history of economic thought papers by young staff anxious to publish rather than perish. There is something in this pure numbers argument, but it is just a little too easy. The number of students studying economics is by no means increasing in all countries. Moreover, academic careers are enhanced by publication in prestigious journals and journals in history of economic thought do not rank high in the estimation of department chairmen.⁴

A more persuasive explanation of the apparently growing number of history of economic thought scholars is that history of economic thought appeals to a different type of mind from that of the average mainstream economist. If you are mathematically inclined, you will find physics, engineering and modern economics congenial to study. If you are philosophically inclined—an intellectual rather than a technocrat—but are attracted to economics because of its policy relevance or the belief that society rests essentially on economic foundations, you may well find yourself drifting towards history of economic thought as one of your specializations in economics. Because papers in history of economic thought rarely contain much mathematics or econometrics, some students may persuade themselves that it is a soft option. Actually, history of economic thought is in many ways more difficult, more subtle, less capable of being cloned on a master mold than standard mainstream economics. Be that as it may, it is a striking fact that conferences in history of economic thought attract Austrians, Marxists, Radical political economists, Sraffians, institutionalists and post-Keynesians in disproportionate numbers, all non-neoclassicals or even anti-neoclassicals who have no place else to go to talk to scholars outside their own narrow intellectual circles (Vaughn, 1993, p. 180). In other words, history of economic thought is a haven for heterodoxy, a heterodoxy which no doubt has many sources but at its foundation takes its departure, I suspect, from a certain type of mind, a certain congenial style of thinking.

⁴ If only someone were to collect numerical data on the growth of history of economic thought conferences and journals compared to other fields of economics, I (and others) could abandon the use of anecdotal evidence. Alas, having participated in an unsatisfactory European effort along these lines, I decline to join in a Kuznets-like exercise to produce the figures for the United States but will eagerly consume them when they appear.

Counsel for the Defense Addresses the Jury

Yes, but what exactly is the case for *any* role of history of economic thought? In the face of this question, the titles of some of the papers on the role of history of economic thought in modern economics adopt a painfully defensive tone: “What Price the History of Economic Thought?” (Winch, 1962); “Does Economics Have a Useful Past?” (Stigler, 1969); “After Samuelson Who Needs Adam Smith?” (Boulding, 1971); “Should Economists Abandon HOPE?” (Corry, 1975); “Does Scholarship in the History of Economic Thought Have a Useful Future?” (Barber, 1990); “Are There Limits to the Past in the History of Economic Thought?” (Backhouse, 1992); and “Why Teach the History of Economics?” (Vaughn, 1993). The note of pain in the defense arises from the fact that it is not easy to provide a compelling defense for an intellectual preference, the more so when the jury is known to be skeptical. Consider the somewhat lame defense offered by Schumpeter (1954, p. 4) in the opening pages of his magisterial *History of Economic Analysis*. He asks why study the history of economics, and replies with a quick list: “pedagogical advantage, new ideas, and insights into the ways of the human mind.”

“Pedagogical advantage,” a term conveying the study of the fundamental building blocks of economics (opportunity costs, decisions at the margin, private vs. social costs, pecuniary incentives, intertemporal coordination, market clearance, market failure, imperfect information, moral hazard, transaction costs, and so on) in a wide variety of historical and intellectual contexts, appears high on almost everybody’s list of reasons for teaching history of economic thought, particularly to undergraduates at, say, the junior level—that is, not before they know some economics and not after they have decided whether to continue with economics in graduate school.

Discovering “new (and perhaps forgotten) ideas” is less frequently mentioned if only because there are not many examples in history of economic thought of the phenomenon in question. The rediscovery of Pareto optimality in the 1930s after 26 years of neglect, or the exploration of transaction costs 30 years after Coase had drawn attention to the idea in 1937, is not a common occurrence in the evolution of economics.

Some commentators in history of economic thought express belief in the notion that the community of economists represents an approximately perfect market in which new ideas are so efficiently transmitted in a communication network of journals, books, seminars and conferences that there is virtually no loss of significant content. This view of an efficient marketplace of ideas implies that history of economic thought can be safely neglected by modern economists, because what is valuable in the ideas is fully contained in the present curriculum. Stigler (1969) managed somehow to combine this dismissal of the history of economic thought with a great deal of invaluable and still classic research on particular topics in history of economic thought. The objections against taking the market-of-ideas as anything other than a

stimulating metaphor are so obvious as hardly to require discussion. In particular, markets as arbiters of quality in scholarly (or any other kind of) goods are excessively subject to bandwagon and snob effects, particularly as scholars are typically rewarded by employment in nonprofit and frequently subsidized institutions of higher learning.⁵ Besides, the loss of content that we are worried about in history of economic thought is not so much ideas that are irretrievably and totally lost but rather embryonic ideas (like, say, transaction costs) whose critical insights on current economic problems have not yet been adequately explored. Consider how long it took to connect Edgeworth's concept of "the core" of an economy with Nash equilibrium in noncooperative games!

The only utilitarian function that Stigler ever offered for history of economic thought was, curiously enough, that it taught one "how to read and how to react to what we read" and this art is best practiced, he argued, on the truly great books of the past, so as to take advantage of the perspective of distance. This is a defense that we also find in Boulding (1971, p. 235): reading the Great Books of Economics, such as *The Wealth of Nations*, he remarked, "give us some inkling of the way in which a really exceptional intellect works." This argument picks up the third of Schumpeter's arguments for history of economic thought—"insights into the ways of the human mind"—but it was much more than that for Boulding. His case was that a modern graduate education in economics that left out history of economic thought was perfectly calculated to produce *idiots savant*.⁶

When Schumpeter defended history of economic thought he was thinking of the history of economic *analysis*, whereas almost all the subsequent arguments for the teaching of history of economic thought, like those of Stigler and Boulding just discussed, have been about the history of economic *doctrines*: the relationship of economic theory to economic policy, the influence of social, philosophical and political preconceptions on the development economic ideas, the methodological views of the great economists, the sociology of the economics profession, the international diffusion of economic ideas, and similar wide-ranging questions about the history of ideas as applied to economics (see, in particular, Winch, 1962;

⁵ For critiques of the efficient market-of-ideas hypothesis, see Anderson and Tollison (1986), Strassman (1993), Khalil (1995, pp. 717), and Yeager (1997). The market-of-ideas concept is central to the new "economics of science." For discussion, see Hands (1997, pp. 728–29) and at book-length, Wible (1998).

⁶ Boulding (1971, p. 232–33) wrote: "The antihistorical school, which is now so common in the United States, where the history of thought is regarded as slightly depraved entertainment, fit only for people who really like medieval Latin, so that one became a fully-fledged, chartered Ph.D. economist without ever reading anything that was published more than ten years ago . . . leads to the development of slick technicians who know how to use computers, run massive correlations and regressions but who do not really know which side of anybody's bread is buttered, who are incredibly ignorant of economic institutions, who have no sense at all of the blood, sweat and tears that have gone into the making of economics and very little sense of any reality which lies beyond their data." That was in 1971. What would Boulding say today faced with the findings of Klammer and Colander (1990) and the Commission on Graduate Education in Economics (Krueger, 1991)?

Corry, 1975 p. 260; Cesarano, 1983). From this wider perspective, the case for teaching and studying the history of economic thought is easy to make. It is in fact the only way to give students some sense of the place of economics in the larger community of social science, and to raise the famous questions of the advantages and disadvantages of an intellectual division of labor. If we teach the ideas of the great economists of the past with due attention to their intellectual background, their philosophical preconceptions and the institutional context in which they wrote, we end up fulfilling the third of Schumpeter's reasons for studying history of economic thought: "insights into the ways of the human mind." But more pertinently, we end up with insights into the ways economics got to where it now is. In the words of Karen Vaughn's (1993, p 178) fierce defense of history of economic thought: "We need to say straight out that the history of economics is 'useful' not because it helps students to sharpen theoretical skills or because it gives them a little interdisciplinary breadth, but because it can affect the understanding of economics itself, its potential accomplishment and its important limitations." She concludes that there is a good case, at the margin, for a little more history of economic thought and a little less mathematical economics and advanced econometrics. I could not agree more.

Knowledge has multi-dimensional depth as well as breadth, and some of the dimensions of economic knowledge include analysis, data, history, institutions and policy questions. There is a raw kind of conceptual depth where concepts are only understood when they are differentiated relative to other closely related ones so that the extent of one's knowledge depends on the fineness with which one can differentiate. In different contexts, these dimensions of knowledge vary in importance. The history of economic thought seems to me of potentially wide applicability for many students in getting a "deeper" or "gut-level" understanding of a wide range of concepts.

How Do We Reconstruct the Past?

Some historians of economic thought have tried to sell the subject to their departmental colleagues by reducing history of economic thought to the history of economic analysis, and then by dressing up past ideas in modern garb, often in the form of mathematical models that look just like something that might have appeared in the latest issue of the *American Economic Review* or the *Journal of Political Economy*. I call these "rational reconstructions" and I contrast them with "historical reconstructions," borrowing the terminology from Richard Rorty's historiography of philosophy (Blaug, 1990; see also Backhouse, 1992, p. 24; Khalil, 1995, pp. 46–49). What Schumpeter (1954) called "history of economic analysis" is in fact a history of rational reconstructions, but despite his announced intentions in the opening chapter of his great book, he almost continuously lapsed into what he then

called “intellectual history” or “*geistesgeschichte*” (p. 303), which is virtually the same thing as what I call “historical reconstructions.”⁷

The noun “reconstruction” is deliberate, paying tribute to the structuralist insights of Jacques Derrida and Michael Foucault, namely, that all texts of the past need to be reconstructed because they do not speak with one voice and are never unambiguous; even the authors of these texts are never in complete control of their own meaning. Given the fact that texts must be reconstructed, the question is how are we to do so: in the light of all that we now know or as faithfully as possible to the times in which they were written?

The temptation to choose the first alternative is almost irresistible. In so doing, we make history of economic thought transparently relevant to modern economists and, at the same time, we exercise our technical expertise, honing it for contemporary applications. Of course, it is an anachronism to express Adam Smith in a three-equational growth model or the Malthusian theory of population in two differential equations. But that is a small price to pay for the exultant feeling that, finally, we understand Ricardo almost as well as Samuelson.

In contrast, historical reconstructions, which involve accounting for the ideas of past thinkers in terms that these thinkers and their contemporary followers would have accepted as a correct description of what they intended to say, are very difficult to carry out. They require careful reading not only of the texts of the economists that one is studying, but also of the previous generation of thinkers in order to understand the context in which the economists in question were writing. Historical reconstructions require us to travel backwards in time, to drive the intellectual vehicle of economics by looking in the rearview mirror. At that point, we are inclined to take comfort in the undoubted fact that historical reconstructions are, strictly speaking, psychologically, intellectually and even logically impossible. How can we possibly forget, or even pretend to forget, modern economics when we read Karl Marx? Why did the poor fellow try to attribute the value of the product to a single input, labor, without knowing anything about marginal productivity? One might well talk to a psychoanalyst about one’s childhood while pretending that amnesia occurred at puberty.

As we read the great masters of rational reconstruction, such as Paul Samuelson, Michio Morishima, Hans Brems, Jurgen Niehans and Takashi Negishi,⁸ we no

⁷ Rational reconstructions are better known by the pejorative label of “Whig interpretations of history” after the title of a 1951 book by the English historian Herbert Butterfield. It attacked the dominant tradition of English historiography to depict the history of England as a story of steady progress towards the liberal ideals that the Whig party represented. The term Whig Interpretation soon passed into general currency as a name for a practice that historians ought to avoid but critics delighted in showing that Butterfield’s own *Origins of Modern Science, 1300–1800* carried a strong Whiggish flavor. My distinction between rational and historical reconstructions is identical to the distinction historians of science commonly draw between the “anachronical” and “diachronical” view of the history of science (Kragh, 1987, ch. 9).

⁸ For an uncompromising example of the same thing from the pre-war generation, see Knight (1935) and Stigler’s Ph.D. thesis (1941) supervised by Knight.

doubt learn much about mathematics modelling of economic ideas and even a little about Smith, Ricardo, Mill, Walras and Wicksell but, however legitimate the exercise, at some time the futility of the operation becomes overwhelming. Once we have written down a mathematical model of an agrarian economy subject to irremediable land scarcities, which is Ricardo in modern dress provided we cut a few corners, why do we need Ricardo at all except as an advertising logo? Of course, if a person has a hammer, everything looks like a nail and if an economist has modern tools, then every issue looks like a chance to apply those tools. It is always nice to have new nails to hammer but rational reconstructions ultimately make the history of economic thought dispensable because if the only point is to use one's modern tools, there are many other places to do it.

Although I have been guilty myself of the very sin I have just deplored, I have come to the conclusion that the only approach to the history of economic thought that respects the unique nature of the subject material, rather than just turning it into grist for the use of modern analytical techniques, is to labor at historical reconstructions, however difficult they are. Rational reconstruction makes past thinkers appear to be a bit more like us than they were; historical reconstructions make them out to be a little less like us than they were. We cannot recreate the mindset of Adam Smith nor the intellectual legacy that he inherited, but we can try to come closer to it. There is progress in history of economic thought just as there is in economics as a whole: to read even such great scholars of yesterday as Jacob Hollander and Jacob Viner on Adam Smith is to realize how far we have come in Smithian studies in recent decades. Besides, the logical impossibility of ever reconstructing past ideas "as they were actually thought" is no greater than recreating the revolutionary fervor of Paris in 1792 or Moscow in 1917. If the problems of historical reconstruction in the history of ideas were truly insurmountable, so would be attempts to write any social, political or economic history.

Having Your Cake and Eating It Too

If rational and historical reconstructions would always come neatly separated in different packages, distinguishing the two types of interpretation would be plain sailing. Alas, most practitioners of the art of rational reconstruction persuade themselves that they have achieved a deeper historical reading of the great thinkers than was ever achieved by those purist historians of economic thought who claim no more than that of bringing the past to life. I have elsewhere given numerous examples of this phenomenon of having your cake and eating it too (Blaug, 1990), but here let me offer just a couple of telling examples that convey the power of history of economic thought to illuminate the central questions of economics and to teach fundamental lessons about the unfolding of economic ideas.

Adam Smith's "invisible hand" must be one of the most famous metaphors

in the literature of economics. In the “historical introduction” to their *General Competitive Analysis*, Kenneth Arrow and Frank Hahn (1971, pp. 1–2) pay tribute to Adam Smith for having dimly perceived 200 years ago that perfect competition secures Pareto optimal multi-market equilibrium and this reading of Smith is echoed in numerous introductory textbooks (Blaug, 1997a, p. 82). This tribute is a historical travesty. What Adam Smith meant by competition is what modern Austrians call “process competition.” What we nowadays call competition was for him “the obvious and simple system of natural liberty,” meaning an absence of artificial restraints and, in particular, restraints on free entry into industries and occupations. Neither competition nor monopoly was a matter of the number of sellers in a market; monopoly did not imply a single seller but a situation of less-than-perfect factor mobility and hence inelastic supply; and the opposite of competition was not monopoly but cooperation. In short, competition denoted that pattern of business behavior which we conjure up by the verb “to compete:” to invade profitable industries, to expand one’s share of the market by price cutting, and to jockey for advantage by any and all possible means. It was Auguste Cournot who in 1838 first invented the idea of perfect competition as a market structure in which business firms are so numerous that each firm must take price as given, being free only to adjust the quantity it produced. Not only was this conception of firms as “price takers” rather than “price makers” totally foreign to the way Adam Smith and all subsequent classical economists thought about competition, but to imagine that the “dynamic efficiency” which they clearly ascribed to the competitive process is exactly the same thing as the “static efficiency” of Pareto and Arrow-Debreu is to pile travesty on travesty (Hutchison, 1999).

Moreover, Adam Smith used the phrase “invisible hand” on three dissimilar occasions in his writings and in each case it was employed, not to exemplify the Panglossian conclusion that markets always convert private “vices” like selfishness into public “virtues” like income and employment for all, but to demonstrate that, in Robert Burns’s words, “the best-laid schemes o’ mice and men/Gang aft a-gley” (Rothschild, 1994). In *The Wealth of Nations*, the “invisible hand” phrase occurs only once in Book IV, Chapter 2, with reference to international trade, where Smith argues that the selfish preference for domestic over foreign industry unintentionally contributes to the defense of the nation (Grampp, 2000), adding sardonically that the deliberate promotion of public welfare by disingenuous merchants usually does more harm than good. Adam Ferguson’s (1978) contemporary doctrine of the “unintended social consequences of private action,” rediscovered by Friedrich Hayek and Robert Nozick in the twentieth century—so-called “invisible-hand explanations” of social institutions—is *not* found in Adam Smith. So what? Well, if we are going to invoke the past to endorse current beliefs, it is philistine to ignore the textual record. Moreover, if we want to understand the process-conception of competition in opposition to the now dominant end-state conception (Blaug, 1997a),

it pays to come to terms with what Adam Smith actually believed about the benefits of the price system.

My next example comes nearer to home. I am sure that many mainstream economists rate history of economic thought rather low in part because they think of it as the history of economics long ago, dealing perhaps with Aristotle on money, or the scholastics on usury, or the mercantilists on import tariffs. But the most recent issue of *Econometrica* is just as much history of economic thought as what Pigou contemptuously called “the wrong opinions of dead men.” The history of economic thought runs right up to yesterday and living economists are as much grist to the mill of historians of economic thought as dead economists. Robert Lucas’s (1996) Nobel Lecture on the long-run neutrality of money nicely makes the point. Lucas follows Milton Friedman’s practice of starting any discussion of monetary theory with an exposition of what David Hume really meant in his two “marvellous essays” of 1752, *Of Money* and *Of Interest*. He cites two of Hume’s statements on “what we now call the quantity theory of money:” namely, 1) that a change in the money supply will change money prices proportionately; and 2) the corollary that it will produce no real effects on output and employment. Lucas (pp. 661–663) also notes Hume’s declaration that “there is always an interval before matters be adjusted to their new situation.” But if agents hold rational expectations, Lucas asks, why is the initial effect of monetary expansion or contraction different from their ultimate effects? How can neutral unit changes in money ever induce movements in employment and production in the same direction?

Lucas’s (1996, p. 664) explanation for what he views as Hume’s inconsistency is that Hume lacked modern economic tools: “I think the fact is that this [question] is just too difficult for an economist equipped with only verbal methods, even someone of Hume’s remarkable powers.” Moreover, Lucas goes on to argue, the lack of systematic data on money and prices in the eighteenth century forced Hume to rely largely on purely theoretical reasoning: “though tested informally against his vast historical knowledge, his belief in short-run correlations between changes in money and changes in production was based mainly on his everyday knowledge.” Amplifying this historical reconstruction of Hume’s argument, Lucas (1996, pp. 668–69) adds:

Hume was able to theorize vigorously and, as we have seen, with great empirical success, about comparisons of long-run average behavior across economies with different average rates of money growth. For short-run purposes, on the other hand, he was obliged to rely on much looser reasoning and rough empirical generalizations. As economic theory evolved in the last century and most of this one, the double standard that characterized Hume’s argument was perpetuated. The quantity-theoretic “neutrality theorems” were stated with increasing precision and worked through rigorously, using the latest equipment of static general equilibrium theory. The dynamics had a kind of patched in quality.

Wicksell, Keynes, Hayek and even Patinkin, Lucas tells us, wanted to think in general equilibrium terms in which people are conceived as maximizing over time and solving intertemporal substitution problems but they end up, like Hume, resorting to loose equilibrium dynamics because the analytical equipment available to them offered no alternative. For Lucas, all of this “serves only to underscore the futility of attempting to talk through hard dynamic problems without any of the equipment of modern mathematical economics.” The rest of the Nobel Lecture is then devoted to showing that we can now do general equilibrium macroeconomics properly, demonstrating that the long-run Phillips curve must be vertical if agents have perfect information about the stochastic outcomes of their decisions.

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. Money was granted to be neutral in the long run—and what a stunning indictment that made of the mercantilist obsession with chronic export surpluses—but nonneutral in the short run. Indeed, the short-run nonneutrality of money was so heavily underlined by Hume and later on by Marshall, Fisher, Wicksell, Mises, Hayek and Keynes that the much touted long-run neutrality of money, the proportionality theorem, and all that practically disappeared from view (Mayer, 1980; Patinkin, 1990; Humphrey, 1991; Blaug, 1997c, pp. 19–21, 614–28, 638–40). “The good policy of the magistrate,” declared Hume, “consists only in keeping . . . Money, if possible still increasing because, by that means, he keeps alive a spirit of industry in the nation.” Creeping inflation is what Hume recommended. In that sense, Hume is even more modern than Friedman or Lucas ever imagined.

It is this emphasis on the short-run, an emphasis that is virtually ignored by Lucas and by almost every modern textbook statement of the quantity theory of money, which became the hallmark of interwar macroeconomics right up to Keynes (Laidler, 1991, pp. 18–19, 79; 1999). I conclude, therefore, that Lucas’s rational reconstruction of Hume is a poor historical reconstruction, which as a consequence seriously misreads the history of monetary theory over two centuries.

Lucas simply cannot interpret a text that departs from his own theoretical framework according to which the only concern of an economist is the properties of long-run equilibria. While rational expectations and modern stochastic tools have certainly helped to spell out some of the grounds behind Hume’s claim about the long-run neutrality of money, it is untrue that previous generations of economists were seeking to model long-run neutrality, and the arguments over whether monetary policy should be aimed at short-run business cycle effects, or guided even in the short-run by an assumption of long-run neutrality, is not addressed at all by the ability of certain analytical tools to spell out the implications of neutral money under particular assumptions about expectations.

An Ultimate Justification for Studying the History of Economic Thought

I want to return now to the earlier \$64 question: How can one justify the study of the history of economic thought as a specialization within economics? We have surveyed a number of persuasive arguments from others, none of which I dare say was absolutely convincing to diehard skeptics. I want to offer my own knock-down argument, knowing full well that this argument is also not absolutely compelling. It is this: No idea or theory in economics, physics, chemistry, biology, philosophy and even mathematics is ever thoroughly understood except as the end-product of a slice of history, the result of some previous intellectual development. I never understood the calculus I learned in school until I read accounts of the Newton-Leibniz disputes about “the fundamental theorem of the calculus,” grounded in the metaphysical meaning of an infinitesimally small increment or decrement; it was only then that the penny dropped and suddenly I could see exactly why differentiation is the opposite of integration. I never understood the Keynesian Revolution until I read Hayek’s tortured *Prices and Production* (1931) and Robbins’s confusing explanation of *The Great Depression* (1934). So it is, I think, with all economic theories. Economic knowledge is path-dependent. What we now know about the economic system is not something we have just discovered, but it is the sum of all discoveries, insights and false starts in the past. Without Hayek and Robbins and Pigou, no Keynes; without Keynes, no Friedman; without Friedman, no Lucas; without Lucas, no . . . Leijonhufvud (1999) once likened history of economic thought to a decision tree, a trunk with many branches, some of which grow lustily but others atrophy and die, at which point the sap runs back to the trunk to revitalize another branch. There is nothing predetermined about our current theories and if years ago, economics had taken another turn at a critical nodal point, we would today be advocating a different theory.

I could give literally dozens of examples of this fundamental idea but I will here limit myself to only one. Imagine trying to grasp the full meaning of the so-called Coase theorem, the very centerpiece of the modern law and economics movements, from its announcement in the third edition of Stigler’s (1966, p. 113) *Theory of Price* as the proposition that “under perfect competition private and social costs will be equal” and hence that “the composition of output will not be affected by the manner in which the law assigns liability for damage.” Stigler’s brisk summation combines two claims in one: an efficiency claim that perfect competition is always optimal if voluntary bargaining between the affected parties is possible at zero transaction costs, and an invariance claim that the final allocation of resources is invariant to different initial assignments of property rights. A voluminous subsequent literature has shown that both propositions are either highly contentious outside of a world of perfect competition, or a tautology if perfect competition, perfect information, and zero transaction costs are rigorously defined (Usher, 1998; Medema and Zerbe, 2000). But when

we turn to Coase's (1960) famous article on "The Problem of Social Cost" to find the Coase theorem, it is not even alluded to in these terms. Moreover, transaction costs in this article are only defined as "marketing costs," whatever that means, 23 years after the earlier neglected Coase (1937) paper on "The Theory of the Firm" that first introduced the concept of transaction costs. It took Coase himself several years of Coasean economics by others to define transaction costs more precisely as the costs of negotiating contracts, whether explicit or implicit, and of monitoring and policing the enforcement of contracts, and to argue ever more vehemently that transaction costs can be minimized but that they always remain positive even under perfect competition. In short, the "Coase theorem" as described by Stigler and many others is what Coase himself referred to as "blackboard economics" which never could apply to the real world (Medema, 1994, ch. 4; 1995).

This wonderful comedy of errors teaches us, first of all, that even great thinkers never fully grasp their own innovations, and, secondly, that the full potential of great ideas is only exploited by disciples and critics over what may be years or even decades. We now casually refer to Coase's 1960 article as teaching us that "government failure" is as big a problem as Pigou's "market failure," so that the failure of private and social costs to coincide is itself an insufficient reason for government intervention, but that inference is by no means apparent when we read the paper afresh.

The Coase Theorem is not an unique example of what the passage of time will do to texts and that is why economics as a profession must throw off the disdain of history of economic thought. History of economic thought is not a specialization within economics. It *is* economics—sliced vertically against the horizontal axis of time.

New Currents in the History of Economic Thought

Conversations with other economists have brought home to me that a widely held impression views the history of economic thought as a sort of intellectual archaeology: it may turn up new manuscripts and documents from time to time, but it itself remains unaffected by these discoveries and, unlike other branches of economics, shows no development or progress over time. This is a totally misleading impression. Here, I wish to convey a little of the flavor of how drastically some areas in history of economic thought have been transformed in recent years. Even so, what I can say is deeply affected by my own interests. I am no Jacob Viner, who was famous for having read everything.

Let us go all the way back to the 1980s to salute the writings of Odd Langholm (1987) whose three closely connected books on scholastic economics have essentially rewritten the previous studies of post-Roman pre-classical economic thought. In fact, his *Price and Value Theory in the Aristotelian Tradition* (1979) deserves recognition as one of the classics of history of economic thought, ranking with Heckscher

(1935) on mercantilism, Viner (1937) on international trade theory, Collison Black (1960) on classical development economics (in relation to Ireland) and Fetter (1965) on British monetary orthodoxy.

Thinking about Adam Smith has not been the same since the Glasgow edition of his complete works and correspondence in the 1970s. The editors of the Glasgow edition worked hard to lay to rest the so-called “Adam Smith problem:” the relationship between his *Theory of Moral Sentiments*, a daring tour de force of a theory of ethics based on the psychological concept of empathy, and *The Wealth of Nations*, a study of economy growth based on self-interest moderated by the norms and conventions of a commercial society. They argued that the inconsistency between the altruism that motivates people in the *Theory of Moral Sentiments* and the selfishness that motivates them in the *Wealth of Nations* is more apparent than real because the crucial concept of “sympathy” or empathy in the former book is not at all what we mean by altruism, namely, caring for others to the point of sacrificing ourselves for them; besides, despite his puzzling failure ever to cross-reference the two books, Smith meant to integrate them in a projected but never written third volume on jurisprudence.

For a short time, we thought that this cleared up the problem. But the problem refuses to go away and rears its ugly head in every other paper of the Adam Smith industry. Over the years, Smith has turned out to be one of the subtlest and most complex thinkers in the whole of history of economic thought. The flood of books and articles on various aspects of his writings have been nothing short of amazing and we are sorely in need of a new stock-taking.⁹

There has been much less activity on David Ricardo and John Stuart Mill, although there is the “new view” of Samuel Hollander on Ricardo’s theory of wages (Peach, 1988) and the radical reassessment of a number of minor classical writers (O’Brien, 1988), who have never previously been given their full due as independent thinkers. Over and above all that, or perhaps a little to one side, there has been the steady, relentless attempt by Sraffians to construct a fundamentally new interpretation of all classical economics as “surplus theory” that would once and for all destroy the notion that modern neoclassical economics deserves the name *neoclassical* (Kurz and Salvador, 1998). However, these efforts have attracted relatively little attention, particularly on the American side of the Atlantic, and remain an unresolved issue (but see Blaug, 1999a.)¹⁰

The classical economist who has undergone the most radical reinterpretation in recent years is Thomas Robert Malthus. On the one hand, we have Samuel Hollander’s (1996) mammoth rational reconstruction of Malthus as the relentless opponent of Ricardo’s value and distribution theory, who was only incidentally the population theorist that we (and his contemporaries) know. On the other hand, we

⁹ West’s (1988) last survey is now over a decade old. Tribe (1999) falls well short of a general stock-taking, but it does suffice to show the richness of Smith’s writings.

¹⁰ See also Backhouse (1996, p. 11–12) for the ahistorical use of the label “classical” in leading current textbooks of macroeconomics.

have Donald Winch's (1987, 1996) cogent historical reconstruction of Malthus as the economist that every Victorian had read. Malthus's religious views, which have hitherto been slighted, are now given full scope in a new understanding of Christian Political Economy in the classical period (Waterman, 1991). The reappraisal of Malthus does not entirely dispel the old Marxist libel that Malthus was a propagandist for the landed classes, nor the liberal indictment that he was an apologist for a reactionary social order, but nevertheless, these older views of Malthus are barely recognizable in the new interpretations (Waterman, 1998; Pullen, 1998).

A new appreciation of nineteenth-century German and French economic thought has recently overturned yesterday's treatment of the marginal revolution of the 1870s. We always knew that there were anomalies in the standard account of how the triumvirate of Jevons, Menger and Walras discovered the new marginal economics simultaneously but independently between the years 1870 and 1874. Examples of the anomalies include such notables as Cournot in 1838, Dupuit in 1844, von Thünen in 1852 and Gossen in 1855, but they were largely considered as distractions. However, Erich Streissler (1990, 1994) has drawn attention to a proto-neoclassical German tradition around the middle of the nineteenth century—including von Thünen and lesser-known writers such as Hermann, Rau, Hufeland, Mangoldt and Schäßle—who all understood diminishing marginal utility, diminishing marginal productivity, opportunity costs and substitutability at the margin, so that "the basic marginal concepts were all there for Menger to use" (Streissler, 1990, p. 46). Indeed, the first appearance of subjective value theory and a demand and supply diagram—with price on the vertical axis as in Marshall—was in the fourth 1841 edition of Rau's *Grundsätze der Volkswirtschaftslehre* (1826), the first standard German textbook that ran to eight editions in the next 40 years.¹¹

Similarly, we have become aware in recent years that Cournot and Dupuit were not isolated figures in French economics. Ekelund and Hébert (1989) have shown that a whole group of French engineers, centered on the Ecole National des Ponts et Chaussées, independently elaborated the basic concepts of modern microeconomics as much as half a century in advance of Jevons, Menger and Walras. These engineers were not academics, and their analysis was focused on the practical problems of roads, canals and railways. This practical bent in their studies gave them a fresh approach to economic problems, but it also explains why their ideas were never systematically developed and failed to communicate with traditional economics taught in the French universities.

Putting Streissler together with Ekelund and Hébert destroys the usual history of economic thought textbook account of the Marginal Revolution as a curiously isolated event in Manchester, Vienna and Lausanne, which then took three or four

¹¹ Hutchison (1953, p. 132) notes: "In their analysis of value, production, and distribution, one or two of the German 'Classics' were, on many questions, several decades ahead of their English contemporaries." He was only wrong in surmising "one or two" when he should have said "four or five."

or even five decades to pervade the economics profession as a whole. Indeed, the new historical revision makes it even more difficult to explain why the marginalist revolution took so long to succeed. How can an evolution of 30 to 40 years be called a revolution?

Of the three members of the triumvirate, Leon Walras has proved to be the most complex and contradictory, as enigmatic in his theoretical intentions as was Adam Smith. We have long known that this most abstract of economic theorists was also an avid applied economist, but the precise relationship between positive and normative economics in Walras is only slowly being understood (Jolink, 1996). Donald Walker (1996) has shown how Walras travelled in successive editions of his *Pure Elements of Economics* towards an ever more formalistic solution of what has long been known as “the existence problem”—is simultaneous market clearing possible in all the markets of an economy?—at the expense of his earlier semi-realistic account of “the stability problem” via a tâtonnement process. As equilibrium came increasingly into the limelight, disequilibrium virtually dropped out of the picture and with it any minimal description of market institutions (Walker, 1997).

At long last, it can be said that the history of general equilibrium theory from Walras to Arrow–Debreu has been a journey down a blind alley, and it is historians of economic thought who seem to have finally hammered down the nails in this coffin (Ingrao and Israel, 1991). It has been a dead alley because the most rigorous solution of the existence problem by Arrow and Debreu turns general equilibrium theory into a mathematical puzzle applied to a virtual economy that can be imagined but could not possibly exist, while the extremely relevant “stability problem” has never been solved either rigorously or sloppily. General equilibrium theory is simply a research program that has run into the sands (Blaug, 1997a). This is clearly a debatable appraisal but only so, I believe, because every reference to interdependence among the various parts of an economy is sanctioned by appeal to something called “general equilibrium theory.” But the fact that everything depends on everything else is a far cry from the theory that Walras invented.

General equilibrium theory, which had been dying a slow death ever since Walras’s own death, was revived in the 1930s by Hicks, Lange, Hotelling and Samuelson even as the Keynesian revolution, the monopolistic competition revolution, the social accounting revolution and the new welfare economics rose to a crescendo, while in the background, so to speak, the Austrian process-conception of competition gradually emerged out of the so-called socialist calculation debate. For years it was fondly believed that Oskar Lange won that debate hands down, demonstrating that general equilibrium theory could address practical issues like the feasibility of a socialist economy. A re-examination of that debate (Lavore, 1985; Blaug, 1997a) has revealed the absurdity of that claim, revealing once again that the history of economics is being constantly rewritten, and that rewriting is directly related to how we understand current doctrines like general equilibrium theory.

The 1930s was a decade of simple unprecedented fecundity in economic thought but it was succeeded by an even more fertile decade after the end of World War II. The publication of the Arrow-Debreu paper of 1954, proving the existence of general equilibrium, and Samuelson's announcement of "the neoclassical synthesis" in the third edition of his *Economics: An Introduction* (1955) marks the true birth of what has ever since been called "neoclassical economics." This was what I have called the "Formalist Revolution" (Blaug, 1999b) and its explanation—why 1945–55 and not earlier or later?—remains a question that is only slowly being addressed in new literature on that fateful decade that is appearing even as I write.

The really new history of economic thought that has just come into view is what the Germans used to call *Geistesgeschichte*: overarching themes in the history of economic thought or entire eras caught in one great sweep. It is strikingly exemplified by the books of Philip Mirowski, particularly *More Heat Than Light* (1989), in which the history of economic thought is seen in an entirely new light as a story inseparable from the ways in which physicists and other natural scientists have viewed the world. While often wrong on historical detail—for example, De Marchi (1993) sharply criticized Mirowski's treatment of such key figures as Walras, Marshall and Wicksell—the book continues to offer a style of history in which we care less what the great minds of the past actually said and care more about the intellectual milieu in which they said it.

Finally, there has been a virtual explosion of books on the history of econometrics, beginning with Epstein (1987), Morgan (1990) and Duo Qin (1993) on the probability approach of the 1940s and the structural estimation methods of the Cowles Commission, and culminating in the magnificent study of Hendry and Morgan (1995) in which all the great papers in the twentieth-century history of econometrics are discussed and many of the original empirical verifications are reworked with modern techniques (see also Darnell, 1994). After this initial outburst, it is my impression that interest on the history of econometrics has once again subsided. To the extent that the role of econometrics in economics remains a live issue today, further exploration of why the subject has evolved in the last few decades in the way it has would surely pay handsome dividends. Macroeconomics has become ever more theoretical in the 1980s and 1990s while econometrics has become ever more atheoretical, with many leading econometricians giving prominence to data exploration before venturing into empirical generalization. How come? I dare say most of us do not think of that as a question historians of economic thought address. But, yes, they do.

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