

Earw(h)ig: I can't hear you because your ideas are old

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This paper provides a critical challenge to the Whig view of economic ideas, which holds that good ideas from the past are embodied in the common scientific wisdom. In contrast to this position, we contend that the market for ideas, while no doubt competitive in terms of scientific rivalry, is not free of distortions in the incentives and signals that guide economic scientists. As a result, ideas that are flawed can come to dominate the profession, while useful ideas are left on the proverbial sidewalk of intellectual affairs. The smooth evolution of economic thought from falsehood to truth that underlies the Whig perspective is complicated by both historical circumstances and the intimate relationship between economics and politics that follows from the attraction of public policy for those who enter the discipline.

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1. Introduction

The financial crisis that has engulfed the Western democracies since the autumn of 2008 has brought with it renewed interest in the history of economic ideas. Many explanations of the crisis emphasise the wrong direction economic theory took after the paradigmatic shift in macroeconomics in the 1970s and 1980s. The microfoundations revolution of New Classical Economics, the argument goes, with its excessive formalism and blind faith in the rationality postulate at the individual level and the competitive market at the system level, misled an entire generation of professional economists in academia and in public policy. The mainstream of the profession believed that aggregate volatility had been eliminated by perfected knowledge of monetary policy rules and the evolution of refinements in finance theory that appropriately priced and micromanaged risk in a way that had never been possible. Two factors contributed to this confidence of mainstream macroeconomists and modern finance theorists. The first was the period of dampened macroeconomic volatility in the USA and UK between 1980 and 2008. The second was the tremendous financial innovation during this same period.

But when aggregate volatility returned, it returned with a vengeance and the ideas of theorists of aggregate volatility returned as well. Hyman Minsky was one of the first

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theorists to be resurrected, but the subsequent professional and public debate has not resulted in the ‘Minsky moment’ becoming the main explanation for the crisis.¹ Instead, John Maynard Keynes has captured the moment (see, e.g., Skidelsky, 2009) and, just as in the 1930s, his main adversary is F. A. Hayek. A recent YouTube² music video that has had over 2 million views summed up the situation with the refrain, ‘We have been going back and forth for a century.’ ‘I want to steer markets’ [Keynes]. ‘I want them set free’ [Hayek].³

Of course, the confidence that mainstream economists had in their theories tended to gloss over some troublesome data points that heterodox critics were always quick to point out. The experience with Long-Term Capital Management, the Internet bubble and the Asian financial crisis could in retrospect be pointed to as reasons why a red flag may have been raised concerning the claim that aggregate volatility had been conquered. But the empirical record also seems to show that the aggregate volatility did not increase with those events, and instead markets corrected and asset values continued to increase. Heterodox critics on the left tended to focus on the excessive *laissez-faire* thrust of economic theory and policy from 1980 onwards, while heterodox critics on the right tended to focus on the continued reliance on credit expansion and excessive government spending to artificially fuel economic growth and the continuation (and expansion in other areas) of government regulation of the economy. Critics left and right, though, agreed that the mainstream of economic analysis had become occupied with excessively formal and unrealistic models and with an obsession for empirical tests of statistical significance rather than deeper historical and institutional knowledge. The teaching and research of economics had gone astray.⁴

Critics of the practice of mainstream economics also united in a desire to see more attention placed on the history of the discipline. Part of this was to refocus energy on

¹ See David Prychitko (2010) for a re-evaluation of Minsky’s contributions to macroeconomics and the explanation of financial instability in comparison to the theories of Mises and Hayek.

² See <http://www.youtube.com/watch?v=d0nERTFo-Sk>. The video is the product of EconStories and on their website they have a follow up that is more along the lines of traditional economic source material for the debate between Keynes and Hayek (<http://econstories.tv/learn.html>). A second video, *The Fight of the Century: Keynes versus Hayek Round Two* (<http://www.youtube.com/watch?v=GTQnarzmTOc>) has now attracted more than 1 million views.

³ Hicks’s (1967, p. 203) judgement seems to be not only right for the period he wrote about, but for our time as well. ‘When the definitive history of economic analysis during the nineteen thirties comes to be written,’ Hicks wrote, ‘a leading character in the drama (it was quite a drama) will be Professor Hayek ... Hayek’s economic writings ... are almost unknown to the modern student; it is hardly remembered that there was a time when the new theories of Hayek were the principal rival of the new theories of Keynes. Which was right, Keynes or Hayek?’

⁴ In the UK, leading economists actually wrote an apology to the Queen (<http://media.ft.com/cms/3e3b6ca8-7a08-11de-b86f-00144feabdc0.pdf>). With the financial assistance of George Soros, the Institute for New Economic Thinking was established to reassess the state of the art in economics and its public policy application (<http://ineteconomics.org/>). In the USA, John Cassidy ran a set of interviews with Chicago school economists about the financial crisis in *The New Yorker* (<http://www.newyorker.com/online/blogs/johncassidy/chicago-interviews/>) and there was the Congressional testimony of David Colander, who argued that modern models fail to account for economic complexity (<http://causesofthe-crisis.blogspot.com/2009/09/failure-of-economists-to-account-for.html>), and Robert Solow, who argued that the pre-occupation with dynamic stochastic general equilibrium (DSGE) models in modern macroeconomics was a problem (<http://www2.econ.iastate.edu/classes/econ502/tesfatsion/Solow.StateOfMacro.CongressionalTestimony.July2010.pdf>). But the call to reform economic education has much better traction in Europe than in the USA.

particular thinkers, such as Keynes or Hayek. But some made the more important argument that a serious study of the history of economic thought would give some perspective in the training of economists. A serious study of the continuities and discontinuities in the history of the discourse over economic theory and economic policy could perhaps prevent the next generation from going down blind alleys or could even give modern practitioners ideas and even approaches that would be useful to addressing the pressing problems of today.

The idea that the past could help us today is actually a heretical notion in the modern scientific literature. Modern science is grounded in the idea that the more mature a discipline, the less it pays attention to its past. It is only disciplines such as those in the humanities that look backwards for wisdom and insight; sciences only look forwards. As Alfred North Whitehead (1929, p. 162) argued: 'A science that hesitates to forget its founders is lost.' The working hypothesis is that if an idea from the past is any good, it is embodied in the common scientific wisdom that current practitioners have as background when starting their investigation into the new. There is no need to study older ideas in their original presentation: what is useful is now common knowledge. Further, what isn't useful has been discarded, and it is this process of critical weeding out of bad ideas and adoption of good ideas that leads to scientific progress. This vision of science is essentially a Whig theory of the history of ideas.

The purpose of this paper is to challenge the Whig vision of progress in economic thought and public policy. We contend that the market for ideas, while no doubt competitive in the sense of scientific rivalry, is not free of distortions in the incentives and signals that guide economic scientists. As a result, ideas that are flawed can come to dominate the profession, while useful ideas are left on the proverbial sidewalk of intellectual affairs. The smooth evolution of economic thought from falsehood to truth that underlies the Whig perspective is complicated by both historical circumstances and the intimate relationship between economics and politics that follows from the attraction of public policy for those who enter the discipline.

We proceed as follows. The next section asks the question: does economics have a useful past? We argue that it does, precisely because the evolution of science is 'lumpy' as compared with linear, which implies that mistakes in thought can be made and must be corrected. Section 3 is motivated by the question: does the past have a useful economics? Again, we contend that the answer is 'yes' and argue that there is much that the contemporary economic theorist can learn from the past. Section 4 deals with a tension in the position developed in the preceding sections. An economist has to have a basic disciplinary and technical competence to assess economic arguments. However, Stigler noted that those who possess these skills will find little professional reward to the study of the history of ideas and will therefore tend to be supportive of the Whig position. To resolve this tension, we draw on the concepts of the 'endogenous past' and the 'extended present'. Section 5 concludes.

2. Does economics have a useful past?

Leading representatives of twentieth-century mainstream economics, such as Paul Samuelson (1987) and George Stigler (1969), argued that only good economic ideas survive in the competitive business of scientific publication. The best ideas not only get published in the top journals but they receive the most citations, and the developers of those ideas get the best appointments in the scientific community and earn the highest

honours that the profession offers to its practitioners. The market for ideas, in this rendering, conforms to the perfectly competitive model and survivorship is a strong indicator of efficiency.

Ironically, both Samuelson and Stigler had career-long fascinations with the history of economic ideas, from their student days onwards. But the implication of their Whig argument is that such study of the past is too costly an endeavour for the benefit received in terms of scientific progress. The underlying logic is that the history of ideas is incapable of affecting current scientific progress and modern aspiring economists have too many other important things to learn during their graduate training. As Samuelson put it, ‘something had to give in the economic curriculum. What gave, and gave out, was history of thought’ (1987, p. 52). All that is good has already been incorporated into modern theory and thus all that we can learn from the past is the wrong ideas of long-defunct economists. Stigler (1969, p. 217) states the proposition simply: ‘one need not read in the history of economics—that is, past economics—to master present economics’. ‘This will not be news,’ he goes on, ‘to the present generation of economists. The young theorist, working with an increasing formal, abstract, and systematic corpus of knowledge, will seldom find it necessary to consult even a late-nineteenth-century economist.’

The more advanced a science is, the less attention it will pay to its past. In fact most practitioners of modern economics will view an interest in older ideas as a signal that anyone who works in the field of the history of economic thought is either not quite up to the challenge of modern theory and/or in possession of strange preferences. Modern economists will conclude, as do his colleagues in the mature sciences such as physics and chemistry, that ‘the history of the discipline is best left to those underendowed for fully professional work at the modern level’ (Stigler, 1969, p. 218).

Unfortunately, this attitude is self-reinforcing within the economics profession. The incentive system in competitive scientific exploration ensures that the best and brightest analytical minds will steer clear of the history of ideas and focus instead on making contributions to current economic theory and empirical analysis. The attitude is, as Diana Strassmann (1993A, p. 147) put it: ‘Good economists spend their time searching for better theories and more accurate ways of testing their theories. Once a theory has been supplanted by a better theory, there is little point in dwelling on the inferior one unless, of course, it has pedagogical value, as a building block for better theories or as an illustration of a misguided theory rejectable by well-constructed econometric tests.’ Following this line of reasoning, mainstream economists will tend to perceive that those who self-select into studying the history of economic ideas will not be as analytically astute as those who steer clear of the history of thought. But only an analytically astute economist can really understand what an economist is up to in his analytical argument. As a result, the general opinion is that a lot of what passes as the history of thought is actually incompetent readings of the analytical presentations of older thinkers. In short, the secondary literature in the field is worse than the primary literature that was already being ignored. In addition to exhibiting bad analytical skills, the secondary literature is characterised by either a hypercritical attitude towards those the interpreter doesn’t appreciate and most likely doesn’t understand or an adulatory attitude that seeks to praise the great works and most often perceived heroic struggle of the earlier unjustly neglected thinker. Neither hypercriticism of mainstream thinkers nor hagiographic essays on unorthodox thinkers of yesterday will be that welcomed by current practitioners of economics.

Perhaps the history of thought could be useful to the workaday economist, but given the perspective described above, practitioners will have to demonstrate that usefulness to the modern economist. It is, Mark Blaug (2001, p. 145) states, no secret that the history of economic thought is held in low esteem, perhaps even contempt, by most mainstream economists. The real challenge is to demonstrate that they shouldn't hold the subdiscipline in just disrespect. Economics, Stigler concedes, may in fact have a useful past, but there are many possibly useful goods and services in society that are not produced because they are worth less than what they cost to produce. It remains the task of historians of economic thought to prove to their fellow economists that the subject is worth the cost. To date that task remains unfulfilled (Stigler, 1969, p. 229). Samuelson (1987, p. 51) is equally blunt: 'When I began graduate study a million years ago, history of thought was a dying industry.' In the period since that time, the field of history of thought has died completely. And, Samuelson argues, the economics profession was right to be contemptuous of the past, especially since the only reason the history of economic thought hung around as long as it did was because of the 'decadence of literary economics'.

Since Samuelson and Stigler both possess an interest in the history of economic ideas, they do try to provide some reason for their fascination other than peculiar intellectual preferences or accident of their educational path. Stigler argues that the history of thought helps us learn how to read and also how to react to what we read. But as we have seen, he argues that this method of learning may be too costly given the benefit; it is an empirical question as to whether the serious study of the history of thought could be beneficial to a current practitioner. Samuelson is more pro-active and argues that a programme for Whig history can be fulfilled through rational reconstruction of the best arguments from the past through the analytical lens of modern theory. In both cases the challenge is thrown down to historians of thought to demonstrate that their work is professionally competent and of value to their workaday contemporary colleagues in economics.

There is a certain logical consistency to the Samuelson–Stigler position, but it assumes that the market for ideas operates efficiently. But what if Kenneth Boulding (1971) is correct and the market for economic ideas does not operate as smoothly as assumed? Economic ideas are prone to momentary political and intellectual fads and fashions. As much as we would like to delude ourselves into believing this, debates are not always determined in economics on the basis of logic and evidence. Sometimes positions are discarded due to their inconvenience for the moment. Sometimes positions are accepted due to their political expediency. The discipline can become derailed not just by politics but by shifts in philosophical movements.

A pre-occupation with methodology by an economist, like the pre-occupation with the history of ideas, is often attributed to an inability to master contemporary technical economics. But this clearly wasn't true for thinkers such as Frank Knight, Lionel Robbins, Milton Friedman and Paul Samuelson, who made pivotal methodological arguments during their respective careers. Scientists cannot completely ignore philosophy, no matter how hard they would like to. As Daniel Dennett put it in *Darwin's Dangerous Idea* (1995, p. 21), 'Scientists sometimes deceive themselves into thinking that philosophical ideas are only, at best, decorations or parasitic commentaries on the hard, objective triumphs of science, and that they themselves are immune to the confusions that philosophers devote their lives to dissolving. But there is no such thing as philosophy-free science; there is only science whose philosophical baggage is taken on board without examination.'

The philosophy of science and methodology matter because they ultimately determine the acceptable questions to pursue, while also providing the criteria to judge what a good answer would be to those questions. When Samuelson, for example, refers to the ‘decadence of literary economics’, he is making a philosophical statement more than an analytical judgement. When Smith, or Ricardo, or Mill were writing out economic arguments, the philosophical baggage was different from that taken on board by Keynes, or Samuelson, or Friedman, or Hayek, or Buchanan. Robert Lucas once described himself as the bastard child of Friedman and Samuelson, and he meant that philosophically in both the sense of methodology of economics and political ideology.⁵ Lucas, no doubt, had philosophical baggage coming on board that related to not only meta-questions such as the role that economic forces play in the grand scheme of history, but also how one studies those economic forces theoretically and how one measures the impact of those economic forces empirically. Scientific practice is determined by philosophical positions, which practising scientists do not in general study with the same care that they bring to mastering their discipline.

We are not arguing that economists should stop learning and doing analytical economics and techniques of empirical analysis in order to pick up tomes in the philosophy of science. There are very sound reasons for an intellectual division of labour. All our argument implies is that the confidence that one has in whether all that is valuable from the ancients is embodied in the moderns will depend on a set of philosophical beliefs that are in fact contested and not settled. Furthermore, even if we could agree that there is potential scientific progress from continually submitting theoretical conjectures to empirical test, we need to recognise that such a vision of science is more murky than is commonly understood, because the procedures of testing, let alone the interpretation of the results of the tests, are a bit more difficult than is commonly assumed. The Duhem–Quine thesis is particularly challenging in the field of economics, because the economy is an example of a complex phenomenon and the ability even to mimic the controlled experiments that are conducted in the more mature sciences that economists strive to imitate is restricted. If the Duhem–Quine thesis holds for physics, then it certainly holds for economics. The upshot is that empirical testing is never unambiguous; it is always difficult to discern whether the specific theoretical hypothesis being tested has been rejected or whether it is a subsidiary argument in the network of theoretical arguments that was employed that is being rejected.

Philosophical positions are being brought on board unexamined, empirical tests are ambiguous and we have always to remember that science—even the most natural of the sciences—are human endeavours. All science is human science because only humans practise science (see McCloskey, 1985). And with that come at least two other difficulties—one sociological and the other economic. First, science is a social enterprise and not an isolated endeavour, despite the long hours scientists spend in the laboratory, or in the library, or at their desks writing up results. It is a communicative process and it has strong rules associated with this social interaction. Reputations are hard-earned and continuously at risk. Michael Polanyi (1962) argues that activity in the ‘republic of science’ is characterised by three forces that judge contributions: (i) the intrinsic interests of the scientific community; (ii) the plausibility of the results; and (iii) the creativity demonstrated by the scientist in posing a new question and providing a new answer or offering a new approach. The first two forces are ‘conservative’; the

⁵ See Arjo Klammer’s interview with Lucas in *Conversations with Economists* (1983, pp. 29–57).

last is 'revolutionary'. The play between these three forces is the essential tension that exists in the scientific community that disciplines participants while also permitting scientific progress. The tug and pull of science is a lumpy, and not a smooth, process and this lumpiness leaves space for errors. This means that ideas that are thought to be dead may actually have much more to offer, while ideas that are thought to be live may actually lead down dead ends. Science self-corrects, but not instantaneously; and just as the market requires entrepreneurs for self-correction, science requires intellectual entrepreneurs who see the current inefficiencies in the marketplace of ideas (e.g. those ideas lying on the sidewalk) and act upon them through acts of intellectual arbitrage and/or intellectual innovation to contribute to scientific progress.⁶

The economic consideration of scientific processes is that scientists in physics, chemistry, biology, etc., let alone economics, are human actors; they are rational choosers. We pursue our goals as effectively as we can in our scientific endeavours in the same way as we do in our commercial behaviour. This does not rule out 'truth seeking' as a primary motivation in science, but it does mean that we have to recognise that scientists, like other actors we examine with the tools of economic reasoning, are rational choosers who are making choices against a background of constraints. There are incentives that the scientist responds to and there are informational signals that guide behaviour and provide disciplinary feedback.

The lumpy nature of the process of scientific discovery means that at each step along the way, error exists throughout the system that individuals are busy working to eradicate. The 'efficiency' of the scientific process is not to be gleaned from the approximation of existing knowledge to some objective notion of timeless truth, but instead from the mechanisms at work that highlight flaws in the existing knowledge and steer activity in less erroneous directions than before. Ironically, Mises perhaps captured the picture of science as an ongoing process better than any other economist:

It is certainty for many people to blame economists for being backward. Now it is quite obvious that our economic theory is not perfect. There is no such thing as perfection in human knowledge, nor for that matter in any other human achievement. Omniscience is denied to man. The most elaborate theory that seems to satisfy completely our thirst for knowledge may one day be amended or supplanted by a new theory. Science does not give us absolute and final certainty. It only gives us assurance within the limits of our mental abilities and the prevailing state of scientific thought. A scientific system is but one station in an endlessly progressing search for knowledge. It is necessarily affected by the insufficiency inherent in every human effort. But to acknowledge these facts does not mean that present-day economics is backward. It merely means that economics is a living thing—and to live implies both imperfection and change.⁷ (Mises, 1969 [1949], p. 7)

Ultimately, this implies that existing judgements may be wrong for a variety of reasons, ranging from delusion to vested interest. In short, knowledge that was once had can get lost, not just displaced by superior knowledge, and knowledge that is presented

⁶ See Leeson et al. (2006) for a discussion of the market process theory of self-correction; our vision of the scientific process is analogous. See Butois and Boettke (2002) for a discussion of the similarities and differences between the entrepreneurial process in the market and in science. Also see Strassmann (1993B), where she argues that the lack of a true contestation of ideas in economics limits the use of the market metaphor in discussions of the exchange of economic ideas. Instead, the monopoly power exhibited by the mainstream means that there are distortions in the valuation of economic ideas due to the barriers to entry.

⁷ This is ironic only because critics of Mises, such as Samuelson, attribute to Mises a position of extreme dogmatism about his economics.

as superior may in fact be inferior.⁸ Science is a competitive process, but, in the field of economics at least, it is also a field of inquiry whose most immediate practical application is public policy, and thus politics.

Philosophy, politics and our humanity are inescapable, but they also mean that economic science does not progress in a linear fashion. There can indeed be significant wastes of intellectual resources at any point in time within the scientific process due to intellectual fads and fashions (see Boulding, 1970). It is our contention that, once that argument is accepted, then the sort of depiction of the Whig history of economics as laid out by Stigler and Samuelson is untenable. Instead, as Boulding (1971, p. 227) argued, ideas from older economists that may not be currently fashionable can nevertheless be part of our ‘extended present’. ‘The question of whether economists who are not primarily historians,’ Boulding (1971, p. 230) states, ‘but who are practicing their art, trade, science, or whatever it is, need to pay any attention to the classical economists, or to any writers of the past, depends on one’s estimate of the extent to which the evolutionary potential of these past authors has been realized or exhausted.’

We don’t just study the evolution of economic ideas to get a better grasp on current theory, as Schumpeter (1954, p. 4) could be interpreted as saying.⁹ We certainly don’t study the history of economic thought only as a method of learning how to read and react to economic argument, as Stigler said. And it is an act of extreme hubris to suggest that the only way to treat older thinkers is to find in their work the models that present-day thinkers are working with, as Samuelson suggests. No, in Boulding’s argument we continue to read Adam Smith because Adam Smith’s ideas still have evolutionary potential for our efforts in contemporary theorising. In a world where the evolution of ideas is lumpy and not a continuous straight line, reading classic works in economic thought (and perhaps some not so classic works), like reading the most recent issue of the *American Economic Review* or the *Economic Journal*, can prove to be a necessary input into our efforts as workaday economists. The history of thought is one way, among several ways, of doing contemporary theorising.¹⁰

3. Does the past have a useful economics?

In the Section 2 we argued that economics does indeed have a useful past. We should be clear that we believe the historian of economics must shoulder the burden of proof against the arguments used by Samuelson and Stigler. The bottom line is that there

⁸ The idea here is that we must recognise the potential for a ‘Kuhnian loss’ in the evolution of economic ideas. In the Whig theory, the judgement is that the loss is either negligible or non-existent (and clearly the costs are far less than the benefits of the paradigmatic shift), but to the contra-Whig, the ‘Kuhnian loss’ may indeed be significant and thus a great intellectual entrepreneurial opportunity exists for those who can capture what was lost in the paradigmatic shift. In Boettke (2012), the argument is made that it is useful to distinguish in between ‘mainline’ and ‘mainstream’ economics within the scientific discipline, and that the emergence of various schools of thought can be explained by the acts of intellectual entrepreneurship at times when the ‘mainline’ and the ‘mainstream’ deviate from one another significantly.

⁹ Though if one reads to the end of the second full paragraph on that page, Schumpeter actually makes an argument similar to the one we have made: ‘Scientific analysis is not simply a logically consistent process that starts with some primitive notions and then adds to the stock in a straight-line fashion.’ Science is instead ‘an incessant struggle with creations of our own and our predecessors’ minds and it “progresses”, if at all, in a criss-cross fashion, not as logic, but as the impact of new ideas or observations or needs, and also as the bents and temperaments of new men, dictate’. All modern thought, Schumpeter argues, is ‘historically conditioned’ and is rendered meaningful only ‘with reference to the historical background from which they spring’.

¹⁰ See Boettke (2001) where he argues that this is how Hayek in many instances approached the history of ideas in political and economic thought.

are opportunity costs associated with studying the history of ideas in economics, and it would be foolish for anyone to deny that. But if science is non-linear in its progression, then it is very possible that older ideas might actually outperform the so-called newer ideas that contemporary economists are working with.¹¹ In the ongoing development of economic doctrine, it turns out we can find several instances where what is true isn't new and what is new isn't true. But this assessment isn't always made in a timely manner due to implicit philosophical shifts, political expediency and human fallibility.

Historical context matters. Ideas do not emerge in an acontextual matter, but instead as part of a conversation. To treat ideas as disembodied from the discourse context is to abuse those ideas. This abuse can be justified to a considerable extent when using the history of thought as an input into contemporary theorising. Exercises focused on mining the history of ideas for theoretical construction purposes, almost by necessity, treat ideas 'opportunistically' rather than 'faithfully'. The contra-Whig position does entail a certain abuse of ideas from the perspective of the pure historian of ideas. It is perhaps useful here, though, to remember Stigler's two errors in reading: hypercriticism and adulation. Of the two errors, the Whig historian of ideas would tend to commit the error of hypercriticism of earlier writers who were incapable of availing themselves of modern sophisticated tools and techniques of analysis; while the contra-Whig historian of ideas would tend to commit the error of adulation of the misunderstood and unjustly neglected genius from the past. We have been trying to suggest that there is an intellectual path that can avoid both errors.

In our task as historians of ideas, we would argue that the contextualisation of thinkers in their time and place is essential for understanding. Ideas cannot be treated as disembodied entities to be assessed coldly, but instead should be treated as products of a specific discourse in time and place. The goal is neither to criticise nor to praise, but rather to situate and understand. But this is in our task as historians, not as economists.

Our task as economists is to think about how the economic system works, to get a better understanding of how the economic forces at work interact and affect the human condition. Ideas, in this context, are to be assessed for their logical validity and their pragmatic value in solving the problems we are addressing. Economics is the study of man; his choices against given constraints, his social relations in exchange and his material well-being constitute the subject matter of the discipline. The Boulding criterion is clear: are the ideas of a Smith, a Ricardo, a Say, a Mill, a Keynes, a Hayek, a Friedman, etc. still in possession of evolutionary potential for the purposes that we theorists today are trying to tackle? If so, then they are part of our extended present; if not, then it is best to leave the examination of those works to the historian.

A dispute over whether historians of economic thought should continue to work in economics departments or move into the field of the history of science broke out in the subdiscipline of the history of economic thought close to 20 years ago.¹² The position

¹¹ In other words, the 'Kuhnian loss' is greater than the perceived scientific benefits of the new development. In order to encourage original thinking, however, the scientific community (as Michael Polanyi, 1962, argued in 'The Republic of Science') must balance interest in the puzzle, plausibility of the solution and original thinking about what puzzles we should be studying, how we should be thinking about them and where we think the answers to the puzzles are to be found. There are conservative and revolutionary forces in science that exist in tension with each other in the play of the scientific game even under the best of circumstances. We are simply arguing that those who believe that a 'Kuhnian loss' is significant must face the burden of establishing that fact with their scientific peers.

¹² See Ross Emmett (2010) for an overview of recent contributions.

we are staking out straddles the divide and simply suggests that (i) the history of economic thought as a subdiscipline within academic economics should be large enough to tolerate both producers and consumers of the history of ideas, and (ii) that in their capacity as consumers of the history of ideas in economics, contemporary theorists can find many useful ideas that will help them in their efforts to build new ideas. We readily admit that the task of the historian is radically different from the task of the theorist. What we are arguing is that the work of the historian, as well as the primary text that the historian is working with, can be useful inputs into the production process of a modern theorist trying to tackle problems in their specific context of professional dispute and the historical purpose of economics.

All science, economics included, is shaped by historical context. In economics this is not just a matter of the public policy debates that arise in each generation, but also of the broader cultural *Zeitgeist*. What is considered science and what is considered art does determine what is a contribution and what is considered nonsense. The historian and the theorist negotiate this terrain differently. But make no mistake about it; neither of them can escape it. It is what it is and if you cut against the prevailing *Zeitgeist*, the road travelled to find acceptance for your ideas will be a difficult one, just as if you travel with the prevailing *Zeitgeist* you will find acceptance much easier. The trick is that the true innovators in science challenge not only the received wisdom in their chosen discipline, but also the *Zeitgeist* at least on some margin. This is just another way of stating the Polanyi point we alluded to earlier about the essential tension in the ‘republic of science’ between conservative and revolutionary forces.

There is much that the contemporary theorist of economics can learn by studying the evolution of economic ideas. Given the lumpy and non-linear progression of economic thought, older ideas can possess an evolutionary potential that is far from exhausted. It is also the case that we can understand those ideas and their potential by coming to understand the historical circumstances and the intellectual context within which the ideas we find intriguing developed. Economics has a useful past and the past has a useful economics, and both work with one another to make, in our view, a strong argument for the subdiscipline of the history of economic thought to remain open and in fact to encourage both producers and consumers of intellectual history. However, there is also a tension with this position that is actually somewhat delicate; this we will attempt to address in the next section.

4. When history and practice collide

Recall that we argued that the historian of thought needs to shoulder the burden of argument after the challenges of Samuelson and Stigler. One of the significant challenges is the social stigma that both alluded to for those who work on the history of ideas as opposed to contemporary work in economics. We have tried to counter the respective positions of Samuelson and Stigler by way of Boulding and the idea of the extended present. In short, we have tried to argue that reading in the history of ideas can be a useful way to engage in contemporary theory construction.

But this approach glossed over the more blunt challenge that Stigler, especially, put forward. Only an economist can really understand what an economist is up to. You have to have a basic disciplinary competence to assess economic arguments. Great writing, deep historical knowledge and philosophical sophistication, in the end, do not, and cannot, substitute for the ability to understand and work through the logic of an economic

argument. Unfortunately, those who possess that skill, Stigler contends, will find that (i) there are no professional rewards for treating the history of ideas seriously and (ii) will most likely be of the same mindset as their peers in the more mature disciplines.

There is a problem of self-reinforcing outcome from this intellectual predicament. In order to address this tension we draw on the notion of the ‘endogenous past’. The problem of the endogenous past is that the outcome of the evolution of ideas is a function of the previous acceptance or rejection of ideas. This implies that there is a sort of ‘path dependence’ or lock-in in intellectual affairs.

In its most blunt form, the endogenous past can take the form of a theoretical warning—e.g. Hayek’s *The Road to Serfdom* (1944) about the potential slippery slope of democratic socialism; precisely because it was heeded, the predicted outcome that was warned against was averted. In such an instance, the theoretical warning was not ‘refuted’ by the subsequent empirical record, even though it did not actually materialise.

A more subtle rendering of the problem of the endogenous past occurs when an idea or practice that is not as sound as one might like nevertheless becomes so widely utilised in the discipline and perhaps in the economy that it takes on a new meaning. An example of this is the notion of GDP accounting. Simon Kuznets, one of the pioneers of national income accounting, expressed many times his concern with the misuse of this measure in public policy. In order for the aggregate measure of economic activity to have meaning, prices must be assumed to be competitive equilibrium prices so as to make sure that the full opportunity costs of production were taken into account. Administered prices, as was the case in the wartime economy, do not work to capture the appropriate value of economic activity. If market prices were not at the competitive equilibrium (where $P = MC$), then the aggregation of prices would have little to no meaning in reference to the value created by economic activity.¹³ Economists at the time, such as Mises and Hayek, were highly sceptical of efforts to measure aggregate economic performance with national income statistics. Hayek (1979 [1952], pp. 108–9) went as far as to make a *reductio ad absurdum* that aggregate income statistics tell as much about the underlying structural relationships in an economy as the aggregate tally of words on a page tells us about the content of the message being conveyed on that page.

Hayek was arguing that national income statistics mask the underlying economic relationships that must be explored. Such aggregate statistics are meaningless, he was arguing. And as Kuznets’s warnings suggest, Hayek may in fact have been right. We don’t have to take a position on this for our purpose. The point is that in the subsequent years after Kuznets and Hayek, national income statistics have taken on a life of their own. Governments throughout the world have continually kept such statistics and based public policy decisions on them. For our purposes the more important point is that the market participants engaged in commercial life utilise such statistics. Investment decisions are made based on these aggregate statistics; elections are won and lost based on these aggregate statistics. National income measures have become very meaningful to many individuals throughout the world. Whether or not they are an accurate measure of well-being in an economy is beside the point; what matters is that what was once legitimately questioned as meaningless has taken on a very real economic meaning precisely because the questions about their meaningfulness were ignored.

¹³ See Robert Higgs (2006), especially the chapter on wartime prosperity.

As a more subtle instance of the logic of the endogenous past, consider also that the original proposition need not be rejected by subsequent developments, but instead can be rendered mute by the actions and declarations of leading professionals. To illustrate this, let us look back at the statements of Samuelson and Stigler and the impact they have had on the subsequent development of history of economic thought.

As we quoted Samuelson as saying, even when he was in graduate school, the history of thought was a dying industry. But Samuelson and Stigler continued throughout their careers to write contributions to the subdiscipline of the history of economic thought. William Baumol is another significant modern thinker to share their fascination with the evolution of economic ideas. However, they all (Baumol included) were major proponents of the argument that studying the history of economic thought is too costly compared with the benefit for the contemporary graduate student, let alone practitioner, of economic theory.

When the leading representatives of the mainstream who also happen to share a passion for the history of ideas make such an argument, the consequences of their position cannot help but shape the professional direction of the history of thought. Boulding's bold entrepreneurial effort to push back in the other direction takes on an entirely new significance. It is important to remember Boulding's stature within the economics profession; he was a John Bates Clark Award winner in 1949 and the President of the American Economic Association in 1968. This was no voice in the wilderness, pushing back against the tide of professional opinion. He was a professional insider who took an outsider position, but demanded the insiders' intellectual attentions.¹⁴ But Boulding's argument did not win the day. As Samuelson put it, something had to give in the curriculum of graduate education and that something was the history of economic thought. Stigler's projection that anyone who worked in the field must not be up to the task of modern analysis, simply by the force of him saying it, reinforced professional prejudices in that direction.

The problem with the endogenous past in this instance is that it creates a self-reinforcing system. The history of economic thought is irrelevant to the modern economist and those 'economists' who go into the history of thought are not capable of doing modern economics. Therefore, it is very prudent to ignore the subdiscipline of the history of economic thought.

Boulding is the only major thinker of professional weight since 1950 to try to counter this self-reinforcing path of professional irrelevance for historians of economic thought. His argument was not directed at the historian but at the contemporary theorist, and he attempted to demonstrate that ideas from the past can be part of our extended present and as such present great evolutionary potential for scientific progress. The title of his essay, 'After Samuelson, Who Needs Adam Smith?', suggested that he did not shy away from tackling the issue of the endogenous past head-on. But the fact that he didn't win the day is reflected by the increasing decline of the history of thought in the graduate curriculum since the time he wrote. There has been rise of membership in the History of Economics Society, but there is nobody of the professional stature of a Samuelson, or a Stigler, or a Boulding, let alone a Baumol, who even finds the subdiscipline a worthy pastime and is willing to

¹⁴ See Boettke (1998) for an argument about how effective economic dissent conforms to Thomas Kuhn's model of scientific change, not in the sense of the 'paradigm shift' but in terms of 'the essential tension' that is evident in the shifts in paradigms in science.

say so. If anything, the professional opinion among practising economists has grown more contemptuous, and this is seen by the low ranking on the Social Science Citation Index (SSCI) and other measures of citation impact of the leading journals in the field—*History of Political Economy*, *Journal of History of Economic Thought* and *European Journal of the History of Economic Thought*. The number of people writing in the field has grown, but the number of economists listening had dwindled (see [Blaug, 2001](#)). Mainstream economists cannot hear anything because ideas are deemed as too old unless they have been published within the last decade in the main journals of the profession.

5. Conclusion

The point we have tried to make is relatively simple—what if the counterfactual was realised and Boulding had in fact won the intellectual day rather than the positions articulated respectively by Samuelson and Stigler? If the history of thought was seen not as merely a tool to learn how to read or as raw material for rational reconstruction, but, instead, as part of our extended present, then young minds capable of making contributions to contemporary analysis would have found the contributions of historians of thought valuable. The historian of thought could be part of an ongoing conversation within economics about how best to think about problems, how to contextualise the problem in the light of historical background and how both to learn from the past and to use the past to better understand the present.

As Schumpeter argued, the evolution of economic ideas does not follow a straight line. It progresses in a criss-cross manner. We could even postulate a sort of corkscrew-type progression. Some philosophers of history seem to stress a sort of circle of ideas in human history, where the same ideas continue to surface and are debated over and over. Others postulate a smooth linear path from falsehood to truth. We are arguing that the history of ideas does not continuously repeat itself, but there are broad themes that do keep coming back to the surface of economic disputes (e.g. Malthus and Say; Keynes and Hayek), and that while progress is made, the path is non-linear and very lumpy due to shifts in philosophical perspectives, political expediency and human fallibility. It is such a non-linear path that enables the Boulding position to hold and for the history of ideas to be neither limited to a scriptural agenda (as it would in a circular world) nor be completely disregarded (as it would in a linear world).

The contra-Whig historian of economic ideas sees opportunities to build new theoretical constructions with the aid of past thinkers that involve taking the ideas of these past thinkers in directions that they themselves could never imagine. Some of the finest minds in the history of the discipline actually produced their theoretical contributions in this manner. Economic theory emerges in an ongoing dialogue with past and present minds who have wrestled with the most difficult problems in the worldly philosophy.

But we must unfortunately conclude that unless the problem of the endogenous past is met head-on, the discussions over the use of the history of economic thought will remain a private conversation among those who do history of ideas in economics or economics as history of science in science studies. Neither of these approaches is capable of addressing the opportunity, posed by the current financial crisis, for a re-evaluation of the economics profession, its contemporary practice as a science and its training of the next generation of economists. Those of us who believe the history of thought is an important subdiscipline that provides insight and perspective as well as guidance in theoretical construction, must be willing to shoulder the burden of

argument that both Samuelson and Stigler challenged us with. Boulding tried to provide an answer; he didn't persuade. We have to do better if we hope to change the attitude of our peers in the discipline and reverse the degenerative path of the discipline.

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