

The Historical Logic of Economics Imperialism and Meeting the Challenges of Contemporary
Orthodoxy: Or Twelve Hypotheses on Economics, and What is to Be Done

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Introduction

In describing developments within economics between 1945 and 1955 as a “formalist revolution”, Blaug (1999, 2001 and 2003) highlights what is an uncontroversial process of change in which mathematical and statistical techniques have assumed much greater, and ultimately, overwhelming significance. This point of this paper is not to question whether, in this respect, the decade pinpointed by Blaug marked a revolution either in initiating or extent of change, thereby creating a fundamental break between before and after. Rather, the intention is to set the formalist revolution in the wider contexts both of the history of economic thought and of the relationship between economics and the other social sciences. In this sense, the paper is concerned less with the rise of formal techniques than with shift in substantive content.

In doing so, however, it is accepted that the decade of the formalist revolution does represent a watershed in the evolution of economic theory and in the relationship between it and the other social sciences, as significant over the long term, if not necessarily so acute and dramatic, as the process of formalisation itself. During this decade, the core technical apparatus associated with what is now neoclassical orthodoxy reached its analytical pinnacle and became the standard for what was to follow. But the earlier process by which it was established involved a narrowing in scope of application both within economics itself and through detachment from the other social sciences. By contrast, once established, the core apparatus has become the foundation on which both to appropriate the discipline internally to an almost exhaustive extent (otherwise discarding what does not fit) and to colonise the subject matter of other disciplines, economics imperialism.

Inevitably, this is a big picture that cannot be filled out sufficiently in content and justification in a relatively short article across both the history of economic thought and its relations to the other social sciences. Consequently, the argument is presented through a series of hypotheses around how economics has been and has changed, with these hypotheses supported by illustrative material. A fuller, if far from comprehensive account, is to be found in Fine and Milonakis (2008) although this does not use the watershed around the formalist revolution as an organising framework. Those with expertise in the different branches of economics, and their own personal histories (if long enough), will be able to support, qualify or reject these hypotheses. But it is equally if not more important not only to reflect on the past but also to anticipate and prepare, make even, the future. Whether intended or not, the formalist revolution has ultimately led to an economics that has marginalised heterodoxy, history of economic thought and conscious and consistent methodology to an unprecedented degree both within its own boundaries by comparison with the past and by comparison with any other discipline.

This is a dismal picture within the dismal science. But prospects are not exclusively bleak since the prospects for political economy within the other social sciences are currently brighter than for two or more decades, despite the challenges posed by “economics imperialism”.¹ What political economy will be adopted and how is also remarkably open. This implies that the current generation of heterodox economists have a major responsibility in sustaining their critique of orthodoxy in and of itself and in constructively offering alternatives especially in relation to interdisciplinarity – rather than retreating into a strategy for tenuous survival on or outside the margins of orthodoxy.

Hypothesis 1: Economics is NOT Mathematically, Let Alone Conceptually, Rigorous

Blaug’s notion of a formalist revolution raises a number of issues. If correct, it implies that there was both pre-revolutionary and post-revolutionary periods as well as what is necessarily a relatively short period of revolutionary action itself. What is the nature and significance of this periodisation over and above what is presumably a passage from the informal, aformal, or is it non-formal to the formal, and what were the mechanisms that brought this about?

The first of these questions – the nature of the revolution if such it is – is open to a number of differing interpretations each of which is subject to contestation. Inevitably, for example, of significance is the status of mathematics in scientific inquiry in general, across social as opposed to natural science, and in application to economics in particular. Whilst mainstream economists have an image of themselves as rigorous and scientific in view of their use of mathematics, methodological debate within or, more exactly, around economics has pinpointed certain limitations that are imposed by the increasing reliance upon mathematical methods. These include undue attachment to deductivism, determinism and conceptual impoverishment, Chick and Dow (2001) for an overview.

To these insights, I would add two further considerations. First, the use of mathematics within economics is extremely limited. A colleague of mine has deplored the way in which economics has become the plaything of third-rate mathematicians. This is certainly true in the sense of the breadth of mathematics that is used in economics, deriving primarily from calculus and statistical methods. Whilst the breadth and depth of the mathematics used by economists has undoubtedly ratcheted up across all levels of the subject, it almost certainly has not kept pace with developments within mathematics itself. It is extremely easy for a mathematician,² or someone from the natural sciences trained in the necessary techniques, to become an economist but the transition in the opposite direction is, I suspect, almost unknown.

In addition, complementary to the limited use and knowledge of mathematics, economics has displayed little or no interest in the inner limitations of mathematics itself, as opposed to the greater, if now marginalised, attention paid to the limitations in applying it to economics. As I have argued elsewhere, problems in the philosophical foundations of mathematics – those associated with Russell's paradox – mean that there are corresponding problems in the application of mathematics to social science and so to economics, Fine (2006b).³ Specifically, if we build up our theory from the aggregated behaviour of individuals or individual interactions, this imposes certain limitations upon the social properties that can be derived. In particular, for example, concepts such as liquidity, institutions, the state and identity cannot be satisfactorily accommodated within a mathematical approach, not as such, but in one that relies upon methodological individualism as does, of course, the core of mainstream economics and increasingly so in its deployment of mathematics in practice. Not surprisingly, the leap from aggregation over individuals to the social is one of faith, and neither conceptual nor mathematical logic.⁴

My first hypothesis, then, is that following the formalist revolution, if not before, the use of mathematics by economics has been for instrumental and deductive purposes and, as such, has been highly selective and careless in its use of mathematics. This has been most strongly highlighted, and criticised, by the critical realist school of economics, most closely associated with Tony Lawson. But it is also implicitly accepted by Backhouse (2007), for example, in part in opposition to critical realism on the grounds of the primary role that mathematics plays in providing models as instruments for problem solving rather than satisfying methodological niceties. One commentator on my point about the implications of Russell's paradox for economics suggested that neither bridges nor economic theory fall down because of the philosophical foundations of mathematics. But the latter does shed light both on the nature and limitations of classical mechanics and neoclassical economics irrespective of the motives, and success, of engineers and economists. At a more superficial level, the idea that the formalisation of economics has removed it from an attachment to real world problems has been strenuously denied by both Solow (2005) and Dasgupta (2002) in light of the extent to which theory has been heavily and increasingly attached to empirical work, Mäki (2002) for a broader assessment. There is a questionable assumption here of essentially equating realism (and problem-solving) with econometrics or more general interrogation of the data/facts. This is more evidence of the carelessness with which mathematical models and statistical methods are deployed, not least for example in the Barro-type regressions associated with new growth theory.⁵

As I have argued elsewhere against the critical realism school, the nature of, and opposition to, mainstream economics and its mathematical methods cannot be taken much further without critically engaging with the substance of its economic theory, Fine (2004, 2006a and 2007a). This is not least because mainstream economists themselves show little interest in, or self-awareness of, the nature of their own methodology and its weaknesses, or even that their claimed parallel with the methods of the natural sciences has long been superseded in principle and, to a large extent, in practice.⁶ Thus, significantly, in his defence of orthodoxy, Dasgupta (2002, p. 57) opens by confessing that, "Most economists ... have little time for the philosophy of economics as an intellectual discipline. They have

even less patience with economic methodology. They prefer instead to *do* economics ... There is much to be said for this habit ... I know of no contemporary practicing economist whose investigations have been aided by the writings of professional methodologists". Further, neglect of history of economic thought is justified by reference to the methods of the natural sciences, "You can emerge from your graduate studies in economics without having read any of the classics, or indeed, without having anything other than a vague notion of what the great thinkers of the past had written", for "She reads Ricardo no more than the contemporary physicist reads James Clerk Maxwell", p. 61.

Given, though, that the formalisation of economics did involve the heavy adoption of mathematical techniques and that this also involved questionable change in both the form and content of economic theory, how (and why) was this brought about? I do not have a complete answer to this although I suspect that the technical professionalisation of economics around core principles in rapidly expanding US university departments over the course of the formalist revolution is of decisive importance. There was a corresponding shift of the centre of gravity of the frontiers of economics from England to the United States alongside the Americanisation of the discipline. I am not sure that this story has been fully told. It requires close empirical scrutiny, where possible, of university appointments and curricula, of the sort currently being undertaken of the effects of the RAE in the UK, especially by Fred Lee (2007) most recently, and the emergence of new journals, etc. And it is important not to neglect the presence of mathematics within economics prior to the second world war as well as the mathematical accomplishments of economists such as Marshall and Keynes, although their reservations over its use have tended to be forgotten (other than by those who applaud them).

It is also important to take account of external influences on the evolution of the discipline, not least the funding of research in the United States to promote neo-liberalism and rational choice through the RAND Corporation, Amadae (2003). Yet such interventions were not decisive and closely determining in and of themselves and were balanced, if not negated, by the more interventionist and Keynesian Cowles Commission, with Arrow neatly capturing the contradictions involved. He was employed by RAND to study how to form a Soviet welfare function for the purposes of game-theoretically modelling the Cold War. Social choice theory, and the impossibility of treating society as if it were an individual, resulted. More generally, Arrow's own work represents a correspondingly remarkable evolution of having your cake and eating it too – both pushing forward the application of the technical apparatus and architecture of the mainstream, see below, whilst also qualifying if not undermining it. Tarty, Mirowski (2007a) dubs Arrow the "Cowles poster boy", with his popularity within the profession reflecting the irony of repudiating at one time or another each of the mainstream advances that he has himself made, see also Mirowski (2007b).⁷

Thus, Arrow (1994) is drawn to the rejection of methodological individualism within economics on the grounds that it cannot be realised in practice, practitioners' prejudices to the contrary. This is because of the need to take something social as given - the price system, the rules of the game (theory), or externalities in access to the pool of knowledge, for example. It is apparent, then, that there were considerable reservations, certainly qualifications, over the formalisation of economics prior to and during the formalism itself. How were these handled? There have been two methods. One is to have acknowledged such reservations as part and parcel of pushing forward with the formalism in any case. I suspect that this is more common in those who initiate change than those who follow, and more common than the other way of proceeding which is to disregard or to dismiss reservations. Interestingly, these two different attitudes towards formalisation are to be found in Arrow and Debreu, respectively, co-founders of one of the most important events in the formalist revolution, the proofs for general equilibrium theory. Debreu's (1986, p. 1265) commitment to formalism with economics, in a sense, as an add on, could not be plainer:

An axiomatized theory first selects its primitive concepts and represents each one of them by a mathematical object ... Next assumptions on the objects representing the primitive concepts are specified, and consequences are mathematically derived from them. The economic interpretation of the theorems so obtained is the last step of the analysis. According to the schema, an axiomatized theory has a mathematical form that is completely separated from its economic content. If one removes the economic interpretation of the primitive concepts, of the assumptions ... its bare mathematical structure must still stand.

Less well-known of Debreu is his corresponding commitment to methodological individualism. In response to Alan Krueger's (2003, p. 190) question on whether "we don't need a separate field of

macroeconomics”, Edmond Malinvaud responds, “That was a vision Gerard Debreu was arguing with me in our interchanges ... at the Cowles Commission”.

Hypothesis 2: Economics Progresses by Stating Limitations Only to Ignore Them

This all leads me to my second hypothesis that changes in economic theory are in part propelled by those who make them, inadvertently or not, by clear statement of reservations and limitations only for these to become secondary and, ultimately, discarded in the progress of the discipline. As a corollary, this also suggests that such changes can be rapid, within a generation or less, and become consolidated as a new conventional wisdom with little regard for what has come before. A further corollary is that the explicit identification of such reservations by those pushing economic theory forward has decreased with the passage of time, see tenth hypothesis below. This is because the process has become more Debreu-like than Arrow-like as deductive and speculative formalism has come to the fore at the expense of more rounded conceptual and methodological considerations, not least as a result of the consequences of formalism itself as it becomes an object in its own right. Blaug (1998, p. 11) appropriately reports from a survey of a lack of interest in the real world on the part of elite graduate economics students as opposed to honing their skills in the latest econometrics and mathematical economics.

I will return to this hypothesis and its corollaries later in the context of the pre- and post-revolutionary periods as demarcated by the formalist revolution (the later message being that reservations are more prominent before than after). Before doing so, I wish to focus on the substance of what was formalised as opposed to formalisation itself. Here I would emphasise first and foremost the technical apparatus of neoclassical economics. By the 1950s, the utility and production theory of marginalism had been more or less perfected in two respects. On the one hand, the consequences for supply and demand of individual optimisation had been more or less completely investigated through the Slutsky-Hicks-Samuelson analysis. On the other hand, Arrow-Debreu had equally established sufficient conditions for the existence of general equilibrium, itself drawing upon aggregating across the optimising behaviour of individual agents, creating a technical architecture around notions of equilibrium and efficiency as part of the conceptual focus for formalised models.

Hypothesis 3: Utility and Production Functions Rule, OK

My third hypothesis is that the technical apparatus takes precedence over all else – conceptual content, realism, method, empirical evidence, and alternative approaches. This is even so of mathematical rigour in the sense that should such rigour come into conflict with the technical apparatus, then it is the latter that prevails. The evidence for this is compelling in the ubiquity of the technical apparatus across neoclassical economics, as well as the latter’s disregard or contempt for anything that does not incorporate it. More specifically, we can see the imperative of the technical apparatus in the observation only in the breach of the results even within neoclassical economics that offer profound challenges to its continuing use. This begins with acknowledgement of the failure to guarantee existence, uniqueness and stability of general equilibrium. Assume such problems away. The same applies to the profoundly destructive implications for use of production functions that arise out of the Cambridge Critique. As Samuelson (1966: 582) concluded that debate from the neoclassical side:

If all this causes headaches for those nostalgic for the old time parables of neoclassical writing, we must remind ourselves that scholars are not born to live an easy existence. We must respect, and appraise, the facts of life.

Yet neoclassical economics continues as if such lessons need not be learnt.⁸ Much the same applies to the absence of money in anything other than a formal sense within economic models; the theory of the second best, the need to attach the technical apparatus to convexity and other assumptions, and the absence of factor reversals in trade theory, of externalities in many models, and of a mode of setting prices other than through a fictional auctioneer. The point is not that neoclassical economics never confronts these issues. On the contrary, the fact that it does so reveals how important they are in principle for its modelling and why they must be overlooked as a matter of formal convenience for the technical apparatus to be able to solve problems within a framework of its own making.

It now makes sense to confront our second and third hypotheses (and corollaries) with one another. To what extent were reservations around the technical apparatus noted only to be disregarded by those

who themselves were pushing the theory forward; did this hasten the process; and have the limitations been less observed in the wake of the formalist revolution? This is a matter of some weight and detail, and it is beyond the ambition of this paper other than to pinpoint how much of it began and how much of it ended. For the latter, it is only necessary to observe the unconsidered confidence with which Robert Lucas (1987), for example, considers the functioning of the economy as a whole in terms of representative individuals alongside assertions to the effect that there is no such thing as macroeconomics distinct from microeconomics.

The corresponding theory of the optimising individual has its most profound if not earliest origins in the *Principles of Economics* of Alfred Marshall. There can be no doubt that he sought to establish an “organon” of optimising individual behaviour. But he saw this as only part of economic and human behaviour and far from a basis on which to understand the functioning of the economic system as a whole. Indeed, forging a link between marginalism and macroeconomics proved to be a project that he failed to realise, not through his own inadequacies but because a solution would only lie along the unacceptable lines adopted by Lucas almost a century later. Significantly, though, Marshall’s provision of an acceptable organon of marginalist principles to the economists of his own time depended upon rendering it palatable through stated limitations and reservations, not least in relation to historical and methodological concessions.

Hypothesis 4: The Foundation for the Formalist Revolution Was Technically Driven

As mentioned, to what extent Marshall at the outset and Lucas on the contemporary scene are representative of our hypotheses for those who came between is a matter for close empirical scholarly investigation of those shakers and mover in the interim. But, if correct, in light of the formalist revolution, it is suggestive of a fourth hypothesis, albeit one that is possibly unduly teleological. This is that as the formalist revolution is defined substantively by its corresponding technical apparatus, so the period leading to it was driven by the imperative of establishing that technical apparatus. In particular, from Marshall onwards, marginalist principles were modified in order to obtain required results without regard, other than in passing and temporary qualification, to other considerations.

There is a sense in which this must be so. To go from A to B requires dropping the conditions within A that make B impossible and adopting those within B that do make it possible. So it is a way of reading, and not of explaining, what happened although it is not thereby without explanatory content particularly in light of our earlier hypotheses. Consider, for example, the notion of utility. By the end of the process, it has become attached to the utility function of an anonymous individual without identity exercising a logic of choice over equally unspecified goods. This involves a number of requirements over and above the technical conditions to allow optimisation to proceed. These requirements are reductionist in the sense of stripping out conceptual content and context. This is most apparent for utility, something that was more akin to well-being in the utilitarianism of the nineteenth century but has become the undifferentiated consumption of goods by the time of the formalist revolution, a logic of choice. This has to be made so. Even for Robbins (1932, p. 87), notorious for his definition of economics as the allocation of scarce resources between competing ends, “economic subjects can be pure egoists, pure altruists, pure ascetics, pure sensualists or – what is much more likely – mixed bundles of all of these impulses”. There is a fair bit of individual character to ponder in this. Yet as Waller (2004, p. 1112) puts it in review of Davis (2003), “But if choices are the only characteristics of atomistic individuals, the theory of the individual becomes so reductionist that it ceases to be about human beings”. For Davis (2007a, p. 203) himself, “if the basis of the atomistic individual was its inner life, and that inner life is now black-boxed into non-existence, then it follows that this neoclassical individual also ceases to exist”.

As already indicated, then, that goods might have content derived from their social or historical context has also been excised, although the evolutionary approach to economics associated with Veblen – not least demonstration effects and conspicuous consumption of the leisure class – remained strong throughout the interwar period. By the same token, the classic work of Knight in identifying the distinction between risk and uncertainty had the effect of allowing the latter to be dropped, more or less unconsciously once we have rational expectations and information economics. That this has happened is put most beautifully by Barzel and Kochin (1992, p. 19), “Knight implicitly introduced significant transaction cost considerations to the analysis of the firm. His uncertainty – the driving force in his theory of the firm – is best understood as being like risk except that it is too costly to transact in the market”, see also reference to Demsetz below. So uncertainty is simply very expensive risk. But the

most notable reductionism has been to and of the individual, with the identification of the individual with the utility function, and its maximisation as the sole goal of individuals and the determinant of outcomes.

Hypothesis 5: Prior to the Formalist Revolution, Economic Rationality Is Only Part of Economics

The reaction to such reductionism gives rise to my fifth hypothesis that it led less to hostile rejection than acceptance of a direction of research that corresponded at most to one part of an economic analysis. This is already apparent in case of Marshall but it is also true of Weber and Schumpeter in their search for “social economics”, of the Austrian school in their complementary appeal to uncertainty and inventiveness, and in Keynes and others in providing complementary material in macroeconomics, business cycle analysis, economic history and other areas of applied economics. This is not to deny presence of outright opposition to the newly emerging homo economicus from time to time. But the more general response was one of acceptance of the legitimacy of economic rationality as long as it be complemented by what was increasingly perceived as otherwise irrational although it is more correctly seen as social or non-rational to the extent that economic rationality is associated with the single-minded pursuit of self-interest through utility maximisation.

Hypothesis 6: The Rise of Economic Rationality Detaches Itself from Other Social Sciences

And this yields a sixth hypothesis that the technical apparatus was confined not only to a part of economics as a discipline but that it was thereby increasingly detached and isolated from the other social sciences. This means that other forms of now heterodox economics remained strong, American Institutionalism, for example, and old or classic development economics could emerge untainted by the formalist revolution and its antecedents at the same time as the formalist revolution itself. In addition, economic material and analyses found themselves located in other disciplines, not least with the emergence of economic history and the economic elements covered by sociology. Thus, in a stringent and strategic response to Robbins in the Quarterly Journal of Economics, Talcott Parsons (1934, p. 533), founder of functionalist sociology, suggests:

economic institutions are in the causal sense a specifically *non-economic* factor ... they form at least one fundamental element in accounting for the specific qualitative form of organization of any particular “economy” ... But neither Robbins nor Souter, tho both vaguely note their existence, have any clear conception of the relation of institutions to economic activities, nor any systematic place for a theory of institutions in their scheme of the social sciences. In my opinion it is one of the central elements of sociology.

In short, the process of creating the technical apparatus that provided the foundation for, and culminated in, the formalist revolution of the 1950s, reflected something of an implosion in relation to the discipline of economics itself as well as in relationship to other disciplines. Apart from the reductionism required to establish the technical apparatus, it was also acknowledged only to have a limited scope of application.

Hypothesis 7: Economic Rationality Reverses Its Decline in Scope after the Formalist Revolution

This is in complete contrast to the post-revolutionary situation, giving rise to our seventh hypothesis. The formalist revolution signals a watershed in the evolution of economic theory, with an implosion followed by an explosion of scope of application of the core formalist principles. As corollary, it should be added, the hypothesis bears upon developments both within the discipline of economics itself and between it and the other social sciences.

This hypothesis is grounded in what we have termed the historical logic of economics imperialism. Whilst the marginalist revolution began the process establishing the technical apparatus of the emerging neoclassical economics, it did so only by accepting its own limitations both analytically and in scope of application as the preconditions necessary to derive its formal results. If not absolutely, rational, optimising homo economicus was traditionally confined to the market and supply and demand of traded goods. This seems to have been taken for granted to a large extent well after the formalist revolution. For Boulding (1969, p. 4), in his AEA Presidential Address, “Economics specializes in the study of that part of the total social system which is organized through exchanges and which deals with exchangeables. This to my mind is a better definition of economics than those which define it as

relating to scarcity or allocation, for the allocation of scarce resources is a universal problem which applies to political decisions and political structures through coercion, threat, and even to love and community, just as it does to exchange". And for Coase (1978, p. 209), implicitly acknowledging the attachment of economics to market exchange through monetary value, "Economics, it must be admitted, does appear to be more developed than the other social sciences. But the great advantage which economics has possessed is that economists are able to use the 'measuring rod of money'". But this advantage for study of the economy, and the corresponding techniques, have their limitations. Indeed, "the analysis developed in economics is not likely to be successfully applied in other subjects" not least because of "the relative unimportance of technique" for "subject matter is really the dominant factor", p. 209.

Such a compromise in practice and by tradition, however, over the intra- and inter-disciplinary boundaries around marginalist principles, was at odds with the universal nature, or unlimited scope of application of the technical apparatus itself. Utility and production functions are generalised, and a-social and a-historical, instruments without necessarily being confined to optimisation in response to the constraints imposed by prices and incomes.

As a result, once the formalist revolution had established itself through its corresponding technical apparatus, it was inevitable that the attempt should be made to recapture the ground conceded in scope of analysis. Over the post-war period there has been a dual process of colonising subject matter both within economics as a discipline and between itself and other disciplines. I take it that the process of internal recolonisation is uncontroversial in the sense of being familiar to those who either are old enough to have lived through it to a greater or lesser extent and/or who have cared to look back upon it. Nonetheless, it is worth offering a few observations upon the process and on its substance and timing.

Hypothesis 8: Resistance to Expanding the Scope of Economic Rationality Was Initially Strong ...

First, there can be little doubt that the formalist revolution promoted the recolonisation but, equally, as an eighth hypothesis, it did not determine either its content or its timing. Thus, not least with the standard and universal IS/LM reduction of Keynes to the neoclassical synthesis, the focus on macroeconomic aggregates that make up aggregate supply and demand were undoubtedly formalised but not, in general, by appeal to optimising individuals attached to utility and production functions. Notably absent from IS/LM Keynesianism is Keynes' emphasis on uncertainty and his antipathy both to mathematical modelling and, especially, econometrics. Initially, though, Keynes' rejection of methodological individualism was both unknown and so, to a large extent, unwittingly accepted. Nonetheless, reductionism to individualism, with this label deployed by Coddington (1976), had to wait upon the reappraisal of Keynes, significantly offered as a radical rupture with the neoclassical synthesis. This enjoyed a brief but glittering career, Backhouse and Boianovsky (2005), until the stagflation of the 1970s and emergence of the monetarist counter-revolution, ultimately, the New Classical Economics, NCE, had dealt it a devastating blow (not least through perfect market clearing in place of fixed prices).

With the NCE, and increasing reliance upon the representative individual in macroeconomics, the internal colonisation of macro by micro is explicitly recognised to have been completed, if not at the expense of potential for further development. As observed, for the leading new classical economist, Lucas (1987, p. 108), "the term 'macroeconomic' will simply disappear from use and the modifier 'micro' will be superfluous", cited by Davis (2003, p. 35).

But this is not the full story. The first major application of the micro technical apparatus to the macroeconomy long preceded NCE, with the emergence of the old growth theory in the mid-1950s, and the presumption that an economy can be represented by a single production function (something carried over mindlessly into the new growth theory). In addition, what is striking about the internal recolonisation is its limited progress relative to micro- not just macroeconomics. Of course, this is not true of its technical apparatus so much as the application of that apparatus to what were perceived to be applied topics – not least industrial economics, for example, and other fields presumed to incorporate a particularly heavy empirical or policy content. Applied economics, at both micro and macro levels, did not necessarily thrive without mathematical modelling, and econometrics, but nor did it fully, even partially on many occasions, embrace either methodological individualism or its corresponding neoclassical technical apparatus.

Hypothesis 9: ... And Affected the Content and Timing of Formalism

Comparison with today is marked, signifying some obstacle(s) to internal recolonisation in the past. It has been natural to offer explanation in terms of lower levels of technical capacity in the past, the most explicit case being that of the old or classic development economics as suggested by Krugman (1992). But I doubt whether the old classical development economists were incapable of matching Krugman in technique as opposed to not wishing to do so. Hence a ninth hypothesis, or corollary to the eighth, is that the content, form and timing of the formalisation of economics following the formalist revolution is heavily influenced by a continuing commitment to its limited applicability within economics itself, especially in light of systemic and/or applied considerations (as opposed to undue reliance upon axiomatic deductive theory).

There are three compelling pieces of evidence, or symbolic illustrations, to support this hypothesis although, I suspect, interpretation of them will remain controversial over their influence on the evolution of the history of economic thought. One is the various contributions made by Coase both on the existence of the firm and the settling of externalities through property rights in case of zero transaction costs. As is now apparent, these insights are now fully compatible with the technical apparatus of the mainstream but they did not become fully incorporated until decades after the event, especially as far as the firm is concerned (and the firm was soon followed by institutions in general). Why is this?

Coase's own take on the issue is revealing as opposed to the current focus on whether markets work well (in absence of transaction costs) or badly (because of their presence). First, he considers what he has to contribute to be simple but so much so that its failure to be accepted over such a long period necessarily reflected the economists way of thinking about things that precluded it from being accepted Coase (1988, p. 1):⁹

My point of view has not in general commanded assent, nor has my argument, for the most part, been understood ... As the argument in these papers is, I believe, simple, so simple indeed as almost to make their propositions fall into the category of truths which can be deemed self-evident, their rejection or apparent incomprehensibility would seem to imply that most economists have a different way of looking at economic problems and do not share my conception of the nature of the subject. This I believe to be true.

Second, his own mission was to understand the world of non-zero transaction costs, something that he saw as profoundly empirical in content, dependent upon study within other disciplines, and not amenable to pure economic theory. Thus, p. 3:

This preoccupation of economists with the logic of choice, while it may ultimately rejuvenate the study of law, political science and sociology, has nonetheless had, in my view, serious adverse effects on economics itself. One result of this divorce of the theory from its subject matter has been ... [w]e have consumers without humanity, firms without organization, and even exchange without markets.

Indeed, Coase (1978) denies the likelihood of a prospective economics imperialism for "Economists may, however, study other social systems, such as the legal and political ones, not with the aim of contributing to law or political science, but because it is necessary if they are to understand the workings of the economic system itself", p. 210. Indeed, "we may expect the scope of economics to be permanently enlarged to include studies in other social sciences. But the purpose will be to enable us to understand better the working of the economic system", p. 211 in his closing this piece. In this light he is particularly disparaging about formalism within economics, citing his own opinion that judges "seemed to show a better understanding of the economic problem than did many economists ... I did this not to praise the judges but to shame the economists", Coase (1996, p. 105) and, ultimately concluding, "In my youth it was said that what was too silly to be said may be sung. In modern economics it may be put into mathematics", Coase (1988, p. 185).

What this all indicates is that, for Coase, economists inhabit a silly world in which there are zero transaction costs. "Most objections to the Coase Theorem seem to underestimate what costless transacting could accomplish", Coase (1988, p. 163). In such a world, there is no need either for firms or for externalities since everything can be worked out perfectly. Far from this being a reason for

leaving things to the market (or to the state), it means paying analytical attention to a different world, one in which there are positive transaction costs, whose consequences need to be investigated, and this has to be an empirical investigation of how the economy works undertaken through the contribution of other social sciences and, for him in particular, law. “The world of zero transaction costs has often been described as a Coasian world. Nothing could be further from the truth. It is the world of modern economic theory, one which I was hoping to persuade economists to leave ... in such a world the allocation of resources would be independent of the legal position ... even the qualifying phrase ‘under perfect competition’ can be omitted”, p. 175/6.

Indeed, for him, the Coase theorem, dubbed as such by Stigler and contra Pigou, “follows from the standard assumptions of economic theory. Its logic cannot be questioned, only its domain”, p. 10 – negligible or non-existent for Coase. “It undermines the Pigovian system ... My conclusion: Let us study the world of positive transaction costs”, especially the legal system “and the rights to perform certain action ... which individuals possess ... As a result, the legal system will have a profound effect on the working of the economic system and may in certain respects be said to control it” , p. 11. He suggests this is not accepted by economists because it goes against the grain of the orthodox Kuhnian paradigm to incorporate non-zero transaction costs. And ignorance over these means “What we need is more empirical work”, Coase (1994/1991, p. 12/13). In particular, Coase finds an absence of a theory of industrial organisation in microeconomics, “What is studied is a system which lives in the minds of economists but not on earth. I have called the result ‘blackboard economics’”, p. 5.

In short, as Coase is himself acutely aware, there is limited application of blackboard economics other than for its own idealised world of zero transaction costs, and beyond which a more empirically grounded analysis is required. In this respect, Coase is not idiosyncratic insofar as his fellow economists also hesitated in applying the techniques on a wider terrain. Where Coase has been entirely wrong, as will be seen, is in believing that the blackboard would be abandoned and economics imperialism limited in deference to the empirical realities to be highlighted by other disciplines and turned to a better economics and understanding of the economy.

Coase offers a particularly sharp illustration of delayed formalisation of economics through its technical apparatus and beyond. Other examples can be offered much more briefly. A second is provided by rational expectations. Muth’s original contribution had to wait upon a decade or more for adoption within the NCE. Far from uncertainty being reducible to costly risk, macroeconomists in the Keynesian tradition must surely have retained some lingering understanding of, and commitment to, uncertainty as understood by Knight (and Keynes, Hayek and others).¹⁰ It is also a moot point whether general equilibrium theory, replete with a full set of timeless (or chronologically indefinite) contingent markets would have served to have promoted or impeded the attraction of risk in place of uncertainty within a systemic understanding of macroeconomics. But with NCE, expectations had been detached from uncertainty and attached to information in a way that is tellingly revealed by Barzel and Kochin (1992), see above, albeit for a later and different purpose, one of dealing in non-zero transactions costs as required by the now acceptable Coase.

How, and with what confidence, this is to be done is now strikingly revealed by Demsetz (1997, p. 1). He opens, “The strong export surplus economics maintains in its trade in ideas and methods with the other social sciences is an important indicator of the success of economics. Not much has been said about the source of this success, but it has been attributed largely to advantages offered to other social sciences by the economics tool kit ... The emphasis here is on the broad scope of the phenomena that can be explained by our tool kit”, emphases added. Further, he continues by boasting of the achievements of the discipline, “Economics may be judged the more successful social science because it has explained phenomena within its traditional boundaries better than the other social sciences have explained phenomena within their respective traditional boundaries. The primacy of economics may be established in this sense even if economics never influenced the other social sciences”, p. 2. Although no evidence is given for this claim – it is presumably self-evident – it is immediately repeated but with the caution that such “success breeds confidence, and sometimes over-confidence”. For he accepts that “institutional arrangements in neoclassical theory are correctly described as ‘black boxes’”, p. 4. Nonetheless he rejects Knight for “arm-chair psychoanalyzing” and he and Simon “are virtually (I would say entirely) empty of empirical relevance”, p. 7/8. Opening the black boxes “waited upon completion of the central inquiry of economics” despite an acknowledged foretaste derived from Coase, p. 11, whose more inductive methodology he deplores in deference to the deductivism of Becker, p. 1, especially as far as extension of the boundaries of economics are concerned. In other

words, the economist tool kit rules and anything that does not fit is empty (rather than vice-versa as for Coase).

A third example of delayed formalisation is the use of game theory, so important in recent times for the formalisation of so much microeconomics on the basis of its technical apparatus. Game theory was ready for adoption within economics at an early stage, certainly coincident with the formalist revolution itself. As revealed by Amadae (2003), it was heavily promoted by the RAND Corporation after the second world war and, although familiar to economists situated there and at the Cowles Commission, made little inroad into economics. Whilst Amadae does see this promotion of game theory as an important element in the rise of rational choice theory within economics, and beyond, it did not have that effect immediately.

Hypothesis 10: Over Time, Limitations of Economic Rationality Are Lesser Highlighted and More Rapidly and Fully Discarded

Just as the formalist revolution marked a watershed in the derivation and application of the neoclassical technical apparatus within economics, so it also signals a watershed in the relationship between economics and the other social sciences. With the increasing acceptance of the technical apparatus as a core component of the discipline, the historical logic of economics imperialism dictated that it should be applied more widely than within economics alone. Our tenth hypothesis is that this is done with much less attention to the reservations and qualifications that were expressed in deriving the technical apparatus for the even more limited application to economic behaviour alone. Thus, paradoxically, there were much greater concerns expressed in making the assumptions to allow for the derivation and use of utility and production functions for the narrow application to supply and demand than there were in extending their application across the social sciences.

One example of this is provided by economic history and the rise of cliometrics, Fine and Milonakis (2003 and 2008) and Milonakis and Fine (2007 and 2009) for a fuller discussion. North (1963, p. 128), one of its pioneers, serves notice at an early stage of major change in the academic wind, "A revolution is taking place in economic history in the United States". It involves the application of economic theory to economic history. North suggests taking any leading article and seeing whether it is susceptible to formal modelling even if needing to rely upon the most favourable (unrealistic?) assumptions. More generally, a manifesto is provided for the fledgling field, North (1965, p. 91):

In summary ... we need to sweep out the door a good deal of the old economic history, to improve the quality of the new ... and it is incumbent upon economists to cast a skeptical eye upon the research produced by their economic history colleagues to see that it lives up to standards which they would expect in other areas of economics.

Economic theory without history, then, is to provide the standard by which to judge (economic) history itself.

This is a significant but not an isolated instance since it expresses perfectly the approach to economics imperialism adopted by Gary Becker. Demsetz (1997, p. 1) who, in the context of economics imperialism, describes Becker as having "earned Commander-in-chief ranking in the EEF (Economics Expeditionary Forces)". And Demsetz's own position, as already revealed, offers an almost ideal illustration of some of the hypotheses posited here.

But it would be a mistake to turn such hypotheses into absolute truths since many economists by training, tradition and inclination remained wedded to more restrictive notions of the compass of economics, and this did arise by way of (subsequently overlooked) reservation even whilst pushing forward the boundaries of economics imperialism. This is apparent in case of public choice theory, for example, as illustrated by one of its leading exponents. For McKenzie (1979, p. 145), for example, a pioneer in public choice theory:

The purpose of this article is not to extol the virtue of economic analysis but rather to reflect on its limitations. In these times, given much talk of the expanding domain of economic science and an inclination on the part of economists to claim that there are no boundaries to economic analysis, my purpose may seem unusual.

Amongst a number of objections to economics imperialism, McKenzie (1978, p. 639) suggests that with it:

There is denial, at least in part, of the creative consciousness of individuals; this is simply because predictive theories in the Becker tradition require that goods be objectively specified, which leaves little room for raw emotions. Indeed, once the good which people are assumed to maximize is specified and the nature of the demand curve and cost functions are defined, the theory becomes totally deterministic: The curves then become the theoretical equivalent of the walls of the rat maze through which the individual must run.

To a large extent, the qualifications attached to public choice theory are veiled by, if not the product of, the variety of intellectual sources from which it derives, and with which it engages through being wedded to an irreducible antipathy to collective action from Keynesianism through to communism. These alternative origins within the liberal tradition, from Adam Smith through to Hayek, have, however, been stripped down to rational choice across the social sciences and to the technical apparatus within economics.

Significantly, though, Becker is McKenzie's explicit target despite both being responsible for pushing out the boundaries of economics imperialism. Some like Becker may have lacked the sensibilities displayed by McKenzie. Economists in general, though, during Becker's command of the colonising forces, were often far from sympathetic, displaying greater respect both for other disciplines and for the limited scope of application of their own. Akerlof (1990, p. 73), for example, even lampoons Becker in terms of his having learnt how to spell banana but not knowing when to stop!

In this light, it is paradoxical that it is with Akerlof's own contributions to economics that the floodgates are opened for a deluge of economics imperialism, both in tempering reservations from within the discipline and in rendering its incursions more palatable to the colonised. In a nutshell, the information-theoretic approach to economics that he pioneered has yielded the following despite continuing to be based on the optimising behaviour of individuals in circumstances of imperfect markets:

- 1) Market failures explain economic structures.
- 2) Response to these explains non-market structures.
- 3) Hence history, institutions, culture, customs, norms, etc, matter and can be endogenised.

As a result, a new phase of economics imperialism has been prompted, literally promoting a whole range of "new" fields or rejuvenating the old – new growth theory, new institutional economics, new economic sociology, new economic geography, new labour economics, etc. Some of these build upon the old, others are new ventures. But what they all continue to share in common is continuing commitment to the core technical apparatus. It can all be summarised by the two equations:

$$e=mi^2 \text{ and } ss=e$$

where e is economics, ss is social science and mi is both methodological individualism and market imperfections.

Hypothesis 11: The Information-Theoretic Trumps the Economic Approach in Promoting Economics Imperialism

This has the effect of dividing the social sciences in relation to neoclassical economics, as previously, into three broad areas around the rational/irrational divide. Pure models depend upon optimising agents only; mixed models somewhat inconsistently combine rational with irrational motives, behaviour, culture, or whatever; and the rest that seeks to escape the rational/irrational dualism as the basis for social theory. Our eleventh hypothesis is that the boundaries of these first two categories have been symbiotically pushed out to a significant degree by the new phase of economics imperialism, as indicated by the range of new or newer fields, thereby appropriating both economic heterodoxy and the subject matter of other disciplines.

Hypothesis 12: The Impact of Economics Imperialism Is Diverse and Open

But these incursions do not necessarily prosper at the expense of the third category although there is presumably some crowding out involved as well as developments in parallel. This leads to an twelfth hypothesis, focusing more on the reaction to economics imperialism across the social sciences as opposed to the action of the aggressor. The impact of the economics imperialism across the social sciences is diverse, varying from discipline to discipline and from topic to topic, according to the continuing traditions and dynamics of scholarship. Economics imperialism is liable to be more influential to the extent that rational choice is already present (a host factor) and attention to the meaning of concepts is absent (a hostile factor). But the latter is no guarantee of protection as economic imperialism can proceed by stripping out interpretative content of categories like culture in deference to its own meaningless technical apparatus. A striking example of this is provided by the economics of identity pioneered by Akerlof in which it appears as an otherwise unspecified variable in a utility function!¹¹

What Is to Be Done

The previous discussion has addressed what economics has become as a discipline in terms of core dependence on its core technical apparatus. Such an assessment is not always accepted since Critical Realism in Economics, for example, perceives orthodoxy in terms of its deficient ontology, emphasising closed deductivism as its foremost feature. As argued elsewhere, irrespective of the merits of its own alternative, both interpretatively and strategically this is to engage insufficiently if at all with economic theory. Further, Lawson (2006) has drawn upon Colander (2004) to pinpoint methodology as the defining and unchanging feature of orthodoxy because the latter has varied across a range of non-standard methods such as evolutionary models. These are confessed to be at the margins of the discipline but thereby projected to the prospective frontier, and then new core, with the mainstream disintegrating into a pluralism, Davis (2006). In contrast in interpretation here, these new methods generally rely upon an otherwise unchanged core technical apparatus and, in any case, that they should become the new orthodoxy is purely speculative with little or no justification on the basis of current status and past experience. Ruccio and Amariglio (2007, p. 227) are appropriately unconvinced that “modernism in economics is on the wane ... the evacuation of the strong concept of the utility-maximizing agent as a universal form of economic subjectivity/identity and its replacement by a rule-driven, habit-inflected, institutionally affected, ‘local’, processual individual does nothing, in itself, to counter economics as a ‘process with a subject’ ... neither game theory nor the new behavioural economics dispenses with a modernist conception of the subject, and in any event, it is still the case ... that hundreds of thousands of students worldwide begin their training in academic economics using introductory textbooks that codify over and over and over again rational choice and utility maximization (and, for the ‘self’ of the firm, profit maximization)”.

What these new developments on the frontiers/margins of economics can highlight is the intellectual fragility of the orthodoxy once it is confronted by the narrowness of its methodology and its chaotic conceptual content with limited substance. But this is hardly new as far as orthodoxy is concerned as has been demonstrated by the preceding hypotheses. Exactly what is striking is how the process of bringing back in, BBI, what has been left out is filtered through its compatibility, not its consistency, with the technical apparatus, with imperfect information being the most notable recent example.¹² But the same applies to endogeneity, for example, of preferences and technology quite apart from more longstanding technical assumptions around increasing returns to scale and externalities.

Further, and paradoxically, despite its continuing intellectual fragility, the strength of the orthodoxy professionally within the discipline borders on the absolute and, in the phase of economics imperialism based on BBI and accepting that the “social” matters, has strengthened its status across other social sciences however fully and unevenly given continuing intimidation by, and yet antipathy to, its mathematical and statistical methods and conceptual impoverishment. The orthodox hold over the discipline is reflected in the dual movement of inventively appropriating any heterodoxy that it can through $e=mi^2$ and otherwise dismissing it.¹³ The same applies to the history of economic thought, economic methodology, and applied inductive work (with econometrics as its deficient surrogate).

This has placed heterodoxy in a highly vulnerable position, questioning its very survival within economics as a discipline. Of course, heterodoxy covers a wide range of approaches and content, with opposition to orthodoxy the only common denominator. And the absolute intolerance of the orthodoxy has forced or encouraged heterodoxy to locate itself outside economics altogether in order to create a niche or two for itself, and otherwise to rely upon more or less formal organisations and journals.

The situation is dire as is evidenced by testimony of the heterodoxy as opposing force. For Kurz (2006, p. 2), for example, “Historians of economic thought are an endangered species and their natural habitat – faculties of economics – are becoming less and less hospitable. The marginalization of the subject has been going on for quite some time”. He advises a strategy for survival through combining history of economic thought, HET, in combination with contribution around current theoretical concerns – although this raises issues of how to compromise with the currency without being totally devalued. Weintraub (2006) offers an even bleaker picture. For him, the “science war” engaged by HET with current economic theory is over. “Because historians and methodologists of economics are not seen as any ‘threat’ to mainstream economists ... the economic science war is over, and we historians of economics have lost”, p. 13. He advises that HET seek sanctuary in the more general milieu of the history of science, as does Schabas (2002). Although he offers some small and questionable comfort in the tendency for academic economists to take an interest in history of economic thought as they age, and for it to be found in disciplines other than economics, Backhouse (2002, pp. 93-4) suggests of the UK:

The age profile of HET staff raises the possibility that when the current generation currently in its fifties retires, the number of staff available to teach the subject will plummet, and the subject will be in danger of dying out altogether. The supply of new Ph.Ds in HET is tiny, and even if there were more of them, universities would favor those working in the mainstream fields.

Bateman (2002) offers some optimism for HET through liberal arts colleges in the United States. But, for Gayer (2002, p. 57), survey results in the US “suggest that most current Ph.D. students are not exposed to HET. This is particularly true among the top Ph.D. programs, which more frequently place their Ph.D. students in tenure-track jobs in Ph.D.-granting programs”.

The situation elsewhere with HET is uneven, as indicated across the other articles covering the non-Anglo-Saxon world in the special issue of History of Political Economy, vol 34, no 4, Annual Supplement, 2002, devoted to its prospects. But Deleplace (2002, p. 122) captures the general trend:

a consensus about tools has been established in economics at the international level ... More profoundly, it is not only the existence of the consensus in economics that marginalizes HET, but also its content.

Similar stories can be told of economic methodology, as it too is exiled from within the discipline, and for the survival of heterodoxy in general as revealed by Lee’s (2006) introduction to the special issue devoted to how it can be sustained, with Stilwell’s (2006) contribution particularly poignant given the subsequent closure of the Department of Political Economy at the University of Sydney.

Such perspectives are grimly realistic but arguably too narrow in their assessment and in their ambition. To some extent this is because of an understandable, if far from universal, psychological predisposition to seek survival by waiting upon an orthodoxy’s willingness to engage, a sort of McCloskey syndrome but one in which the crumbs of comfort that fall from the mainstream feast are heavily outweighed by the vicious kicks under the table. The goal is one of patiently persuading orthodoxy of the narrowness and limitations of its approach and substance by reference to methodology, history of economic thought, empirical regularities and/or alternative approaches and considerations. Irrespective of the futility or otherwise of such a response to orthodoxy’s current and strengthening intolerance and narrowness, insufficient account is taken of the broader intellectual climate across the social sciences and the opportunities and responsibilities that this offers for heterodoxy.

Currently the social sciences are marked by a dual retreat from the extremes of neo-liberalism and postmodernism, with a corresponding renewal of interest in systemic understanding of the nature of contemporary capitalism. The rise of “globalisation” has been the single most important marker of this broad intellectual trend over the last two decades. Economics, having been immune to postmodernism in the first place, has only participated in the dual retreat on one leg, with $e=mi^2$ the form in which neo-liberalism has been rejected and milder forms of Keynesianism and state correction of market imperfections restored. The other social sciences have, however, inevitably been drawn to political economy in order to confront the globalisation for which mainstream economics offers little by way of

systemic analysis incorporating power, conflict, class, context and dynamics that are traditional categories of the other social sciences.

With economics imperialism on the basis of $ss=e=mi^2$, however, economics has increasingly incorporated such considerations on its own terms. This reflects a disciplinary schizophrenia, Mavroudeas (2006). On the one hand, within the discipline, there is an absolute confidence in the technical apparatus and in its capacity to address economic and other problems. On the other hand, there is an increasing and widening recognition within economics that the economy cannot itself be understood on the basis of economic variables alone. This has meant that economics imperialism has not only sought to appropriate the subject matter of other disciplines on their own terrain but also to incorporate it into economic analysis. Some have seen this as a potential source of crisis for the discipline as the implications of incorporating the non-economic has the methodological, conceptual and theoretical potential to undermine the technical apparatus and its standard applications. But, as already argued, there is little evidence from the past that such conundrums as do arise and are acknowledged prompt anything other than continuing intellectual fragility and inconsistency as opposed to major change. Inner problems with economics in and of themselves do not lead to radical reassessment.

Nonetheless, the opening up of economics in this way is part of a much broader and genuine opening up of the position of economics, or political economy, within the other social sciences. In this respect, and more generally, the dual retreat from neo-liberalism and postmodernism means that the direction and dynamic across disciplines and topics is difficult to anticipate. To some degree it will depend upon the response to economics imperialism and, where it is negative, whether this leads to a retreat from economic considerations altogether (in some new form of post-Xism) or to the incorporation of alternatives rooted in political economy, history of economic thought, and systemic methodologies and concepts. It is precisely here that heterodox economics has an indispensable role to play, not by retreating into its separate and, to some extent, esoteric and marginalised concerns, but through forging a more or less conscious collective enterprise of sustaining critique of orthodoxy, in relation both to its economic and its non-economic analysis, and offering systemic alternatives especially where of appeal to interdisciplinarity. It is necessary to be mindful that the current generations of heterodox economists, trained in but rejecting the orthodoxy, are liable to be reproduced in the future in ever dwindling numbers from within the discipline. They are already predominantly white, middle-aged males, and disgruntled. Are they going to go out with a bang or a whimper?

Footnotes

¹ For my work on economics imperialism, see <http://www.soas.ac.uk/departments/departmentsinfo.cfm?navid=490>

² For my own case, see entry in Arestis and Sawyer (eds) (2001).

³ Note that such limitations also apply to the application of mathematics to the natural sciences, as was highlighted by those investigating the philosophical foundations of mathematics.

⁴ Fine (1980) makes the same point in arguing that external causal content within neoclassical economics is a consequence of the social content that is taken as given for the purposes of individual optimisation. And Hodgson (2007) has questioned whether a pure form of methodological individualism is to be found in practice, let alone that it is possible giving the necessity of taking something social as given in the first instance.

⁵ Whilst Sala-i-Martin (1997) reduced the number of regressions he ran from four to two million, this was subsequently raised to 89 million although 33 million of these proved sufficient for his purposes, Sala-i-Martin et al (2004). This is despite the extraordinarily unrealistic assumptions that need to be made in order to justify such statistical profligacy, Rodríguez (2006) for example.

⁶ See Mosini (2007, p. 5) for the very different relationship between theory and evidence in natural sciences as opposed to economics, with close “reality checks” for the former throughout in contrast to the latter.

⁷ Mirowski’s work here concerns the impossibility of the mainstream’s economics of knowledge and, in its own way, offers an excellent illustration of some of the hypotheses offered here.

⁸ See Hodgson (1997) theory and, more recently, symposium in *Eastern Economic Journal*, vol 31, no 3, 2005, Han and Schefold (2006), and Fine (2006c) in context of new growth theory.

⁹ References to Coase (1988 and 1994) are to collections of essays, with original dates of publication indicated where differing from that of the volume concerned.

¹⁰ How appropriate that Samuels (2007, p. 166) should quote Knight (2005[1933], p. 35) to the effect that, “There is *no established* economic usage for anything in economics”, the notion of expectations being a notable illustration.

¹¹ See Akerlof and Kranton (2000) for first contribution and Davis (2007b) and Fine (2007b) for their further contributions and complementary critiques.

¹² As Mirowski demonstrates for the economics of information.

¹³ For Samuels and Medema (1997, p. 179), for example transaction costs via Coase “has opened up and focused for many mainstream economists ideas which hitherto were left to the heterodox economist. He has not been alone in this, but he has been significant”.

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