

Economics as progress: the LSE approach to econometric modelling and critical realism as programmes for research

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In the face of claims that economics is increasingly driven merely by fashion, this paper draws out certain similarities and differences between two self-consciously progressive and developmental research programmes—namely the *LSE approach* to econometric modelling and *critical realism in economics*. The argument is that, while these two programmes of research share a common point of departure and possess many common elements, what at root distinguishes them is their adoption of opposing philosophical orientations. The comparison enables both the nature of each programme, and the relevance of their common concerns, to be more easily appreciated and helps clarify the sort of evidence that would provide a basis for selecting one project over the other.

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1. Introduction

Modern economics is driven very largely by fashion, with topics, themes, questions, categories appearing and disappearing without any clear line of progression.¹ The natural sciences, by contrast, are recognised as supporting numerous sustained projects. Think in particular of medical research and the attempts to identify and

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¹ This is a frequently made observation, Turnovsky (1992, p. 143), for example, writes: 'There are several aspects of economics as it is currently practised which I find to be troubling and which I hope will be reversed over the next several years. First, economics particularly in the United States, is very much subject to fads. Certain topics become hot for a period, consuming a lot of research effort, only to become obsolete in a relatively short period of time and to be superseded by something else'. As David Hamilton notes 'Fashions in economics are almost as fickle as those in dress and automobiles' (1984, p. 143).

understand diseases. Moreover, in such disciplines it is not unusual to find long-term projects in competition with one another. That is, one can find programmes of research addressing the same problems or phenomena but coming from different angles or perspectives, perhaps drawing upon different basic convictions about the nature of the problem or its solution. When two sustained competing programmes are found, a comparison of their relative strengths and weaknesses often facilitates insights which would otherwise not be apparent. Such comparisons allow us to see more clearly what is essential to the respective projects by deepening our understanding of their methodological presuppositions. Contrastive analysis of this type may also suggest ways in which it is vital to go forward by indicating more precisely what it is that requires further investigation and clarification for a reasoned choice to be made between the alternatives on offer.

While the characterisation of economics as fashion driven is undoubtedly correct as a broad generalisation, economics too can boast some sustained and competing programmes. And a failure to appreciate this can mean that the potential insights that can be achieved from contrastive exercises remain unrealised. In order to illustrate, and indeed to draw insight for modern economics, a comparison of two very important, if nominally quite different, programmes, which have each been under way for more than 20 years, is undertaken. Both programmes are addressing the same problem. Each has attracted a significant group of contributors. Numerous, albeit very different, advances have been made in the two projects. And the common question they address remains as important as it did when the two projects were initiated. By comparing the two, the nature of each, and the relevance of their common concerns, can be more easily appreciated. The comparison also enables us to identify the sort of evidence which would be required to move things forward, in the sense of providing a basis for selecting one project over the other. In this particular case, such a comparative assessment has a further rationale in that it helps us clear up a confusion perpetrated by the two main proponents of the respective projects more than 20 years ago. A disagreement between the two who have done most to develop these projects raised in an initial exchange but left unresolved can be clarified with the hindsight of seeing how the projects have developed.

The first of the two sustained, developmentally progressive, projects to be considered is the econometrics project of David Hendry and his colleagues, sometimes referred to as the *British tradition* or *LSE approach*.¹ The second is the project of Tony Lawson and his colleagues, mostly emanating from Cambridge, and systematised as *critical realism in economics*.² Focusing on these two particular programmes is not intended to suggest that there are no other constructive programmes of research in economics. A number of projects, especially those within heterodox traditions, such as Post-Keynesian economics and Old Institutional economics, can be seen in this light. However, it is important to demonstrate that progressive long-term projects do exist,

¹ The persistence of the LSE perspective has been highlighted as an aspect of the approach which marks it out from the changing and transitory fashions of much applied economics see (Faust and Whiteman, 1995, p. 171).

² No attempt is made here to provide a general assessment of, or introduction to, either of these programmes. Rather the emphasis is upon relevant points of contact which highlight the similarities and contrasts between them. For a more general discussion of Hendry's approach, see Cook (1999), Gilbert (1986), Pagan (1987) and Keuzenkamp (1995). For one useful attempt to place critical realism in context, see Fleetwood (1999); for a general introduction see Boylan and O'Gorman (1995).

for the contrary opinion is widely held. Here, two examples are provided that are important in many ways: their focus is of interest to all economics and their differences sum up the essence of the choices facing modern economists concerned with illuminating the social world.

Each project is seen by its proponents as developmental and progressive. Hendry, for example, explicitly invokes the notion of a *progressive research strategy for empirical modelling* (see Hendry, 1993, pp. 177, 210) and stresses the developmental nature of the project (see Hendry *et al.*, 1990, p. 257). Critical realism, meanwhile, differentiates itself from certain projects in economic methodology which are, more or less, exclusively methodologically focused by insisting that the objective is to move *economics* forward (see Lawson, 1999B). In what sense can these self-consciously progressive and developmental research programmes be seen as dealing with the same problem? Both projects are, in fact, responding to the observation that modern econometrics has traditionally failed to identify event regularities of any satisfactory degree of strictness or durability, certainly not of a sort to facilitate successful forecasts. Hendry, for example, emphasises that '[f]ew of the main macroeconomic forecasting systems can produce sensible forecasts without the tender loving care of their proprietors. Many econometric equations in common use show significant deterioration in the accuracy with which they characterize data as time passes. And many economists are sceptical about empirical evidence, feeling it "lacks credibility"' (1985, p. 272). He even argues that: '[a]ll models are crude simplifications, but many are hopelessly crude because of the methods used in their formulation, which camouflaged their inherent flaws: i.e. they were very badly designed. *The first hot blast of post sample reality ensured the conflagration of these straw-houses*' (italics added, 1985, p. 279).¹ In similar vein, Lawson notes: 'Although econometric failure is manifest at many levels an outwardly familiar sign is the poor forecasting record of "econometric models" designed to track developments in the economy' (1997A, p. 301). More generally he notes:

The most telling point against this econometrics project is the *ex posteriori* result that significant invariant event regularities, whether of a probabilistic kind or otherwise, have yet to be uncovered in economics, despite the resources continually allocated to their pursuit. Fifty years ago Haavelmo (1944) justified his efforts in developing 'the probabilistic approach to econometrics' with the observation that 'economics, so far, has not led to very accurate and universal laws like those obtaining in the natural sciences' (1944, p. 15). With the passage of time this situation does not seem to have changed significantly. *Econometricians continually puzzle over why it is that 'estimated relationships' repeatedly 'break down', usually as soon as new observations become available.* (Lawson, 1997A, p. 70; italics added)

The aim of this paper is to provide a comparison of these two programmes to see what can be learnt. In particular, the objective is to understand better the separate contributions of the two projects and to discover on what basis a choice between them could rationally be made by highlighting their many similarities and crucial differences in philosophical orientation. From such a comparison, it will be seen that it becomes possible to resolve the confusion which characterised an early brief exchange in this

¹ Hendry, in fact, could not be more severe in his criticism of the conventional approach to econometric modelling. He suggests that the conventional approach 'is inherently non-scientific and it would be surprising if it threw light on anything more than the investigators' personal prejudices' (1985, p. 273).

journal between the two most prominent contributors to these programmes of research. Central to the original debate is an approach to econometrics delineated by Lawson (1981A) and designated the 'standard approach'. Lawson wrote of '[o]ne approach—which for the purposes of this essay will be labelled the 'standard' approach—accepts the existence of 'correctly specified' equations and 'true' models and assumes the role of the econometrician is to utilise existing data in order to identify such structures' (1981A, p. 317). These true models, argued Lawson, were usually assumed to have stable parameters and a white noise error process. Against this 'standard', Lawson delineated a second approach, identified then as Keynesian, but thereafter systematised first as realist and eventually as critical realism in economics,¹ which does not start out with the presumption that correct econometric equations or (stable parameter) models exist only to be empirically identified.

At one stage, Lawson referred to Hendry and his colleagues as working within this standard approach. Hendry replied explicitly rejecting the claim that he worked within the approach Lawson designated as standard, arguing that, in his framework, all models are 'false' by definition. Hendry also accepted the possibility of unstable parameters (that structural breaks occur). Indeed, Hendry seemed even to accept many of the criticisms that Lawson levelled at much work carried out under the rubric of econometrics. Despite these clarifications, Lawson replied insisting that he had not misrepresented Hendry and that contrasting stances on the issues on which he focused, specifically the role of presuppositions of true models with constant parameters, were what divided the two projects. *Prima facie* both could not be correct. With the passage of time, it is clear that both participants have stuck to the basics of their own assessments of their own projects. But with the greater clarification, elaboration and progress that both projects have achieved, we can see how early misunderstandings occurred. As it happens, it is not that one was right and the other wrong. Rather, both were mostly right on their own terms. By examining how this is so, we obtain a better insight into both projects and are able to determine more clearly than hitherto the issues on which the relative performances of the two projects turn. The issues so brought into relief are fundamental to modern questions concerning the practice of empirical economics.

2. A common perception of the problem

In considering the output from the two programmes over the last 20 years, it would seem that Lawson must have misunderstood Hendry quite significantly. To situate Hendry's project as part of a 'standard' approach to econometrics that needs to be countered seems, at one level, to be almost wilful misrepresentation and, what is more, it appears to ignore certain striking correspondences between the two programmes. Lawson and Hendry's approaches actually share a common understanding of the difficulties of doing empirical research and also make similar criticisms of key questionable practices carried out within conventional econometrics. The features that the two projects share have grown to be quite substantial. In particular, both

¹ The re-examination of this debate serves as a useful corrective to the view sometimes expressed that critical realism in economics represents simply the attempt to impose a philosophy of science literature from outside as it were (see Mäki, 1998, p. 408). It is clear that the development of critical realism in economics has emerged from the problems and characteristics of economics itself.

delineate a real world independent of our analysis of it and view it as complicated and evolving, and subject to structural changes. Both highlight the invalidity of much econometric practice, pointing out its inconsistency with most econometric theory. Both projects reject the idea that econometrics is analogous to experimental control. And both allow that models with 'nice' properties may be unobtainable. Yet the two projects proceed very differently. In eventually seeing how they do, we shall not only gain further insight into these projects but also see that Lawson was not so misguided in his initial drawing of contrasts after all. First, though, let me briefly elaborate on these claimed commonalities.

2.1 Realist propositions

Both programmes can be seen as adopting a realist orientation. Within critical realism, the thesis of the existential intransitivity of objects or ontological realism (the claim that things in general exist and act independently of our knowledge of them) is tied to the thesis of the historical transitivity of knowledge or epistemological relativism (that there is no way to know what exists except under particular, historically transient, descriptions). To elaborate, there are two types of objects of knowledge: the things which are being expressed or described, etc., and the prior theories, hunches, facts, hypotheses, guesses, intuitions, speculations etc., that are being transformed and drawn upon in arriving at current formulations. To describe certain objects or features as intransitive is to indicate that they exist at least in part independently of any knowledge of which they are the referent. Intransitive objects of knowledge need be no more fixed or enduring than transitive ones. With regard to the social realm, at the moment a social investigation is initiated, the putative objects of that enquiry either do or do not exist and, if the former, possess whatever properties that they do quite independently of the process of investigation which eventually ensues. This remains the case even if that investigation in due course informs a transformation in the object concerned. At the same time, if knowledge is not merely given in experience neither can it be seen as being created out of nothing. Rather, it must come about through a transformation of pre-existing knowledge-like materials. The argument critical realism develops is that:

the pervasive phenomena of scientific continuity and change is intelligible only if we recognise this distinction between intransitive and transitive objects of knowledge. For only then can we make sense of scientific discovery and change in our knowledge of the world on the one hand, and changes in the world itself on the other. That is unless knowledge and its intransitive objects possess relatively distinct beings and histories, scientific change and changes in the nature of things could not be distinguished, with the consequence that the former would be unintelligible while the latter would never be noted ... the intelligibility of the phenomena of scientific continuity and change entails not only that creative subjects do not constitute the world (as in idealism including its post-modernist or pragmatist variants) but also that thought could be regarded as a mechanical function of the world (as in empiricism). Rather, knowledge must be seen as consisting of stuff that is socially produced, typically symbolically mediated and formed, historically specific, and materially irreducible to its intransitive objects. Both inside and outside science, when we talk or write of things, structures, events and states of affairs, we can speak or write of, and know, them only under particular descriptions that are neither reducible to, nor isomorphic with, the objects which ... they nevertheless express. (Lawson, 1997A, p. 239)

Hendry, at times, explicitly adopts a realist perspective in the sense that the ultimate objects of scientific investigation are taken to exist independently of, or at least prior

to, their investigation. For Hendry, '[t]here is an economic mechanism which operates in reality: This comprises the transacting, producing, transporting (etc.) behaviour of economic agents at some time and place' (1995, p. 55). In adopting a realist orientation, Hendry is also concerned to provide some explicit commentary upon the nature of the object of analysis. Specifically, he suggests that the economic realm is highly complex:

I certainly think that the real world exists. There are agents behaving in that world, and their interactions generate outcomes. We have a measurement process and a good idea of what it measures. The observables are generated by that joint mechanism, which is more complex than any model we think of. (Hendry *et al.*, 1990, p. 189)

Hendry's references to an economic mechanism and a data generation process appear at times to be pointing to the distinction between an intransitive and transitive realm.¹ He notes: 'I find it very useful to think of the data generating process (DGP), the actual economic mechanism, as something external, and my model as something I am using to understand the DGP' (Hendry *et al.*, 1990, p. 197). Elsewhere he notes that: 'We need different terms for the mechanism and models thereof, and will use the term DGP only for the actual process which generated the data' (1995, p. 55). It would seem from these kind of remarks that Hendry is suggesting that the economic mechanism and models exist in distinct realms, the actual economic mechanism and the DGP in the intransitive realm, the model in the transitive realm.

2.2 Structural breaks and the intrinsically dynamic nature of social reality

In a series of recent papers, Hendry and his colleagues have considered the implications for econometric modelling and forecasting of the existence of *structural breaks*. Hendry recognises that economies constitute highly dynamic complex processes and emphasises that they 'are often subject to major institutional, political, financial, and technological changes which manifest themselves as structural breaks in econometric models relative to the underlying data generation process' (Clements and Hendry, 1998, p. 1). Hendry (1996, p. 412) provides an illustrative list of the kind of legislative, social, institutional and financial regime shifts that can generate such structural breaks. The existence of regime shifts and associated structural breaks is seen to undermine traditional forecasting procedures severely:

Unfortunately, the historical track record [of economic forecasting] is littered with less than brilliant forecasts. There have been major episodes of predictive failure when the model forecasts have systematically over—or under predicted for substantial periods, and realised outcomes have been well outside any reasonable *ex ante* confidence interval computed from the uncertainties due to parameter estimation and lack of fit. Examples include the forecasts made by the major UK model based forecasting teams over the 1974–5 and 1980–81 recessions ... This suggests that it is inappropriate to use a theory of forecasting based on assuming a stationary process with constant parameters which are accurately captured by the model. Nevertheless, the majority of analyses adopt just such a perspective, and concentrate on a world in which a constant-parameter, unchanging relation ... holds. (Clements and Hendry, 1996, p. 102)

¹ While highlighting the relevance of such a distinction seems to lie behind Hendry's emphasis on the DGP, there appears to remain some ambiguity concerning the way in which Hendry himself deploys the term; for discussion see footnote 1 on p. 190.

Essentially, the criticism of traditional forecasting procedures would seem here to be that they fail to take assessments of the nature of the underlying object of analysis sufficiently into account. Thus Clements and Hendry observe that:

periods of economic turbulence resulting from structural breaks or regime shifts historically go hand in hand with episodes of dramatic predictive failure ... The correlation between turbulence and predictive failure is to be expected, but what is surprising is that most analyses of economic forecasting are firmly rooted in the assumption of a constant data generation process. For an econometric theory of forecasting to deliver relevant conclusions about empirical forecasting, it must be based on assumptions that adequately capture the appropriate aspects of the real world to be forecast. (1996, pp. 104-5)

The task Hendry and his colleagues set themselves, then, is that of considering how econometric modelling can contribute to forecasting while acknowledging this feature of social reality.

To the extent that Hendry and his co-workers emphasise and acknowledge the dynamic and evolutionary nature of social reality, there is considerable correspondence with critical realism. Critical realism in economics argues that individuals are internally complex and cannot be conceptualised as passive automata. It insists that the subject matter of social science, including economics, cannot be reduced to principles governing the behaviour of individuals and descriptions of their situations. In particular, in addition to acknowledging the centrality of human beings, human practices and contexts of action to all social life, critical realists establish the irreducibility of social structures including rules, relationships and positions. The conception of the social world sustained is of a network of continually reproduced interdependencies. As Lawson notes: 'Social reality is conceived as intrinsically dynamic and complexly structured, consisting in human agency, structures and contexts of action, none of which are given or fixed, and where each presupposes each other without being reducible to, identifiable with, or explicable completely in terms of, any other' (1997A, p. 159). On this view 'social structures ... cannot be regarded as somehow fixed; they can never be reified. There is always fluidity and movement. Even when, over-restricted regions of time and space, a structure might be held to have been reproduced "intact" as it were (and ... given the holistic nature of much social structure all reproduction doubtless entails some change, just as change will rarely be total) this is always on the basis of the intrinsically dynamic and always potentially transformative, human practice' (*ibid.*, p. 171).¹ Just as Hendry and Clements criticise the traditional theories of forecasting for retaining the assumption of a constant *data generation process*, similarly Lawson criticises the ontological neglect he sees as characterising much economic modelling: 'any willingness of economic modellers to keep on attempting to forecast sufficiently accurately cannot establish that achieving this goal is a real possibility—that its conditions are everywhere or anywhere satisfied' (Lawson, 1998A, 359).

¹ More concretely, Lawson writes: 'We live in an open economy, subject to the influences of erratic movements in world trade, multiple regionalised wars, frequent institutional collapses, the regular re-arranging of trading policies, agricultural gluts and failures, novel political developments including arbitrary, unannounced foreign policy changes bearing on matters relevant to domestic consumers and traders etc' (1997A, p. 83).

2.3 *Econometric theory and practice*

Lawson and Hendry also both acknowledge that econometric theory and practice are mutually inconsistent and see the respective programmes they support as responding constructively to this feature of contemporary economics. Lawson explicitly motivates his book *Economics and Reality* by identifying a series of theory–practice inconsistencies, which he associates with contemporary mainstream economics. One such inconsistency relates to the way ‘economists frequently employ methods, practices and techniques of enquiry and modes of inference, that are inconsistent with the theoretical perspectives on method which they claim to draw upon’ (1997A, p. 4). According to Lawson:

Econometrics is the paradigm example here. It is widely observed, for example, that when econometricians formulate and (often simultaneously) estimate their ‘models’ they frequently ‘run’ hundreds if not thousands of regressions, and in doing so contradict the classical theory of inference they explicitly acknowledge. Moreover, when their models are used to forecast unobserved (typically future) states of the economy, econometricians repeatedly make *ad hoc* revisions to estimated parameter values, or introduce ‘add on’ factors in order to generate results that are ‘sensible’ or believable, thereby contravening what Lucas designates the ‘theory of economic policy’. (Lawson, 1997A, p. 6)

Lawson then goes on to attempt to render such inconsistencies intelligible.

Hendry too emphasises the distance between econometric theory and practice:

At present there are peculiar gaps between theory and what people actually do: I think the sinners and preachers analogy in Leamer (1978) is the correct one here. The theoretical econometrician says one thing but as a practitioner does something different. I am trying to understand why economists do that, given that they know the theory, and they are obviously trying to solve practical problems. (Hendry *et al.*, 1990, p. 179)

In their recent book on forecasting, Clements and Hendry (1999, p. xxiii) situate their own contribution as one which attempts ‘to reduce the present discrepancy between theory and practice’. In an earlier paper, Hendry points to just the same set of tensions to which Lawson later refers when motivating his book. Hendry suggests that within conventional modelling:

any misfit should lead to rejection of the model. Instead, to rescue their pet hypotheses, investigators may run literally ‘hundreds of regressions’ (cf. Friedman and Schwartz, 1982, p. 266), hoping that one of these will ‘corroborate’ their ideas. Thus, difficulties are camouflaged or papered over, not revealed; the resulting models are not robust and it is little wonder they break down when confronted with new data and/or new economic policies ... they do not even adequately characterise existing data. (1985, p. 277)

For Hendry, such tensions can be overcome only once an alternative approach to applied economics is adopted.

2.4 *Experimental control and the data of the social realm*

An explicit recognition that the data of the social realm are not amenable to being treated as if they were generated in conditions of experimental control is a further common component which the two programmes share. For Hendry, ‘[e]xperimentation may be useful as part of scientific method, and is a powerful instrument for discovery and evaluation, but it is not essential to either science or progressivity’ (1997A, p. 168). He robustly criticises the view that econometrics represents a counterpart to experimental control in economics. He argues:

Econometrics is sometimes viewed as the economics equivalent of experimental control. In such a framework, it is argued that although economists primarily obtain non-experimental data, the impact of uncontrolled variables can be removed after the event by partialling out their effects using techniques such as multiple regression and its many sophisticated derivatives ... However, that 'model' of econometrics would require that all relevant uncontrolled effects were measured and were indeed removed as covariates: since a major objective of econometric analyses is to determine what factors are relevant, there seems little hope of attaining that requirement. (1997A, p. 173)

According to Hendry (1995, p. 28), within the experimental context the modeller controls the environment and some of the factors (i.e., the inputs), traces their effects on certain other factors (i.e., the outputs) and establishes a causal relationship between inputs and outputs. Hendry sees it as highly misleading to treat observational data of the type typically available to the econometrician as if they represent measurements obtained in the context of a controlled experiment:

To apply the logic used by physicists (say) to the empirical analysis of data in econometrics requires the omniscient assumption that the model is the mechanism which generated the data. In economics, we do not know how the data were actually generated, and we do not control the economy in the way that a physical scientist can control an experiment. (1995, p. 28)

Hendry suggests that it is important to recognise that economics departs significantly from the physical sciences in so far as 'rather little is known about the relevance of the states considered to the actual data generation process' (1997A, p. 171). Conventional econometrics fails to appreciate this difference: 'conventional modelling assumes that the model and data process coincide at the outset; i.e. that the data were actually generated by the factors in the model plus a random innovation impinging from nature' (1985, p. 277). As a consequence, conventional econometrics proceeds by asserting the existence of correct models whose parameters have only to be estimated.¹

Lawson is equally critical of 'the pretence that economic phenomena are ... generated under conditions equivalent to those achieved through experimental control' (1997A, p. 235). As we have seen, Lawson suggests that social reality is open, dynamic and holistic; the implication drawn from this is that neither it, nor stable bits of it, are amenable to isolation in conditions analogous to those of experimental control facilitating the production of event regularities. Thus he notes: '[i]t is certainly reasonable to doubt that controlled experimentation will ever be particularly meaningful in economics due to the impracticality of manipulating social structures and mechanisms in order more clearly to identify them' (1997A, pp. 203–4). Specifically, he suggests that 'the environment in which any mechanism acts need not be sufficiently homogeneous. In the social realm, indeed, there will usually be a potentially very large number of countervailing factors acting at any one time and sporadically over time, and possibly each with varying strength' (1997A, p. 218). Further, 'the mechanisms or

¹ Hendry writes: 'Somewhat as a caricature the conventional approach in empirical economics is as follows: (i) postulate an arbitrary theory (arbitrary in the literal sense of being at the free choice of the investigator); (ii) find a set of data with the same names as the theory variables (such as 'money', 'incomes', 'interest rates', 'inflation' etc.); (iii) make a range of auxiliary simplifying assumptions (e.g., choosing a linear model, assuming away measurement errors); (iv) fit the theory-model to the data to see the degree of match. Corroboration is sought, and accepted, with minimal testing to check whether rejection is possible against interesting alternative hypotheses (such as non-random errors, and changing parameters)' (Hendry, 1985, p. 273).

processes which are being identified are themselves likely to be unstable to a degree over time and space' (1997A, p. 219).

2.5 Event regularities and constant parameters

Both programmes see the existence of event regularities, and so models that pre-suppose constant parameters, as a fundamental issue, and Lawson and Hendry each consider the possibility of the absence of such regularities. Here Lawson's reference to 'event regularities' and Hendry's 'constant parameters' are viewed as essentially relating to the same phenomena. Hendry acknowledges that the assumption of data generation processes characterised by constant parameters can be questioned:

the relevance to data analysis of arbitrarily postulated DGPs with constant parameters ... is open to question, since it is not obvious that there need exist any parameters in the empirical representations which economists consider ... For example, there may not be a marginal propensity to consume for any agents, any society, or any entities, however many theories postulate its central role, or data analyses claim its determination. I believe such constructions do exist, because (e.g.) the aggregate marginal propensity to consume apparently can be altered by changing taxes. Moreover, a substantial number of econometric relationships seem to persist through time. Finally, human behaviour, at both the individual and the social level, evolves slowly relative to the time-span of most econometric studies. Just as there is no criterion of truth for empirical models, so we cannot verify that parameters of interest exist to be discovered. Nevertheless, parameters of interest motivate and structure a study to come to a better understanding of the world in which we live. Historically, the existence of the initially postulated parameters may not be essential to scientific progress since, in the process of analysis, a better framework may emerge. Conversely, faster progress is likely if the selected parameters do reflect salient and constant features of reality. Parameters which prove to be constant empirically seem naturally to be of interest. (1995, pp. 348–9)

Hendry suggests that the adequacy of any underlying assumption concerning the existence of constant parameters can only be assessed *ex post*:

economies seem prone to regime shifts, structural breaks and technological and financial innovations which require adaptation and learning by economic agents. All of these induce different forms of non-stationarity, which require careful empirical modelling if invariant parameters are to be established but do not *per se* preclude doing so ... However, it is unsurprising that evidence in economics is less secure than in the natural sciences, a difficulty exacerbated by the tiny allocation of resources to data collection in economics. *The only issue of principle involved is the existence of underlying invariances, and on that, the proof of the pudding is in the eating.* (italics added, 1997A, p. 178)

Just as Hendry is prepared to question the validity of assuming the existence of constant parameters, so Lawson examines the assumption of the existence, and doubts the prevalence of constant conjunctions of events. In order to facilitate discussion, Lawson sets out a number of definitions relating to event regularities, perhaps the most basic of which concerns *regularity stochasticism*. Regularity stochasticism is for Lawson the thesis that 'for every (measurable) economic event or state of affairs y there exists a set of conditions or events, etc., x_1, x_2, \dots, x_n say, such that y and x_1, x_2, \dots, x_n are regularly conjoined under some (set of) 'well behaved' probabilistic formulations. In other words ... for any (measurable) economic event y a stable and recoverable relationship between a set of conditions x_1, x_2, \dots, x_n and the average or expected value of y (conditional upon x_1, x_2, \dots, x_n) or some such, is postulated' (1997A, p. 76). Lawson and others developing critical realism have been especially

concerned to identify the set of conditions on a system which would ensure that such event regularities hold. These have been found to be rather strict. Systems in which constant event regularities do not feature are referred to as open. Closed systems relate to those situations where constant event regularities obtain.

If Hendry insists that the existence of constant parameters is something that can only be assessed *ex post*, then for Lawson too the question of whether or not event regularities occur in the social realm remains essentially an empirical matter. Thus he notes:

In *Economics and Reality* I maintain, as an *ex posteriori* assessment, that event regularities that are both of sufficient strictness to be of use to economic modellers, and of scientific interest (i.e., containing some revelatory insight; acting behind our backs, as it were), are found to be rather rare. Amongst other things, if fifty years of econometric failure (in the face of billions of 'regressions') is suggestive of anything it is surely that regularities of the sort being sought after may after all be thin on the ground. In consequence, it seems at least reasonable that the hypothesis of the non-occurrence of event regularities of the sort being pursued be taken seriously. (Lawson, 1998A, p. 357)

There exist, then, numerous points of contact between the LSE approach to econometrics and critical realism in economics. Both programmes can be seen as explicitly realist and provide commentaries on the nature of the object of analysis in elaborating the approaches they respectively defend. Both remain critical of others who do not take assessments of the nature of the underlying object sufficiently into account. Both recognise that econometric theory and practice are mutually inconsistent. Both explicitly recognise that the track record of econometric forecasting is poor. Both elaborate important sets of categories. Both programmes emphasise that the data of the social realm are not amenable to being treated as if generated in conditions of experimental control. Both understand the social world as evolving complex processes subject to structural breaks. Both see the existence of event regularities (and so constant parameter models, etc) as a point in contention. And both acknowledge that the existence or otherwise of event regularities (and hence constant parameters etc.) is ultimately an empirical issue. Given these similarities, Lawson's original interpretation of Hendry's approach as a standard approach within econometrics does seem *prima facie* extraordinary. In fact, with so much in common the relevant issue would seem to be how to differentiate at all between the two projects.

3. Identifying and accounting for the differences

The difference between the two programmes, in fact, begins with a further commonality. Most conventional econometric projects start by asserting the existence of correct models whose parameters have only to be estimated. They assume that social outcomes can be treated like phenomena produced in well-controlled experiments. Lawson and Hendry both reject the parallels with controlled experiments, both think that models with constant parameters are a special occurrence, and both set out to interpret such occurrences should they obtain. That is, both set out to provide general frameworks, against which specific occurrences of models with relatively stable parameters are a special case, and in terms of which they can be given a particular interpretation. Thus, in providing these general frameworks, Lawson and Hendry are again following similar lines, lines which take them away from the standard

approaches. We can see again, then, why Hendry felt aggrieved in being referred to by Lawson as adopting a ‘standard approach’. It is at this point, though, that their ways do part company. And we shall see that they do so in a manner that renders Lawson’s assessment intelligible, and on its own specific terms, actually correct. Let me consider the two general frameworks in turn, starting with Hendry’s approach.

3.1 *Hendry on the DGP and the method of reduction*

A fundamental feature of Hendry’s approach is that it starts from the idea of a complex (considered to be) unknowable Data Generation Process or DGP. There are, in fact, (at least) two ideas in play here. The first is the one already referred to above of a complex real world (ontological) mechanism (including measurement system) that gave rise to the economic data of interest. The second is a joint probability distribution defined over the entire sample space for all the observed random variables. Following Haavelmo (1944), it is *as if* the observable variables are produced in accordance with this distribution. It is the second idea, of the joint distribution, that is essential to Hendry’s method of reduction, i.e., his account of ‘how to analyse data in a non-experimental world’ (1995, p. 29). There may be some slippage in Hendry’s writings, though, in that sometimes it is the former mechanism with a measurement system superimposed on it that is regarded as the DGP while, at other times, the term is used to refer to this *and/or* a probability distribution.¹ However, for our purposes it is sufficient that the concept of a joint distribution is relied upon. Thus Cook and Hendry write:

¹ For example, Hendry writes: ‘More generally, it is postulated that there exists a stochastic process generating all the variables (denoted by w_t) which are, and/or are believed to be, relevant (allowing for whatever measurement methods are involved). This vast complex is called the data generation process (abbreviated to DGP). In practice, the DGP may well be unknowable to our limited intellect, important variables may be unobservable, and/or the stochastic mechanism need not be constant over time. Nevertheless, I assume that there does exist a “meta-parameterization”, denoted by θ , which characterises what is relatively constant in the process. Less restrictively, the mechanism is assumed to generate outcomes sequentially over time. Empirical models result from reparameterizing the process, through eliminating (marginalizing with respect to) all but a small subset of variables (those remaining being denoted by x_t) and conditioning one sub vector y_t (called endogenous) of that remaining subset on another z_t (called “exogenous” because it is not determined within the model). Here the basic statistical operations of conditioning and marginalising are used in their conventional senses: given any two continuous random variables a and b then their joint probability distribution $D(a, b)$ can be expressed as

$$D(a, b) = D(a|b)D(b) \text{ (or } D(b|a)D(a)) \quad (18.1)$$

where $D(a|b)$ is the conditional distribution of a given b and $D(b)$ is the marginal distribution $b \dots$

Thus, the conceptual framework is that there exists some joint distribution denoted $D(w_1 \dots w_T/\theta)$ which by repeated application of (18.1) corresponding to the notion of sequentially generating data yields the DGP:

$$D(w_1 \dots w_T|\theta) = \Pi D(w_t|w_{t-1} \dots w_1; \theta) \quad (18.2)'$$

(1983B, p. 422). Hendry appears at least at times then to use the term the DGP to refer both to the underlying mechanism (plus measurement system) *and* to a probability distribution. In more recent work the argument is that the Haavelmo distribution constitutes a convenient statistical representation of the DGP. The implication is that the DGP and the probability distribution are different kinds of entity. This interpretation of the DGP is also evident in earlier work (see Hendry and Richard, 1982, 1983). This ambiguity has been commented upon by Keuzenkamp who suggests that ‘The DGP is reality and a model of reality at the same time. Philosophers call this “reification”. Once this position is taken, weird consequences follow’ (1995, p. 236).

In theory, one starts with the DGP which gives rise to the data, defined by the economic mechanism and the measurement system. In a non-experimental discipline such as economics, the DGP is a complicated, unknown function which is unlikely to be discovered ... In practice, however, analyses are conducted within a framework defined by the Haavelmo distribution rather than the DGP (after Haavelmo who drew the attention of economists to the need to model in terms of joint distributions ...) This joint distribution of the observed random variables over the entire sample period, which provides the most general description of the probabilistic information in the data and links the DGP to probability theory. (Cook and Hendry, 1994, pp. 75-6)

They suggest:

The analysis begins with the complete set of random variables U_T^{-1} relevant to the economy under investigation where $U_T^{-1} = (u_1, \dots, u_T)$ defined on a probability space (Ω, F, P) where Ω is the sample space, F the event space, and P an appropriate probability measure. The assumption that economic events are measurable does not seem unreasonable: indeed Schumpeter (1933) went so far as to argue that economics was inherently the most quantitative of the sciences since its measures arose from the ordinary business of life! The $\{u_t\}$ are not just the data to be analysed in the investigation, but also comprise all the potential data from the economic mechanism under study, so the DGP operates at the level of U_T^{-1} . Thus, the vector u comprises details of every transaction of every agent at time t in all the regions of the geographical space relevant to the analysis. Obviously, U_T^{-1} is unmanageably large, and many of its components are either unobserved or so badly measured as to be unusable. The assumption that the behaviour of $\{u_t\}$ can be characterised by a joint distribution function is much more tenuous, although Haavelmo (1944) presented a powerful case for its generality. We make that assumption here ... (Cook and Hendry, 1994, p. 78)

Now given this idea of a complex joint probability distribution, it is possible to see all actual econometric models as reduced forms of it. Conceptually, any model posited can be seen as derived from the general model through a reduction process which involves marginalisation, conditioning on certain variables (Hendry currently lists 12 distinct steps for reducing a complex distribution to a simple model, see Hendry, 2000A).

So if all models can be interpreted as reductions of the general probability model defined over all relevant observable outcomes, the significant question, for Hendry, is whether reductions are achieved in the most useful way in some sense. The status of all the empirical models is the same: they are reductions of the general model. The aim, then, is not so much truth-seeking as one of engineering. The goal is to design a model that is useful.¹ And, typically, being useful means being found, first of all, to have a residual term that is unsystematic (and has relatively constant variance). It also means having parameters which are as constant as possible on subsets of existing data. It includes being consistent with economic theory. And it is desirable that variables treated as given are ones which economic agents could act contingently upon. It is held, too, that an acceptable model will encompass previously estimated models. Other criteria are also sometimes given, but those mentioned here should indicate the nature of the exercise. The important point is that, for Hendry, getting a 'good model' according to the sorts of criteria just listed is a question essentially of engineering or

¹ Thus Hendry writes: 'Without an assumption of omniscience, there is no escaping the implication that all empirical econometric models are designed according to criteria specified by their proprietors ... The interesting issue is not whether or not models should be designed, but how they should be designed, and thus how the gap between theory and empirical evidence should be bridged' (1995, p. 359).

design. Consider how Hendry contrasts his approach to that which treats economic phenomena as if generated in a well-controlled experiment:

there is a key difference between a fully-controlled experiment described by a linear model say, and a linear econometric model. The former can be represented schematically as:

$$\begin{array}{ccccc} y_t & = & f(z_t) & + & v_t \\ \text{(output)} & & \text{(input)} & & \text{(perturbation)} \end{array} \quad (13.1)$$

where y_t is the observed outcome of the experiment when z_t is the experimental input, $f(\cdot)$ is the mapping from input to output, and v_t is a (hopefully small) perturbation which varies between experiments conducted at the same values of z . This equation entails that given the same inputs, repeating the experiment will generate essentially the same outputs. The point is that causation is going from the right-hand side to the left-hand side in equation (13.1). Alternatively expressed, equation (13.1) is the DGP for y_t . It is this feature which validates, for example, regression analysis of the relation between y and z .

For econometrics to mimic experimental control requires data in which the outputs are in fact generated by the inputs, so the model must coincide with the mechanism which generated the data. But the economic mechanism is too complicated to be precisely modelled, and all econometric models must be simplifications and hence false. Since we neither know how the data were generated, nor control the economy, even though econometric equations might look like equation (13.1), there is in fact a fundamental difference, shown in equation (13.2)

$$\begin{array}{ccccc} y_t & = & g(z_t) & + & \varepsilon_t \\ \text{(observed)} & & \text{(explanation)} & & \text{(remainder)} \end{array} \quad (13.2)$$

Now, the left hand side determines the right, rather than the other way round as in equation (13.1), and equation (13.2) merely shows that y_t can be decomposed into two components, $g(z_t)$ (a part which can be explained by z) and ε_t (a part which is unexplained). Such a partition is possible even when y_t does not even depend on $g(z_t)$, but is determined by completely different factors $h(x_t)$ say. In econometrics:

$$\varepsilon_t = y_t - g(z_t) \quad (13.3)$$

describes empirical models: changing the choice or specification of z_t on the right hand side alters the left hand side, so (ε_t) is a derived process. In contrast to the process (v_t) in equation (13.1), (ε_t) in equation (13.2) is not a random drawing from nature: it is defined by what is left over from y_t after extracting $g(z_t)$. (1997A, pp. 174–5)

Hendry insists that in such circumstances it is necessary to approach

modelling in a more engineering spirit—that is as a process of designing models to achieve certain (albeit limited) objectives. Thus, the model is viewed as an inherently simplified mimic of behaviour, not a facsimile of the data process, and its unexplained component (residual) is derived as ‘everything not elsewhere specified’. Then one designs the model such that (i)(a) the residual is unsystematic (i.e., is an innovation or ‘news’ relative to the available data) and has a relatively constant variance; (i)(b) the variables treated as given are ones which economic agents could act contingently upon; (i)(c) the parameters are as constant as possible on subsets of the existing data; (ii) it is consistent with theory; (iii) it is admissible given the properties of the measurement system (e.g., predictions of prices are positive, or of unemployment are less than 100 per cent etc); and (iv) it encompasses previous models (either historically or that and their forecasts). (1985, p. 277)¹

In recent years, this framework has been supplemented with the category of a local DGP or LDGP. This is a particular reduction of the general probability distribution

¹ Hendry repeatedly draws a connection between econometrics and engineering; see Hendry and Wallis (1984, p. 4).

underpinning the whole analysis. It is defined over that subset of economic variables being considered in any analysis, and is conceived of as possessing unsystematic residuals, allowing that, should the LDGP be known, outcomes could be predicted up until an innovation error. The modelling exercise is then, in effect, to identify (or mimic) the LDGP:

From the theory of reduction, there always exists a unique 'local DGP' (denoted LDGP) of the variables being modelled, formally derived from the actual DGP by reduction ... Any economy under study may comprise billions of decisions and millions of recorded variables, generated by the DGP of agents' behaviour and the recording procedures. The LDGP is the corresponding representation for the subset of variables under analysis such that, were the LDGP known, the outcomes could be predicted up to an innovation error. Thus computer-generated data from the LDGP would differ only randomly from the actual values, and would do so in the same way as equivalent data generated from the DGP itself ... The implication is that selection must be judged against the LDGP. (2000B, p. 471)

Notice that the analysis does not at any stage require that the parameters of the LDGP or local model must be constant. Hendry is consistent throughout on this. If they are, this is an *a posteriori* occurrence, in effect it means that the DGP over the relevant period has been of such a nature that a model representing a specific reduction is found to possess constant parameters. Even where a constant parameter model is found, these parameters may not be easily interpretable in terms of economic theory. They do, after all, result from the complex reduction of a complex distribution. And Hendry emphasises this. Even so, he hopes that constant parameter models will be found which can be interpreted. Otherwise, as he admits, the models achieved would hardly be useful, even interpreted according to his general framework:

No assumptions about 'constant parameters' or stationary data are needed in justifying this analysis, although highly non-constant coefficients in any resulting econometric model would render it useless. (2000B, p. 471)

When invariant features of reality exist, progressive research can discover them in part without prior knowledge of the whole ... Indeed, a sequence of mutually encompassing LDGPs can be visualised, each of which is a valid representation of the phenomena under analysis. Should no invariant features of reality exist, neither theories nor econometric models would be of much practical value. (2000B, p. 474)

To summarise, within the actively controlled environment of the experiment, Hendry argues that it is possible to identify the DGP. In experimental contexts, it is feasible to specify a relationship between the data in which different observations (repetitions of the same experiment) are connected by a common description (the same equation). The significance of the experimental context is that it allows an equation to be formulated which captures what is constant in the data. In economics, where researchers are dealing with a complex mechanism and historical, non-experimental data reported using limited measurement techniques, the DGP remains unknown and inaccessible. But Hendry insists a coherent and useful theory of econometrics can still be constructed. Crucially, he accepts that the relevance of his preferred method of reduction depends upon one central, essentially meta-physical, belief:

The nature of observations in macroeconomics is very different from that in some of the other sciences and must pose problems for any methodology. First economic time series are

contingent on the particular historical path followed by the economy under study. This does not pose insuperable difficulties for reduction theory provided there exists an ahistorical (for example, stationary ergodic) innovation impinging on the dynamic evolution. Belief in the latter lies at a metaphysical level, although aspects are testable (for example, by developing homogenous explanations which survive for prolonged time periods). (1997A, p. 177)

Hendry remains confident that it is possible to promote a progressive research strategy within econometrics despite the limitations of conventional approaches. He expresses hope not only that invariant features of reality exist but also that such constancy or invariance is systematically expressed at the level of regularities amongst measurable events. He admits that, were these latter to be absent, econometric models would be stripped of much of their relevance or usefulness. Once Hendry's bold metaphysical assumption is accepted and if a sympathetic attitude is adopted toward his expression of hope that constant parameters obtain, his recommendations for the reorientation of econometrics appear coherent.

3.2 *Lawson's depth realism*

Lawson's approach actually takes off where Hendry's leaves off. Lawson would certainly agree with Hendry that 'should no invariant features of reality exist, neither theories nor econometric models would be of much practical value'. The difference between Lawson and Hendry is where they look for the invariant features of reality. Hendry's approach ultimately relies on reasonably invariant correlations holding between observable variables, as we have seen. Lawson does not rule out the possibility of these being uncovered. But he develops an alternative approach in which explanatory success does not depend on their being found. This is a long story, and I must summarise.¹

Like Hendry, Lawson starts by giving an interpretation of such event regularities as do occur. But Lawson does not invoke the framework of DGPs and methods of reduction. Like Hendry, Lawson indicates the essentials of his approach by comparing it with what goes on in the well-controlled experiment. But Lawson provides a different interpretation of experimental work. By asking why natural scientific event regularities are mostly restricted to conditions of well-controlled experimentation and how it is that experimental results can be successfully applied outside the experiment where event regularities do not occur, Lawson provides a particular take on what is going on. He argues that we can only make sense of all this if we see experiments as seeking to isolate the stable causal mechanisms in which we are interested from countervailing mechanisms, in order to identify them empirically. The event regularity achieved is a correlation between the events triggering the isolated mechanism and its (undisturbed) effects. The results achieved can be applied outside the experiment because they relate to the underlying causal mechanism, not to the event regularity corresponding to its empirical identification. Thus, as Lawson often reminds us, the gravitational mechanism which gives rise to an event regularity when objects are dropped in a vacuum equally affects the paths of leaves as they fly over roof tops and chimneys.

¹ See Lawson (1997A) for detailed elaboration and defence.

Now the significant point here is that Lawson interprets the primary goal of experimental and all scientific work to be not the production of an event-regularity *per se* but the identification of the mechanism responsible for it. In Lawson's terminology, this is causal explanation. Correlation analysis can be an aide to this process but it is not essential. And, according to Lawson, it is a knowledge of causal mechanisms that scientists mainly seek, and which can be used to send rockets to the moon or cure disease. Data patterns are viewed more as useful epistemological features, where they occur, or can be produced, which enable underlying causal mechanisms to be identified.

Of course, because experimental control is typically not possible in the social realm, specific procedures are required to identify social causes. For this, Lawson advances his theory of contrast explanation. It is not necessary to discuss this idea here other than to say that the aim is to explain a surprising outcome: to explain 'why this rather than that (as expected)'. Whereas experimental work seeks to isolate a single mechanism physically, Lawson seeks to isolate one (or aspect of one) conceptually. He does this by focusing on two outcomes that unexpectedly have turned out to be different, when our current understandings led us to expect that they would have an identical causal history. *Prima facie* there is a case, in such situations, for supposing a single causal mechanism to be responsible.¹

3.3 Competing philosophical frameworks

Lawson generalises his framework in a different manner to Hendry. Hendry produces a framework in which a joint probability distribution is defined over all observables, and against which any particular model is perceived as a reduced form. The generality comes with the move from variables of interest to a framework in effect covering all observable variables. Lawson's generalising move is from events of interest to the structures or mechanisms behind them, and responsible for them. Lawson's is a vertical ontological extension, Hendry's is a horizontal one. Lawson goes deeper; Hendry goes wider. Lawson seeks to uncover the nature of social reality, and advances a social ontology against which event regularities are a special case. On this conception, the limited occurrence of event regularities does not matter, because the explanatory goal is to uncover the (relatively enduring) mechanisms responsible for the movements in events or observables.

Why does Hendry not adopt this move as well? After all, there is nothing in Lawson's approach that necessarily invalidates Hendry's approach and interpretation. It mostly provides a more general way of proceeding, one that can be fruitful whether or not event regularities occur. The answer seems to be Hendry's prior philosophical orientation. Hendry acknowledges:

I think 'causality' is only definable within a theory. I am a Humean in that I believe we cannot perceive necessary connections in reality. All we can do is to set up a theoretical model in which we define the word 'causality' precisely, as economists do with $y=f(x)$. What they mean by that in their theory is that if we change x (and it is possible to change x), y will change. And the way y will change is mapped by f , so we have a causal theory. They could give a precise or formal definition of the mapping $f(\cdot)$. Empirically, concepts such as causality are extraordinarily hard to pin down. In my methodology, at the empirical level, causality

¹ For a discussion of a contrast explanation, see Bhaskar and Lawson (1998), Lawson (1997A, ch. 15; 2003, ch. 4).

plays a small role. Nevertheless, one is looking for models which mimic causal properties so that we can implement in the empirical world what the theorist analyses; namely, if you change the inputs, the outputs behave exactly as expected over a range of interesting interventions on the inputs. (Hendry *et al.*, p. 184)

Hume denied the possibility of establishing the independent existence of things or the operation of natural necessity. In Humean philosophy, the only properties of which we can have any knowledge are those which give rise to distinct impressions. These properties include the perceptible qualities of bodies, such as their shape, size, colour, etc. We can observe the speeds and directions of, say, two colliding billiard balls immediately before and after they collide, and we may identify a regularity in the way these speeds and directions are connected. What we do not observe is something beyond this that constitutes the capacity of one billiard ball to move another. On Hume's account of perception and causality, as traditionally interpreted, it is experiences, constituting atomic events and their conjunctions, that are viewed as exhausting our knowledge of nature. On such a view, generalities of significance in science must take the form of event regularities for these are the only sort of generalities that such an ontological position can sustain. Hendry is restricted by his Humeanism to searching out correlations at the level of events and to sophisticated forms of data analysis.

Lawson is not restricted in this manner. Critical realism adopts a thoroughly *a posteriori* position, more Aristotelian in orientation, which defends the reality and knowledge of not only surface phenomena but also underlying structures, mechanisms, powers, tendencies etc. As Lawson notes for critical realists:

it is real things and their powers or ways of acting that are considered to be knowable and are taken to endure. Specific kinds of things have powers to act in definite ways in appropriate circumstances by virtue of certain relatively constant intrinsic structures or constitutions, or more generally, natures—which are discerned *a posteriori* in the process of science and general experience. It is these essential natures that designate what things are. And once we know what a thing is then, if certain 'activating' or 'triggering' conditions hold, we know how it will behave. (Lawson, 1989, p. 239)¹

Without a structured ontology, the absence of strict event regularities necessarily threatens the search for scientific generalities. With a structured ontology of the kind elaborated within critical realism, persistence and generality can obtain at a different level. Moreover, as we have seen above, the realist account of deeper structures and mechanisms can render significant aspects of scientific activity, i.e., experimental activity and the application of scientific knowledge outside the experimental set-up, intelligible. Lawson, in committing himself to knowable deeper levels of reality, sets about elaborating ways of identifying underlying causes.

We can now clearly see the differences in the two projects despite their numerous commonalities, including their criticisms of others. Ultimately, they differ because Hendry's philosophical orientation prevents him from following the sort of path taken by Lawson. Probably Lawson would endorse Hendry's approach to the extent that models are found *a posteriori* to be successful. But Lawson does not need them and, as he does not expect useful econometric models to arise often, he has

¹ On the contemporary relevance of Aristotelian essentialism and its connections with critical realism, see Fleetwood (1997). For further historical background, see Cartwright (1992).

committed himself to the alternative, ontologically explicit approach often systematised as critical realism.

4. Making sense of the original debate

Despite numerous similarities between the two programmes concerning the problems associated with carrying out applied research, and similar criticisms of conventional econometrics and reservations as to whether it is coherently grounded, there are after all key differences. Hendry's responses to the identified failures of conventional econometrics are from within a framework which holds fast to the hope that there are nonetheless event regularities of significance and durability in the social realm to be uncovered. The task is then to look both for procedures which would facilitate the identification of stable event regularities despite previous failures, and for ways to model what is going on after 'structural breaks' have undermined what were previously treated as regularised event patterns. Lawson's response is to fashion an alternative approach, one that makes no a priori commitment concerning the existence of strict event regularities. Both projects set out to provide general frameworks, against which special occurrences of models with relatively stable parameters are a special case, but herein lies the differences: for while Hendry might be said to consider the wider domain of surface phenomena, Lawson focuses on the underlying causal mechanisms ultimately generating the events and states of affairs that econometricians wish to correlate. These differences arise as a result of competing basic philosophical orientations being adopted. Hendry admits to being a Humean. This leads him to focus more or less exclusively upon measurable data. Hendry, in fact, defines science as 'a public approach to the measurement and analysis of observable phenomena' (1997A, p. 167). Lawson adopts a thoroughly *a posteriori*, more Aristotelian, position which concerns itself not only with surface phenomena but also with underlying structures, mechanisms, powers, tendencies, etc. Having identified where precisely and why these programmes diverge, can this help to explain the confusions which characterised the original exchange between Hendry and Lawson? Given Lawson's and Hendry's competing philosophical orientations to science, both more fully elaborated over time, it now comes as no surprise that they differed as much as they did in their earlier debate. It is even possible to identify why misunderstandings arose.

4.1 In what sense does Hendry's approach remain 'standard'?

In his original paper, Lawson (1981A) distinguished *standard* and other approaches according to belief in the likelihood of social event regularities being uncovered. His language though was about the possibility of finding *true* models. Thus he notes: 'Two different approaches to econometric modelling have been identified. The first assumes that there exists a 'true' or 'correctly' specified set of equations which have only to be identified' (1981A, p. 322). Given this notion of a true model, it can be seen that true models and event regularities amount to very much the same thing. Moreover, Lawson was adopting this language in order to connect up explicitly with the terminology of Lucas (1976). For Lawson was responding to the Lucas critique of Keynesian modelling. Lucas used the language of true models of structures. Specifically, Lucas pointed out that his critique did not turn on the difficulty of finding a true model, but on the problem for modelling caused by the fact that the true model after a government

intervention was not the same as the true model before it. Lawson was arguing that this criticism did not affect Keynesians because there was no presupposition of a true stable parameter model anyway: '[t]he Keynesian approach to modelling does not accept that reality has a "correct" representation in equation form' (1981A, p. 322). At another point in the paper, Lawson draws the contrast explicitly between 'The standard approach which accepts the existence of a 'true' model—that reality has a correct representation in equation form—and the Keynesian approach which, essentially, does not' (Lawson 1981A, p. 319). In this context, Lawson argued that Lucas's criticism applied not to the Keynesian approach but only to those who adopted the 'standard econometric approach'. And as an illustrative example Lawson mentioned Hendry:

On reading the various contributions by Hendry and others, it is difficult not to be aware of the heavy emphasis on tests of, or ways of increasing, parameter stability and predictive accuracy. *This emphasis reflects an underlying belief that there exists a correct equation which needs only to be identified and which apparently possesses constant and stable parameters and usually a white noise error process.* (1981A, p. 318; italics added)

When Lawson said Hendry's approach was 'standard', he meant one thing only and something very specific: that its usefulness required invariant regularities or constant parameter models (however we refer to them). In all other senses, Hendry's approach is clearly non-standard. In this particular sense, it remains standard. And it is obvious why Lawson picked out this one feature. For it is on this one feature that Lawson parted company with all other approaches to econometrics, including Hendry's.

It is now possible to see how confusion arose. If Lawson had said he was not convinced that social event regularities exist to be uncovered, Hendry could have expressed his optimistic view to the contrary, both could have agreed it was ultimately an empirical matter, and they could have left it like that. Instead, Lawson expressed it as an (*a posteriori*) scepticism in the possibility of true models, noting Hendry's optimism. When Hendry replied, saying that he did not claim to have identified a true model, Hendry seems to have misinterpreted Lawson's talk about possibilities as a claim that he—Hendry—had found a true model. Hendry mistook an ontological claim for an epistemological one. In particular, Hendry was eager to correct what he saw as a misrepresentation of his work, and this involved quoting exactly a particular passage from an earlier jointly authored paper. Specifically, Hendry reproduced the following passage: 'However we do not [*sic*] conclude that our model represents the "true" structural relationship . . .' (Hendry *et al.*, quoted in Hendry, 1983A, p. 69). Lawson replied that he never claimed that Hendry had found a true model, only that Hendry accepted the possibility. He took the following longer version of the above statement by Hendry as evidence of this: 'However we do not conclude that our model represents the "true" structural relationship since there are several important issues yet to be considered' (Hendry *et al.*, as quoted by Lawson, 1983, p. 78). Lawson pointed to other examples where phrases such as correct models were deployed. Clearly, by phrasing his remarks in terms of a "'true" model assumption', Lawson left himself open to misunderstanding. Nevertheless, it is clear that his main point was that, within the standard approach, a specific representation of economic reality was being relied upon: for the standard approach

to be coherent and useful, it needs to be assumed that event regularities which could be expressed in equation form exist. Significantly, Lawson notes that this assumption is typically made a priori or adopted as an act of faith: 'the notion of a "correct" model or equation (and the usual presumption that this will involve stable parameters and a white noise error process) is ... no more than an act of faith or an assumption' (1981A, p. 319). In his reply, Lawson states that: 'I wished to distinguish those econometricians who appear not to doubt the existence of (but not that they necessarily claimed to have found) true or correct relationships—that reality has a correct formal (if probabilistic) representation in (simple) equation form—which can in principle be identified, from those who at the very least are not convinced of this' (1983, p. 78). While Hendry is prepared to accept that it is an empirical issue as to whether event regularities obtain, the hope that they do exist is what renders his project coherent. In confusing Lawson's claim about possibilities for a claim about actual models, Hendry misread the basis on which Lawson was interpreting the standard approach. Hence, Hendry thought it enough merely to point out that he never considered any particular model as the true one. Here we have an explanation of Lawson's reply noted above.

4.2 *Conceptions of truth and the status of econometric models*

So far, I have suggested that, in attempting to clear up the confusions that surrounded the initial exchange, it is important to recognise that Lawson, in characterising the standard approach, was concerned with the possibility of uncovering significant event regularities and was not suggesting that Hendry was claiming to have himself found a true model. However, this only constitutes part of the explanation. It is also crucial to recognise that Lawson and Hendry define truth in different ways. Both define it in relation to their own frameworks, and this leads to further differing views about the status of models.

For Lawson, truth is a relation (an expressive referential one) between theory and reality. For Hendry, it is a relation between a model and a more complex probability distribution or DGP (or statistical representation of the DGP). On Hendry's account, for truth the model and DGP must coincide (essentially be the same thing). But the whole point of Hendry's approach is that this rarely happens in the economic sphere. Indeed, the question is what to do *because it does not happen*. Hence Hendry's starting point is that all econometric models are false (because partial). When Hendry suggested in his response to Lawson that he accepted, and never claimed, that a true model or correct equation existed, he merely meant that any model derived, whether or not the parameters are stable, will be derived from a DGP and so will be necessarily simpler than it and so in *this sense* false. He wrote:

A model is a simplified representation of some set of phenomena; as Hayek (1967, p. 14) expressed the matter '... a model always represents only some but not all the features of the original'. All models are, therefore, 'incomplete' pictures of reality and in that sense are 'false' by definition. Consequently, I generally drop the redundant qualifier, retaining the description 'false model' for a model that is rejected against another model. A 'true' model is virtually a self-contradiction as it would have to be as complicated as whatever it is supposed to represent. (Hendry 1983A, p. 70)

This view of truth is one which Hendry has repeated and elaborated upon in later papers. For Hendry, models are engineered (designed) reductions of the DGP and,

while they can be viewed as good/legitimate or not, it is not helpful to regard them as true. Hendry argues 'in favour of replacing a search for truth, which cannot be ascertained in economics, by a search for congruent encompassing empirical models which have a consistent and comprehensive theoretical interpretation' (Cook and Hendry, 1994, p. 73).

Lawson's goal is actively to seek truth. For truth is achieved when a theory or model adequately expresses that part of reality it is about. Lawson does not claim that we must obtain true theories, or even that we would know if we did. But he does not deny the possibility of truth, and recognises its importance as a regulatory ideal. In elaborating upon his expressive theory of truth, Lawson tries to draw links with what he takes to be the intuitions of many econometricians:

To accept the fallibility and transformability of knowledge, however, is not thereby to deny the possibility of objective truth. Rather propositions are true, or contain truth, by virtue of the way that the world is. There is no need or possibility here of rejecting that implicit premise of the correspondence theory that truth can, under at least some of its numerous aspects, figure as an objective property of propositions. Here, the intuitions of many econometricians can be accepted. For it seems clear that when they reject either the possibility, or the actuality, of truth in formulating their models, especially when they declare that these models are not yet true ones, truth is being interpreted as something objective. It is being acknowledged as something which holds or exists independently of any individuals beliefs, and presumably, given the often found emphasis upon truth being unobtainable because of the complexity of reality, which turns upon the way that the world is. The implicit recognition of an objective aspect to truth is something I clearly retain.

But in place of the conception of correspondence (or mapping) in knowledge and truth, the notion of expression is preferable. This term, as much as any, has connotations which capture that characteristic of knowledge that it is indeed fallible, historically transient and transformable. It reminds us that there is no necessary relation of identity or correspondence involving knowledge. It indicates that what is expressed, the referent of any expression, can never be reduced to or mapped onto, that which is expressed. But this latter situation, if to repeat, does not undermine the possibility of propositional truth. Indeed, the statement that 'contemporary western society is relatively complex', which in effect appears to be a premise for the familiar (if erroneous) claim that economic modelling must be unrealistic or false, may even be absolutely true—by virtue of the way the world is. Clearly, though, whether or not this is so, there is no question of a mapping or correspondence involved. (Lawson, 1997A, pp. 239–40)

Thus, when Hendry says his models are partial (and so in his terminology false), Lawson (in his terminology) would be likely to describe Hendry as making a probably true statement about the social world. For Lawson, partial claims can be true, and he explicitly argues that partiality does not necessitate falsity:

to focus upon particular features of something, for example a social system or a human being, is not per se to treat those features as existing in isolation or otherwise subject to necessary distortion. If I focus on a person's eyes in an attempt to gauge his or her reaction to what I am saying, or if I describe them in reporting my impression to others, I do not suppose that they exist in isolation; nor do I otherwise necessarily misrepresent the person's reaction in any way. (Of course ... there may not even be any distortion involved in expressing something as an idealisation. It depends on the referent of that expression. Idealisations can be real). To take a partial approach is not per se to deform. It can involve distortion; but the one is not equivalent to the other. (Lawson, 1997A, p. 240)

4.3 Assessment and implications

With hindsight, the central issue of the debate, masked by the discussion of true models, is whether there is any alternative for empirical economics other than persisting with the narrow search for event regularities. Hendry's view rooted in his Humeanism is that essentially there is no alternative, Lawson's view, albeit initially only partially expressed,¹ is that there is. With the acknowledged *a posteriori* difficulty of uncovering event regularities, or uncovering those that predict successfully, Hendry's Humean orientation has taken his project one way; Lawson's Aristotelianism has taken the project of critical realism in economics a different way. Within their own specific frameworks, each is understandable, and clearly progressive. Hendry's approach ultimately rests (with the rest of econometrics) on the faith that there are relatively stable event regularities to uncover (as Lawson in fact originally claimed). However, his approach is not standard in any other sense. For Hendry, econometric models are never true (thus their ability to express truth is not a helpful/meaningful criterion). Rather, they are reductions that are (or are not) legitimate/valid and

¹ Lawson clearly attempts to identify two approaches to economic modelling. The standard approach is seen as presupposing that social reality has a correct representation in equation form while within the Keynesian framework: 'The presumption is neither that the models are in some sense "true" nor even that there is a possibility of a true model in equation form' (Lawson 1981A, p. 318). Despite this, it is sometimes unclear what precisely Lawson is defending. In particular, at times, he appears to be defending one, more or less, formalistic approach to modelling, namely Keynesian macroeconomic modelling, against other formalistic approaches which presuppose event regularities, i.e., the standard approach. The obvious question which arises here is, if the objective of the Keynesian macroeconomic modellers is not to identify underlying event regularities: What are the objectives they pursue? Occasionally, the task Lawson seems to have set himself is to provide some such rationale. In certain passages (Lawson, 1981A, p. 322) the emphasis appears to be on formal modelling being one useful heuristic device. Running alongside and intermingled with this attempt to provide a rationale for formal Keynesian modelling is a second line of argument questioning the relevance of formalistic modelling altogether and pointing tentatively to some possible alternatives. For example, Lawson throughout stresses the continuously evolving and context-specific nature of social reality and also the relevance of case study work (Lawson, 1981A, p. 319). Indeed, if one considers Lawson's own substantive contributions, dating from roughly the same period (see, in particular, Lawson, 1981B; Kilpatrick and Lawson, 1980), it is clear that the favoured approach to empirical work is one of detailed case study analysis with little or no reference to formal modelling exercises. Interestingly, Lawson has recently returned to these two studies (1998B, 1997A, ch. 18, respectively) demonstrating how they can both be seen as consistent with critical realism. Lawson has not returned to the task of defending formal modelling from a developed critical realist stance. Indeed, it would seem he has become rather pessimistic about the contribution that such formal modelling strategies can perform in systems which are characteristically open (see, for example, Lawson, 2001). He notes that: 'Fundamental to the mainstream position is an insistence on working with formalistic models. Indeed, the primary objective of this mainstream project is to produce theories that facilitate mathematical tractability. In contrast, my goal (naïve though it may sound) is to pursue true theories, or at least to achieve those that are explanatory powerful. I have found that the two sets of objectives, explanatory powerful theories and tractable models, are usually incompatible, just because of the nature of the social world. For whereas the latter has been found to be quintessentially open and seemingly unsusceptible to scientifically interesting local closures, the generalised use of formalistic economic methods presuppose that the social world is everywhere closed' (1999B, p. 273). In retrospect, it is apparent that what Lawson was groping toward was not an alternative basis for formalistic modelling at all but a rigorous alternative to it. Lawson's focus has increasingly been upon elaborating explanatory strategies which may prove fruitful within the structured and open social realm where the experimental production of event regularities is not feasible, and where few strict event regularities of any interest seem to occur spontaneously. It is crucial here to note that there is no consensus among critical realists concerning the likely contribution of formal methods within economics or more generally social science. Many argue from a critical realist position, i.e., accepting the structured and open ontology outlined within the project, that specific types of formal techniques can be of considerable value; for discussion, see Finch and McMaster (2002), Porpora (2001) and Downward and Mearman (2002).

potentially useful. Hendry's approach provides a framework of interpretation, of constructive comparison and much more. But it does not provide a method for progressing in a world without some reasonably stable correlations between events. Lawson's approach does. Given the relative paucity of social event regularities so far, the Aristotelian approach at this point in time at least, appears the more promising.

There is a fundamental choice here which carries significant implications for how best to do empirical economics and it is at root a choice between the two philosophical frameworks. At this fundamental level it would seem that Lawson comes off best, in the sense that, while Hendry's Humeanism is largely *a priori* (and in any case now discredited in modern philosophy¹), Lawson's orientation is *a posteriori*, grounded and, indeed, can sustain Hendry's as a special case. For success to be achieved in identifying even limited event regularities, closures (albeit local ones) must obtain. Now as Lawson notes:

closures themselves have been shown, within critical realism, to presuppose, and indeed be a special configuration of, an open structured system, that is, a special case of the sort of system that does obtain ... Critical realism thus cannot and does not rule out *a priori* their limited occurrence. Rather, critical realism adopts an essentially *ex posteriori* orientation. And if the primary aim of the project of critical realism in economics in particular is to bring ontological considerations (back) into the economics picture and to indicate real possibilities in the social realm, it cannot determine *a priori* which possibilities are to be actualised in any local context. It can explain why *ex posteriori* closures do not seem to occur very often in the social realm (given the latter's human agency dependent, intrinsically dynamic and highly internally related nature), and it can and does indicate ways of proceeding in their absence. (1999A, p. 7)

Thus critical realism can accommodate the possibility of event regularities obtaining in the social realm. Were strict event regularities to be identified in the social realm, this would represent an interesting phenomenon requiring explanation, especially in the light of critical realist arguments concerning the nature of social material. Significantly, though, critical realism can also accommodate, and adequately account for the non-prevalence of event regularities in the social realm, as well as the record of failure on the part of those searching for event regularities. Moreover, critical realism further provides some guidance concerning how to proceed with the task of identifying causal mechanisms in an essentially open social world.

5. Concluding remarks

In the face of claims that economics is increasingly driven merely by fashion, this paper has set out to consider two self-consciously progressive and developmental programmes which have been promoted within the discipline over the last 20 years. The objective has been to draw out certain similarities and identify contrasts between these programmes. There are clearly points of contact between the two projects. Both programmes are seen by their proponents as standing opposed to an entrenched conventional or orthodox position. Both involve engagement at a methodological level, both have undergone considerable and significant extension, and both have undertaken reassessments at the level of the history of thought. The paper has shown that there are also more substantial commonalities linking the two programmes. Both projects can be seen as responding to

¹ See Bhaskar (1978), Chalmers (1992, 1999) and Humphreys (1988).

the observation that traditional attempts to identify event regularities within economics have hit upon severe problems. Both projects are realist at least to a degree, both provide explicit commentaries on the nature of the object of analysis in elaborating the approach defended, and are critical of others who do not take assessments of the nature of the underlying object into account. Both recognise that econometric theory and practice are inconsistent. Both emphasise that it is not useful to treat the data of the social realm as if they are generated in conditions of experimental control. Both understand the social world as an evolving complex process subject to structural breaks, and so on. Yet, despite all this, the responses advocated by the programmes are very distinct. The different strategies advocated reflect their respective philosophical orientations and associated ontological commitments. Hendry adopts essentially a Humean orientation, Lawson does not. Hendry looks for patterns in surface phenomena; Lawson looks behind these for their deeper or underlying causes.

The present comparison of the two developed programmes has allowed some of the confusion surrounding an earlier exchange between the two main proponents of the programmes to be resolved. The point at issue in this earlier debate ultimately comes down to whether there is any alternative approach to empirical economics other than looking for patterns in surface phenomena. Lawson and Hendry's competing positions on this issue reflect the adoption of differing ontological positions, just as the developed programmes diverge as a consequence of their adherence to competing philosophical frameworks. Hendry accepts that, at root, his programme rests on a metaphysical or ontological claim: this is something to be welcomed, since it constitutes a recognition that ontology is inexorable. What remains perplexing is the lack of concern with the sustainability of his ontological position. It is as if, for Hendry, it is enough to state his metaphysical claims. The sorts of circumstances which might lead Hendry to abandon or modify his ontological commitments are never identified. In considering a way forward for empirical economics—in choosing between Hendry's and Lawson's respective approaches—what is required is reflection on the presuppositions being made by the competing projects in the light of an analysis of the ontological conditions of social reality.

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