Reorienting economics?

Simon Mohun and Roberto Veneziani

2010

Reorienting Economics?*

Simon Mohun†

and

Roberto Veneziani‡

Review essay based on: Lawson Tony, *Reorienting Economics*, Routledge, London (2003) ISBN 0-415 25336-5 Pages xxvi, 383.

Abstract:

Reorienting economics analyses many important issues in the social sciences. This paper focuses on Lawson's key methodological and epistemological claims concerning the role of mathematics in social theory. Lawson provides several forceful criticisms of the search of mathematical rigour for the mere sake of formalism. Yet his stronger claims on the extremely limited, if nonexistent, scope for formal analysis in the social sciences are less convincing. In general, his purely methodological approach does not provide robust foundations for reorienting economics.

Keywords: mathematical models; causal explanations; open systems; social ontology.

---

* We are grateful to Alessandro Vercelli for many detailed comments and suggestions. The usual disclaimer applies.

† School of Business and Management, Queen Mary University of London, Mile End Road, London E1 4NS. E-mail: s.mohun@qmul.ac.uk.

‡ (Corresponding author): Department of Economics, Queen Mary University of London, Mile End Road, London E1 4NS. E-mail: r.veneziani@qmul.ac.uk.

1. Introduction

Reorienting economics (henceforth, RE) analyses many important and extensively debated issues in the social sciences. It offers interesting insights on a wide range of topics, from social ontology – often neglected by social scientists – to the current status of economics. Whatever one thinks of the answers provided in RE, Tony Lawson asks some of the searching questions in the social sciences.

The starting point of RE is the unhealthy condition of modern economics, as acknowledged even by prominent mainstream economists. As Lawson forcefully shows, orthodox economics has failed according to its own standards. It has become a game of model-building played for its own sake, with little explanatory or predictive relevance. The central thesis of part I is that this failure is essentially due to the application of mathematical-deductivist methods in conditions for which they are not appropriate. Mainstream economics is defined, according to Lawson, by its *insistence* on the use of mathematics. However, our best understanding of social ontology suggests that social reality is open, internally related, organic, structured, and intrinsically dynamic. This implies that the strict regularities necessary meaningfully to apply mathematical-deductivist methods (the type of regularities observed in experimental settings) almost never occur in the social world. Therefore far from being a guarantor of scientific practice, the adoption of formal models yields few explanatory insights, and is generally inappropriate. An ontological turn, concludes Lawson, is necessary in economics to acknowledge the fundamental limitations of the tools used by the vast majority of economists.

In part II, Lawson argues that even if mathematical-deductivist methods have significant limitations, social science *is* possible, and it should aim to explain causal mechanisms. Social reality is structured, and therefore "To pursue causal explanation as interpreted here, we require a mode of inference that takes us behind the surface phenomenon to its causes, or more generally from phenomena lying at one level to causes often lying at a different deeper one. This is *retroduction*" (RE, p.80). Whereas deductive reasoning is forward-looking – and oriented towards prediction – the essence of retroduction is backward-looking and it aims at explaining some unexpected event *a posteriori*. The explanatory endeavour is started "by feelings of surprise, doubt, concern, or interest, that accompany some contrastive observations" (RE, p.93): social science starts *after* something unexpected (in some relevant sense) occurs. The causal forces at play in the social realm can be discovered by using *contrast explanations*, whereby "the question posed is not of the form 'why *x*?', but of the form 'why *x* rather than *y*?'" (RE, p.86). According to Lawson, scientific knowledge in the form of the discovery of causal structures is possible because causal forces are reasonably stable over time/space, even though they very rarely produce the closed regularities necessary to permit the use of mathematics.

In part III, Lawson analyses various heterodox schools in economics. The main claim is that – albeit often implicitly – all heterodox approaches share a common understanding of social reality, a common social ontology, which is broadly consonant with that set out in part I of RE. Consistent with this view, heterodox approaches are defined by their rejection of the mainstream *insistence* on mathematical-deductivist methods. The book concludes with a detailed historical analysis of the forces that have led to the dominance of the mathematical-deductivist approach in economics.

While the scope and relevance of the issues analysed in RE is thus very broad, this paper will concentrate on one key methodological issue, which is central in current debates, namely the role of formal models in the social sciences. At the heart of Lawson's theory lies a fundamental methodological concern: given the nature of social reality, the adoption of formal models represents a severe shortcoming of modern economics. According to Lawson (2009b), for example, the inability of economists to analyse, let alone foresee the financial crisis, derives to a large extent from their emphasis on formalism. After its mathematical turn, economics has forgotten its nature as a social theory, and has lost sight of social reality. Reorienting economics thus entails an explicit ontological perspective in the discipline, which in turn implies a fundamental change in its methodology.

2. The strong methodological thesis

An important preliminary point is that there is some ambiguity about the scope of Lawson's claims on mathematics. In fact, he repeatedly states that he does not provide an impossibility claim on the use of mathematics as such, and he does not suggest that formal methods should be banished from the social scientist's toolbox (see also Lawson, 2004; 2009a). Further, many of Lawson's claims are carefully qualified so that, for instance, "reliance on methods of mathematical deductive modelling *more or less* necessitates … a focus on conceptions of atomistic individuals and closure" (Lawson, 2004, p.334, italics added); or critique focuses on "*certain sorts* of mathematical methods" (ibid., p.337, italics added) and of "methods of mathematical-deductivist modelling *as employed in modern economics*" (RE, p.13, italics added). Based on these qualified claims, one can interpret Lawson's analysis as supporting a *weak methodological thesis* which provides a healthy warning against the *excessive* formalism characteristic of the work of many social scientists, and in particular of economists.

But as various commentators have noted, despite all caveats, Lawson's arguments lead precisely to the conclusion that formal tools are (almost) always inappropriate. Mathematical-deductivist methods presume "constant conjunctions of events or states of affairs", such that "whenever event *x*, then event *y*" (Lawson, 1997, pp.16-17; RE, pp.105ff). The latter characterise closed systems which display the strong regularities typical of the experimental sciences, in which the phenomenon is analysed in a system 'closed' – or isolated – from external influences. But this type of closure, argues Lawson, (almost) never occurs in the social realm, which is "open, structured, intrinsically dynamic in a manner dependent on social transformation, and highly internally related through social relations" (Lawson, 2006, p.500). It immediately follows that "If formal models require strict local closure, then formal models are never appropriate" (Hodgson, 2004, p.4).[1]

Lawson (2009a) rejects the accusation that his stance on mathematics is dogmatic by arguing that his analysis spells out the conditions for the successful application of formal methods, and he is not ruling out such methods "as an *a priori* disposition" (Lawson, 2009a, p.198). Granting the absence of a dogmatic *a priori disposition*, his analysis of the nature of social reality nevertheless does lead to the *conclusion* that mathematical models are (almost) never likely to succeed in explaining social phenomena. In Lawson's writings, it is difficult to find more than a handful of examples of the *successful* use of formal models (e.g., RE, pp.20-21). Moreover, despite all qualifications, Lawson's arguments are *not* restricted in application to *certain types* of models, or to mathematical methods *as used in mainstream economics*. Indeed, it is rather unclear from his analysis what alternative models might be useful.[2]

It is therefore appropriate to focus on Lawson's more controversial *strong methodological thesis* (henceforth, SMT) according to which, given the nature of social reality, formal approaches are (almost) never useful in the social sciences. Two types of criticism can be made against the latter argument. First, one may reject Lawson's description of social reality and, based on an alternative ontology, add an argument to conclude that formal models may well be useful/appropriate. Given the methodological focus of this paper, however, a different approach is adopted: *even if* one finds Lawson's broad description of social reality persuasive, the SMT does not hold. An ontological claim that reality is – at the most general level – open, structured, internally related, etc. has little bearing on methodology. As Dow forcefully puts it, "Any further discussion requires that we depart from this transcendental notion of openness. As soon as we start conceptualizing the economic system, we inevitably invoke closures (normally of a provisional, incomplete, sort). Epistemology cannot be conceived of as an open system in the same pure sense as social ontology" (Dow, 2004, p.309).

In the next section, several doubts are raised on the *negative* part of Lawson's argument, according to which mathematical techniques are (almost always) useless in economics. In the following sections, the *positive* part of Lawson's analysis is considered and it is argued that his account of contrast explanations, and of knowledge creation, does not provide an alternative model of scientific theorising that is inherently inconsistent with formal tools.

3. The *pars destruens*

Even granting Lawson's broad description of social reality, the SMT is not compelling because it depends both on an overly restrictive notion of 'closure' and on a narrow (implicit) definition of mathematical methods and of economics.

First of all, assume, for the sake of the argument, that Lawson's definition of economics and of formal methods (as essentially oriented towards prediction) is satisfactory. Although the social realm may be in general open, structured, etc., it is unclear that *every* social phenomenon possesses these features, and that it possesses them *to such a degree* as to make formal models inadequate. Lawson concedes that models can be used, for example, "where a relationship (of causal sequence) holds between measurable economic variables, and does so with *sufficient strictness* as to facilitate the successful application of standard techniques of econometrics, albeit only within a limited span of time and place" (RE, p.105, italics added). So, a reasoning of the type 'whenever *x* then *y*' can be applied, albeit in a specific context. But this weakens the claim that the conditions for application of formal models (almost) never occur. For it both explicitly admits that local closures may occur, and forcefully suggests that the notion of closure itself, and thus the appropriateness of formal methods, may be interpreted as a matter of degree (see the notion of "*sufficient* strictness"), thus further expanding, in principle, the scope for the application of mathematical techniques.

Lawson rejects the latter conclusion by adopting an extremely strong definition of closed systems (RE, pp.105ff) which allows him to conclude that local closures (when they occur) are likely to be transient, and that (almost) all social systems are open. According to Lawson, a system is open (and therefore deductivist methods cannot be used) if it "includes at least one occasion when the antecedent event occurs but is not followed by the usual consequent" (RE, p.106). Even if the statement 'whenever x, then y' (formally, $y = f(x)$) expresses a relation between a single cause x and its effect y, this notion of openness sets an impossible epistemological hurdle: the existence of an outlier (or a discordant element in a retroductive explanation *à la* Lawson) does not seem sufficient to reject a causal hypothesis.[3] Outliers are outliers. That "most observed regularities are not uniform or without exception even within limited stretches of space-time" (RE, p.106) is obviously true, but nobody would sensibly discard a model or a theory based on any *one* observation at odds with the claims of the theory (deductivist or otherwise).

This is even more evident if the statement $y = f(x)$ expresses a potentially large set of causal influences (a vector of causes x), and the analysis is developed under a *ceteris paribus* clause. Again, the logical relation is not falsified or rendered irrelevant *simply* by one observation out of line with the assertions of the theory, given that the antecedent is appropriately qualified. Even if Lawson's statement is meant to apply to major breaks beyond outliers, it is unclear that one structural break *per se* makes formal methods irrelevant or misleading.

In other words, the ontological thesis that all social systems are open requires an exceedingly weak notion of openness. But it is unclear that this notion can support the claim that the nature of social reality makes mathematical methods inadequate. For this latter, a stronger notion of openness is necessary, and on this basis one can then investigate whether at least *some* social phenomena exhibit openness *to such an extent and degree* that formal models are inappropriate.

A second problem of Lawson's analysis relates to his (largely implicit) definitions of economics and of mathematical methods. In fact, although Lawson's claims concern the nature of (the whole of) economics as a social science and the usefulness of (all types of) formal theorising, virtually all of the examples – and certainly the most compelling ones – of the failure of economic theorising concern econometrics. The latter choice lends Lawson's argument a great deal of its persuasiveness, but at the same time it is unclear that his negative conclusions can be extended to other areas of economics and beyond econometric models. Most of the arguments on the limits of mathematics in economics as a whole are essentially *a fortiori*.

As Hodgson (2004, p.5) has noted, by focusing on econometrics Lawson "gives insufficient attention to other applications of mathematical techniques, which serve primary purposes other than the prediction or explanation of measurable variables". Hodgson suggests that formal models are useful as a heuristic, "to identify possible causal mechanisms that form part of a more complex and inevitably open system" (ibid.). He analyses Schelling's (1969) celebrated ethnic segregation model as an example of a successful heuristic, which shows that segregation does not necessarily result from racist preferences.

Lawson's arguments in RE do not speak to this issue. In his reply to Hodgson, however, Lawson (2009a, p.218) denies that models can serve as heuristics because “the heuristic assumptions can go to work in economic methodology only after causal mechanisms of interest have already been identified, or at least hypothesised”. In particular, he denies that Schelling’s model uncovered any relevant mechanism: “I doubt this [mechanism] was ever news. Indeed, do we not all experience situations in which a tendency of this sort is so dominant that it is even actualised? ... My earliest memories include glimpses of physical education lessons in primary school where the teacher regularly asked the class of about 30 children to form four or so groups. Invariably, as I recall, the groups were wholly male or wholly female but not mixed” (ibid., p.216). This reply trivialises Schelling’s contribution and it misses its main point. For the model’s contribution is not to show that segregation occurs, nor that it occurs when people do not like to be located in an ethnically mixed area. The model’s contribution is to show that even if people’s preferences are favourable to integration, the response dynamics of the game generate segregation. This is hardly a trivial observation, contrary to what Lawson’s school example suggests, and Schelling’s model does clarify a possible causal mechanism leading to segregation. Indeed, it allows the formulation of a causal assumption, to be tested empirically, about the determinants of segregation.

There are many other examples in mainstream economics in which formal deductive reasoning has produced results that were not obvious. Here are ten.

1. *Under certain conditions*, universally ‘selfish’ behaviour can be Pareto-efficient, and any efficient situation can be supported by a price mechanism.

2. That excess demand (supply) generates price rises (falls) is insufficient to show the stability of a market mechanism.

3. The conditions for the uniqueness of equilibrium are extremely strong. In general therefore, equilibrium cannot be used for predictive purposes and comparative statics analysis cannot be performed.

4. If the conditions for Pareto efficiency are not met in more than one area, a Pareto improvement in one area might make the situation worse.

5. Public goods cannot be allocated by a decentralised price mechanism.

6. Aggregation is deeply problematic, whether of 'capital' or of individual excess demand functions.

7. Outside of economies producing and exchanging one good, there is no one-to-one correspondence between wage or profit rates and techniques of production, and consequently a theory of distribution relying on a monotonic relationship between marginal products and factor prices is in general invalid.

8. Under intuitively appealing conditions it is in general impossible to produce a social ranking over alternative states from individual ones.

9. Liberalism and democracy (in the sense of the principle of unanimity embodied in the notion of Pareto efficiency) are incompatible.

10. Asymmetric information between agents may destroy markets.

None of these results seems to fit Lawson's (implicit) definition of modern economics, since they are by no means oriented towards prediction. Yet they are core results of mainstream economics and provide good examples of the successful use of mathematical models as heuristics, clarifying theoretical concepts. As in the case of Schelling's model, these results, and the mechanisms generating them, are far from trivial and they have been established by formal reasoning. As Amartya Sen has noted in his Nobel Memorial Lecture, about

Arrow's impossibility theorem (result 8 above): it "can hardly be anticipated on the basis of common sense or informal reasoning … Informal insights, important as they are, cannot replace the formal investigations that are needed to examine the congruity and cogency of combination of values and of apparently plausible demands" (Sen, 1999, p.353).

Deductive formalism has also been useful in areas not conventionally defined as mainstream. Considerable effort has been devoted to the exploration of linear models in a number of directions. Examples include von Neumann's work on general equilibrium and multi-sectoral growth models; Leontief's work culminating in the input-output methods that underpin national income accounting; Seton's clarification of the Marxian transformation problem; Sraffa's analysis of prices of production; Okishio's work on the rate of profit; Morishima's formalisation of much of the corpus of Marxian economic theory; and Steedman's application of a Sraffian methodology to Marxian economics. This body of work exploring linear economics – some of it constructive and some of it destructive – has clarified what can and cannot be said on the basis of certain assumptions. These theorisations have greatly helped in clarifying a number of key conceptual issues in heterodox and Marxian economics, in ways that would not have been accessible with informal reasoning. As Roemer (1981, p.3) has noted, "A model is necessarily one schematic image of a theory, and one must not be so myopic as to believe other schematic images cannot exist. Nevertheless … the production of different and contradicting models of the same theory can be the very process that directs our focus to the gray areas of the theory."

In sum, Lawson's analysis has little or no bearing on a number of key areas of economics as a social science, where the application of formal methods is not only natural but necessary to derive insights. Further examples can be easily be provided. The definitions and analyses of inequalities, exploitation, and classes, for instance, require the construction of appropriate indices to measure such phenomena, and to analyse their pattern over time and across countries. Moreover axiomatic analysis is necessary to state the positive and normative features of the relevant measures, and the differences between them. Again, this type of formal analysis is by no means oriented towards prediction; yet it is hard to deny that it contributes to our understanding of social reality. Adopting less narrow definitions of formal methods and of economics as a social science than those implicit in Lawson makes it quite unclear that mathematical techniques are (almost always) useless. The next sections show that, if anything, Lawson's theory of social scientific explanations provides further evidence of the usefulness of formal methods in the social sciences.

4. The *pars construens*: contrast explanations

Does Lawson's account of contrast explanations provide an alternative model of scientific theorising that is inherently inconsistent with formal tools? The problem is that Lawson's model of knowledge creation is open-ended, if not underdetermined (so much so that the actual explanatory power of Lawson's contrast questions is unclear, as argued below), and this leaves significant scope for the use of formal models. Not only are contrast explanations compatible with the use of mathematics; it can also be shown that Lawson's own model of knowledge creation, whereby “Knowledge … is … a produced means of production of further knowledge” (RE, p.92), can be formalised. The former issue is analysed in this section, and the latter is considered in Section 5.

A first important point to note is that the *structure* of contrast explanations *per se* has no implication about the usefulness of mathematics. There is no reason why contrastive questions (‘why *x* rather than *y*?’) cannot be tackled with formal tools. For example, a neoclassical economist may ask why workers in a company town are not paid their marginal product and answer with a theory of monopsony. Consider Lawson’s own plant breeding example and the type of controlled experiments which involve ‘contrast spaces’ such that in some plots of land a chemical compound is used and in others it is not (RE, p.88ff). Clearly it is not the *structure* of the experiment that makes mathematical-deductive reasoning inappropriate. According to Lawson, in this case “the focus is not on a specific outcome *per se*, i.e. the level of yield of the crop, but on a comparison or contrast: whether or not the yield is significantly higher where the compound has been added” (RE, p.88). But here, in principle, formal modelling may usefully enter in at least two ways: (i) for the experiment to have some scientific value (rather than being a product of mere chance), there must be some *a priori* hypothesis concerning the causal relation between the chemical compound and average crop yields, and the latter can naturally be expressed in functional, and so in this sense, formal terms. And (ii) even if only contrast, and thus *difference* matters, the latter can certainly be expressed formally (*ceteris paribus*, *whenever* the compound is used, *then* average crop yield increases).

Indeed, even though, as already noted, Lawson provides no example of fruitful mathematical techniques, he explicitly notes that mathematics is not limited to deductivist modelling, and "if it is ever appropriate to 'associate' a method with an ontology I see no reason to suggest that some mathematical methods could not be associated with a causalist ontology" (Lawson, 2004, p.338). This would suggest that the causal explanations sought by Lawson can in principle be expressed formally. Even more forcefully, prominent critical realist Erik Wright argues that "lurking behind every informal causal explanation is a tacit formal model. All explanatory theories contain assumptions, claims about the conditions under which the explanations hold" (Wright, 1989, p.45). From this perspective, proper formal modelling *is* appropriate to analyse causal relations of the type outlined by Lawson, because it makes the relevant theoretical assumptions explicit and open to critical scrutiny.

Further, there are some features of contrast explanations that provide room for the use of formal methods in social scientific investigations. Suppose that the difference between the two types of questions ('why x?' or 'why x rather than y?') is indeed strict. Then many important issues remain outside the scope of scientific inquiry, such as all questions concerning the existence or the definition of certain phenomena. Consider the Marxian notion of exploitation: the question 'Why is there exploitation in capitalist economies?' cannot even be formulated. In fact, according to Marx, although exploitation is definitionally a feature of any class society, the only thing that can be observed by comparing capitalism and feudalism is that the form of appearance of that exploitation is different. How can the social scientist move from this observation to a notion of exploitation itself?

Lawson acknowledges the explanatory limitations of a strict interpretation of the contrast and therefore allows for the possibility that y = *not* x (RE, p.104), so that the relevant question becomes 'why x rather than *not* x?' A first implication of this move is that in many instances the contrastive scenario y = *not* x, inevitably entails counterfactual analyses. For example, in order to conceptualise exploitation a counterfactual comparison is needed of the form: 'what is a society without exploitation?' But if counterfactual comparisons fall within the scope of contrast explanations, then 'fictional theorising' of the sort rejected by Lawson (being associated with deductivist reasoning) becomes central to his epistemology. Perhaps more importantly, Lawson acknowledges that "at first sight this might seem to render all outcomes open to contrast explanation, and thereby perhaps to trivialise the explanatory approach being elaborated. But this is not so. For an essential condition of contrast explanation is surprise, interest, doubt, etc." (RE, p.104). This strategy has some undesirable implications for Lawson's theory, concerning both the epistemological status of contrast explanations in general, and the role of formal models in contrast explanations.

First, it is unclear that the latter strategy does not trivialise the approach, because the necessary conditions for contrast explanation (surprise, etc.) are entirely subjective and *a priori* undetermined: *anything* can be a source of surprise and therefore, *contra* Lawson, all outcomes are potentially open to contrast explanations. Besides, according to Lawson, the *state* of things cannot be known: we cannot explain the *level* of crop yield (RE, p.88), or the behaviour, or *state*, of cows (RE, p.95). Since the basic form of knowledge creation is contrast explanation, argues Lawson, we can only explain variations, or differences. But then, if the state of things cannot be explained, it is unclear how the original expectations about events can be rationally formed: where does the contrasting hypothesis come from? There is no guarantee that the original expectation, say y, is rationally formed, let alone the product of some sort of causal reasoning.

Indeed, the epistemological approach developed by Lawson provides no well-defined restrictions so that *any* initial assumption is in principle legitimate, including customary beliefs, religious creeds, superstitions, etc. But then, why should the feeling of 'surprise' (arising out of observing x rather than y) lead us to search for causal phenomena? To state that surprise "[presupposes] a concerned and knowledgeable orientation" (RE, p.94) is unwarranted, because nothing in the element of surprise definitionally requires such orientation. And to *define* surprise as requiring such orientation is equivalent to assuming what needs to be proved.[4]

The second, somewhat ironic, implication of this specific subjectivist aspect of Lawson's epistemology is that, by leaving the entry point of the explanatory process (i.e. the initial assumption concerning the relevant phenomenon) undetermined, it provides a natural place for mathematical models: they may play an important heuristic role in the construction of the initial assumptions of the theoretical explanation. After all, the essence of the method is that knowledge advances "by getting things knowledgeably wrong" (RE, p.101), and Lawson is emphatic that mathematical methods should not be banned *a priori*.

True, according to Lawson, in most occasions, formal models will prove wrong, since strict closures do not obtain. But this can only be known *a posteriori* and therefore nothing prevents social scientists from formulating their initial assumptions in a formal guise. Indeed, in Lawson's epistemology, the very fact that a formal model is falsified might tell us something relevant concerning the nature of a social phenomenon, in addition to the causal forces at hand. Using a formal model to build an initial assumption which may be falsified may therefore prove doubly informative. The trouble with this is that if formal models are allowed as heuristics in the formulation of initial assumptions (and nothing in Lawson's theory rules this out), then it is unclear that an element of surprise or contrast generated by some unexpected outcome would necessarily lead to their abandonment *ex post*. Once an unexpected event occurs, it is unclear how one can distinguish between the failure of an otherwise appropriate formal model to take account of all relevant causal forces, and the inappropriateness of a formal approach *per se*.

In sum, contrast explanations seem neither necessary nor sufficient to create knowledge (as maintained, instead, in RE, pp.92-93). But in any case, nothing in the structure of contrast explanations implies that formal models are inappropriate in social scientific inquiry, and indeed the open-ended nature of Lawson's theory itself suggests useful applications of mathematical-deductivist reasoning.

5. The *pars construens*: a deductivist model of knowledge creation

Setting aside the doubts on contrast explanations raised in section 4, a striking example of deductivist reasoning is given by Lawson's own model of knowledge creation, which can be described as follows: "we start out, at any point in time, with a stock of knowledge, hunches, data, anomalies, suspicions, guesses, interests, etc." (RE, p.92). Based on this stock of knowledge, we form some 'reasonable expectations' on events, or outcomes. When expectations are violated, we start a process of revision of our knowledge by means of contrast explanations, which leads to uncover some previously unexpected causal mechanism. “Knowledge, then, is found to be a produced means of production of further knowledge” (RE, p.92).

This account can be naturally translated into the deductive statement: ‘Whenever event x (x = a knowledgeable mistake), then event y (y = knowledge increases)’. Hence, quite interestingly, Lawson’s approach provides a natural framework for a formalisation of the process of accumulation of knowledge. Let K_t denote the stock of knowledge at time t. Let $\mathrm{E}o_t$ denote the expected outcome (of a certain causal process) at time t on the basis of knowledge K_t, so that we can write $\mathrm{E}o_t = f(K_t)$, where f indicates the relevant process of formulation of hypothesis. (Note that $\mathrm{E}o_t$ could denote a *vector* of expected outcomes.)

If o_t denotes the actual outcome, then the process of creation of knowledge according to Lawson can be modelled as $K_{t+1} - K_t = g(\mathrm{E}o_t - o_t)$: the variation in the stock of knowledge depends on the discrepancy between expectations and realisations. The key assumptions of Lawson’s approach can be stated as $g(0) = 0$, (there can be no knowledge creation without some pre-existing knowledge), and $g(\mathrm{E}o_t - o_t) \geq 0$ *whenever* $\mathrm{E}o_t \neq o_t$, (knowledge is created *whenever* expectations are shown to be wrong). Substituting for $\mathrm{E}o_t$, one obtains $K_{t+1} - K_t = g(f(K_t) - o_t)$: this can be called ‘Lawson’s law of knowledge accumulation’ and it formalises the creation of knowledge by means of knowledge and ‘surprise’.

If contrast explanations are considered to be sufficient for the creation of knowledge, then the previous model can be seen as a possible alternative to mainstream models of knowledge and human capital accumulation.[5] And one may argue that the adoption of a formal model helps to clarify the critical features of Lawson's model of knowledge creation. For example, lacking a clear analysis of K_t, the model does not provide an understanding of the creation of scientific knowledge but only a law of motion of acquired beliefs, rational or irrational. Further, since f is *a priori* undetermined, any form of creation of expectations leads to a change in the stock of knowledge. Lawson also seems to suggest that g is strictly monotone in $\mathrm{E}o_t$ - o_t: the more significant the mistake in expectations, the higher the creation of knowledge.

Be that as it may, if one accepts Lawson's epistemological approach, then his model of knowledge accumulation can be formalised, and a number of properties of the theory can be investigated by analysing the properties of the postulated functional relations. And this conclusion is independent of any assumptions on measurability: the fact that some of the variables in the equation of notion of the stock of knowledge are difficult, or even impossible to measure, does not make Lawson's analysis any less 'deductivist', nor does it make formal methods inadequate (see, for example, Katzner, 2009).

6. Reorienting economics?

The previous analysis raises serious doubts on the SMT: even granting Lawson's broad account of social ontology, formal models can play an important role in economics, and in the social sciences, as heuristics and in the analysis of theoretical constructions. The SMT provides doubtful foundations for a strategy aimed at reorienting economics. Can the *weak* methodological thesis (henceforth, WMT) provide more satisfactory foundations for reorienting economics?

As noted above, the WMT is more persuasive, and a critical reconsideration of the use of formal models in economics is long overdue. It is unclear, however, that the WMT can provide clear and persuasive guidance for reorienting economics. An analysis pitched only at the most abstract ontological and methodological level, focusing generically on mathematics (rather than on specific models and theorisations) and explicitly refraining from any consideration of substantive concepts and propositions, is unlikely to lead far.

Consider Lawson's definition of the orthodox vs. heterodox divide, which is (at least in principle) based on the WMT. 'Orthodoxy' is characterised by the *insistence* on the use of mathematics, and 'heterodoxy' as the negation of the claim that "forms of mathematical deductive method should everywhere be utilised" (Lawson, 2006, p.492). Reorienting economics can then be interpreted as the substitution of a more pluralistic heterodox attitude for the orthodox one.

It is almost uncontroversial that most economists place an excessive emphasis on mathematics; that this methodological bias is not neutral in terms of substantive conclusions; and that formal models are not necessarily the most fruitful way of understanding every social phenomenon (see Elster, 2009). Yet Lawson's definition does not help to understand the current status of the discipline and its internal dynamic, and it is not particularly useful for reorienting it.

First, taken literally, the criterion for orthodoxy is overly restrictive. Despite the (clear) mathematising inclination of most economists, few of them would actually insist that the use of mathematics should be 'universalised', i.e. it should be literally used "always and everywhere" (Lawson, 2006, p.492).

Second, the emphasis on 'the insistence in the use of mathematics' shifts the focus of analysis on to the *subjective* dispositions and inclinations of economists. Thus, the concept of 'mainstream' is hardly an *analytical* category, because orthodox becomes synonymous with (methodologically) 'closed-minded'. Further, this approach characterises *economists*, but not orthodox and heterodox *economics*, nor even economists' *work*. Therefore, lacking some evidence of the subjective attitude towards mathematics in general (e.g., methodological meditations), it is impossible to tell whether an economist (and his/her work) is heterodox or orthodox. Thus there is potentially a very large set of economists who are impossible (and not just difficult) to 'classify' in a non arbitrary way.

Indeed, it is impossible *a priori* to tell whether an article, or the entire lifetime contribution of an economist, or even a journal is orthodox, by simply analysing its content. Even the fact that all of the contributions (by the economist or in the journal considered) focus on formal models says nothing about the *insistence* on mathematics, and provides at most *prima facie* evidence of a methodological stance. After all, as Lawson repeatedly notes, not all uses of mathematics are *a priori* illegitimate, so that a methodologically open-minded economist may still be using mathematics in a specific setting where formal methods are appropriate.

In sum, if Lawson's subjectivist interpretation of the WMT and of the orthodox/heterodox divide is taken at face value, then it does not provide robust foundations for reorienting economics. Indeed, it is unclear that the replacement of the current orthodoxy would lead to *any* substantive change in the practices of economists beyond a generically more pluralistic (subjective) attitude. Economists may become more open-minded, but their work and practices may remain substantially unaltered.

To be sure, one might argue that the orthodox persistent *use* of formalisations represents sufficient evidence of their *insistence* on the use of mathematics. This interpretation makes Lawson's approach less vague, but it remains unclear that it can provide the foundations for reorienting economics. For the purely methodological orientation of Lawson's approach does not allow a proper understanding of the current status of economics and the role of mathematics in it. Consider the examples of economic theorising analysed in section 3 above. For Lawson, all the contributions to the analysis of linear economies, from von Neumann to Sraffa and Morishima would be mainstream economics because of the methodology employed. Not only is this counterintuitive; it strongly suggests that there is something wrong with Lawson's definition.

More importantly, it is worth stressing again that all of the results 1-10 above have been derived *within* mainstream economics. What is interesting about mainstream economics is not that these results are controversial. They are not. They are well-established. *It is rather that, as results of mainstream economics, they are routinely ignored by mainstream economics.* This suggests that Lawson's definition of mainstream economics in terms of its commitment to deductive formalism is inadequate because it does not engage with its substantive content. Reorienting economics requires an appropriate definition of the mainstream, and this in turn requires an analysis of substantive issues. Although space constraints preclude a proper treatment of this issue, the next, and concluding, section offers some preliminary remarks that represent a first step in this direction.

7. What is mainstream economics?

The 1870s turn in economics is often and with reason termed the 'marginalist revolution'. Marginalism is a feature of optimisation (at least under conditions of continuity), and so it is a small step to assert that the defining feature of mainstream economics is the realisation of that revolution (via Samuelson's 1947 *Foundations of Economic Analysis*), and this is what Lawson does. The problem with this, as we have repeatedly hinted above, is that the focus on method is unhelpful because it does not in fact distinguish orthodoxy from heterodoxy. To do that requires some focus on content. Nevertheless, the 'marginalist revolution' is indeed a good starting point, not however because of its marginalism, but because of its 'subjectivism' (it is also occasionally known as the 'subjectivist revolution').

The marginalist revolution focused attention on exchange rather than production, and hence concentrated on the market in a self-consciously different way from for example Marx. Rather than seeing competition as a war to the death, competition became the means of attaining social harmony as individuals maximised their utility (constrained by income and given prices). Utility functions therefore took centre-stage, and the preferences they expressed were assumed exogenous to any explanation of social phenomena. This in turn rendered individuals asocial, collapsing the social into a collection of individuals none of whom could affect the preferences of any other.

This remains a defining feature of the mainstream: individual preferences are exogenous, and attempts to endogenise preferences are destructive to the whole project. Indeed, the exogeneity of preferences in large part defines the ontology of mainstream economics. For the ontology of the mainstream is that it is a characteristic of human nature to be acquisitive, but scarcity is pervasive because wants are unlimited relative to the resources available to satisfy them; hence choices must be made, and economics is the study of these choices and their consequences. Typically (although not essentially), these choices involve rational agents optimising, and typically (although not essentially) analysis concentrates on the compatibility of choices through the study of equilibria.

But this ontology emerges from a particular contingent characteristic of capitalist societies. For the incessant expansion of capital requires its realization in money form, which requires continual output growth in the form of commodities that can, indeed must, be sold. This in turn requires ever-increasing demand, which in turn requires acquisitive consumers. So mainstream ontology universalizes a behavioural specificity of contemporary society, and then treats the present as a particular realization of that universal. It is not then surprising that detailed historical analysis of economic behaviour is rare in the mainstream, for it is quite unnecessary. If all empirical instances, no matter how historically different, are treated as hypostatizations of the same universal, the social disappears. Wage-labour becomes labour; capital becomes the instruments of production; the labour process becomes a production function; acquisitive behaviour becomes human nature, and so on. On this basis, axioms are postulated and theory is developed through a succession of models. But the problem is not any insistence on deductive formalism. It is rather a content which hypostasises the present as an instance of the universal, and this in turn is a consequence of a methodology of individualism with a content of exogenous preferences. Anything that threatens this structure, even if it is a consequence of modelling within this structure (such as results 1-10 above), is routinely ignored.

It is not then surprising that individual endowments are taken as exogenous too. For they are obviously historically determined, and when history has been so comprehensively excised from the mainstream, there is little that can be said. For the same reason, technology is generally taken as exogenous. Sawn-off versions of the mainstream commonly use production functions, collections of *pre-existing* blueprints among which profit-maximising firms choose on the basis of given prices. Technical change (manna from heaven) then shifts production functions, measured as total factor productivity (the 'dark matter' of economics). These concepts have repeatedly been shown to be deeply problematic, but to no avail. More general versions of the mainstream eschew production functions in favour of convex production sets, but the generality achieved (and the exclusion of all but the most limited increasing returns to scale) has no purchase on applied analysis.

The mainstream is not then defined by its pervasive use of deductivist modelling. It is defined by a methodological individualism in which preferences, endowments and technology are treated as exogenous. It is quite true that this is associated with a focus on deductivist modelling. But deductivist modelling is a consequence of the original stance, which elides the social and the historical through its treatment of individuals and their preferences as concrete expressions of abstract universals. It is not that the atomism is a functional consequence of an insistence on the use of mathematical deductive methods; rather the converse is the case. And this requires a notion of history and historical specificity that is absent from Lawson's methodological approach.

Heterodoxy then broadly encompasses all of those analyses that see the social as more than a collection of abstract individuals with exogenous preferences, and that see the present as the outcome of history. It also encompasses those analyses that are subversive of the orthodoxy through immanent critique. Whether any, some or all of these analyses use formal deductive methods is not germane. Rather, the important question is what formal models can be used to analyse which social phenomena.

REFERENCES

Dow, S.C. 2004. *Reorienting Economics*: Some epistemological issues, *Journal of Economic Methodology*, vol. 11, 307-12.

Elster, J. 2009. Excessive ambitions, *Capitalism and Society*, vol. 4, 1-30.

Hodgson, G. M. 2004. On the problem of formalism in economics, *Post-Autistic Economics Review*, vol. 28, 3-11.

Katzner, D. W. 2009 [2nd edition]. *Analysis without Measurement*, Cambridge: Cambridge University Press.

Lawson, T. 1997. *Economics and Reality*, London: Routledge

Lawson, T. 2003. *Reorienting Economics*, London, Routledge.

Lawson, T. 2004. On heterodox economics, themata and the use of mathematics in economics, *Journal of Economic Methodology*, vol. 11, 329-40.

Lawson, T. 2006. The nature of heterodox economics, *Cambridge Journal of Economics*, vol. 30, 483-505.

Lawson, T. 2009a. On the Nature and Role of Formalism in Economics, in E. Fullbrook (ed) *Ontology and Economics: Tony Lawson and his critics*, London: Routledge.

Lawson, T. 2009b. The current economic crisis: its nature and the course of academic economics, *Cambridge Journal of Economics*, vol. 33, 759-77.

Roemer, J.E. 1981. *Analytical Foundations of Marxian Economic Theory*, Cambridge, Harvard University Press.

Schelling, T. 1969. Models of Segregation, *American Economic Review* vol.59, 488-93.

Sen, A.K. 1999. The possibility of social choice, *American Economic Review*, vol.89, 349-378.

Vercelli, A. 1991. *Methodological foundations of macroeconomics: Keynes and Lucas*, Cambridge, Cambridge University Press.

Wright, E.O. 1989. What is Analytical Marxism?, *Socialist Review*, vol.19, 35-56.

---

[1] A similar conclusion holds if *stochastic* closures (Lawson, 1997, p.76) are considered, whereby the relevant regularity connects a set of random, rather than deterministic, variables.

[2] This is ambiguity is especially clear in Lawson (2009b), where he first insists that the fundamental problem of modern economics is its *insistence* on mathematical deductivist modelling; then, second, asserts that he is *not* saying that such modelling can never provide insight; and then third, states, "mathematical deductive methods are … of little assistance in the analysis of most social phenomena" (Lawson, 2009b, p.763).

[3] See, for example, the analysis of probabilistic causality in Vercelli (1991).

[4] Actually, the assumption that the state of things is unknown, and the overall description of the process of accumulation of knowledge, imply that contrast explanations provide at best necessary conditions for *scientific* explanations: that the starting point is a *rational* set of beliefs and knowledge, and that the outcome of the revision of established beliefs will be a *rational* causal explanation is simply assumed. The process outlined by Lawson describes also a set of customary attitudes (e.g. in the management of crops) that is changed for another set of customary attitudes that by chance have proved to provide better yields on average. From a strictly logical viewpoint, the study of a causal mechanism is redundant in this process.

[5] Lawson may argue that the law of knowledge accumulation does not reflect the anti-positivistic essence of his method, which rejects 'accounts wherein knowledge is the accumulation of incorrigible facts' (RE, p.101). But the interpretation of the stock of knowledge (and its variation) in the model is open, and nothing requires that K_t be given a positivistic interpretation.