

Introduction to

## ***Ontology and Economics: Tony Lawson and his***

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### Lawson's Reorientation

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Tony Lawson has become a major figure of intellectual controversy on the back of juxtaposing two relatively simple and seemingly innocuous ideas. In two books and over fifty papers he has argued:

1. that success in science depends on finding and using methods, including modes of reasoning, appropriate to the nature of the phenomena being studied, and
2. that there are important differences between the nature of the objects of study of natural sciences and those of social science.

Taken together, these two ideas lead to the conclusion that the methods found to be successful in natural sciences are generally not the ones that should be used in social science.

By relentlessly focusing on this pair of ideas, Lawson has in a short space of time changed one of economics' key conversations. His chapter, "A Realist Theory for Economics", published in Roger Backhouse's 1994 landmark collection *New Directions in Economics Methodology*, stands out like someone standing alone at a party. As recently as then the ideas of three thinkers, none of them economists, none social scientists and all of them dead, dominated economics' literature on methodology. The index of Backhouse's wonderful book powerfully illustrates this. It lists 47 pages that refer to Thomas Kuhn, 69 to Karl Popper and 73 to Imre Lakatos. Twelve of the book's sixteen chapters (excluding Lawson's) refer to one or more of the three and eight, as well as the back cover, to all three. Lawson does not refer to any of them. More significant, Lawson's key reference point is *ontology*, a word that, except in the Introduction when Backhouse is introducing his collection's odd man out, appears in none of the other chapters. Notably, when Lawson first uses "ontology" he feels it necessary, despite his highly specialized audience, to explain what the word means: "enquiry into the nature of *being*, of what exists, including the nature of the objects of study." [Lawson 1994, p. 257]

Thirteen years later and anyone in economics who knows anything about methodology knows what "ontology" means. They also have come to realize that if Lawson's basic conclusion were applied it would entail a programme of reform that would fundamentally change economics. A quick check with Google shows just how phenomenally successful Lawson has been at changing the conversation. Below are listed the number of web pages turned up for four trios of words. [30/03/07]

Popper, economics, methodology	300,000
Kuhn, economics, methodology	391,000
Lakatos, economics, methodology	82,300

Lawson, economics, methodology	264,000
ontology, economics, methodology	1,050,000

To appreciate the significance of the huge debate begun by Lawson, we need to look at its historical background.

### **Physics, economics and the philosophy of science**

For those of you too young to remember, philosophy of science took off in a big way in the 1960s. Not for the first time, philosophy struggled to update its teachings to make them consistent with developments in science. Traditionally philosophers told the story, and the educated classes repeated it, that science, especially physics, progressed on the basis of the application of theories empirically proven true beyond question. But the first half of the 20<sup>th</sup> century witnessed two “revolutions” in physics that made a mockery of that narrative. Physicists came to accept the theory of relativity and then quantum theory, both of which contradicted in fundamental ways Newton’s theory, the most empirically confirmed theory in the history of science.

In an ideal world epistemologists would have jumped at this chance to develop new ideas. But even after the solar eclipse of 1919, which disproved Newton and confirmed Einstein, philosophers of science, under the banner of “logical positivism”, persisted in telling the same old story. It was not until late 1934 that Popper published, in its original German, *The Logic of Scientific Discovery*, a book that ventured to rewrite epistemology in line with the no longer so recent events in physics. But two more decades passed before Popper and other innovators succeeded in forcing themselves past the gate keepers of the philosophical establishment. When resistance to the need for new ideas about how science succeeds and fails finally crumbled, a half-century of repressed questions shot to the surface. In consequence, the decades that followed rank among the most productive and interesting in modern philosophy.

Inevitably, economists joined the fun. So too did other social scientists, but for economists there was a special and virtually irresistible attraction, especially to the Popper-Kuhn-Lakatos triad. From the mid 19<sup>th</sup> century onward economics has fancied itself as methodologically akin to physics. Therefore, almost inevitably economists saw the physics-related revolution in the philosophy of science as relevant to economics as well. Meanwhile the identification of economics with physics in the economist’s mind had become so strong that it almost completely obscured *the most fundamental difference* between the practice of physics (and indeed of all the natural sciences) and the practice of economics. Whereas physics invents and chooses its methods on the basis of the nature of the phenomena that it studies, economics does not. Let me explain.

### **1843 to today**

John Stuart Mill not only turned economics primary concerns away from production and distribution to those of value, he also made the case that economics, and the social

sciences in general, should ape the methodology of astronomy and physics. In *System of Logic* Mill appealed to Newton and in particular to a “law of nature” that

is called, in dynamics, the principle of the Composition of Forces: and in imitation of that well-chosen expression, I shall give the name of the Composition of Causes to the principle which is exemplified in all cases in which the joint effect of several causes is identical with the sum of their separate effects. [1843, Book III, Ch. VI, sec. 1]

Mill then cautions that “This principle, however, by no means prevails in all departments of the field of nature.” [1843, Book III, Ch. VI, sec. 1] But later in the book when considering the social sciences, without supporting argument, Mill divinely declares: “In social phenomena the Composition of Causes is the universal law”. [System of Logic, Book 6, chapter VI section 1] He has previously identified this linear relation between causes as what enables the application of the deductive method. [Book III, Chapter XI, Section 1] So in this *a priori* and pre-emptive way Mill declared that what he understood to be the method of Newtonian physics was the only proper one for economics.

Within a couple of decades major economists had got the message. Jevons and Walras certainly had when in the 1870s they set about inventing neoclassical economics. In the preface to his *The Theory of Political Economy* (1871) Jevons wrote:

But as all the physical sciences have their basis more or less obviously in the general principles of mechanics, so all branches and divisions of economic science must be pervaded by certain general principles. It is to the investigation of such principles – to the tracing out of *the mechanics of self-interest and utility*, that this essay has been devoted. The establishment of such a theory is a necessary preliminary to any definite drafting of the superstructure of the aggregate science. [emphasis added] [Jevons 1970, p. 50]

Walras began and proceeded in the same vain in his *Elements of Pure Economics*. (1874-77) Alluding to the role of force and velocity in mechanics, he says: “Similarly, . . . this pure theory of economics is a science which resembles the physico-mathematical sciences in every respect.” [Walras 1984, p. 71]

Walras does not have just any mathematics in mind, but rather that of classical mechanics. Like Mill, Walras, beyond some rhetorical flourishes, offers no argument in support of the presumed isomorphism between the mechanical and economic realms. What matters to Mill, Jevons and Walras is not the methodological fit but rather the method itself, *the method used in their day by physics*. Adopting this approach to methodology means that instead of being led by ontological enquiry, one defines *a priori* the ontology to fit the method. Nothing could be more against the procedures and mindset that have dominated the natural sciences from Copernicus on. In applying a system of analysis, mathematical or otherwise, to an empirical domain, the key question for the real scientist is always whether or not the structures described by the former are isomorphic to those found in the latter. For the scientist, although not for the mathematician, the mathematics is supposed to illuminate empirical reality rather than the other way around. This means that *ultimately* the choice of method, like the question of whether or not Mill’s Composition of Causes pertains to a particular domain, is a question of ontology. In real science an ontology, however imperfect, decides the method, not the opposite. The birth of classical mechanics is a paragon case. Rather than pretend that the mechanical universe had properties isomorphic to an existing

math, Newton invented one, calculus, which did. Instead of bending his ontology to fit the mathematics, he created mathematics, a method, to fit his ontology. A similar sequence of events has characterized the development of 20<sup>th</sup> century physics, especially the theory of general relativity. In the twentieth century the natural sciences, not just physics but also biology, underwent radical and more or less continuous ontological revision. The elementary entities and fundamental properties that populate the minds of physicists today are light-years removed from those of Newton's time or even of Maxwell's.

The 20th century, especially its second half, witnessed a gradual intensification of economics' obsession with dressing up in the methodological clothes of physics. Some economists, so carried away by their masquerade, even developed a taste for pretending that their achievements merited comparison with those of the great names of physics. The science historian Yves Gingras [2002] has described one such case:

Paul Samuelson (1970 winner) wrote about his 'Nobel coronation' – not his 'Bank of Sweden Coronation' – and filled his talk with references to Einstein (4 times) Bohr (2 times) and eight other winners of the (real) physics Nobel prize (not to mention, of course, Newton) plus a few other names as if he were part of this family.

But some more recent winners of the Swedish prize have not, at least with hindsight, been so taken in. Milton Friedman [1999, p. 137] has acknowledged that "economics has become increasingly an arcane branch of mathematics rather than dealing with real economic problems", and similarly Ronald Coase [1999, p. 2] has written "Existing economics is a theoretical [meaning mathematical] system which floats in the air and which bears little relation to what happens in the real world". Method counts for virtually everything, substance for little or nothing, and disconnection from "real economic problems" and "the real world" is general in scope. In the typical research seminar, observes Bruce Caldwell in this volume, "No claims are ever defended with anything like the vigor with which one defends one's choice of econometric techniques." [p. 16]

## **Ontologies**

By unveiling the mainstream's ontology entailed by its methodology and by calling attention to economics' scientism, Lawson seeks to win the minds of the young and thereby bring about a reversal of the discipline's traditional order of priority between method and substance. Above all Lawson's project is one of persuading economists to do as physicists have always done: to take cognizance as best they can of the basic characteristics of their domain of inquiry and then proceed to develop and choose their methods accordingly.

Lawson builds his prescriptive analysis on the ontological platform of the social-philosophical school of thought called Critical Realism. This movement, a predominately Anglo-American affair, can through Continental eyes appear rather hackneyed. Lawson lists five key properties which, "according to the (philosophical) ontological account" that underwrites his project, social phenomena possess. [Reply to Davidsen, 15]

1. They are produced in *open systems*.
2. They possess *emergent* powers or properties.
3. They are *structured*.
4. They are *internally-related*.
5. They are *processual*.

These core ontological ideas of Lawson's project include nothing that at the time of Critical Realism's inception in the 1970s was not already part of the woodwork of Continental philosophy and social theory. One example well illustrates the case. In Simone de Beauvoir's *The Second Sex* (1949), one of the last century's most influential books, the concept of gender and the ontological framework that supports it incorporate all five of the properties of social phenomena that Lawson embraces.

1. *open systems*:  
"humanity is something more than a mere species: it is a historical development;" [Beauvoir, p. 725]
2. *emergent*:  
"Woman is not a completed reality, but rather a becoming," [p. 66]  
"One is not born, but rather becomes, a woman." [p. 295]
3. *structured*:  
"For us woman is defined as a human being in quest of values in a world of values, a world of which it is indispensable to know the economic and social structure. We shall study woman in existential perspective with due regard to her total situation." [p. 83]
4. *internally-related*:  
"Otherness is a fundamental category of human thought." [p. 17]  
"The Other is posed as such by the One in defining himself as the One." [p. 18]
5. *processual*:  
"An existent is nothing other than what he does;" [p. 287]

And of course above all Beauvoir was an existentialist so that, in Lawson's words, "there is no one-to-one mapping from social structure to individual pathways, experience or personal identities [p. 65, this volume]," and in Beauvoir's words, "she acquires this consciousness under circumstance dependent upon the society of which she is a member. . . . But a life is a relation to the world, and the individual defines himself by making his own choices through the world about him." SS, 80-1]<sup>1</sup>

Pointing out the historical pedigree of Lawson's core ontological ideas is not a criticism but, on the contrary, an endorsement. It is the unoriginality that so suits Critical Realism for the task of critiquing mainstream economics. The legitimacy and fecundity of the ontological ideas that it pushes are so well-tested and so widely embraced outside of economics that it makes an ideal replacement for the ontology implicitly assumed by mainstream formalist methods. To my knowledge no one of repute in economics has dared to come forward to argue, against Lawson, that the economy is a closed system, that it is not characterized by the property of emergence, that it is not structured, that in its internal relations do not play a pivotal role and that it does not consist of an inter-related series of unending processes. Only a fool would publicly take up these arguments. And most economists, but

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<sup>1</sup> For more on Beauvoir's ontology see Edward Fullbrook and Kate Fullbrook, *Simone de Beauvoir: A Critical Introduction*, Cambridge, UK: Polity Press, 1998, or Edward Fullbrook and Margaret A Simons, "Simone de Beauvoir and Jean-Paul Sartre" *Gendering Western Philosophy: Pairs of Men and Women Philosophies from the 4<sup>th</sup> century B.C.E. to the Present*, New York: Rowman and Littlefield, 2008, or "Chapter 14: Gender and Ethics" and "Chapter 15: The Second Sex" in Edward Fullbrook and Kate Fullbrook, *Sex and Philosophy*, London: Continuum, 2008, or the many papers in "Première Partie: La Philosophie du Deuxième Sexe" in *Cinquatenaire du Deuxième Sexe*, editors Christine Delphy, Sylvie Chaperon, Kate Fullbrook and Edward Fullbrook, Paris: Éditions Syllepse, 2002, pp. 19-190.

not all, are also too sensible to suggest that economics should not take cognizance of the fundamental properties of its object of enquiry. In consequence, defenders of the status-quo find it difficult to frontally attack Lawson's ideas. They tend to settle instead for indirect approaches. Easiest and in the short run probably strategically the wisest is just to ignore him. Another has been to hurl personal abuse at him, as in Herbert Gintis's amazon.com review of *Reorienting Economics*. Another and increasingly common tactic has been to misrepresent the current situation in economics. There can be a big payoff for this approach when addressing a non-economist public, including economics students, or when addressing oneself in bad-faith. Out of the tens of thousands of papers published in mainstream economics journals over the past half century, one can easily find some, which having slipped past the gate keepers, embody one or more of the five properties. Wave these papers about vigorously enough and some people will be convinced that economics is already as Lawson would like it to be. Alternatively, one can misrepresent the formal properties of various methodologies, as when it is suggested that standard game theory describes an open system.

### **Thirteen years on**

Thirteen years on, Backhouse's collection belongs not just to another century but also to a different era. Although many economists, especially older ones, still entertain kissing-cousin fantasies about their relation to physicists, inhibitions have developed about acting them out in public. It is hard to imagine anyone accepting the Swedish prize today behaving as Samuelson did. Among methodologists the shift has been especially pronounced and quick. The majority may still in their heart of hearts prefer to view economic method through the physical science prism. But in the main they have, even if begrudgingly, taken on board the fact that any methodological commitment is also an ontological one. Questions concerning the fundamental nature of economic phenomena are not yet basic to the practice of economics, as the corresponding questions are in physics, but neither are they still treated as totally beneath attention. Today nearly all methodologists are either conversing with Lawson or heckling him from the edges of the room.

Many people, including all of the contributors to this collection (several in particular), have played a part in bringing about this shift, this *new new* direction in economic methodology. But more than anyone, I believe, Tony Lawson deserves credit for the swing away from judging method in economics as an end in itself to judging it as a means to substantive knowledge and hence its ontological fit. It will be a long struggle to reverse the wrong turn that Mill made for economics in 1843. But Lawson's *Economics and Reality* in 1997 and *Reorienting Economics* in 2003 together with his many papers have provided the growing number of reformists in the profession with a formidable and expandable arsenal, and with the likes of which dissenters have not previously been armed.

### **Lawson's Critics**

Over a period of 18 months I commissioned for the *post-autistic economics review* the ten critical essays around which this volume is formed. I chose the critics partly on the basis of the particular approach I anticipated that they would take to Lawson's work and partly

because in each case I held their critical powers in special regard. None of them disappointed me. Very briefly I will run through the arguments of the critics, whose essays have been ordered alphabetically.

**Bruce Caldwell** declares his “substantial agreement with Lawson’s fundamental complaint that the economics profession is dominated by a mainstream orthodoxy” [1] that is unhealthy because of its methodological approach. He also finds attractive Lawson’s description of structured social reality. But unlike Lawson, Caldwell retains a strong faith in traditional “basic economic reasoning” as “a powerful tool” that enables us to understand the world, improve our decisions and order human behaviour. [4] He cautions us not to “worry about establishing causes” [4] in lieu of using the tools we already have, and would like to see research into “why such reasoning works”. [4]

**Bjørn-Ivar Davidsen** argues that the social ontology upon which the critical realism project in economics bases itself lacks “epistemological credibility beyond a reasonable amount of doubt.” [8] Consequently, he sees it as “ill advised” to rely on critical realism in its present form as the basis for critiquing and reforming “scientific practices” in economics. Davidsen calls instead for a critical realist project that would develop “domain specific ontological theories” and then apply them to “scientific work directed toward analysis of substantive economic questions and issues.” [9] Critical realism would then be judged by its success in offering improved accounts of old and new economic topics. If successful, the epistemological status of the critical realist ontology would be enhanced and acceptance from mainstream economics might follow.

**John B. Davis** believes that today heterodox economists have a choice between two strategies for reforming economics. They can hope for a “big scientific revolution” or they can gradually chip away at the mainstream core. Lawson’s view of heterodoxy, says Davis, conceals this choice. He sets about establishing its existence by inventing and applying a classification system to economics. This includes three principles shared by heterodox economic approaches, and that “draw the dividing line between orthodox and heterodox economics circa 1980” [p. 6], and four ways by which an approach could become heterodox or vice-a-versa. Davis’s argument also grows out of his recognition of promising new research programs in economics and their characteristics.

**Paul Downward** and **Andrew Mearman**, while generally backing Lawson’s analysis, argue that there needs to be more emphasis on practical methodology for guiding research projects informed by critical realism. To this end, they advocate a principle that they call *triangulation*, a “commitment in research design to investigation and inference via multiple methods which are not placed in any *a priori* hierarchy.” [2] They argue that this approach makes operational Lawson’s principle of *retroduction*, promotes pluralism, co-operation with other social sciences and leaves the door open to quantitative methods that otherwise would be excluded. In this way they see triangulation as a means for realizing Lawson’s project of transforming economics.

Like Lawson, **Bernard Guerrien** was a mathematician before turning to economics. Unlike Lawson, he identifies the type of social structure, not the type of economic agent, implicitly assumed in the models of modern economics as what makes them so irrelevant. When they assume that households and firms are price-takers, they describe not a market but

a centralized economy. When they reduce the whole economy to the choice of a “representative” agent, they are indulging in blatant nonsense. Guerrien argues that the real reason why intelligent people can propose and endlessly study “such *stupid* models” is ideology and that to overcome it ontological debates are no or little help.

**Geoffrey M. Hodgson** agrees with Lawson that modern economics’ malaise stems from “the victory of technique over substance,” and its dogmatic insistence on the use of formalism. [1] But he largely rejects Lawson’s critique of formalism and, more significantly, accuses him of a dogmatism of his own. Hodgson makes the case that Lawson’s criterion of local closure for the use of mathematics together with his critical realist ontology, which rules out virtually all such closures, in effect denies almost all possibility for legitimate use of mathematics in economics. Alternatively, Hodgson rejects strict local closure as a criterion for the use of formal modelling, citing biology in support. He then explores two types of situation in economics, heuristics and internal critiques, where applications of formalism, including “using closed models to help understand an open reality”, have proved useful.

**Bruce R. McFarling** makes the case that epistemology, not ontology, should be given the “starring role” when it comes to reorienting economics. Ontological choices, he notes, ought to be founded on epistemology. His argument centres, however, on the mainstream mode of explanation, which he identifies as the root cause “of why sixty years of determined empirical testing has left the mainstream project stalled.” [p. 3] The failure stems from the method’s unit of analysis, the problem solving isolated individual, which renders this approach “blind to important aspects of the economy”. [p. 3] Researchers, wedded to the method, systematically ignore all those features of the economy incompatible with the standard unit of analysis. Degenerately, the method’s failure perpetuates its use. Researchers, instead of reconsidering their methodology, reapply it but with a different selection of variables and parameters, hoping that at last success will come.

**David Ruccio** applauds Lawson’s efforts to make economists self-conscious about the conceptual schemes and ontological presuppositions of contemporary economic discourse. But he objects to what he sees as Lawson’s attempt to have the critical realist ontology adopted as “the singular reality appropriate for economic science“. [p. 6] *the* conception of reality. Ruccio points to the existence of other ontologies, especially Marxism and post-modernism, which have proven useful, both in their own right and as critiques of mainstream economics. He elaborates on the contributions that have come through the application of these ontologies and which emanate from their particular characteristics. He concludes by withholding support for “the project of finding or producing a single ontology that will serve as the shared foundation of the various schools of thought that have come together in the post-autistic economics movement.” [p. 8]

**Irene van Staveren** identifies Lawson as a strong supporter of the feminist cause in economics. Nonetheless, she levels three criticisms regarding feminism against him. In his encouraging feminists to study gender as an ontological category, she sees him as advancing a universalist and essentialist “claim about the nature of human beings, a claim against which the whole project of feminism is set up.” [p. 2] Straveren then makes the case that Lawson’s rejection of formalistic modelling can work against the aims of feminist economics. Feminists cannot afford to ignore either theoretical or empirical modelling, regardless of their ontological legitimacy, because they influence the way people think of society. She considers the



example of modelling work on unpaid labour and the care economy, where the modeller is faced with the choice between constructing a model that permits changing gender relations and one that does not. Finally, she criticizes Lawson for failing to make the learning relations between critical realism and feminism run in both directions.

**Jack Vromen** takes strong exception to what he characterizes as Lawson's "presumption that adherence to a mathematical-deductivist style of modelling imposes a 'flat', non-layered empiricist ontology." [p. 1] He also argues, against Lawson, that mainstream economists believe both in underlying mechanisms, although different ones, and that a satisfactory economic theory should identify them. But unlike Lawson, mainstream economists do not think that it is necessary to model them. Vromen explains why. He then sets out an argument against using ontology as "a final arbiter in assessing economic theories," [p. 3] especially the presumption that there "are many uncontested generalised observations about social reality." [p. 4] He concludes that ontological considerations should serve as "heuristic principles" for developing new economic theory.

As a year passed and these critical essays accumulated I came to fear their combined effect – that perhaps I was doing Lawson a disservice. This fear grew when he declined to respond to any of his critics until the series was finished. Then a further silence followed, as he insisted upon writing all ten of his replies before revealing any of them.

Finally, his replies arrived on my desk. The week that followed, with its close back-to-back reading of the critiques and Lawson's replies, proved one of the most satisfying of my professional life. This is a collection of fine minds, stretching to near their limits, interacting with each other and being changed by the process. I was changed by reading it. I hope you will be too.

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