Ontology and *Economics*

TONY LAWSON & HIS CRITICS

EDITED BY
EDWARD FULLBROOK

Ontology and Economics

In the social sciences 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 many papers he has argued first that success in science depends on finding and using methods, including modes of reasoning, appropriate to the nature of the phenomena being studied, and also that there are important differences between the nature of the objects of study of natural sciences and those of social science.

This original book brings together some of the world's leading critics of economics orthodoxy to debate Lawson's contribution to the economics literature. The debate centres on *ontology*, which means enquiry into the nature of what exists, and in this collection scholars such as Bruce Caldwell, John B. Davis and Geoffrey M. Hodgson present their thoughtful criticisms of Lawson's work while Lawson himself presents his reactions. Of course many social scientists disagree with him, but Lawson's arguments are so powerful that few economists now feel that his case can be ignored. Bringing Lawson head-to-head with eleven of his most capable critics, this is a book of intellectual drama. More than that, it is a collection of fine minds interacting with each other and being changed by the process.

This book is particularly useful for students and researchers concerned primarily with methodology and future development of economics. It is also relevant to the concerns of philosophers of science and to all social scientists interested in methodological issues.

Edward Fullbrook is the founder and editor of the *Real-World Economics Review* and a research fellow in the School of Economics at the University of the West of England.

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Edited by Edward Fullbrook

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Ontology and Economics

Tony Lawson and his critics

Edited by Edward Fullbrook

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Foreword

As a researcher, I cannot think of anything more gratifying than to have a group of respected scholars engage with one's own research output. I am enormously appreciative of the group of individuals that have taken the time to produce the critical assessments found in the following pages.

I am especially pleased in that whilst the analyses provided each picks out fundamental issues in my writing, collectively the commentaries span a wide range of its aspects.

As is usual in academic debate, each reviewer mostly concentrates on our differences; and so shall I. But I think it is worth recording at the outset that the scope of agreement between myself and each of the individual contributors appears substantial.

In fact, we will see that the common ground is actually somewhat larger than a few of the contributors at times appreciate. Where this has not been recognised, the reason, much of the time no doubt, is that I have been insufficiently clear. I thus welcome this opportunity to clarify relevant features of my various positions, although in the pages that follow I do further develop certain aspects of my arguments as well.

A process of clarification and development, of course, can take a good deal of space, whilst the expressing of a disagreement can be brief. Whereas the critics are mostly on the offensive, my role as respondent, and indeed defendant, means that I need to do most of the explaining, elaborating and justifying. For this reason, amongst others, my responses, tend to be rather longer than the original comments.

My rejoinders have been produced in a somewhat disjointed fashion, at different points over a period of roughly a year, with their writing being fitted into gaps between carrying through numerous other tasks. Perhaps because of this (and my poor memory) I found, on eventually looking over the whole manuscript, that I do sometimes cover similar ground in different responses; more so anyway than I had anticipated. In consequence, I have subsequently trimmed some of the repetition and used cross-referencing as a substitute. But I resisted eradicating all overlap, not least because I suspect different readers will be drawn to specific interchanges and not to others. It can be a nuisance to switch chapters (and topics) to follow an argument of interest. So, I have left each response as reasonably self-contained.

As always, my debts are huge and mostly impossible to pin down. I am sure that very many people who have interacted with me in an intellectual way over the last twenty years or more have made a difference to my thinking, and affected the beliefs I currently hold and the ideas expressed in the following pages. I am certainly aware of the enormous influence of those who have attended the Cambridge Realist Workshop since its inception in 1990, especially its most regular participants. And I am particularly grateful to the Cambridge Social Ontology Group. I must especially single out those of its members, namely Ismael Al-Amoudi, Vinca Bigo, Philip Faulkner, John Latsis, Clive Lawson, Nuno Martins, Nitya Mohan and Stephen Pratten, who, having read first drafts of my responses, communicated their impressions, comments and criticisms at an all-day seminar sometime in early October 2006. This group, or combinations of the individuals involved, also met with me at subsequent shorter meetings, and in this way and others, continued to supply criticisms on revised drafts. My gratitude to this collection of people is obviously enormous.

Finally, I owe a very special debt of gratitude to Edward Fullbrook who both invited the various contributors to submit their critical comments to the *post-autistic economics review* and conceived the idea of collecting these comments together, along with my responses, for publication in the current volume. I am enormously flattered and grateful that anyone, let alone such an important figure in the movement for a more relevant economics, should consider such a project to be of value. I can only hope that he feels that the result renders his efforts worthwhile.

Tony Lawson
January 2007

Introduction

Lawson's reorientation

Edward Fullbrook

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 forty-seven pages that refer to Thomas Kuhn, sixty-nine to Karl Popper and seventy-three 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 March 2007):

- 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 twentieth 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-nineteenth 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 became 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, sect. 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" (1843, Book VI, Ch. VI, sect. 1). He has previously identified this linear relation between causes as what enables the application of the deductive method (Book III, Ch. XI, sect. 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 *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.
>
> (Jevons 1970, p. 50, emphasis added)

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 mathematics, 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 twentieth 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 twentieth 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, p. 71, this volume).

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 illustrates the case well. 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 that a mere species: it is a historical development" (Beauvior 1949, 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," (see Chapter 5, 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
>
> (1949, pp. 80–1)[1]

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 it internal relations do not play a pivotal role and that it does not consist of an interrelated series of unending processes. Only a fool would publicly take up these arguments. And most economists, but 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 when confronted with Lawson's ideas immediately find themselves in a tight corner. They don't have the option of frontally critiquing his ideas. They have to settle instead for a less attractive and less admirable approach. 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 than 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* 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 eighteen 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 chapters 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" (p. 13) 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 (p. 16). He cautions us not to "worry about establishing causes" (p. 18) in lieu of using the tools we already have, and would like to see research into "why such reasoning works" (p. 18).

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" (p. 48). 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" (p. 50). 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. 84), and four ways by which an approach could become heterodox or vice versa. Davis's argument also grows out of his recognition of promising new research programmes 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" (p. 131). They argue that this approach makes operational Lawson's principle of *retroduction*, promotes pluralism, cooperation 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 (p. 175). 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. 235). 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. 236). 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 F. 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. 269), *the* conception of reality. Ruccio points to the existence of other ontologies, especially Marxism and postmodernism, 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. 272).

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. 299). Staveren 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. 325). 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. 328), especially the presumption that there "are many uncontested generalised observations about social reality" (p. 329). 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 disfavour. 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.

Note

1 For more on Beauvoir's ontology see Edward Fullbrook and Kate Fullbrook, *A Critical Introduction*, Cambridge, UK: Polity Press, 1998, or Edward Fullbrook and Margaret A. Simons, "Simone de Beauvoir and Jean-Paul Sartre," in Karen J. Warren (ed.) *Gendering Western Philosophy: Pairs of Men and Women Philosophies from the 4th century B.C.E. to the Present*, New York: Rowman and Littlefield, 2008.

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1 Some comments on Lawson's *Reorienting Economics*

Same facts, different conclusions

Bruce Caldwell

I welcome the opportunity to reflect on Tony Lawson's *Reorienting Economics* (2003). Lawson covers a considerable amount of ground in his book, so my comments will of necessity be selective.

I will begin by stating that, for what it is worth, I am in substantial agreement with Lawson's fundamental complaint that the economics profession is dominated by a mainstream orthodoxy which is "not in too healthy a condition" due to its insistence on following a specific methodological approach, one that is not well matched to the social reality it wishes to investigate (p. 3). I make similar complaints in the final chapter of my book on Hayek (Caldwell 2004), and indeed I quote liberally from Lawson's earlier book (Lawson 1997) in that chapter. In this regard I consider Lawson a colleague who shares a quest, that of figuring out why economics turned out the way it did in the twentieth century. This quest has historical, methodological, ideological, sociological, and even pedagogical dimensions, and we are but two of many who have contributed to it (a selective sample might include Mäki 1999, Mirowski 1989, 2002, Weintraub 2002, and selected articles in Colander and Brenner 1992).

As an aside, I will add that Lawson's broad-brushed description of structured social reality is quite attractive. For those who have read Hayek, it is also familiar: many of the things that Lawson identifies as features of social reality were similarly identified by the Austrian social theorist. For example, that "human social activity is intelligible" (p. 33), that we follow social rules (pp. 36–38), that human actions are "intentional human doings, meaning doings in the performance of which reasons have functioned causally, where reasons are beliefs grounded in the practical interests of life" (p. 47), that many actions are based on tacit knowledge (ibid.), that humans form plans that are forward-looking (pp. 50–51), and that all human agency takes place within given social structures, but also produce changes in those structures (pp. 48–49), are all Hayekian themes.

That such claims appear in both Hayek and Lawson is perhaps not altogether surprising, for they are also recognizable in the writings of other heterodox economists, post Keynesians (at least of the Shacklian variety) for example. Lawson explicitly recognizes this in Chapter 7, where he suggests that different heterodox traditions share the broad-based description of social reality, and are to be distinguished from one another according to the different aspects of that reality upon which each chooses to focus (pp. 180–183). Given the richness of the complex reality before us, this too makes sense. It may also help to explain why (especially if one accepts the proposition that many issues that separate such groups are empirically undecidable, more on which in a moment) such groups inevitably persist. Some may agree with Lawson and me that pluralism makes good sense; the complex nature of social reality may also mean that it is inevitable.

In Chapter 4 Lawson recommends that economists reorient their discipline by resolving to seek causal explanations. He lays out an explanatory strategy for accomplishing this, which he breaks into three steps: identify event regularities, form causal hypotheses that can account for them, and then discriminate among the competing causal hypotheses that are consistent with the regularities (p. 81). Though he does not say so explicitly in his general formulation, it may be that Lawson is calling for more long run causal explanations here, or, put another way, for more economic history. Some of Lawson's examples (e.g., explanations of differential measured productivity growth rates, or of relative changes in primary versus produced goods prices over the last century) support this reading, as does Lawson's italicized statement at the end of the chapter that "*the explanatory process so facilitated is necessarily backward looking*" (p. 108).

If Lawson is advocating that economists do more economic history when he says that we should seek causal explanations, I have no quarrel, though as will be clear, I believe that there are other things that we can be doing as well. However, it may be that Lawson is calling for what might be termed *short run* causal explanations as well. In my opinion, seeking to produce valid *short run* causal explanations is an extremely ambitious goal, and in many instances an unreachable one. The complex nature of the open system that constitutes social reality, one that poses such problems for mainstream efforts at its analysis, will cause similar problems for any such program.

A homely example will illustrate the problem. I work in a largely empirical department of economics. Though the kind of research that I like to do is very different from theirs, I have come to admire and respect the carefulness with which my colleagues undertake their work. This is best revealed in departmental seminars, countless numbers of which I have attended (the high price of good departmental citizenship). Over the years certain features of a "typical" empirical seminar have emerged. A problem or puzzle is posed. Sometimes the problem arises from surprising relationships that have been discovered among the data (e.g., one colleague found that, during recessions, a number of variables associated with "better health" improved); other times it is an attempt to identify the impact of some policy change on some set of variables of interest (e.g., the impact of changes in the welfare laws on household and labor market variables of interest, or of the institution of charter schools on variables associated with educational outcomes). As the speaker goes through her presentation, typical questions arise. If the data set is a well-known and frequently used one, the speaker is asked about how she handled the equally well-known problems associated with it. If it is a new data set, there are questions about how the variables of interest were constructed, and whether their composition raises problems for the questions that the speaker seeks to answer. Usually they do. The peculiarities of the data dictate which subset of econometric methods should be used to correct for the problems. A good speaker knows the limitations of her data, and has chosen the subset of methods that hold the best chance of correcting for them. Speakers judged as ineffective are either unaware of problems or of the appropriate tools for correcting for them, or worse, both.

Sometimes the speaker draws policy conclusions from the study. This typically provokes animated discussion, for a number of reasons. First, the relations among the data are correlations. To move from there to policy conclusions, one must speculate about *causes*, and there are typically many plausible interpretations on offer. Next, all empirical economists recognize that adding new variables to an existing set of variables, or using new data sets that include different variables or which cover different time periods, or using different types of corrections, all typically yield different results, always in terms of the coefficients attached to various variables of interest, and sometimes in terms of their signs. The latter phenomenon is sufficiently ubiquitous that an economist who has studied them has given them a name: "emerging recalcitrant results." Robert Goldfarb draws the obvious inference about such findings:

> These emerging contrary results or "potential reversals" present a dilemma for the conscientious economist who is part of an empirical literature's audience. How is he or she to make believable inferences from such a literature, when results may have already been, or in the future be, challenged and even conceivably overturned?
>
> (1997, p. 222)

The implications are evidently quite profound if one wants to take the step towards making policy recommendations. As a result, the most successful seminar presenters (the most "scientific") are very careful about trying to discuss the policy implications of their papers. It is usually done only in the last five minutes, when the substance of the talk is over, sometimes with a bit of a smile or other body language to suggest that this is the speculative part, always using very careful language ("this study would seem to suggest…"). No claims are ever defended with anything like the vigor with which one defends one's choice of econometric techniques.

The main reason why making the jump from the empirical results of a study to policy conclusions is so difficult is that a given set of facts always give rise to multiple plausible interpretations as to why the facts are as we find them. In my estimation, precisely the same holds true when one seeks short run causal explanations. To restate this using Lawson's own framework, my point is that the third stage of his recommended strategy, that of formulating ways of discriminating among competing causal hypotheses, is in the short run extremely problematical. People are always able to reach different conclusions from the same set of facts.

The bedrock claim that underlies this pessimistic conclusion is that the complexity of social phenomena implies severe limitations on what we can expect of empirical work in economics. This does not mean that progress in the empirical domain is impossible. We now have better and more varied statistical methods, more powerful computers, and more detailed data, so that we can describe the economy at a point in time much better than we could even a generation ago. But even with all of these advances, the complexity of the phenomena we analyze means that forecasting will be difficult, it means that making the move from an econometric study to a policy conclusion will be difficult, and it means that discriminating among competing causal hypotheses, at least in the short run, will be difficult. These are not problems that will go away through time, once we have better tools. They are a permanent feature and are due to the nature of the open system that we study. This pessimistic conclusion is probably the most important implication that I drew from my study of Hayek's writings on the study of complex phenomena. My working subtitle for my book, and one I had wished now that I had retained, was "F.A. Hayek and the Limits of Social Science."

Does providing long run causal explanations exhaust the contributions that economists can make? No, there are other things that we can and should do. For example, economists have long contributed a method of analysis that helps all of us to make better sense of the world. I have discussed this contribution both in my book and on the pages of the *post-autistic economics review* under the not very well-defined label "basic economic reasoning" (Caldwell 2002, 2004, pp. 382–388). What constitutes basic economic reasoning is hard to describe (though I am tempted to say, like pornography, I know it when I see it), so instead of offering a definition I have provided a number of examples of what I have in mind in my article and book.

Basic economic reasoning uses simple tools, like production possibility curves or demand and supply curves, to facilitate understanding of real world events. Such diagrams almost "think for themselves." They embody common sense, even proverbial knowledge (e.g., the notion of opportunity cost suggests the adage, "you can't have your cake and eat it too"), knowledge that has survived and been passed down through time in various forms because it has proved useful.

Because they embody common sense, the diagrams themselves are not really even necessary. Last week I read in the paper that, due to the hurricanes that hit Florida in the summer and fall of 2004, Americans should expect that the prices of certain produce (oranges, grapefruit), of lumber and other products used in construction, and of certain types of insurance to rise, and that east coast resort beaches outside of Florida should experience more business. One could use a demand and supply diagram to show why we might expect such things to happen, one carefully hedged with *ceteris paribus* clauses, but one doesn't need to do all that, and they certainly did not do it in the newspaper. Nor does such reasoning depend on humans acting like the perfectly rational agents that are necessary for deriving such predictions in our formal models.

So what is the status of such knowledge? In a recent paper on Frank Knight and pragmatism, Wade Hands (2006) describes Knight's views about economic science. Knight's views are helpful here, because what he describes is very similar to what I have in mind when I talk about basic economic reasoning.

> For Knight … even though economics is not a positivistic science, it *is* a type of science: an intentional or *common-sense* science based on the beliefs and desires of economic agents. Such economic science is essentially a formalization of age-old common sense, but it successfully provides both predictions and explanations of human behavior (though a different type of prediction than available in the natural sciences). Given the particular character of the objects in its domain – humans – this intentional common sense science is not only useful, it actually predicts better than the application of positivistic science to the human domain. As Knight says, "in this instance the position of common sense is better grounded in terms of the ultimate and inclusive facts of experience than is that of scientific logic".
>
> (p. 580; the quotation from Knight is from Knight 1935, p. 81)

Basic economic reasoning is a powerful tool, it helps us to make sense of the world, it allows us to make better decisions, and it makes human behavior more ordered. It is part and parcel of what makes human behavior intelligible, and predictable in certain domains, to the extent that it is at all. Seeking to explicate and to expand the domain of such reasoning is one of the most important contributions that economists can make.

Yet as Hands' passage makes clear, the status of such knowledge is ambiguous. It clearly does not meet the criteria of positivistic science. Nor, as far as I can see, does its use fit easily into the categories that Lawson provides.

But perhaps I am wrong. It may be that the phenomena that basic economic reasoning identifies are event regularities, or "demi-regs." So it may be that I am saying that we should not worry about establishing causes, but simply use these tools that have proven to be so useful in identifying event regularities in the past, even if we do not know precisely why they work. Alternatively, I also suggested in both my article and my book that exploring just why such reasoning works might also be a fruitful research endeavor: this may well be equivalent to Lawson's call for forming and discriminating among causal hypotheses. But such activity should not, in my view, obscure the fact that such reasoning is essential, and should be retained even if we are not sure (because we are unsure of the underlying causal mechanisms) why it works as well as it often does. In any event, I would welcome hearing Lawson's views on such matters.

In conclusion, though Lawson and I share much common ground in terms of our descriptions of what ails the economics profession, our "policy conclusions" as to the best way forward appear, at least, to be different. Given all that I have said above, the fact that we might reach different conclusions starting from the same set of facts is not surprising to me.

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2 History, causal explanation and "basic economic reasoning"

Reply to Caldwell

Tony Lawson

It is hugely desirable to be charitable in interpreting an opponent's arguments. Being so constitutes not only an expression of respect, but also a principle of good methodological practice. For revealing the limitations of a simplistic caricature of someone else's position rarely constitutes much of an advance in understanding.

I expect most commentators will agree with this. But I am aware of few who seek more to put such a principle into practice than Bruce Caldwell. Caldwell's commendable desire to be charitable to those whom he engages or studies, no doubt accounts for his balanced contributions to the history of economic thought in particular. His recent biography of Hayek (Caldwell, 2004) is an especially impressive and insightful work of this sort.

This avowedly fair-minded orientation is equally manifest in the continued patience Caldwell shows for the mainstream tradition. Although Caldwell himself contributes mostly to heterodoxy, economic methodology and the history of economic thought, he keeps abreast of mainstream developments and seeks to rescue as much as he can from heterodox or philosophical critique.

I do worry, though, that this may involve him in ultimately claiming more worth in the mainstream project than is tenable. This in itself is not an overly bad thing, unless this emphasis results in too little attention being paid to critically formulated alternatives. I wonder if this is not the case with his discussion of the approach he terms short run causal explanation. Caldwell, it seems to me, dismisses the possibility of success in this domain a bit too quickly. And I think he does so because he believes that the application of mainstream tools under the head of "basic economic reasoning" is a fitting substitute that succeeds in giving us a good deal of what we need. On this, I am not yet convinced.

History

But let me start closer to the beginning of Caldwell's piece. After first surveying parts of *Reorienting Economics*, Caldwell asks whether I am advocating that economists do more economic history. My short answer is that not only am I advocating that they indeed do more history, I believe that, *qua* economists, they should almost *never not be doing history*. For all of reality is in time, and so has a historical dimension.

This applies equally to phenomena of the natural realm of course; it is even conceivable that gravity operates differently the further we are from any "big bang" (see e.g. Richard P. Feynman, 1988, p. 206). But if the speed of change of certain natural phenomena is sufficiently slow that, for most practical purposes that concern us, we can treat them as approximately constant, this is usually not the case regarding social developments. Social reality is that realm of phenomena that depends for its existence (at least in part) on us; it is constituted, and so is being continually reproduced and/or transformed, through variable human practice. Hence, most social phenomena are not only space–time grounded but also inherently more quickly transformed or more transient than most natural phenomena. So, for a discipline like economics, the temporal dimension of its objects is always likely to be fundamental to its analysis.

This does not mean, of course, that economics cannot, or should not, be theoretical. It merely follows that good social theory warrants the skilful combining of abstract and concrete history, and particularly of pure and applied explanatory endeavour. I know of no better illustration than Marx's theoretical, and yet just as obviously historical, analysis of the nature or mode of functioning of the specific human system that is capitalism.

Of course, social reality also stretches over space as well as time (or perhaps better over space–time). Hence, all social theory, including any serious economics, is inherently geographical as well as historical.

Clearly in setting out these assessments, I am taking the view that the nature of economics ought in some way to reflect the nature of its subject matter. That is, I am not wishing economics to be tied to some particular method in an a priori fashion, as is the practice of the current mainstream, but suggesting that its orientation be tailored, at least to some degree, to ontological insight.

Parenthetically, once we take this latter route of orienting processes of investigation to ontological insight, it is reasonable that (as is the case in the natural sciences) the social disciplines more widely be carved up according to any differences found in the *sorts* of structures, processes and principles being studied. However, if the social domain is as I describe in *Reorienting Economics* it is clear that economics, sociology, politics, anthropology and human geography, etc., deal not only with the same spatially temporally rooted reality, but also with the same sorts of structures and processes.

So in answering Caldwell's question I find myself (given the nature of my answer) setting off to address the more fundamental one: can we find a non-arbitrary basis for distinguishing economics from any of the other disciplines that study the social realm? And I have to conclude that I believe there is really only the basis for a single social science.

Such a contention does not undermine the need to retain divisions of labour, along the lines found in other sciences, such as physics, with its various sub-branches. On this conception, the division of labour adopted by the various sub-disciplines of social science like economics will be a matter largely of each sub-discipline's own history. In *Reorienting Economics*, I make the case for economics being traditionally the study of those social factors bearing on the material conditions of well-being (see Lawson, 2003, Chapter 6).

Here, though, I am straying increasingly far from the point. My simple answer to Caldwell is: yes, I believe actual economic analysis needs to become more historical. In fact, to be explanatorily successful, it can only be an intrinsically historical discipline; any ahistorical economics is likely to be an irrelevance. My reasons for this answer, though, lead me also to conclude that economics ought to be far more integrated with all the other branches of a single (geo-historical) social science.

Long run and short run causal explanation

So far so good, in the sense that Caldwell and I seem to agree that economics needs more history. However, agreement is less clear when Caldwell writes:

> If Lawson is advocating that economists do more economic history when he says that we should seek causal explanations, I have no quarrel [...]. However, it may be that Lawson is calling for what might be termed *short run* causal explanations as well. In my opinion, seeking to produce valid *short run* causal explanations is an extremely ambitious goal, and in many instances an unreachable one. The complex nature of the open system that constitutes social reality, one that poses such problems for mainstream efforts at its analysis, will cause similar problems for any such program.

I must admit I am not completely sure of Caldwell's distinction between long run and short run causal explanation. Most likely long run causal explanations are distinguished from short run explanations simply in referring to causal processes that are relatively more enduring. But whatever the distinction, it should already be clear from the discussion above that I am of the view that all causal processes (long run, short run or whatever) operate in time and so warrant historical analysis.

I take it, though, that Caldwell's main point is that causal processes that are less enduring are more difficult to identify. I suspect this is sometimes so. But I am not yet convinced that an understanding of them is typically unattainable. Let me briefly explore Caldwell's reasons for his pessimistic conclusion.

The problem of short run causal explanation

The most direct reason for Caldwell's pessimism is already given in the passage above: the complex nature of the open system that constitutes social reality. But this begs the question as to why, in the face of such a complex open system, similarly pessimistic conclusions are not reached about long run causal explanation. The key seems to lie in Caldwell's conception of economic history, but I am not sure he elaborates further.

Rather he focuses on the problems of short run explanation. And Caldwell offers to illustrate the nature of such problems with a homely example. In it, Caldwell describes, in admirably respectful terms, some of the practices of his colleagues in his own "largely empirical" economics department. The picture that emerges is of research that mostly comprises one or both of two stages: first empirical analysis and second the (more optional) drawing of policy prescriptions or other implications.

The carrying out of the first stage is described as typically thorough and competent, the second as cautious and speculative. Caldwell further informs us that in a typical seminar presentation, only a small amount of time (and sometimes no time) is allocated to the discussion of the implications of the analysis; and when it occurs it is always situated at the end. However, when it does take place, this part of the presentation usually generates the most animated response or debate.

The reason for this, Caldwell explains, is that to move from data correlations "to policy conclusions, one must speculate about *causes*, and there are typically many plausible interpretations on offer". In addition,

> all empirical economists recognize that adding new variables to an existing set of variables, or using new data sets that include different variables or which cover different time periods, or using different types of corrections, all typically yields different results, always in terms of the coefficients attached to various variables of interest, and sometimes in terms of their signs.

Furthermore, reasons Caldwell,

> The main reason why making the jump from the empirical results of a study to policy conclusions is so difficult is that a given set of facts always give rise to multiple plausible interpretations as to why the facts are as we find them. In my estimation, precisely the same holds true when one seeks short run causal explanations. To restate this using Lawson's own framework, my point is that the third stage of

> his recommended strategy, that of formulating ways of discriminating among competing causal hypotheses, is in the short run extremely problematical. People are always able to reach different conclusions from the same set of facts.

I find Caldwell's observations on academic economic practice to be familiar and convincing. But the first question that pulls on my mind (but apparently not on Caldwell's) is what use is *any* of this? If policy discussions based on such studies warrant being animated and critical, do not discussions of the empirical econometric work warrant the same?

Specifically, what is the value of the correlations reported? As Caldwell in effect notes, they do not reflect (the triggering and effects of) isolated causes (unlike the event regularities produced in controlled experimental conditions), and they vary according to who is using the data set (i.e. they are sensitive to variables included in the model, etc.). Is it not the case that just about all such reported correlations are essentially spurious; the result of repeatedly manipulating/transforming the data until something presentable is concocted? Indeed, it is no secret that reported econometric results are usually the outcome of very many (sometimes thousands of) econometric estimation exercises being carried out (thereby of course contravening the stipulations of classical statistical theory), with only those that conform most to prior expectations being reported. Furthermore, even the "correlations" that are eventually reported typically "break down" once new/additional data is obtained.

Some econometricians do suppose that their own results "this time" will not break down and can at least be used for purposes of forecasting, even if, as Caldwell argues, the causal forces giving rise to actual outcomes cannot be identified. But in an open system of multiple countervailing causes, it seems an especially heroic act of faith to believe that closures (systems supporting event regularities), facilitating successful forecasting, will nevertheless occur very often. And nor is there any evidence that they do.

In any case, this latter orientation or act of faith is clearly not Caldwell's. For in the paragraph following the two passages just noted, after observing that economists "now have better and more varied statistical methods, more powerful computers, and more detailed data", Caldwell adds:

> But even with all of these advances, the complexity of the phenomena we analyse means that forecasting will be difficult, it means that making the move from an econometric study to a policy conclusion will be difficult, and it means that discriminating among competing causal hypotheses, at least in the short run, will be difficult. These are not problems that will go away through time, once we have better tools. They are a permanent feature and are due to the nature of the open system that we study.

But if empirical econometric analysis of the sort in question fails to facilitate either causal explanatory insight, or successful forecasting, or the derivation of policy implications, what is the point of it? Ultimately, it is possible that Caldwell is no more optimistic than am I about the usefulness of any part of the whole practice. But if so he does not convey his pessimism. Rather he provides passages like the following that seem mostly to encourage an impression that the econometric analysis itself (the choice and application of econometric technique[s] given a body of data, etc.) is somehow more defensible (certainly more vigorously defended) than attempts to make use of the analysis:

> the most successful seminar presenters (the most "scientific") are very careful about trying to discuss the policy implications of their papers. [...] No claims are ever defended with anything like the vigour with which one defends one's choice of econometric techniques.

The point of my running through all this is to clarify (my understanding of) Caldwell's assessment of the nature of the problems of short run explanation. In the light of the foregoing, it seems that by short run causal explanation Caldwell effectively means identifying causes behind the outcomes expressed, or correlations produced, in the rather limited sets of time-series data that econometricians mostly use. And, in observing that econometricians cannot easily identify the causes at work, Caldwell concludes that I cannot do so either, for similar reasons.

But I am not so sure about this; I am rather optimistic that it *is* very often possible to uncover causal mechanisms, whether short run or otherwise.

Of course, I do not want to suggest that there is some foolproof method that allows insights that are not open to question or progressive development. Even in the face of the most successful of natural scientific theories, natural scientists are able to come up with competing hypotheses that provide a real challenge. Thus, despite even the spectacular explanatory power of Newton's theory of gravitation, Einstein was able to produce an alternative that equally accounted for the existing "data". The production of a theory that performs at least as well as that currently most widely accepted, however, is not in itself inherently problematic, but rather an opportunity for advance.

The point, of course, is that the theories of Einstein and of Newton yield different (including conflicting) implications for certain types or spheres of phenomena.[1] As a result, scientists have been able to assess which hypothesis is the more explanatorily powerful by collecting new observations in domains revealed to be relevant for comparative evaluation. No hypothesis, however explanatorily powerful, can be wholly treated as the last word. But where new competing hypotheses are