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This and the next four or five issues of the PAER will be devoted in part to debate on and discussion of Tony Lawson's new book *Reorienting Economics*. The intention is for his book to serve as a focal point for a general discussion on the reform of economics.

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"At our university (the leading one for economics in France) we have succeeded in cutting back the programs of micro, macro and maths, something that would have been inconceivable a few years ago. This is in the aid of an approach more open, more multidisciplinary. The 'orthodoxes' have rather easily given way, having, despite everything, interiorized the arguments advanced against them. In the colloquiums and in the press they feel obliged to justify what they do, thereby admitting at least in part the aptness of the 'anti autistes' criticisms." Bernard Guerrien

Invitation to join a heterodox economics e-mail list

Dear Colleague,

I run an e-mail list that distributes information that is of interest to heterodox economists around the world. I try to restrict the e-mails to one every 2-3 weeks. The e-mails generally take the form of a description of a heterodox conference I have been to, brief comments on various heterodox economic activity around the world, and perhaps a brief obituary of a heterodox economist who has died. Then the rest of the e-mail provides information that I think are of interest to heterodox economists on newly published books, new journals, job announcements, call for papers of conferences that are of interest to heterodox economists, seminars, and distribution of information on heterodox journals, graduate programs, and other things. Nearly all of the material I send out has been sent to me by heterodox economists who want me to make it known to the economists on my e-mail list. Thus, if you have any thing you want to send out that you think is of interest to heterodox economists, please send it to me at leefs@umkc.edu and I'll send it out. If you would like to be part of this e-mail list, please send me your e-mail address. If you find the material I send not matching your interests, you can just e-mail me and ask me to take your name off the list--and I will immediately. If you have a questions please e-mail me.

Sincerely,

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Symposium on Reorienting Economics

On the Problem of Formalism in Economics

Geoffrey M. Hodgson (University of Hertfordshire, UK)

Modern Economics is Sick

In his *Reorienting Economics*, Tony Lawson cites this magnificently appropriate quotation by Mark Blaug (1997, p. 3):

Modern economics is sick. Economics has increasingly become an intellectual game played for its own sake and not for its practical consequences for understanding the economic world. Economists have converted the subject into a sort of social mathematics in which analytical rigour is everything and practical relevance is nothing.

I believe that on this issue, Lawson, Blaug and I are in agreement: the victory of technique over substance is a chronic problem within modern economics. Although the victory of formalism can be dated to the 1950s (Blaug 1999, 2003), by the 1980s the problem had become much more serious. Because mathematics has swamped the curricula in leading universities and graduate schools, student economists are neither encouraged nor equipped to analyze real world economies and institutions. Arjo Klamer and David Colander (1990, p. 18) reported a survey which showed that only 3 per cent of graduate students on top US economics programmes perceived 'having a thorough knowledge of the economy' to be 'very important' for professional success, while 65 per cent thought that 'being smart in the sense of problem-solving' is what matters, and 57 per cent believed that 'excellence in mathematics' was very important.

In 1988 the American Economic Association set up a Commission on the state of graduate education in economics in the US. In a crushing indictment, the Commission expressed its fear that 'graduate programs may be turning out a generation with too many *idiot savants* skilled in technique but innocent of real economic issues' (Krueger *et al*, 1991, pp. 1044–5). Alan Blinder (1990, p. 445), a member of the Commission, commented:

Both students and faculty find economics obsessed with technique over substance . . . the many macro and micro theory exams the Commission examined . . . tested mathematical puzzle-solving ability, not substantive knowledge about economics . . . Only 14 percent of the students report that their core courses put substantial emphasis on 'applying economic theory to real-world problems.'

Alarm bells concerning technique displacing substance in economics have been sounding for many years (Ward, 1972). However, although mainstream economics has made some significant theoretical advances in the 1990s, including an increasing adoption of institutional and evolutionary themes, the situation concerning formalism has not got any better.

Perhaps the most serious emerging problem is that the graduate students of the 1980s and 1990s, who are skilled in technique but who have an impoverished understanding of economic principles and their history, are now beginning to achieve positions of seniority and influence in the university departments, associations and journals of the economics profession. Their growing power and influence will ensure that formalism further consolidates its overwhelming hegemony, to the detriment of wider-ranging conceptual and methodological enquiry. This problem is particularly serious in Britain and America, where formalism has achieved its earliest and most complete victory. But the process is delayed rather than absent elsewhere.

Both Blaug and Lawson face the problem of formalism head-on. But, as I shall elaborate below, their evaluations differ. Blaug complains that formalism has been associated with a detachment of economics from substantial and practical issues. Lawson's (1997, 2003) attack is more extensive and radical. He develops at length a methodological critique of what he calls 'deductivism' and identifies this as the root of the formalist malady. One of my main purposes here is to examine some prominent aspects of Lawson's critique of formalism. I shall argue that his stance is too limiting, with the expected outcome that mathematical and econometric tools will be illegitimate except under 'seemingly rare' (Lawson, 2003, p. 21) conditions.

Tony Lawson's Critique of Formalism

Lawson affirms that the systems addressed by the social sciences are open, in that they are subject to multiple extrinsic and intrinsic disturbances. This makes the task of prediction either difficult or impossible. For Lawson (1997, p. 288), 'event prediction is usually infeasible' and 'in any case not required for a successful science of economics'.

Lawson (1997, pp. 16-17) argues that 'deductivism' presumes 'event regularities' or 'constant conjunctions of events or states of affairs' with regularities of the form 'whenever event x then event y '. Philosophically, this is a rather atypical definition of deductivism, because it refers to empirical regularities concerning events rather than logical deductions concerning propositions. He seems to suggest that logical or mathematical constructions, if they are to be of relevance or use, must be some kind of map of reality at the level of events. For example, Lawson (2003, p. 22) writes of the importance of a "fit" with reality'.

From this stance, his critique of the use of formalism in economics readily follows. Social reality is an open system, generally lacking in 'constant conjunctions of events'. By contrast, formal models cannot be open to an indefinite number of additional relations or variables. In either a strict or a stochastic sense, such formal models generate regularities in the form: if x then y . Such event regularities are highly limited in the social realm. Accordingly, there is a general mismatch between formal models and reality. If economics is to progress, then formal modelling must be limited those cases where such regularities pertain, and these appear to be rather rare.

In the absence of formal models, what does the theorist do? Lawson realizes that no theory (formal or discursive) can proceed without some degree of abstraction: it is impossible to consider all elements and interactions at once. Indeed, he develops his methodological notion of abstraction at length. But here he faces a difficulty. If abstraction is necessary, and it involves the limitation of the sphere of consideration and the exclusion of additional relations or disturbing forces, then doesn't this too imply the assumption of a closed system? Stephen Nash (2004) has recently argued in the affirmative, suggesting that Lawson too must assume conditions or forms of closure. To some extent, however, Lawson (1997, p. 236) anticipates this objection. He proposes a distinction between 'abstraction' and 'isolation' in the following terms:

When we focus upon varying productivity performances here, conditions of work there, rising or falling unemployment rates, and so on, we do not suppose that these features we choose to emphasise exist in isolation, even as a temporary, heuristic, measure. To do so is to assume a totally different world from the one in which we live, and one that has no bearing upon it. ... In short, there is literally a world of difference between leaving something (temporarily) out of focus and treating it as though it does not exist. The achieving of an abstraction and treating something as though it existed in isolation are not the same thing at all.

He uses this distinction to protect his argument against the objection that his method of abstraction also implies the assumption of closure; he argues that abstraction does not imply closure but isolation does. With some important nuances and qualifications, Lawson (1997, pp. 131-3) associates the notion of isolation with the work of Uskali Mäki (1992, 1994) and contrasts

isolation with his own concept of abstraction. However, I shall argue later below that the distinction is, at least in prominent practical instances, difficult to sustain. Lawson takes a relatively extreme position in his attitude to formalism in economics, even among critics of mainstream economics, and even among the school of 'critical realists' to which he belongs. For example, critical realists such as Paul Downard (2000) have defended a more frequent use of some econometric techniques. Lawson points to very few concrete instances where econometrics has been appropriately deployed; Downard points to several. And the critical realist Erik Olin Wright (1994, pp. 183-9) has strongly supported the use of 'explicit abstract models, sometimes highly formalized as in game theory' and other 'rational choice models'. Although of course an extreme position such as Lawson's is not necessarily inappropriate or wrong, it does invite repeated criticism.

Perhaps a consequence, in Lawson's later writing, there has been a slight shift of tone and emphasis, if not substance as well. For example, Lawson (1999, pp. 7-8) proposes that from the fact that 'the world is open and structured, it does not follow' that economists 'ought thereby not to engage at all in formalistic methods such as econometrics.' He continues:

The possibility of successes with the latter requires local closures. ... Critical realism thus cannot and does not rule out *a priori* their limited occurrence. Rather, critical realism adopts an essentially *ex posteriori* orientation ... the opponent is the advocate of any form of *a priori* dogma.

With some amendments, Lawson (2003, pp. xix, 27, 178-9) repeats a similar argument in several places in his latest book. Again and again he insists that he is not against the use of econometrics or models in principle, but that they are of highly limited use given the closure conditions upon which they depend. He writes that 'a blanket rejection of econometrics, or indeed of any other method, is not a stance that is, or could be, sponsored in critical realism.' What is opposed is not econometrics but 'the *reduction* of economics to formalistic analysis.' But he then goes on to say that the 'application of formalistic methods requires certain (closure) conditions constituting special configurations of social reality that (unsurprisingly from the perspective sustained) have turned out to be rather rare.' In a recent essay, Lawson (2004) again repeats his insistence that he is not 'anti-mathematics'. But his expectation remains that the conditions for its effective and proper use would be rare.

In these passages at least two features are emphasized. The first is a strong, sincere and repeated claim of anti-dogmatism concerning whether or not mathematics can or should be used. But he lays down criteria for its use, including the requirement of (approximated) local closure. As a result of these criteria, the specific measure of his own anti-dogmatism, in practice rather than in intention, is how far he would admit that open systems might appear (or be approximated) in reality. Lawson argues that his critical realist perspective suggests at the outset that they are 'limited' or even 'rather rare'. Accordingly, the ontological arguments in Lawson's critical realism lead him right away to expect that the possibilities for formalism are highly restricted. This sets limits on his anti-dogmatist stance, despite his pronounced antidogmatist intentions. Although Lawson imposes no absolute normative ban on the use of mathematics, his arguments limit its legitimate use to 'rare' circumstances only.

Generally, one can also ask if a pervasive anti-dogmatism were possible. The need for some dogmatic presuppositions must be acknowledged by any philosopher or theorist. The removal of all dogma would mean a disabling nihilism of universal scepticism. In such circumstances, no theory could be established. Similarly, human activity would become paralyzed if we ceased to believe in the essential dogma that most of the natural regularities and social institutions of today will survive until tomorrow. We often admire anti-dogmatism as a commendable personality trait, but philosophy of science suggests that some considerable degree of dogmatism is unavoidable.

The second feature is the proclamation of 'an essentially *ex posteriori* orientation', although what precisely is meant by this is insufficiently clear. Critical realists rightly emphasize the

importance and priority of ontological commitments. Consider fundamental ontological commitments such as 'ubiquity determinism' (Bhaskar, 1975, pp. 70-1), which means that every event is deemed to have a cause. We have known at least since the days of David Hume that it is impossible to deduce causes *a posteriori* from our experience of events. It is in the very nature of such primary ontological commitments that they are neither based on nor deduced from evidence or experience. One of the crucial aspects of the philosophical assault on positivism in the middle of the twentieth century was the reaffirmation of the importance of such prior ontological commitments, which cannot be established by appeal to evidence or experience alone (Quine, 1951; Caldwell, 1982). So one is left wondering what 'an essentially *ex posteriori* orientation' means, and how it can be reconciled with an insistence on the primacy of ontology.

Again I detect a slight post-1997 shift of tone and emphasis when Lawson (2003, pp. 20-1) openly discusses the possibility that econometrics might be of use in some instances:

Clive Granger has argued convincingly that it is possible to use econometrics to provide relatively successful short-run forecasts of phenomena such as electricity loads and peaks in regions wherein one factor, temperature, or more specifically the extreme cold, dominates behaviour. ... The point remains, however, that the sorts of conditions in question appear *a posteriori* not to be typical of the social realm. Rather, as I say, social reality is found to be a quintessentially open, structured, dynamic and highly internally related system, amongst other things, whilst the conditions for achieving a local closure are seemingly rare.

This is the only example I can find where Lawson has pointed to a specific piece of econometric analysis and acknowledged its legitimacy. Note, however, the strictness of the key condition involved. According to this passage, for econometrics to be applicable, 'local closure' must be actually achieved, not merely approximated. However, it is clearly the case *a posteriori* that electricity consumption (even in cold regions) is a feature of an open rather than a closed system. For instance, electricity consumption is generally affected by its price. Such prices are heavily influenced by global market conditions. Global markets are far from being closed systems. Granger did not provide an example which establishes local closure. By the logic of his own argument, Lawson should have deemed econometrics to be inapplicable to this situation as well. His single claimed example of the legitimate use of econometrics turns out to be illicit according to his own key criterion.

Indeed, if we require that formal models can only be applied in contexts where local closure is actually achieved, then this would mean that such models were inappropriate in other sciences and disciplines, such as biology, physics or engineering. Generally, in multiple contexts, in both the natural and social world, such closures are absent, as Roy Bhaskar (1975) as well as Lawson himself have emphasized. If formal models require strict local closure, then formal models are never appropriate. But this would overlook the achievement of mathematical models in some sciences. It may be suggested that local closure is sometimes approximated in physics, and because of this some formal models can be of use. But models and simulations have also been used with some success in biology and evolutionary anthropology, which face a degree of complexity and openness comparable to that found in human societies (Murray, 1989; Boyd and Richerson, 1985).

At least in recent seminar presentations, Lawson has amended his position still further, by proposing that econometrics might apply when local closure is 'approximated in reality'. This formulation contrasts with that in his two (1997, 2003) books, which generally insist that local closure conditions must actually apply for formalism to be viable. From his amended standpoint, admitting a degree of approximation to closure, it would be possible to admit the Granger example as a case of the legitimate application of econometric techniques. The general problem for Lawson in applying this revised criterion more widely is that the degree of acceptable approximation is left unspecified. In general, once the insistence on the actual achievement of local closure is removed, and approximations to closure are admitted, then the door to econometrics is unlocked and opened.

Some Key Problems and Omissions in Tony Lawson's Critique

Much of Lawson's discussion of formalism concerns econometrics. He gives insufficient attention to other applications of mathematical techniques, which serve primary purposes other than the prediction or explanation of measurable variables. Such additional applications of formalism include (a) heuristics and (b) internal critiques. I shall address each of these in turn.

The purpose of a heuristic is to identify possible causal mechanisms that form part of a more complex and inevitably open system. Heuristics can be useful without necessarily making adequate predictions or closely matching existing data. Their purpose is to establish a plausible segment of a causal story, without necessarily giving an adequate or complete explanation of the phenomena to which they relate.

An example of a formal heuristic that has been persuasive in economics is the ethnic segregation model constructed by Thomas Schelling (1969). Using a very simple model of housing location, Schelling showed that ethnic segregation can result even from very small feedback effects. Even if people only have a very slight preference for their own ethnic group, this can be enough to cause migration out of mixed ethnic areas, with the end result of segregated ethnic ghettos. The problem is extremely simple, and hardly realistic in its detailed assumptions. However, making the model more complicated and 'realistic' would be beyond the point, partly because it is obvious that similar outcomes might result from a more complicated model. Instead, the Schelling model points to a credible mechanism that shows that ethnic segregation does not necessarily depend upon the actions of bigoted racists. Such racists exist in the real world, so their inclusion in the model would make it more realistic. But this would defeat the object of the model, which is to show that segregation might result even without them. The model abstracts from the more forceful versions of racism that we find in the real world to establish this key point. In this case, the power of the model is helped by its unrealisticness. The power of the model lies in its capacity to abstract a plausible but hitherto neglected causal mechanism.

In a very useful discussion of such 'credible worlds', Robert Sugden (2000) asks probing questions concerning the role and 'realisticness' of this and other heuristic models in economics. These heuristic models have the paradoxical claim that they are literally unrealistic yet they seem to illuminate important aspects of reality. Using the Schelling model alongside George Akerlof's (1970) famous article on the 'market for lemons', which again claims to establish meaningful propositions about the world on the basis of an admittedly unrealistic model, Sugden (p. 28) describes these models as 'credible counterfactual worlds' that give 'some warrant for making inductive inferences from model to the real world.'

In no case can the construction of a heuristic or counterfactual model clinch the argument concerning the causal mechanisms that actually exist in the real world. However, what they sometimes do show – as in the case of the Schelling model – is that outcomes might not necessarily result from the causal factors that may be presumed at first sight. To complete the argument, further theoretical development and empirical enquiry are always required. I have suggested above that heuristics are appropriate if they successfully abstract an important causal mechanism in reality. Accordingly, heuristics relate to the very process of abstraction that Lawson himself highlights. But Lawson suggests that heuristics are isolations rather than abstractions. So here I must return to Lawson's (1997, p. 236) attempted distinction between isolation and abstraction, as quoted above. According to him, the key difference is 'between leaving something (temporarily) out of focus and treating it as though it does not exist'. Again take the Schelling model as an example. Schelling himself accepts that bigoted racists exist, yet he leaves them out of his model. The purpose of the model is not to excuse or deny racism, but the more severe forms of racism are deliberately removed. Nevertheless, the model is extremely and worryingly persuasive.

No-one to my knowledge, including Schelling himself, has suggested that such a model is a complete or adequate causal representation of the processes underlying the emergence of ethnic segregation in reality. The model is simply a heuristic step along the road towards that more complete end. More generally, no sensible mainstream economist would deny that the world is open, and no adequate presentation of a formal model would omit to mention that other (omitted) causal mechanisms exist.

Ultimately, Lawson's attempted distinction between abstraction and isolation hinges on the precise meaning of notions such as 'treating [that which is left out of the picture] as though it does not exist' and the implied distinction between a 'temporary heuristic' and 'leaving something temporarily out of focus'. Yet Lawson is insufficiently precise here. If I 'focus' on the workings of a national economy (perhaps without building a mathematical model) and ignore its trade with other nations, then in what sense might this qualify as a temporary account, rather than a presumption that such exports and imports do not exist? Surely, some verbal statement would be required, acknowledging the existence of international trade, explaining its omission from the current discussion, and suggesting that further work must be done to incorporate it into the analysis. But this is also the kind of necessary qualification that we should expect from the best presentations of heuristic models. On the other hand, it would be impossible to mention all the things that we have left out of the account. In this sense all theory is 'temporary'. But do such unmentioned omissions amount to treating some causal linkages as though they do not exist? If this were the cases, then every theory, including non-formal, discursive theory, by Lawson's criteria is a failure. Once we try to apply Lawson's criteria, then their insufficiency and vagueness become apparent, and his attempted distinction between abstraction and isolation is revealed as highly problematic.

A crucial point here is that in economics we should not and cannot judge models in isolation. Lawson treats any model as if it were alone an intrinsic claim to be a partial map of the world. Yet the meaning of any heuristic model depends upon an interpretive framework that is not contained in the formalities of the model itself. If heuristic models are suitably hedged and qualified, in the manner suggested above, then these qualifications form part of the interpretative apparatus for the model. If heuristic models are treated within an adequate interpretative context, then such heuristic packages can successfully defend themselves against the charge that they treat other aspects of reality as though they do not exist.

By contrast, Lawson himself isolates formal models from their interpretative contexts, treating these as if they do not or need not exist, and denies the validity of even 'temporary' heuristic models *per se*. The strictures of appropriate contextualization that Lawson rightly requires of discursive theory should apply to his treatment of formal models as well. Bringing the interpretative framework of a heuristic model into the picture is highly important in appraising the problem of excessive or misplaced formalism in economics. An alternative diagnosis emerges, in which the malady is not the use of formalism as such but the inadequacy and underdevelopment of the interpretative context in which they are placed. Technique can take priority over substance as a result of the relative neglect of interpretative context. An adequate interpretative framework would depend on the discussion of the genesis, meaning and methodological significance of key concepts that are involved in the model or its interpretation. This is never a small task, and if done properly it will be at least as weighty as the formal technique of the model itself. Yet in modern economics such interpretative and conceptual matters are often marginalized and underdeveloped. I contend that this is one of the main problems with formalism in economics today.

While Lawson implicitly treats formal models as if they were claims to map the world, his explicit metaphor is more frequently of the model as a tool. For example, Lawson (2003, p. 12) notes the ontological mismatch between formal models and reality and suggests that this is grounds to question their use: 'Few people ... would attempt to use a comb to write a letter ... or a drill to clean a window.' This argument is not as illuminating as it may seem at first sight. Of course, we would use a pen to write a letter and a clean cloth to clean a window. Yet the ontology of pens is very different from that of letters, and likewise there is a big ontological difference

between clean cloths and dirty windows. So there is nothing in this appropriateness-of-tools argument that rules out, for instance, using closed models to help understand an open reality.

I now turn to the second use of formalism that is neglected by Lawson: that of an internal critique. Generally, the impact of an effective internal critique is negative rather than positive; it shows the limits of an existing theory rather than building a new one. It is nevertheless important. Consider the example of the critique of mainstream capital theory by Piero Sraffa (1960) and others. By developing a model with disaggregated rather than aggregated physical capital, Sraffa showed that the measure of capital could not be independent of profits, wages or prices. Consequently, any attempt to explain the latter by means of an aggregated capital variable must assume that which it has to explain. The validity of this argument was later accepted by Paul Samuelson and others (Harcourt, 1972). It meant that several of the models and arguments used in the mainstream theory of capital and distribution were either invalid or dependent on highly restrictive assumptions.

A demonstration that a widely adopted approach depends on restrictive or even implausible assumptions is a key feature of many of the successful and significant internal critiques that we find in economics. Other examples include works by Rolf Mantel (1974) and Robert Rowthorn (1999). Mantel and several other theorists showed that even with the assumption of individual utility-maximization, the excess demand functions in an exchange economy can take almost any form, and there is thus no basis in standard general equilibrium theory for the assumption that they are generally downward sloping. Their work proved very influential in bringing the microfoundations project in general equilibrium theory to an end (Rizvi, 1994). Rowthorn showed that prominent models used by governments in macroeconomic policymaking are based on highly restrictive and unwarranted assumptions.

Such critiques do not themselves provide new theories, although they may suggest some appropriate measures and establish some relevant pointers. By their nature, internal critiques are not claims to map the real world. Instead, they are attempts to show that other theories are inadequate or overly restrictive in regard to the kind of world to which they relate. I have not come across an adequate discussion of the role of internal critiques in Lawson's work, despite their prevalence the Cambridge tradition of economics that used to be well established in his university department.

Significantly, neither heuristics nor internal critiques are attempts to map the world with a model. Accordingly, insofar as they are of some scientific use, severe doubt is cast on Lawson's central argument that the adoption of a particular model involves explicit or implicit assumptions about the ontology of the social world. By contrast, it would seem that some models are of use, even if there is a significant misfit with reality. If so, then Lawson's main argument falls.

Conclusion

In regard to formalism, many economists propose the extreme view that it is the principal and necessary means by which economics becomes rigorous and scientific, and thus the dominance of formalism is a positive sign of success. Lawson takes a position near the other extreme. He argues that formalism is justified in 'rare' circumstances only, where local closure exists or is approximated. I propose that both attitudes to formalism are flawed, partly because they both downplay its necessary interface with interpretative structures.

Yet while Lawson and the mainstream are at odds, there are some shared presuppositions. Many mainstream economists assume that their models are sufficient to represent the world, neglecting the interpretative discourses required to make such a claim meaningful. Lawson too believes that the adoption of a formal model intrinsically upholds some substantial claims concerning the nature of reality. I believe that both positions are false.

If modern economics is sick, then what is the nature of the sickness? A good answer to this question is required to help us find an appropriate remedy. Lawson's medicine is to require the application of formalism only when local closure is achieved or perhaps approximated. However this remedy virtually ends up as an inversion of the disease itself, and I have argued that it is based on a faulty diagnosis.

Especially in his recent writing, Lawson has insisted that he is not against formalism as such, and he has no dogmatic prescription concerning its use. However, I am aware of only one example of a piece of econometrics which Lawson has deemed as legitimate, and even here to admit it he has to fudge the criterion of strict closure declared in his 2003 book. More recently (but until now only verbally as far as I am aware), he has relaxed this criterion to allow econometrics to be used when closure is approximated, rather than actually achieved.

The consequent challenge for Lawson is to be more specific about the degree of approximation and to point to still further examples of the legitimate use of mathematical models in economics. Until this is done, Lawson remains in the extreme position of admitting as legitimate only one specific case, among hundreds of thousands of examples that are available to us.

Middle ground solutions are not intrinsically warranted simply because they are middle ground. But part of the tragedy of modern economics is that they have so far received limited attention and consideration, with notable exceptions such as a recent article by Victoria Chick and Sheila Dow (2001).

I suggest that the problem with formalism is not the general inappropriateness of formalism itself, but it is the problem identified by Blaug in the quotation near the beginning of this article. Blaug sees the kind of formalism in modern economics as 'an intellectual game played for its own sake' rather than for its use in explaining and engaging with the real economic world. Blaug complains that in modern economics 'analytical rigour is everything and practical relevance is nothing'. Again the solution here is not necessarily to confine formalism to the very rare conditions of actual or approximated closure, but to ensure that concerns for practical relevance come to the fore. Formal techniques should be the servants rather than the masters of scientific enquiry.

It is also worth bearing in mind that there is an example of a social science in which formal methods and models have hitherto been put to little use, apart from statistics. Yet this discipline is widely acknowledged to be in a state of severe disorder, especially concerning its core presuppositions, its self-identity and boundaries, and its relations with other disciplines, particularly economics and biology. This afflicted social science is sociology. The persistence of its acute scientific maladies alongside its relatively infrequent use of formalism indicates that additional problems exist within the social sciences today. These include the postmodernist affirmation that one theory is as good as another, the frequent choice of a theory on ideological rather than scientific grounds, and an occasional self-inflicted blindness concerning the biological aspect of human nature and its significance for the study of human society.

Despite our differences of view, I wish to emphasize that both Lawson and myself, and others here cited including Blaug, Chick, Dow and Mäki, adopt a realist philosophical perspective. Realism acknowledges that a world exists beyond our perceptions. Realists uphold that, to be adequate, sciences including economics should not be self-contained logical games but attempts to address and understand aspects of the real world. Accordingly, there is no room for a philosophy of science in which 'anything goes'. There is a shared realist imperative: to understand the real world.

However, I argue here that there is a place for mathematics in economics, even when conditions of closure are absent or fail to be approximated. I have emphasized the greater importance of the interpretative structure within which the theory is placed. The pressing agenda

issue for further discussion and enquiry in this area is to explore the inadequately explored middle ground between the unacceptable extremes of unreflecting worship and (at least expectational) denial of formal models and methods.

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Symposium on Reorienting Economics

Feminism And Realism – A Contested Relationship

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Introduction

This paper engages with chapter nine of Tony Lawson's (2003a) *Reorienting Economics*, 'Feminism, Realism, and Universalism'. The chapter appeared as a journal article in *Feminist Economics*, in 1999. That publication provoked a remarkable set of comments by feminist economists – some of these highly critical – which were published in the same journal as a dialogue, in 2003, including two responses by Lawson. Earlier (in 1999), as well as in the set of comments in 2003, feminist philosopher Sandra Harding gave her response to Lawson's views. In my discussion of the chapter/article on feminism and realism, I will regularly refer to this dialogue. But before doing so, let me first briefly give some indication of Lawson's position towards feminist economics as a discipline.

Although I could not find his name in the two latest membership directories of IAFFE, the International Associations For Feminist Economics, he can certainly be characterised as a supporter of feminist economics. In his book series with Routledge, *Economics as Social Theory*, he has published several books by feminist economists (Nancy Folbre, 1994; Julie Nelson, 1996; Irene van Staveren, 2001; Drucilla Barker and Edith Kuiper, 2003). He is member of the editorial board of *Feminist Economics*, the associations' journal, and he has participated in several annual IAFFE conferences over the past ten years. Lawson, together with some other male economists involved in IAFFE and/or *Feminist Economics*, can undoubtedly be characterised as a supporter of feminist economics as a sub-discipline within economics. Acknowledging his clear support for the feminist cause in economics, this paper will now focus on the ideas of critical realism that he brings to feminist economics: what are they, how are they connected to feminist economic research, and how are they evaluated by feminist economists?

The objective of his chapter on feminism is "to argue that (...) there are possible advantages to feminist explanatory and emancipatory projects from engaging (or engaging more fully) in the sort of explicit ontological analysis associated with modern versions (at least) of scientific realism" (Lawson, 2003: 219). In his view, feminists too often reject universalism wholesale (rather than only reject *a priori* universalism as expressed in values, experiences, objectives and interpretations of dominant groups) which would "be debilitating for the feminist project" (ibid). In order to clarify his point, he illustrates his argument with three examples, on formalistic modelling, epistemology, and emancipation. The responses to his article agree unanimously with his critique on formalistic modelling, whereas they disagree almost unanimously (except Julie Nelson, who, however, has a related disagreement) with Lawson's universalism underlying his arguments on epistemology and emancipation.

In this contribution, I will first discuss the strong disagreement of the feminist economists participating in the dialogue with the universalism they detect behind his critical realism. In doing so, I will not only rely on the dialogue following Lawson's article in *Feminist Economics*, but also draw from a recent book (published in the book series under his editorship) that provides a state-of-the-art overview of feminist economic philosophy (Barker and Kuiper, 2003). Secondly, I will critically question the apparent agreement between Lawson and the participants in the dialogue on formalistic modelling as unhelpful for both ontological economic analysis and feminist economics. For this part of my contribution, I will partly make use of a paper on feminist

econometrics by Brigitte Bechtold (1999), who instead argues for a feminist approach to modelling. I will end with a conclusion, arguing for a more explicit two-way relationship between realism and feminism.

Feminist Opposition to Universalism

In his original contribution 'Feminism, Realism, and Universalism', to which I will refer to as chapter nine of his book (Lawson 2003a, with page numbers referring to the book version), Lawson rejects *a priori* universalizing, that is, the mere assumption or assertion of a widespread validity or relevance of a particular position. But he warns feminists for the opposite danger he signals in feminist work, namely that "all approaches or stances are as legitimate as each other" (Lawson, 2003a: 218). Now, what does realism offer to feminism? First and foremost, Lawson claims, realism enables feminists to study gender as an ontological category, that is, as a real kind of entity rather than (only) as a representation of certain beliefs. Since gender, and its derived concepts such as gender-relations, gender-inequality, and gender-roles, is at the heart of feminist research, including feminist economics, the potential contribution of realism to feminist research is not trivial. Indeed, as Drucilla Barker and Edith Kuiper (2003: 2) state in the introduction to their valuable volume on feminist economic philosophy, "Gender analysis remains integral to feminist scholarship." Lawson hastens to emphasise that an ontological understanding of gender does in no way imply essentialism. "... there is nothing essential to scientific or ontological realism that supposes or requires that objects of knowledge are naturalistic or other than transient, that knowledge obtained is other than fallible, partial and itself transient, or that scientists or researchers are other than positioned, biased, interested, and practically, culturally, and socially conditioned" (Lawson, 2003a: 220). The participants in the dialogue, however, are not convinced, as they notice a strong universalist claim in his defence of realism. This disagreement underlies much of the dialogue. Lawson perceives an understanding of realism among feminists which reduces this philosophy to a simple, naive version of realism, from which he distances himself. The feminists in the dialogue, however, perceive a strong version of universalism to his position, that is, essentialism, a claim about the nature of human beings, a claim against which the whole project of feminism is set up, in particular post-structuralist feminism. So, the dialogue centres round the opposition between essentialism on the one hand and relativism on the other hand.

Now, do feminists, and in particular feminist economists, reject or downplay realism as Lawson assumes? Does critical realism indeed have the balanced position that feminists favour between universalism and relativism, as Lawson claims in his assertion that realism is not essentialist? In order to shed light on these questions, let me now review the most important comments and replies from the dialogue on the opposition between essentialism and relativism.

On naïve realism, Sandra Harding (1999) agrees with Lawson that this version does not do justice to realism. At the same time, however, she explains that strategically, feminists have found it more helpful to argue from an epistemological perspective, in order to be heard in the scientific debate (and get research funding, for example), than from a realist perspective, in which they often remain marginalised. She argues that feminists have perceived that ontologies are embedded in moral and political projects, and are in no way disinterested. Therefore, she claims, "... it requires a great deal more than just 'clear thinking' to dislodge such ontologies from their status as obvious" (Harding, 1999: 130). Indeed, feminist economists have analysed the economic importance of unpaid labour and caring, the discriminatory part of the gender wage gap, negative impacts of structural adjustment policies for women in developing countries, to mention only a few feminist economic concerns. But these studies have been largely ignored by the mainstream, as is visible in the selection of articles in the discipline's top-ranking journals and chapters in economic textbooks. That is why, Harding states, feminists have found it more useful to rely more often on an epistemological strategy, focusing on how standards are set for what should count as knowledge, good method, objectivity, or rationality. This is precisely why feminist economists have also spent time and effort in challenging mainstream notions of economic

rationality, efficiency, and work, among others, as well as their underlying gender dichotomies of fact/value, reason/emotion, and efficiency/equity. Lawson agrees with this point. The volume on feminist philosophy of economics by Barker and Kuiper provides several examples of such studies¹. To quote just one author in their rich volume, arguing strongly against an essentialist notion of gender, urging feminist economists to hold “no presumption that gender underlies economic processes except in culturally specific, path-dependent ways” (Eiman Zein-Elabdin, 2003: 333).

But there is more than strategy to the feminist preference for epistemology and standpoint theory, expressing the situatedness of knowledge. Fabienne Peter (2003) draws the attention to Lawson’s assumption of a common human nature, referring to a genetic constitution and species-wide needs and capacities, which could be studied in analogy to the study of physical objects in the natural sciences. This assumption, Peter points out, denies the problematic character of science itself, and the still largely positivist science practices in economics. She argues that Lawson appears to suffer from this bias himself, with his notion of ‘judgemental rationality’ which seems to stem from a positivist conception of objective scientific explanation. Feminist economists, instead, tend to follow Harding’s position of ‘strong objectivity’, as a recognition of the situatedness of the scientist (see, for example, Harding, 1995), acknowledging that the ideal of objectivity is untenable and rather than denying its problems, one would better recognise them explicitly. Referring to Lawson’s example on the emancipatory project of feminist economics, Peter (2003: 99) makes clear that in the face of oppression, “accommodating the potential contestedness of needs is more important than issuing universalizing statements.” Drucilla Barker (2003) elaborates this point by questioning the grounds of the presumably shared interests, needs, and motives of human beings – between women and men, but also between women or any other group. Referring to Donna Haraway (1988), Barker (2003: 107) clarifies that “collective subject positions are always socially constructed and partial”. A good illustration of this point can be found in a recent article in *Feminist Economics* on the different perceptions of labour standards by some western feminists and other activists on the one hand, and women workers in exporting industries in Bangladesh on the other hand. In that article, Naila Kabeer (2004) points out why a different perspective is needed, precisely for the sake of emancipation, rather than a unified image of human interests, needs, and motives. Zein-Elabdin (2003: 333) therefore proposes a feminist economic philosophy of hybrid subalternity, which she defines “as subordination deriving from heterogeneous sources rather than a single axis such as gender or colonial subjectivity”. She explains that such a philosophy should be non-modernist and grounded in a self-critical approach and ethical sensitivity to subaltern difference. “This framework remains feminist to the extent that it is partially anchored in a concern for women’s welfare; however, it is paradigmatically guided by the multiformity and instability of difference, and is deeply aware of its own complicity in the cultural hegemony of economic discourse” (ibid).

Lawson’s reply on the critique of his essentialism is that he pleads for seeking commonality with recognition of differences, and not a priori assuming this, which, however, makes one question what is left of the ontology of human beings’ needs and interests. Since he argues that “despite our interrelatedness and differences, indeed as a result of appreciating these features, I believe that, at an abstract level, we can give a formulation of the sort of society that is desirable” (Lawson, 2003b: 125) But the abstract level does not allow for interrelatedness and differences. Feminists instead tend to discuss what is desirable in a society at the level of the concrete. Feminist economists Barker and Kuiper are therefore explicit on their epistemological and methodological stance for economics, a discipline they see as an integral part of culture, power relations, and change: “Economics is not an abstract notion; it does not exist without people. Rather, it is a state of affairs, always implicated in global politics, regional interests, and local alliances. As economists, we are part of the picture – we study, write, and teach from interested positions. Such interests are affected by intellectual pleasure, ethnical sensibilities, as well as by prestige, uncertainty, and a variety of other institutional constraints. Explicitly recognising our location ties us concretely to the world and enables us to envision effective strategies for change and new perspectives on economic issues (Barker and Kuiper, 2003: 3)”. They add, on page 15, that they adhere to the idea of a desirable society, just like Lawson, but recognise that “what

constitutes the 'social good' can no longer be taken for granted". Lawson appears more convincing in his reply to Barker who labelled his universalism as humanist. He agrees, but explains that his humanism is minimalist as it concerns the recognition of human capacities to flourish in human society, as distinct from the capacities of non-human beings. This conception of humanism is very close to Amartya Sen's and perhaps even closer to Martha Nussbaum's theories of capabilities and human development – a perspective with which feminist economists have engaged to quite an extent – largely supportive as well as, to some extent, critical.

Julie Nelson (2003: 110) shifts the attention to another bias she perceives in critical realism, namely its "privileging reason, abstraction, and precision over emotion, particularity, and what is vaguely known." She argues, that this bias against emotion will not help feminist economists to get the economic analysis of caring out of the margin. My example on modelling care in the next section will illustrate this point. Although agreeing with the general ideas offered by scientific realism, Nelson does not find the particular branch of realism very helpful for this type of feminist research. As an alternative, she points at the work of Alfred North Whitehead on process ontology, which she finds a more organic and inter-connected version of realism than Lawson's. "By emphasizing experience, including human bodily experience, as the fundamental unifying reality, he removes the mind-vs.-matter conundrum" (Nelson, 2003: 113). In his reply to Nelson, Lawson feels misinterpreted, and argues, extensively quoting from Whitehead, that his critical realism is much closer to Whitehead's than Nelson claims. That may be so, but his three examples that aimed to show the usefulness of critical realism for feminist economics do not make this explicit – an example on core feminist concerns such as the economic analysis of childcare, or care as a motivation for certain types of unpaid and paid labour might have been a better choice to bring this point across.

In his reply to the comments, Lawson (2003: 128) re-states the objective of his chapter, as "to encourage consideration of an ontological turn in feminist theorizing." But the dialogue that followed on his initial contribution signals that this objective, modest as it may seem, has a problematic undertone. What about a feminist turn in realist theorizing? In other words, what about a discourse in which both feminism and realism are open to mutual influencing? This seems even more desirable in the light of what Harding recognises as an oversight in Lawson's assumption of a feminist neglect of major messages of realism. She argues that much of Lawson's advice on ontology to feminists is ill-informed about what feminist theorists already do, and for quite some time have developed thoughtfully within feminist discourses of philosophy. She refers to work by feminist theorists from the mid-1970s onwards which "has largely already made the claims Lawson 'proposes' (Harding, 1999: 131)." Indeed, she argues, his suggestions on acknowledging situated knowledge "are the main points of standpoint theories" (ibid), but she finds them argued stronger in standpoint theory than in critical realism. She finds Lawson's arguments helpful additions in the continuous defence that standpoint theory is required to do against common misunderstandings. However, the point she makes, referring to others as well, is that science is embedded in networks of beliefs, a point which she summarizes in a question to Lawson: "Shouldn't we expect critical realism, too, to be a network of ontological, epistemological, moral, political, etc. beliefs, even though it focuses only on ontological issues?" (Harding, 2003: 154).

Formalistic Models in Feminist Economics

In his chapter nine, Lawson uses the example of formalistic modelling to illustrate why realism/ontology matters. He argues that the method of formalistic modelling is not at all well equipped for illuminating the social realm and he notices that feminist economists have also criticised it as masculinist, with which he concurs. He convincingly argues that the basic assumption for phenomena to be appropriately analysed with the method of formalistic modelling is that they occur in closed systems, while the social realm, of which the economy is part, is characterised by openness, as well as by structure and dynamics. Moreover, his critique extends beyond the particular type of models that are formalistic (relying on mathematical proofs) to also

include econometrics (relying on statistical tests). He claims that both types of models – theoretical as well as econometric – have been rather unsuccessful. Feminist economists have, however, used modelling as one among a variety of methods, although the type of models used are far more often econometric than formalistic (except for some household bargaining models). Lawson rejects both types of modelling for feminist economic analysis: formalistic as well as econometric approaches. “I think that feminists may have been too cautious in their criticisms of formalistic modelling” (Lawson, 2003: 228). On econometric models, he continues: “there are grounds for supposing that those empirically-oriented feminists in economics insistent upon applying standard econometric methods in all contexts are proceeding wholly in the wrong direction” (ibid).

Now, how did the feminist economists participating in the dialogue react to this assertion? Surprisingly, perhaps, they agreed. Peter (2003: 94) agrees with Harding’s endorsement of Lawson’s critique, finding “Lawson’s article [...] strongest in its critique of formalistic modelling.” Barker (2003: 104) states: “I think that many feminists will find this argument familiar and persuasive.” But she adds that his ontological critique misses the sociological fact that it is precisely formal modelling which provides mainstream economists with their status as scientists. Finally, Nelson (2003: 111) asserts that the critical-realist approaches shares with the feminist approach a wish “to develop a more adequate investigatory practice, not hidebound by allegiance to formal modelling.” These three confirmative responses to Lawson’s critique on the use of formalistic modelling in general, and within feminist economics in particular, require, however, a bit more detailed discussion than they received in the dialogue. There is not enough space in this article to do that, nor would I be best positioned to do this, as predominantly employing qualitative rather than quantitative methods in my own work. But I do think the matter deserves somewhat more attention than it received in the dialogue. Therefore, I will try to point out briefly how feminist economists tend to make use of modelling, and to what extent the outcomes of this method contributes to the understanding of the economic behaviour of women and men and how economic processes influence differently on the economic lives of women and men.

It seems fruitful to start with the example of the disappointing results of modelling the gender wage gap in an illuminative entry on econometrics by Julie Nelson in the *Elgar Companion to Feminist Economics*. In that chapter, Nelson (1999) points out that econometrics has not been able to settle the dispute on the (extent of) discrimination underlying the pay difference between women and men. “Consider regression results suggesting that a wage gap still exists between men and women, even when they have the same observable skills, experience, and so on. Those who do not believe discrimination exists commonly argue that such results could be explained by the omission of important – perhaps unobservable – variables (for example, greater ambition on the part of men). On the other hand, those who believe that the impact of discrimination is actually understated by such studies will argue that some of the included variables (for example, seniority) themselves reflect labour market discrimination” (Nelson, 1999: 155). She explains the different positions on this as resulting from different underlying beliefs, arguing that econometric testing therefore will never be able to settle the argument. This shows again the importance of epistemology for feminist economists, and the need to challenge masculine beliefs reflected in econometrics.

The ways in which feminists address these biased beliefs is partly through employing a broader set of quantitative tools for empirical investigation, including more refined data sets, the use of survey data, and creative technical specifications of models, as well as triangulation with qualitative methods. At the annual conference of feminist economics held in Oxford, in August of 2004, a roundtable on the relationships between feminist economics and Post Keynesian economics, Lawson re-stated his view on the uselessness of models for feminist purposes, while various of the feminist economists present in the session pointed out that this would throw out the baby with the bath water. Feminists have done valuable econometric work in a different way than the mainstream commonly does: not axiomatic but explorative, not claiming explanation but complementing found correlations with theoretical analysis relating to gendered norms, institutions, and power. An example of technical specifications that point to gender inequalities in

explanatory variables are power parameters in household production models, that reflect male control over household resources such as labour time, land, or income. Such parameters do more justice to the underlying gender mechanisms than including only a few separate variables for men and women, such as male and female labour time. Moreover, feminist econometricians tend to be quite cautious about the explanatory power of models, often being aware that more is needed than just regressions with observable variables in order to explain gender differences.

In a recent paper (van Staveren, forthcoming), I have reviewed modelling work on unpaid labour and the care economy. Of course, modelling is not an end in itself or a substitute for theory (although for some economists there is hardly any difference between model and theory). But given the high status that modelling has in the discipline, as Barker has pointed out, it seems strategically wise not to shy completely away from it, not even in the case of analysing caring, a highly under-measured economic activity. Various feminist economists therefore have expressed a need for experimentation with the modelling of care (Frances Woolley, 1993; Martha MacDonald, 1995; Irene van Staveren, 1999; Sue Himmelweit, 2003) – despite the acknowledged limitations. In a comparison I did of two models, each including a variable for unpaid labour (childcare or a broader variable including domestic work), the feminist model appeared to do more justice to the gendered structure of labour markets and the gender division of labour in the household than the non-feminist model. That is because the non-feminist model assumed high substitution elasticities between paid and unpaid work as well as between male and female labour time. Hence, the only way that model could account for observed inflexibilities and rigidities was by including extra constraints for one or both genders. It did opt for a maximum value for female paid working time of 20 hours a week and a minimum value of 20 hours of female time spent on childcare at home, for women with children. For men, no such limits were set. Such choices by modellers severely limit the explanatory power of the model, as it assumes what needs to be explained and thereby does not allow for changes towards a more gender equal distribution of labour between men and women. In the words of Lawson's example on the emancipatory project of feminist economics, the model excluded such a project from the beginning. The other model, however limited in other aspects, did allow for changing gender relations to some extent, as it modelled low substitution elasticities, reflecting job segregation in the labour market as well as norms and institutions supporting the status quo of the unequal division of paid and unpaid labour between women and men. Admittedly, these needed to be explained outside the model, but at least the choice of low substitution rates made such additional explanation possible, whereas the non-feminist model foreclosed the possibility of emancipation altogether.

My conclusion of the discussion of the models referred to above and several other models of care that I have reviewed is that they may be useful to clarify the *structural* dimensions of economic relationships, including gendered structures. Choices between exogenous and endogenous variables appeared to matter, as well as inclusion of care receivers next to care givers; including care in the savings function (as a substitute for purchased services) appeared to enable one model to show a counter-cyclical trend in women's unpaid labour time; and, finally, the modelling experiences indicated that the care economy can be understood as much richer than only in terms of labour time, by including variables such as caring goods, caring productivity, and substitutability with market goods. Such empirical models remain important in order to bring marginalized topics (such as unpaid labour) and differentiated economic processes (such as the gender wage gap) to the attention of the discipline. Moreover, such models on the structure of gender in the economy often serve to refute commonly held beliefs on the benign effects of markets for all economic agents. To give just one example, the carefully developed regressions with additional institutional analysis, by Stephanie Seguino (2000) on the gender wage gap in the Asian tiger economies has shown that wage discrimination is an important factor behind the export success and economic growth of these countries. Such empirical studies are necessary in the discourse on globalisation, especially in the light of unsubstantiated views held by well-known trade economists such as Jagdish Bhagwati (2004, in particular chapter 7 on women), on a presumed equalizing impact of global trade.

But models appear to be very limited, in line with Lawson's claims, when we are interested in learning about the *causation, dynamics, and meaning of these relationships* in the economy, which relate to motivation, reasons, beliefs, and interaction effects of economic agents. But perhaps we should rather give up the ideal of (full) explanation and use models pragmatically to explore possible relationships between gendered variables, than dismissing models wholesale.

Finally, let me say a few words on the possibility of feminist econometrics as suggested by Brigitte Bechtold (1999). She starts by listing ten practices in econometrics that she labels as non-feminist, including the violation of random sampling for gender differences (as well as class and other differences), the emphasis on monetary variables, and the use of dummy variables as a way to accommodate gender differences. She argues that some types of modelling do better than others, while she deems time-series analysis as particularly problematic. But, she does not imply that we should discard econometrics, but rather use it more carefully and with more attention to data gathering. "While bulky and seemingly detailed simultaneous equation macromodels as well as VAR [vector autoregression, IvS] models have major shortcomings in terms of inclusiveness, and the basic classical regression model is lacking in stochastic qualities required to apply limiting theorems, all is not lost" (Bechtold, 1999: 49-50). Instead, she recommends eight 'feminist econometric habits': look for higher t-values; use limited dependent variable methods; avoid technical corrections for serial correlation (they may hide misspecification); avoid dummy variables; use survey and experimental methods; link to findings obtained in other disciplines; avoid re-affirming the status quo; and replace deductive hypothesis testing with inductive methods of analysis. Interestingly, this last recommendation comes close to Lawson's recommendation of contrastive explanation as an alternative to formalistic modelling – apparently, modelling and inductive methods are not necessarily mutually exclusive. Bechtold even suggests that deduction can be done through formalistic models, applying mathematical proofs².

Conclusion: from Realism to Feminism ... and Back?

Lawson's chapter nine has triggered an important debate among feminist economists, a feminist philosopher, and Tony Lawson, on the importance of realism versus epistemology, the balance between universalism and relativism, and strategic choices for the emancipatory project of feminist economics. Various issues were resolved, as they appeared merely misunderstandings or partial interpretations – for example the role of culture in knowledge construction (not only a hindrance but also productive) and the role of humanism (about human capabilities rather than common interests, needs, and motivations) in realism. Also, the dialogue has shown that much of what Lawson had proposed to feminist economists is already wide-shared practice among many feminist economists. Other issues, however, remain unresolved, in particular because feminist economists reject, more strongly than Lawson does, universalist conceptions of human beings and human agency.

On modelling, the participants in the dialogue largely agreed with Lawson's realist/ontological critique, but the point was also raised that modelling has high status in the discipline as it is driven partly by modellers' beliefs. Therefore, it remains necessary, next to critiquing modelling as a masculinist practice with limited explanatory power, to engage in theoretical and empirical modelling and the discourses that surround these two forms of modelling. Several suggestions were made how this can be approached in a gender-aware and generally more inclusive way.

Finally, a note on the tone of the dialogue and the various misinterpretations that contributed to the somewhat tense tone. It seems to me that part of the critical tone of the dialogue may be arising from a, at least seemingly, one-way interest by Lawson in linking realism and feminism: from realism to feminism, and not also the other way around. Feminist theorists, and particularly feminist philosophers like Sandra Harding, appear to have something valuable to say on realism with implications for realism. In particular in issues of epistemology. Moreover, feminist

economists working on the philosophy of economics, may also have something to contribute to Lawson's critical realism project in economics, for example in the area of identity and agency, as well as from their analysis of caring in economics. Lawson's message in his chapter on feminism is clearly one of urging feminist economists to learn from realism, and not about what realism may learn from feminism. This one-way approach of his critical realism project towards feminism does not stand alone, as he has published similar articles on the relationship from realism to Post Keynesianism (Lawson, 1999b) as well as to institutional economics (Lawson, 2001). This is not to say, of course, that such urging of heterodox traditions in economics to consider more explicitly a certain philosophy of science would not be relevant or legitimate – we have probably seen too little engagement with philosophy in economics over the past decades. But there appears to be an implicit request for a stronger commitment to mutual learning in the dialogue on Lawson's chapter and which should not be ignored, in particular if both strands of thought – realism and feminism – are to benefit from future dialogues.

Notes

1. The volume includes, among other things, a review of work on consumer theory by an early twenty-century economist; a literary analysis of the notion of efficiency; a political-economy study of wage setting; a critical reading of Adam Smith, revealing the construction of masculine identity; a demonstration of cultural biases in social statistics; two studies of the identity gap in rational economic man; four chapters analysing caring and unpaid labour as economic activities; and various chapters focusing partly or completely on the importance of a postcolonial or subaltern perspective for feminist theorizing in economics.
2. "A mathematical proof using induction uses three steps: (a) prove the proposition at hand for the first element, (b) prove it for consecutive representative elements 'k' and 'k+1,' and (c) draw the conclusion that it holds in the sample or population under consideration" (Bechtold, 1999: 51).

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Symposium on Reorienting Economics

Some Comments on Lawson's Reorienting Economics: Same Facts, Different Conclusions

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I welcome the opportunity to reflect on Tony Lawson's *Reorienting Economics*. 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, eds. 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 (p. 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 yields 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 (Goldfarb 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. 382-88). 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 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 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 those 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” (Hands 2004, p. 13; 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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A Post-Autistic Introduction to Economic Behavior

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In past issues of the *post-autistic economics review*, writers have pointed out the many failings of contemporary economics education and research. A variety of alternatives in instruction have been suggested, including a greater emphasis on economic problems (rather than technique) and a wider conceptual base. But how, in fact, can this be implemented?

Take, for example, the idea that people are actually much more psychologically complex than the self-interested rational choice-makers that populate neoclassical theory. How could this be explained, in simple language and with emphasis on economic implications, to students at the start of their economic studies? And since instructors will, at least in the near future, continue to have to incorporate at least some of the neoclassical model—even if only to help their students avoid being duped—how can that model be incorporated? We venture the following passages as an example of how this may be done, and welcome your comments.

These passages are abridged from “Chapter 2: Economic Actors and Organizations” of *Microeconomics in Context* by Neva Goodwin, Julie A. Nelson, Frank Ackerman and Thomas Weisskopf (Boston and New York: Houghton Mifflin, copyright date 2005, available as of August 2004). Reproduced by permission. More information on this textbook and related teaching materials may be found at the Global Development and Environment Institute website, <http://www.ase.tufts.edu/gdae/>. The authors may be contacted at gdae@tufts.edu.

Motivation and Behavior

Economics is a social science--it is about people, and about how we organize ourselves to provide the means for life and its flourishing. Ultimately, all economic behavior is human behavior. What motivates people, and how do these motivations translate into economic behavior? Economists generally make an assumption of purposeful or instrumental behavior. Such behavior is motivated by particular goals, and actions are undertaken as means to those ends. Most often, we assume that these goals are conscious and—at least from a person’s own perspective—are intended to advance individual and/or social well-being.

Intrinsic and Extrinsic Motivations

A first distinction to be drawn concerning goals is to note that people act from both extrinsic and intrinsic motivations.

We say that an action is extrinsically motivated, or motivated by “outside” forces, to the extent that the action is taken for a reason that lies outside of a person’s character and his or her relation to the activity itself. Usually these reasons have to do with either reward or punishment.

Money is obviously one of the primary extrinsic motivators. You may work, run a business, make a deal, or study economics because you believe these activities will bring you financial rewards. Besides having financial motivations, people may also undertake activities because they fear the consequences of doing otherwise, or in the hope of gaining some other extrinsic reward, such as high social status or increased power. People frequently use extrinsic motivators to try to change the behavior of others. Economists talk about the various incentives set up by systems of reward and punishment. Employers offer monetary bonuses or “employee of the week” certificates to encourage good work. The government offers tax rebates to encourage energy conservation and fines the worst polluters. A university may tie scholarship money to

maintenance of a certain grade point average. In all these cases, the organizations are relying on monetary or nonmonetary incentives to change behaviors by acting on extrinsic motivation.

Traditionally, economists have paid a great deal of attention to incentives, and to financial incentives in particular. Because of this emphasis, economists are often able to point out where incentives exist and may have effects on behavior, even when the incentives have been created *unintentionally* and are unnoticed by other analysts.

For example, suppose a civic group is concerned about teens who don't finish high school, and so it creates a center for dropouts. The center offer dropouts individualized instruction, paid child care, and a weekly monetary stipend. The civic group's intent, of course, is simply to support dropouts and help them finish their schooling. But what *incentives* does this create for those students who are still in school but are considering dropping out? These current students will have an increased incentive *to drop out* in order to qualify for the center's greater benefits. In this case, creating a program to solve a problem could cause the problem to increase! The civic group might do more good by devoting some of its resources to improving the support services provided at the school itself.

The attention that economists give to incentives can play a valuable role in evaluating the wisdom of various policies, whether in communities, businesses, or elsewhere. The focus on extrinsic motivations and financial incentives needs to be put in context, however, by also considering other reasons for people's actions.

People are intrinsically motivated, or motivated by "inside" forces, to the extent that the reason for action lies in the person or in the activity itself. Intrinsic motivations include direct enjoyment of the activity itself, as well as ethical values such as honesty and loyalty. They also involve issues of identity, such as the feeling of "who you are" or "what our organization is about." Intrinsic motivations are what make you *want* to do something, without respect to rewards or threats from the outside.

You may produce a superior economics term paper because you enjoy learning or because you feel you "owe it to yourself" always to do your best. A government employee may resist a bribe because it is the honest thing to do. Most people choose their work partly on the basis of extrinsic motivations like money and status, but also partly on the basis of intrinsic motivations concerning what they like to do, what kind of person they want to be, and what kind of mark they want to leave on the world. Often both extrinsic and intrinsic motivations are at work.

Self-Interest, Altruism, and the Common Good

Whose interests do people care about? In a famous statement from *The Wealth of Nations*, written in 1776, Adam Smith declared, "It is not from the benevolence of the butcher, the brewer, or the baker that we expect our dinner, *but from their regard to their own interest.*"¹

Many people coming after Smith have interpreted these words in a special way. They have assumed that if people in an exchange economy just follow their own self-interest, acting in the way that most benefits them as individuals, the goal of societal well-being will follow automatically. Many economists of the 20th century read Smith's words out of context and saw them as clever proof that there is no need to for people to think "benevolently" about each other or about society as a whole. This has been used as an ethical justification for following unfettered economic self-interest.

Adam Smith, among others, would have disagreed with this extreme view. (His other most notable work, *The Theory of Moral Sentiments*, addressed at great length the need to take into

¹ *The Wealth of Nations*, 1776, Book I, Chapter 2. Emphasis in original.

account the welfare of others). Exchange may fail to promote social well-being for a number of reasons. People may be badly informed. The situation may entail positive and negative externalities not taken into account in individual self-interested decisions. And, as also pointed out by all major philosophical and religious teachings, purely self-interested decisions are often at odds with basic ethical concerns.

The opposite of pure self-interest would be pure altruism. In this case, you simply desire to help other people, with no thought about yourself. A soldier who throws himself on a grenade to save his comrades or a mother who pushes her child out of the way of an oncoming car and is crushed herself are classic—and extreme—examples of altruism.

Perhaps more relevant to economics is the fact that much economic behavior may be motivated by a desire to advance the common good—the general good, of which one's own interests are a part. Advancement of the common good means seeing your own well-being as connected to the larger well-being of society. For example, even as children we find that learning to share, and not always grabbing or whining for the best toy, leads to more prolonged games and a much more pleasant social environment for everyone—including us. Social theorist Howard Margolis points out that in many social situations people act according to a rule he calls being "neither selfish nor exploited." That is, people are often willing to participate in the creation of social benefits, as long as they feel that others are also contributing.

More and more, economists are realizing that a well-functioning economy cannot rely only on self-interest; it also depends on a culture that includes taking into account the common good. Without such values as honesty, for example, even the simplest transaction would require elaborate safeguards or policing.

If everyone in business cheated whenever they thought they could get away with it, business would grind to a halt. If everyone in the government took bribes, meaningful governance would disappear. In addition, people have to learn to work together to overcome problems of externalities. In regard to children or the ill, who cannot take care of themselves through market exchange, some "benevolence" is obviously in order as well. Self-interest may indeed, in some cases, serve the common good, but it cannot be the only motor for an economy that serves the well-being goals of the society. Indeed, self-interest alone cannot even be efficient. Imagine if you were afraid to put down your money before having in your hands the merchandise you wished to purchase—and the merchant was afraid that as soon as you had what you wanted you would run out of the store without paying. Such a situation would require police in every store—but what if the police also operated with no ethic of honesty?

Fortunately, recent experimental research on human behavior demonstrates that people really *do* pay attention to social norms, and they are willing to reward those who follow the norms and to punish people who violate them, even when this has a cost in terms of their narrow self-interest.

In the "Ultimatum Game," for example, two people are told they will be given a sum of money, say \$20, to share. One person gets to propose a way of splitting the sum. For example, this first person may offer to share \$10 with the second person, or only \$8 or \$3, and plan to keep the rest. The second person can't offer any input to this decision but gets to decide whether to accept the offer or reject it. If the second person rejects the offer, both people will walk away empty-handed. If the offer is accepted, they get the money and split it as planned. If the two individuals act only from narrow financial self-interest, then the first person should offer the second person the smallest possible amount—say \$1—in order to keep the most for himself or herself. The second person should accept this offer because, from the point of view of pure financial self-interest, \$1 is better than nothing.

In fact, researchers find that deals that vary too far from a 50/50 split tend to be rejected. People would rather walk away with nothing than be treated in a way they perceive to be unfair!

In the context of social relations, even the most selfish person will gain by serving the *common* good, making an offer close to 50/50, and thus walking away with about half the total sum (close to \$10). Players who look only at their own potential gain may end up with their offers being refused, and have to walk away empty-handed.

Concern for the atmosphere we all breathe, and concern about poverty that contributes to crime and violence, are examples of real-world cases in which serving the common good may lead to better living for yourself and your family. In such cases, the assertion attributed to Adam Smith should be turned around: concern for the common good may be the best way of serving your own self-interest!

Habit, Constraint, or Choice?

What did you eat at your last meal? *Why did you eat that, in particular?* Because economists want to explain economic behavior, we need to pay attention to why people act the way they do. Take a minute and think about your answer to the second question.

Perhaps your first thought was that you had “the usual”—you ate those particular foods because that is what you usually eat. In this case, we would say that your behavior arose largely from habit. Behavior that arises from habit or custom tends to be fairly slow-changing and is often related to social roles, family, cultural institutions, and the like. Your particular eating habits are probably related to, for example, your particular age, sex, and ethnic background and where you grew up. Habitual behavior is often performed repetitively and fairly automatically, without conscious thought. You may think that the only “normal” breakfast in the world is cereal and milk. Or you may think, on the contrary, that the only “normal” breakfast is rice and fish. In neither case have you given a lot of thought to what you eat.

Or perhaps you explained your eating in terms of “what the cafeteria was serving” or “what I could afford.” In this case, we would say that your behavior reflected the constraints that you faced. You may have wanted to eat something quite different, but you faced limits on your behavior. In a small way, someone else had power over you. The cafeteria manager’s decisions strongly determined your behavior. You knew the police would arrest you if you left a restaurant or grocery store without paying. In this case, the level of your economic resources was important to your behavior. The more you have—in terms of time, money, and transportation—the more you can go where you want and eat whatever you want, freer of constraints.

Or, lastly, did you think carefully about what you were going to eat, making conscious choices between one item and another, based on factors like your personal taste preferences, your goals concerning weight, and/or what you know about nutrition? This would be an example of choice behavior, in which the important factors are your motivations, your knowledge, and your decision-making capabilities.

Actual behavior may arise from habit, constraint, choice, or combinations of all three factors.

Rationality, Goals, and Information

Traditionally, economists have tended to be especially interested in choice behavior. Given this emphasis, the question “How do people choose?” arises. Economists generally assume that people have the capacity to make rational choices.

In common speech, when we use terms like “rational” or “reasonable” to describe an action, we mean both that the *goal* of the action is rational and that the *process* leading to the action was intelligent, appropriate, and thoughtful. It is not particularly rational, in the sense of “sane,” for example, for a person to base all his actions on the goal of being a rock star if he has no talent, or to have a goal of committing a heinous murder. These goals would generally be

considered crazy because they are not related to achievable states of personal and social well-being. Or an action could be judged irrational if the goal is reasonable but the actions taken are not. For example, it is not irrational for a person to have a goal of maintaining a healthy and attractive body weight. Yet a young woman suffering from the mental illness of anorexia may act on the basis of a belief that her body looks grotesquely fat, while in fact she is emaciated. The anorexic's weight loss may be based on the underlying goal of wanting to be attractive, but in fact her judgment is distorted by a neurotic perception.

Do choices that are rational, in the sense of deriving from a thoughtful and appropriate process, *always* lead toward the desired goals? Perhaps not. Because the information base on which we make our choices is imperfect, and because the processes of human reasoning and group decision-making are also often imperfect, we can only say that rational choices will normally *be expected to* move individuals and organizations towards their goals. Rationality means that people weigh the costs and benefits of alternative actions, relative to their goals, when faced with a significant decision—not that people always make perfect decisions.

Optimization vs. Bounded Rationality

We have worded our discussion of rationality rather carefully so far, trying not to claim too much. However much more ambitious claims have sometimes been made, making this a rather contentious topic. In particular, rational behavior is used in the traditional model, as we will soon see, to mean behavior that *best* moves a person towards his or her goals. This kind of behavior is called optimizing.

In 1978, Herbert Simon, a psychologist, won the Nobel Memorial Prize in economics by zeroing in on the question of information, with some surprising results. He pointed out that optimization is normally not possible for human beings, because it requires making the best decision out of the entire universe of possible choices. "Universe" here does not mean planets and stars, but rather the largest possible imaginable set of choices. Your "universe" of possible breakfasts, for example, includes everything from cereal to snake meat.

Under most circumstances it is not feasible to gather the information that is needed in order to identify the entire range of possibilities. Could someone at least identify the optimum point at which to cease gathering additional information? Simon showed that complete knowledge is required even in order to identify that optimum point. Moreover, the effort to find out what additional information might be out there, and to gather it, can be very costly in time, effort, and money.

Accordingly, Simon said, people rarely optimize: instead they do what he called satisficing; they choose a level of outcome that would be satisfactory and then seek an option that at least reaches that standard.

Satisficing can be done in a way that resembles the search for an optimum outcome. If an individual finds that the "satisfactory" level was set too low, a search for options that meet that level will result in a solution more quickly than expected, or perhaps even multiple solutions; the level may then be adjusted to a tougher standard. Conversely, if the level is set too high, a long search will yield nothing, and the satisficer may lower his or her expectations for the outcome. Even with such adjustments, however, satisficing is not the same as optimizing.

Another explanation for behavior has been called meliorating, which may be defined as starting from the present level of achievement and continuously attempting to do better. A simple example is the fisherman who has found a whole school of haddock but only wants to keep one for his supper. When he catches the second fish he compares it to the first one, keeps the larger, and throws the other back. At the end of the day, the fish he takes home will be the largest of all those caught.

One result of using melioration as the real-world substitute for theoretical optimization is its implication that *history matters*: people view each successive choice in relation to their previous experience. It is commonly observed, for example, that people are reluctant to accept a situation they perceive as inferior to previous situations. This psychological “path-dependency” (that is, where you are going depends on where you have been) is relevant to feelings about rising prices, and even more so to attitudes toward declining wages.

Satisficing and meliorating may both be included under the term “bounded rationality.” The general idea is that, without surveying all possible options, people adopt some more-or-less arbitrarily defined subset of the universe to consider. In your breakfast decision, you probably limited your choices to a narrow subset of possible foods.

The concept of bounded rationality thus limits the universe to which decision making is to be applied. Within this limited universe, processes such as satisficing and meliorating are rational behaviors that would normally be expected to move people toward their goals.

Now or Later?

One last dimension of motivation and behavior is crucially important. What time frame do people consider when they have the chance to make significant choices about how they are going to behave?

At one extreme, you probably know someone who has the attitude “Life is short, and tomorrow is uncertain, so let’s have a good time now.” Economists would tend to say that this person has a very high time discount rate, meaning that in his or her mind, future benefits are very much discounted or diminished, when weighed against the pleasures of today. Such an individual will tend to save little, spend a lot, and not expend much effort worrying about the future.

You might also know people who seem to live by the attitude “I’ve got to work hard and prepare now; enjoying myself will have to wait for later.” Economists would say that people like this have very low time discount rates if by their current work they are gaining benefits for tomorrow. The later benefits loom large (that is, are *not* “discounted”) in their decisions. Such individuals will tend to scrimp and save and expend a lot of effort planning for the future.

Time discount rates are important in all sorts of situations. Economists usually assume that anyone investing in a college education has a relatively low time discount rate, since present pain is involved in forgoing income or relaxation in order to study for some expected future gain. Company leaders with high time discount rates may concentrate on making this quarter’s financial statement look good, whereas those with more concern about the future will look toward longer-term goals. In deciding on environmental regulations, people working at government agencies are forced to make decisions about how much weight to put on the welfare of future generations. The lower the adopted discount rate, the more important safeguarding the well-being of future generations appears.

There is no one “right” time discount rate. An extreme disregard for the future is probably irrational in most cases. But in some circumstances—say, for a person diagnosed with a fatal disease, or who faces a high probability of being killed in street violence—it may be understandable.

Extreme concern for the future is also irrational if it means that an individual never gets around to enjoying the benefits of his or her labors, during a whole lifetime. However, strong arguments can be made for taking the future very seriously when discussing actions with

significant multigenerational consequences, such as environmental policies. The question of “now or later” is important in many economic decisions.

Economic Actors in the Traditional Model

Traditional economic theory focuses on a simple mechanical model of economic activity where the central economic actors are assumed to be profit-maximizing firms and utility-maximizing households. It is worth exploring how this model differs from the broader “contextual” approach we emphasize here.

Only Individual Actors

In the traditional, neoclassical model of economic behavior, the economy is assumed to be made up of *only* individual economic actors. “Firms” are treated as though they simply and seamlessly absorb information, make decisions, and take action. Similarly, “households” are treated as though they were uncomplicated individual persons. Little attention is paid to the people and modes of organization—custom, consent, administration or exchange—that make up these entities *as organizations*. Nor is much attention paid to any ambiguities or conflicts that might arise in coming to decisions, or to how the entities might influence each other (for example, the effect of firms’ advertising on households’ preferences).

Rather than envisioning layers of organizations mutually evolving over time, the traditional model portrays only well-defined entities interacting at arm’s length.

A Limited Set of Activities

The traditional model of the economy includes just three economic activities and makes strong and limiting assumptions about the relationships between activities and actors, as follows: Production, accomplished by firms; exchange, performed in markets; and consumption, done by households. The usual “circular flow diagram” illustrates the workings of the economy portrayed in this model. But such a portrayal leaves out some key actors and activities.

For example, the natural resource base of the economy does not appear in the traditional circular flow diagram. Because of this, the economy portrayed is a little like a “perpetual motion machine,” in that the economy it portrays can apparently keep on generating products forever without any inputs of materials or energy. The necessity of resource maintenance activities is ignored.

Likewise, the governments are absent, as though an economy could function without laws, public goods, and other public services. Community groups and non-profits are also absent. Although households appear, they are assumed not to be involved in production activities. Transfers (one-way distribution, in contrast to two-way exchange) are not mentioned.

Restrictive Assumptions about Behavior

In the basic traditional model, actors are assumed to be extrinsically motivated. That is, people are assumed to work, produce, and engage in buying and selling purely for financial gain (unless explicitly noted otherwise). Purchases of goods and services for personal use are assumed to be made because these will bring consumers “utility” or satisfaction of their desires. Even today, the phrase “he was economically motivated” is usually interpreted as meaning

motivated by money or profit. These goals are taken for granted; in contrast to how we use the term “rationality” in common speech, economists do not question the rationality of agents’ goals.

In a further simplification—which is enormously useful in facilitating mathematical modeling—it is assumed that all behavior is purely motivated by self-interest (except in rare and special cases, which are explicitly noted). The model also focuses on choice behavior and ignores behavior determined by habit or outside influence.

Finally, the model applies a rather extreme definition of rationality and an even more extreme assumption about the informational basis of choice making. Rather than construing rational processes just as something that helps actors move in the direction of their goals, the model assumes that actors behave with perfect rationality. Actors are assumed to choose the absolutely *best* action—the one that actually does maximize profit (the assumed goal of firms) or utility (the assumed goal of households). This idea of rationality can, in mathematical treatments, be reduced to precise statements derived from logic. The basic traditional model also assumes perfect information. In spite of Simon's work on *satisficing* in the late 1970s, it was only near the end of the 20th century that many economists began to consider more carefully the problems caused by costly and/or incomplete information.

The assumptions of extrinsic motivation, self-interest, perfect rationality, and perfect information are simplifications that make it possible to construct many elegant theories. But in order to do justice to real world economies, we will also need to develop tools for analysis for cases where these assumptions do not hold.

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Sweden Debates the Future of Economics' "Nobel"

In the Sunday October 10th issue of Sweden's number one newspaper, *Dagens Nyheter*, a debate was opened by *PAER* contributor Peter Söderbaum regarding the future of the Bank of Sweden's Award in Economics in memory of Alfred Nobel.

Dagens Nyheter, DN-Debatt, Sunday October 10, 2004-10-10

The Nobel Prize in Economics – barrier for new thinking

Either the award should be withdrawn or it should be admitted that the presumption of economics as value-neutral is false, writes professor of economics.

Most Nobel Prize winners in economics belong to the neoclassical school. But many are those who question the dominance of this theory with its narrow-minded focus on economic growth and markets. The Prize in Economics has become an obstacle for new perspectives. One possibility is to withdraw the prize, the other is to admit that economics has ideological content. In the latter case, the economics award should be treated as being in the same category as the Peace award, writes Peter Söderbaum.

The Nobel Prizes in physics, chemistry and medicine are not uncontested but have become reasonably respected. Lately, the Bank of Sweden's award in memory of Alfred Nobel has been added and instituted. It has been argued that economics is an established discipline comparable to physics and chemistry and with similar ideas of good science and scientific progress..

Economists can refer to a distinct paradigm, that is a clear theoretical perspective. The tendency is to stick to this perspective, and today there is a monopoly position for neoclassical economics at almost all university departments of economics. Its theories are useful for some purposes, for instance, as a way of understanding financial and monetary policy.

Confronted with the present challenges related to sustainable development, the limitation to the neoclassical paradigm is a problem. Viewing humans as consumers maximizing their self-interest is not very constructive if you wish to discuss issues of environment and survival. Focusing on profit maximization in business will not make it easy to understand the present debate about corporate social responsibility, environmental certification of organizations and similar phenomena. Interpreting economic phenomena and relationships in terms of markets and prices and monetary indicators is not always a good strategy.

Neoclassical economists can of course continue to refer to their conceptual framework and turn their arguments in the best possible way. But a problem that they cannot get away from is that economics, just as other social sciences, is both science and ideology. As an example, focusing on the role of consumer and her self-interest is not neutral in value terms.

One of the scholars who received the Bank of Sweden's Award in Economics in memory of Alfred Nobel, Gunnar Myrdal, repeatedly argued that "values are always with us" in our research. This being the case, it becomes problematic from a democratic point of view to stick to one and only one paradigm at a university Department of Economics. The ideological features and character of this paradigm mean that the department plays a role of political propaganda centre; "human beings are consumers, forget about other roles as citizen, professional..." "anything connected with business can be reduced to a matter of maximum monetary profits", etc..

The solution to this is a pluralistic attitude, that is, open-mindedness to different possible theoretical perspectives compatible with different valuational or ideological

points of view. Just as economists otherwise celebrate competition, this should also be applied to their own discipline. The Bank of Sweden's Prize in Economic Sciences in memory of Alfred Nobel unfortunately has become an obstacle for new thinking.

Even prize winners that present and support theories that could be connected with a criticism of neoclassical theory, for example Douglas North and Amartya Sen, tend to – for tactical reasons, it seems – profess themselves adherents to orthodoxy. Gunnar Myrdal is the exception among prize winners, with his outspoken criticism of the mainstream and clear declaration in favour of institutional theory.

Today a lot is happening internationally in developing institutional theory, social economics, feminist economics, ecological economics, etc.. Ecological economics can be described as “business management and economics for sustainable development” and in this field neoclassical theory holds a minority position.

Adhering to neoclassical theory with its focus on economic growth in GDP-terms is perceived by an increasing number of people as ‘unsustainable’. For several years this has been a focus of the *post-autistic economics review*, which also stresses the limitations of mathematics as a language for economics. Books are now being published, with contributors from many countries, that caution new students in economics about the narrow-mindedness of the textbooks they are exposed to.

The problem is that these textbooks legitimise simplistic thinking about economic growth and markets in a situation where instead a multidimensional and ethically open analysis is needed. To systematically propagate this simplistic economics to countries such as Russia and China is irresponsible.

Against this background, one possible way of acting is to withdraw the Prize in Economics in memory of Alfred Nobel. The alternative is to admit that economics much like other social sciences has a specific ideological content and therefore belongs to the same category as the Peace Prize.

This would make it natural to return to the term ‘political economy’, the language used in the 19th century. It would also make it clear that the project to develop a ‘pure’ economics has been a failure.

With this change, the idea becomes one of identifying potential winners of the prize who, through their research and other actions, have contributed something to humanity. Choosing people who claim to have developed models useful for predicting shareholder values would then become more difficult.

I am sure that there are those who see markets of different kinds as the salvation of the world, but there also are quite a number of citizens who are less enthusiastic about market solutions.

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