Edward Fullbrook (ed.) *(2009) Ontology and Economics: Tony Lawson and His Critics;* Routledge, London and New York

Featuring: *Tony Lawson*
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Chapter 12

On the Nature and Roles of Formalism in Economics

Response to Hodgson

Geoffrey Hodgson and I have debated numerous issues over the years (not least the nature of old institutionalism and how its contributions are best interpreted), mostly in seminars or over a drink or two. Whether we agree or disagree, I find that Hodgson's views are always interesting and usually very substantial. The issue of formalism seemingly opens up a new area of debate between us. In characteristic style Hodgson's various contentions in his current piece are wide-ranging, thought provoking and backed up by numerous authoritative sources. Thus, it is necessary to do quite a bit of work to show why I nevertheless think Hodgson fails to make too much of a case against my position.

Hodgson's main thesis is that my critique of mainstream mathematical modelling is overly restrictive in the way I imagine models to be useful. Formalistic models can provide insight in numerous ways that I fail to contemplate, and Hodgson is concerned to indicate some of these.

This topic of the alternative roles for formalistic models is actually something I explore at length in *Economics and Reality*. Its consideration takes Hodgson to discussions of methods of abstraction, theoretical isolation, heuristics, and so forth, all issues on which I have previously found a need to say much, but which do perhaps get too little coverage in economic methodology in general. As it happens, I believe that much of what Hodgson has to say on these issues cannot be sustained. But his commentary does provide an opportunity to give my take on these neglected issues a further airing, and perhaps to develop them further.

There is a further related, if ultimately secondary, issue I should also address. Overall, I think there is a danger that Hodgson's contribution conveys the (false) impression that I am opposed to formalism *per se*. This is such a misunderstanding that I need first to get it out of the way before addressing the central topic here, the manner in which formal models might usefully be employed.

Orientation to formalism

Whatever Hodgson's intention, it seems to me likely, as I say, that the dominant impression conveyed by his piece is that my stance is somehow an anti-mathematics one. Hodgson does not attribute such an orientation to me directly; in brief asides, indeed, he even acknowledges that this is not an accurate statement of my position. However, he pursues several lines of argument that, I fear, will mostly encourage the uninformed reader to suppose that I am after all opposed to the use of mathematical formalism *per se*. Little could be further from the truth. Rather I am opposed to the abuse of mathematical formalism, and such abuse is, I believe, typical of the situation in much of modern economics.

What line of argument of Hodgson's do I have in mind? At the start of his commentary Hodgson reproduces a passage by Mark Blaug of which I have previously made much use. In it Blaug suggests that "Modern economics is sick", noting that "Economists have converted the subject into a sort of social mathematics in which analytical rigour is everything and practical relevance is nothing" Mark Blaug (1997, p. 3))

Now the reason I quote this passage is simply that I both agree with it, and I find it significant because Blaug once seemed sympathetic to the mainstream. However, Hodgson asserts that Blaug and I make different evaluations, with my position being "more radical". My own view, though, is that, to the contrary, Blaug's position is simply a summary of a critical assessment that I have spent much time defending. Indeed, I would describe my own position on the use of mathematics as anything but radical; it is simply informed by an appreciation of mathematics. I neither support its universal application in economics as an *a priori* stance nor its universal rejection as an *a priori* stance. Rather I recognise that all tools, including mathematical ones, are limited in their scope of application (I believe that the appreciation of a tool is bound up with an awareness of its limitations), and that the relevance of mathematics in any specific application depends on the context-specific merits of the case.

I am perplexed, therefore, to find that Hodgson portrays me as an extremist on such matters[1]:

> "In regard to formalism, many economists take the extreme view that it is the only 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 opposite 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 […].

And Hodgson ends with the following plea[2]

> "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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[1] In this way, Hodgson claims the middle ground for himself without having to do the ontological groundwork. Thus having noted these supposed extremes Hodgson associates himself with "middle ground solutions" on such matters, and connects this back to his original reference to the passage from Mark Blaug:
"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 at 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 bring concerns for practical relevance to the fore. Formal techniques should become the servants rather than the masters of scientific enquiry"

[2] In a recent presentation at the Cambridge Realist workshop this passage is revised to read as follows:

"We need to investigate the inadequately explored middle ground between unacceptable extreme stances that either treat mathematics as the sine qua non of theory, or practically exclude it in all but 'rare' cases. I advocate this middle ground position, not because central areas are intrinsically or universally superior to extremes, but because the extremes in this case jointly downplay the necessary interpretative structure of any formal theory"

Hodgson's talk here of extremes is misleading if not incoherent. At one supposed extreme is the view that formalism "is the only means by which economics becomes rigorous and scientific"; at the other is the view that the use of "formalism is justified in 'rare' circumstances only". Clearly, such views need not even be incompatible let alone opposite extremes. Of course, I have argued extensively against the former view that formalism is essential to science or rigour. But one could (mistakenly) hold to such a view (that formalism is after all essential to science), conclude that formalism has but limited application to the social realm, and infer thereby that a scientific economics has limited scope.

As I say, this is not my position (my own position to the contrary is that a science of economics is entirely feasible whatever the scope for formalism[3]). But it is logically feasible, and indeed is accepted by many hermeneuticists and others who do accept the implicit conception of science in question, but reject its relevance to the study of society (see Lawson, 1997, chapter 10).

In truth the (mainstream) view that formalism "is the only means by which economics becomes rigorous and scientific" is just a mistake, not an extreme view or form of practice. The real sense in which the mainstream is extreme lies in its insistence that we all must always adopt mathematical deductivist methods of a certain sort, irrespective of how explanatorily successful they are (in potential or practice). If there is an opposite extreme to this it is the insistence that such formalistic methods be always rejected irrespective of how explanatorily successful they are. I doubt anyone accepts such a position.

Why then does Hodgson suggest that I am at an opposite extreme to the mainstream? I have to admit to being unsure about this. Perhaps I have just not expressed myself with sufficient clarity.

It is true that, in attempting to identify conditions under which certain methods of mathematical deductivist reasoning are *guaranteed*, I have suggested that they seem

[3] Thus in Reorienting Economics I write:
"For many modern economists, in fact, mathematical deductive reasoning is regarded as essential to science. It follows, for this group, that to relax the existing emphasis on mathematical methods is effectively to give up on the possibility of economics as science.

One reasonable response to this line of reasoning is to question why economics has to be a science. Many critics indeed have rejected the possibility of economics as science. But in fact this is not my orientation. To the contrary, my concern is more with the recovery of economics as science. Ontology helps us better understand the nature of science, and, as we shall see, to appreciate that mathematics is not essential to it. To suppose that scientific practice reduces to, or even necessitates, mathematical formalism is once more erroneously to universalise *a priori* a special case (in this case a particular form of scientific practice) as we shall see in due course" (Lawson, 2003, pp.xx-xxi).

rarely to have come about in the social realm. But this is a long way from an *a priori* rejection of formalism, from an insistence that formalistic methods be rejected irrespective of how explanatorily successful they are.

Actually, it may be useful if I elaborate a bit on this issue. For although it seems to me that the orientation I adopt is reasonable, Hodgson actually interprets it as setting limits to what he calls "my claim of anti-dogmatism". Perhaps this explains his misunderstanding. Let me briefly elaborate.

Dogmatism and identifying the conditions of closures

First, I must examine what is meant here by dogmatism, or dogma. I do use the term dogma in *Reorienting Economics*, though only occasionally. In fact, I use it twice, in each case to describe the doctrine, advanced by mainstream economists without grounds, that certain sorts of methods only should always be followed.

Thus the preface to *Reorienting Economics* I write:

> "Rather, the primary object of my criticism is as stated. In its most general formulation, my opposition is directed at any kind of *a priori* dogma. The realist approach I defend is contrasted with any kind of ungrounded insistence that certain methods only, or almost only, should be followed" (Lawson, 2003).

and in chapter 7 I write:

> "Rather the opponent is the advocate of any form of *a priori* dogma. In the context of modern economics specifically, the primary target is […] the current mainstream *a priori* insistence that formalistic modelling is the only proper, and a universally valid, method for modern economics, along with its effective prohibition on alternative approaches."

No doubt dogma and dogmatism have various meanings. But it is surely clear that in using it to describe mainstream practice I am here simply taking dogma to be doctrine that is not defended but accepted on authority as unquestionable. Dogmatism as I am interpreting the practice, then, is just the act of accepting dogma, that is, of adhering to something on the basis of authority alone and treating it as beyond question or criticism[4]. Clearly, it is something similar to Guerrien's notion of ideology discussed elsewhere in the current volume.

[4] Can it be doubted that the practice of mathematical-deductivist modelling is treated this way in the academy? I suspect Frank Hahn captures the ruling sentiment well when he insists of

Now, of course, in suggesting that the dogmatic stance of modern mainstream economists be rejected I do not want to imply that all doctrines can always be meaningfully challenged[5]. And I am well aware that in acting at any point in time it is practically impossible to subject all beliefs that condition an action to critical scrutiny at that moment. But I do believe that everything that can be challenged should be treated as *open to critical scrutiny in principle* whether or not the latter is forthcoming. And I certainly believe the mainstream insistence that methods of mathematical formalism be everywhere utilised ought to be open to question and criticism by all. As I say, it is the mainstream refusal to consider the possibility that formalistic methods may actually be limited in their scope of legitimate application that I have suggested is dogmatic.

However, Hodgson suggests that my pointing to such limitations reveals the limits of my anti-dogmatism in practice. How does he reach this view? He writes:

> "In these passages [by Lawson] 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"

Hodgson's logic here is not compelling. Let me momentarily leave aside the seemingly emotive topic of the use of formalistic methods in economics. As I say, I am opposed to dogmatism in the form of any authoritative assertion that only one set of research methods can ever be used in economics. Suppose, though, that having

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any suggestion that the reliance on formalism may be problematic that it is "a view surely not worth discussing" (Hahn, 1985, p.18). In fact, Hahn later councils that we should "avoid discussions of 'mathematics in economics' like the plague and give no thought at all to methodology" (Hahn, 1992a; see also Hahn 1992b).

[5] Hodgson seems to suppose that I believe the contrary, but does not say why. Of course I do not. I believe that I have sense experiences that are of a real world in which I live (not, for example, that I am fooled into supposing this is the case by some evil demon that is sending false signals directly to my brain). Yet I do not know how to subject this belief to critical scrutiny.

made this declaration, I also make the suggestion that the conditions under which, say, electron accelerators or stethoscopes are useful in economic research rarely crop up. Would Hodgson maintain that I am thereby once more setting limits to my otherwise anti-dogmatic stance? I doubt it. Certainly I hope not. For in such examples it is surely clear enough that it is neither I, nor my arguments, that limit the use of electron accelerators or stethoscopes to 'rare' socio-economic situations; rather it is the world, the nature of social reality, that sets the limits to the conditions under which the tools in question are useful. Seeking to identify or theorise these conditions has nothing *per se* to do with dogmatism or anti-dogmatism[6].

The point is that the mathematical methods used by economists are themselves just tools. My claim is merely that the conditions in which they are useful seem not to have occurred much in the social realm.

I may be quite wrong in this assessment. But that does not yet set limits to my anti-dogmatism. It would be dogmatic of me, perhaps, if I used this assessment to advise that formalistic methods be everywhere excluded from the economist's toolbox. But I have never suggested this. My position is always that all methods (that anyone seriously proposes) be retained to some degree, not least because even ontological arguments seeking to uncover their spheres of usefulness are fallible.

For example I write in *Reorienting Economics*:

> "In so concluding I am not at all suggesting that formalistic modelling methods should not exist among the battery of options available. All knowledge is fallible, including ontological theorising. And even accepting the ontological perspective systematised and defended in the chapters, which follow, there may yet be some (greater) scope of application for methods of mathematical modelling than so far uncovered. But just as significantly, although ontology can provide some directionality to social theorising, one of its most useful functions, and perhaps its primary one, is actually to open up analysis. This is precisely its role here. My aim with the discussion of this chapter is not to narrow down the range of methodological options by attempting to prohibit a particular method. Rather it is to widen the range of possibilities through criticising the fact that, and manner in which, the particular method in question is currently and unthinkingly universalised" (Lawson 2003, chapter 1)[7].

[6] A radical constructivist might demur, but Hodgson declares himself a realist in the same vein as myself.

[7] Or as I write elsewhere in the same book:
"The last thing I wish to do is support any efforts to prohibit forms of activity, or limit the range of methodological options.

I do not deny that I am rather pessimistic about the prospects of significant success with mathematical methods in economics. In the light of the conception of social ontology

The surprising aspect of all this is that Hodgson's assessment of my position is thought to carry plausibility. To the extent that it is, I suspect it has more to do with some rhetoric that likely misleads. Consider the final sentence of his passage reproduced above. Here Hodgson writes:

> "Although Lawson imposes no absolute normative ban on the use of mathematics, his arguments limit its legitimate use to 'rare' circumstances only".

I have already noted that it is not my argument but the nature of social reality that serves to limit the legitimate use of any methods. The point to which I want to draw attention here, though, is the first part of the sentence: "Although Lawson imposes no absolute normative ban on the use of mathematics"[8], and in particular Hodgson's use of terms like "imposes" "absolute" and "ban" which seem create a smell of dogmatism. Even use of the term "normative" in this context adds to this effect (it is difficult to imagine a non-normative ban)[9].

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defended in the chapters below, along with the assessment made of the ontological presuppositions of formalistic modelling methods, I find it not at all surprising that these latter methods have fared somewhat poorly. But in acknowledging my pessimism, I do not oppose a share of resources being used in endeavours to study social material mathematically. Amongst other things, all knowledge is fallible, as I have already stressed, so that, in particular, the grounds of my pessimism may yet prove erroneous. Rather, the primary object of my criticism is as stated. In its most general formulation, my opposition is directed at any kind of a priori dogma. The realist approach I defend is contrasted with any kind of ungrounded insistence that certain methods only, or almost only, should be followed. Such an insistence seems especially unfortunate when, as currently, the methods laid down persistently perform rather badly, whilst that unhappy performance can be explained (Lawson, 2003, pp. xix-xx).

[8] Whether or not Hodgson is conscious of the fact, by the very act of (unnecessarily) denying that something applies to me he is conjuring up the possibility that it might, or that something similar might. If I started a sentence with the words "Although person X is not an absolute imbecile (or, say, philosophical novice)…" the nature of the expectation/impression I would be seeking to convey is clear. Similarly it is easy to (or rather it is difficult not to) take Hodgson to be suggesting that I impose at least a partial ban on the use of mathematics. And this affects the way in which the remainder of the sentence – "his arguments limit its legitimate use to 'rare' circumstances only" -- is received.

[9] We might also consider the first part of the passage in question. Here Hodgson writes (with emphasis added by me):
"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".
It is the emboldened terms that I wish to draw to attention. I assume Hodgson means "closed" and not "open" in the last sentence. In any case I do not seek to lay down criteria under which something may be used. Rather I seek to identify conditions under which success with the methods in question are guaranteed. That is all. It is these, in my assessment, that may rarely figure in the social realm.

Now I hope it is clear that I am not in a position to impose anything, whether or not I want to; that I would not like to be in such a position; and even more to the point I want nothing banned, whether absolutely or partially. Such sentence constructions mislead and I fear hinder serious argument.

It is true that I argue that regularities (real or imaginary) of the form "whenever event (or state of affairs) x then event (or state of affairs) y" (or stochastic near equivalents) are a necessary condition if formalistic deductivist methods of the sort economists seek are to be utilised. Systems in which these regularities occur I refer to as closed. But I seek to identify the conditions under which such event regularities are *guaranteed*. I may be wrong in my analysis. And in any case, my analysis does not preclude the possibility of their occurring in other conditions. So however we look at it I am hardly laying down criteria for the use of mathematical deductivist methods, merely trying to understand something about the conditions of their likely successes, if any.

Basically I am trying to help clarify the nature of the situation in modern economics. Whether I speculate that closed social systems are more likely, or less likely, or just as likely, in the future than in the past, this hardly counts as an "admission", and again clearly has no bearing on my position vis-à-vis dogmatism.

Successful econometrics?

Now if in *Reorienting Economics*, I observe that examples of successful deductivist, including econometric, modelling seem not to have occurred especially often in the social realm, I do *not* contend that there could never be any limited success stories. Indeed, I suggest possible examples of past successes. However, presumably because he reads me as seeking to banish the use of econometrics, Hodgson seems to regard this as a logical slip. Let me quickly indicate why this too is not the case.

Very briefly, the reason my noting a (conceivably) successful econometric exercise is not a logical slip is simply that there is nothing in the conception I defend that suggests that a closure (or indeed any configuration) could not occur. I may yet be committing an empirical mistake; I may have assessed the econometric contribution to which I referred in an overly positive manner. But this has nothing to do with the logic of my overall argument.

Remember that a closed system is simply one in which an event regularity of sorts occurs. In particular, a closure is not the same as, and cannot be reduced to, the closure *conditions*. Closure conditions are sufficiency conditions, those that *guarantee* an event regularity; they are not strictly necessary for it. In principle, an event regularity could even emerge by accident, with differing causal factors underpinning any set of found-to-be-correlated events[10].

More to the point, remember too that in perhaps all 'applied' or empirical contexts where measurement is involved (including the well-controlled experiment), the relevant form of closure is a *stochastic* one (see Lawson, 1997, p. 76) not a *deterministic* one. A stochastic closure is a system in which a regularity occurs, connecting or covering a set of random outcomes or 'variables'. Specifically a regularity holds between the conditional mean value of any one 'variable' (conditional upon the realised outcomes of the others) and the realised values of the remaining 'variables'. Notably, such a scenario can include a stochastic component capturing not only any measurement error but also the effects of any relevant factors or conditions that are unobserved (or otherwise excluded from explicit analysis).

Clearly if the conditional distribution of the latter component (and so of the dependent or response variable) is small, the regularity involved, because closely approximating a deterministic regularity, will, if successfully identified, and non-spurious, be useful for predictive purposes. It was such a stochastic closure that (for particular reasons set out in *Reorienting Economics*) I was presuming had been (unusually) uncovered in the econometric exercise to which I referred.

Hodgson recalls that in a seminar discussion somewhere, I referred to a local closure being 'approximated in reality', facilitating the use of econometrics. Unfortunately, I do not remember the occasion or context. But I assume that if I made such a remark, I had in mind a stochastic closure closely approximating (or constituting a stochastic near-equivalent of) a deterministic one. The standards of approximation will be those laid down by econometricians.

I admit that I do not expect social reality to throw up very often such conditions as guarantee stochastic closures of this sort (that are not completely spurious[11], and

[10] Of course, as I discuss in *Reorienting Economics*, the latter scenario is unlikely. But however unlikely, such a situation remains logically feasible; it simply does not follow that a closure, even a deterministic one, *could not* occur if generally operative causal mechanisms are not all explicitly accounted for.

[11] Of course, this has not prevented econometricians producing thousands of models. But we all know that, especially with time series analysis, if we experiment by running very many regressions (some econometricians run literally tens of thousands of regressions on a given data set), exploring all imaginable functional forms (or data transformations), it is usually possible to find a within-sample relationship that is close fitting by conventional criteria, whatever the latter may currently be. In this case, a stochastic closure of sorts is identified. But I do not suppose anyone believes many of the results (in the form in which they are presented, whether or not they are published). The theory and assumptions behind the methods of statistical inference employed to identify the relationship are repeatedly contravened (rendering the interpretations typically attached to such empirical results invalid).

employing econometricians own context specific assessments of acceptable approximations); and nor do they seem frequently to have occurred. That is why, as Hodgson also notes, I am able to provide very few examples of apparent successes. But if they occur, or more to the point, if I assess (fallibly) that they have occurred, this *per se* says nothing about the logic of my overall argument

Notice, too, that the position I am defending, despite Hodgson's suggestion to the contrary, in no way encourages the current emphasis on formalistic modelling, even in respect to econometrics. The situation of modern econometrics is not that econometricians are everywhere manipulating criteria whereby stochastic closures might be said to be close approximations to deterministic ones, in order that the current heavy emphasis on formalism wrongly appears justified. Rather this heavy emphasis continues in the absence of widespread successes as measured by these (conventional) criteria, and indeed in the absence of any other apparent justification.

So, in sum, I hope it is clear that by my giving some qualified support for a particular econometric exercise (whether or not the support is warranted in this particular instant), I am thereby neither revealing a logical inconsistency in my argument nor supporting the current *emphasis* on formalism.

But equally, to return to the broader theme (and as my support for the particular exercise just discussed further illustrates), *I am nowhere opposing the use of formalism* per se *either*. As I have repeatedly stressed, my objective in my critical assessments of the state of modern economics, is to understand why certain research methods have generally failed or occasionally (seemingly) proven successful. Whatever else my project may entail, it has nothing to do with seeking to narrow down the range of research practices on offer. Ultimately, indeed, as I have argued earlier, my findings in fact support just the opposite stance, of expanding the options available and recognised as viable.

In addition,, the right hand side 'independent' variables are usually not at all those that can reasonably be claimed to stand in the causal history of the dependent variable. Most telling of all, perhaps, is that as soon as new observations are obtained, previously 'identified' relationships of this sort are almost uniformly found to break down.

Econometricians, of course, know all this (even if they continue with their project on grounds other than realisticness or believability – see Vromen, this volume). Edward Leamer (1978), for example, talks of the inconsistencies in the theory and practice of econometrics, and of the 'priests' who produce the theory to inform econometric practice, and the 'sinners' who misapply the theory in practice. He even notes that these are the same people on different occasions. He notes too that "Sinners are not expected to avoid sins; they need only confess their errors openly" (Leamer, 1978, p. vi). He further notes that, where realisticness is the issue, "hardly anyone takes anyone else's data analysis seriously" (1983, p. 37).

Previous Statements

Perhaps the reader unfamiliar to my contributions will suspect that there is no smoke without fire, that the overall or basic position I am here accepting is so far from that seemingly attributed to me by Hodgson, that I must myself be saying things now that are inconsistent with the broad message of previous contributions. In case this is so, I hope I can be permitted to reproduce here a few (of the very many) relevant passages.

In *Reorienting Economics* I write:

> "Let me elaborate a little on my orientation to formal modelling. Although parts of this book, and most specifically chapter 1, are critical of the way formal modelling methods are taken up in modern economics, I hope by now the highly conditional nature of my criticism is apparent. It is not, and has never been, my intention to oppose the use of formalistic methods in themselves. My primary opposition, rather, is to the manner in which they are everywhere imposed, to the insistence on their being almost universally wielded, irrespective of, and prior to, considerations of explanatory relevance, and in the face of repeated failures" (Lawson, 2003, p. xix).

Elsewhere in a paper in the *Journal of Economic Methodology* I am just as explicit. Here I write (in response to comments by Julian Reiss):

> "*My argument is not at all an anti-mathematics one; and it never has been*. I have only ever criticised the way (certain) mathematical methods tend to be used in modern economics"[12] (Lawson, 2004, p. 337; emphasis in the original).

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[12] The broader passage forming the context in which the passage in the text is set reads as follows:
"Notice, too, that I do not suggest that the methods of mathematic-deductive modelling which mainstream economists wield can never have an appropriate application. To the contrary, I suggest in Reorienting Economics that the perspective I adopt can identify the sorts of conditions in which success is likely and so explain such successes as occur (see e.g. p. 20). And nor do I suppose that mainstream economists do nothing but mainstream economics; indeed I expressly deny that this is so (see e.g. p. xxi) [….]
Let me […] [stress] that I fully agree that 'there is no necessary link between mathematics and bad practice'. Indeed, I cannot emphasise my agreement with this statement too strongly. *My argument is not at all an anti-mathematics one; and it never has been.* I have only ever criticised the way (certain) mathematical methods tend to be used in modern economics. Indeed it is precisely the belief that mathematics ought not to be applied without due care and consideration, coupled with a conviction that in modern economics it too often is so, that explains the direction of much of my writing. If you like, my concern is that much of economic modelling appears somewhat analogous to a violin being used as a drumstick. To suggest that this may be "bad practice" is in no way to devalue the violin, or to deny it a place in the orchestra.

After a lengthy discussion of my views on the use of mathematics in economics I summarise my position as follows:

> "In short, I do not denigrate the use of mathematics. I do, though, believe that the sorts of mathematical-deductivist methods mainstream economists mostly use presuppose an implicit worldview that is not especially typical of social reality. I also suggest that many of the widely acknowledged failures of the discipline arise just because these methods are being applied in conditions for which they are not especially appropriate. In consequence, in *Reorienting Economics* I argue for a more pluralistic orientation to social theorising, and spend time demonstrating that alternatives methods of relevance do exist. In this I do not suggest that formalistic methods be excluded for the methodological options on offer. But I do insist that methods of mathematical-deductivist reasoning (like any other tools) have limits to their usefulness, and that this be recognised and respected. However, I see this as a pro-, rather than an anti-, mathematics position" (Lawson, 2004, pp. 337-339).

I hope I have said things with sufficient clarity, not to mention repetition, to dispel any worry that I am somehow opposed to the use of mathematical formalism as an *a priori* disposition.

The roles of formalistic models in economics

I turn, then, to the matter that I take to be Hodgson's central concern, the manner in which models can be useful to social illumination. Unfortunately, before we can get to points of substantive disagreement, there are, even here, one or two important issues of interpretation on which Hodgson does not get me right.

In developing his comments on how models can be used Hodgson suggests that I supposedly commit the mainstream error of viewing models as somehow mappings onto, or in correspondence with, reality[13]:

Nor do I limit mathematics to deductivist modelling. Actually if what economists do is mathematics, it is a form of applied mathematics; it is the application of mostly already worked out mathematical systems And if it is ever appropriate to "associate" a method with an ontology I see no reason to suggest that some mathematical methods could not be associated with a causalist ontology.

If *I do* not*, then, limit mathematics to methods of deductivist modelling I do* contend that the sorts of modelling methods mostly utilised by modern economists are deductivist in nature […] (Lawson, 2004, pp. 337-8; emphasis in the original)

[13] In a revised version of Hodgson's paper presented at the realist workshop on November 14 2005, the relevant passage is formulated as follows: "Lawson suggests 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 ongoing events. In one passage Lawson […] writes of the importance

> "[Lawson] 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' ".

I *do* write on the idea of models as mappings of, or as corresponding to, reality. But I defend precisely the opposite position to that which Hodgson imputes to me. What I actually write is the following:

> "It seems likely that [many] […] economists are in effect holding, if implicitly, to conceptions of knowledge and truth as relations of identity or, more generally, correspondence[14]. Certainly the widespread use of the terminology of economic models is consistent with this – as if the latter are somehow smaller, scaled down, or otherwise simplified, versions or mappings of the original. Economists frequently talk of 'mappings' in connection with their modelling, often remarking that any map drawn to a scale of one-to-one would be as complicated as whatever it is supposed to represent, and thereby useless. From this perspective it follows that because of the complex nature of social reality any feasible mapping is never isomorphic. In consequence, the argument seems to run, all models are inevitably distortions; true models or theories are most unlikely to be obtained.
>
> Whether or not any economist holds to this line of reasoning completely (or explicitly), or would express things quite so starkly, it is worth considering this position here anyway as an obvious point of reference. And it should be apparent from everything that has gone before that any conception of knowledge and truth along the lines indicated, i.e., as relations of correspondence between discourse and extra-discursive reality, is profoundly misleading" (Economics and Reality chapter 17, p. 238**).**

Rather than employing the misleading imagery of mappings in my argumentation, I refer over and again to the ontological preconditions of formalistic-deductivist methods, and question whether they hold in the social realm. Rarely do I use the word 'fit', and where I do it is clear that this is precisely my meaning in using the term. Even the passage to which Hodgson explicitly refers is followed by one indicating that 'fit' is to be interpreted in terms of the presence of ontological preconditions:

> "But 'fit' with reality matters too. The problem with the mainstream stance is that the ontological preconditions of its formalistic methods appear to be not only not ubiquitous in the social realm, but actually rather special occurrences. If we knew

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of a '"fit" with reality,' suggesting that all theory has to somehow correspond with the real world"

[14] This conception is presumably influenced by the positivistic conflation of knowledge with direct experience and reduction of reality to events and states of affairs given in experience.

both that social life was everywhere atomistic, and also that for any type of outcome we could effectively isolate a fixed set of causes (treating all other causal processes as a kind of stable, non-intervening or homogeneous backdrop), we would have grounds for feeling confident in the emphasis that mainstream economists place on the sorts of deductivist methods they use. However, our best ontological analysis suggests that closures are a special case of social ontology, whilst our a posteriori experience is that this special case seems not to come about very often at all" (*Reorienting Economics,* p. 22)

Actually I use the word 'fit' twice in *Reorienting Economics*. In the other instance I indicate precisely what I mean:

> "As a result the possibility of a lack of ontological fit (a mis-matching of the presuppositions of these modelling methods with [the nature of] those features of social reality being investigated) does not arise. Yet, as I say, methods of mathematical-deductivist modelling, like all methods, do have ontological presuppositions. And my assessment, simply stated (and defended below), is that these preconditions of mathematical-deductivist methods appear not to arise very often in the social realm" (*Reorienting Economics*, p. 12).

Heuristic and internal critique

An equally surprising assertion made under this head is that I concentrate my critique on econometrics[15] and in doing so fail to consider whether formalistic models might be used for heuristic purposes or as vehicles for internal critiques. As much of the discussion below is about, and draws upon, my account of heuristic as set out in *Economics and Reality* I will not add anything on this topic for the moment. But I should perhaps say something at this point about internal critique.

Now an internal or immanent critique (which I actually discuss fairly extensively in *Economics and Reality*) proceeds by way of working from certain internally accepted features of a project or paradigm, and pointing out that they lead to

[15] Hodgson thus writes:
"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"
As it happens, in *Economics and Reality* I address very many different ways in which it might be thought that mathematical modelling can contribute to social illumination. Only one of the chapters (chapter 7) focuses on econometrics. Chapter 9 deals explicitly with defences that accept some goal other than tracking measurable variables, and, unsurprisingly, I do there discuss the possible heuristic role of formalistic models at some length (see below).

problems, inconsistencies, or limits elsewhere within the project or paradigm, conceived on *that projects own terms*[16].

As I say, Hodgson advances the idea that formalistic models might be used for making internal critiques. Now in *the context of modern mainstream economics* no one could presumably fail to recognise this, simply because the mainstream is everywhere couched in terms of mathematical formalism. The insistence on mathematical deductive methods is the essence of that project. The possibility, to which Hodgson points, arises just as a consequence of the mainstream problematic insistence on formalistic methods[17]. Indeed, as most mainstream contributions advance by precisely addressing perceived (internal) weaknesses, or inconsistencies or limitations of preceding contributions, most mainstream contributions themselves constitute or contain internal critiques. There is no great insight here.

Of course, heterodox economists will likely employ formalistic models for purposes of seeking more destabilising critiques than will mainstream economists themselves. And I do not at all want to belittle their achievements on their own terms. Why then do I not join in (more than I do[18])? The answer is simply because I do not think such critiques lead to any necessary advance in terms of illuminating the world in which we actually live. Mostly they lead to the internal shoring up of an irrelevant conceptual system. And even when a framework is radically transformed or replaced, it almost always is so by an equally irrelevant alternative formalistic system. Witness the recent turn to evolutionary game theory, agent based modelling, complexity theory, and so forth, following the demise of general equilibrium analysis and the like. Does anyone really suppose that these providing advances in understanding social reality? Some commentators do seek to interpret such developments as examples of mainstream pluralism. But in spirit they are no such things. And at the methodological level, in particular, they are quite the opposite. At best they are manifestations of the mainstream flexibility to move from one highly questionable mathematical conception to another.

Is there no scope for progress via internal critique involving formalistic methods? It seems to me that the best bet for those keen on internal critiques utilising formal models is precisely to keep plugging away in the hope that mainstream practitioners

[16] Thus it is not clear that Hodgson's examples – those that reveal that models are based on restrictive or unwarranted assumptions - are after all critiques of this form.

[17] Moreover, certain critiques that do not at all require mathematical models for a point to be made are often put in that form just in order to get a hearing. Hodgson references Sraffa's contribution. But Sraffa's essential point is that the meaning/interpretation of an integral element of a system will (or can) depend on the system as a whole. He made the same point to Wittgenstein (in the context of language systems - and causing Wittgenstein to significantly rethink his position) but without the need/constraint of using mathematics to convey his message. Moreover, in the latter case Sraffa seems to have had the more persuasive impact.

[18] I have engaged in this sort of activity myself a good while ago. See for example, Lawson 1981

eventually grow tired of shoring up existing, or seeking new, formalistic systems (for I doubt that the options by way of formalistic systems will run out), and are willing to turn to something else. But I am not optimistic it will happen.

Is there any better alternative way to proceed? If the problem is that by making an internal critique of (elements of) one mathematical-deductivist approach, mainstream economists merely readjust it or invent an alternative one, the surest way to make real progress, it seems to me, is to orient the internal or immanent critique at features *common to (or presupposed by) the variety of mathematical-deductivist methods or projects* associated with mainstream economics. It is to carry out an immanent critique of the mathematical-deductivist approach to social theorising itself. This anyway has been my approach.

Mainstream economists recognise that event regularities are required (or need to be posited) if their methods are to have application (see e.g. Maurice Allais, 1992, p. 25). It is widely recognised that such regularities (whether deterministic or stochastic) rarely emerge in the social realm. What I have shown is that such regularities, if they are to be guaranteed, require conditions (a closed world of isolated atoms) that are rather unlikely to hold in the social realm. Thus methods that presuppose them (if success is to be guaranteed) are questionable, and the reason for the continuing failure of a project that insists on them is evident.

This is indeed an internal or immanent critique just because (or to the extent that) in footnotes, introductions, public speeches, rhetorical asides, and so forth, mainstream economists talk in a manner that presupposes worldviews according to which social reality is anything but closed and atomistic. The problem is an apparent lack of awareness that their methods prohibit the sorts of theorising that conforms to their implicit broader perspectives.

Still here I am straying rather from the issue. Hodgson seems to suggest that I fail to notice, or consider the possibility, that mathematical-deductivist methods can be used for purposes of internal critique in the context of modern economics. My response is to observe that no one could fail to recognise this possibility, for it mostly exists just because that project is itself intrinsically mathematical. My worry, though, is that the sorts of critiques possible will likely leave this overall and unhelpful mainstream emphasis or orientation (as opposed to specific mainstream examples) untouched. It is for this reason that I have adopted an alternative approach that reveals the limitations of the mainstream deductivist project as a whole. Of course, there is more to effecting change than getting it right (irrespective of whether or not my arguments are correct). If Hodgson believes that continually chipping away via internal critiques employing mathematical methods will prove more effective in the long run, whatever the intellectual case, then let him chip away (also see John Davis' commentary in this volume and my response). For reasons I have given, though, if a more realistic economics is the objective, I am not optimistic that the outcome will be worth the effort.

Points of Substantive Disagreement

Now that the main misunderstandings at least have been cleared out of the way I can turn to those of Hodgson's comments that, in part at least, do point to some real substantive differences between us.

A central plank of Hodgson's critique is the charge that the distinction I draw between methods of abstraction and theoretical isolation does not hold. Or rather Hodgson claims "the distinction is, at least in prominent practical instances, difficult to sustain". I believe, to the contrary, that the distinction (elaborated below) is very easy to sustain, and that it is vital to social analysis that we do sustain it.

Why does Hodgson single out this particular issue? The answer is not totally clear to me. Hodgson notes that I regard abstraction as both unavoidable and useful but that I am rather wary of the method of theoretical isolation, associating it implicitly with the mainstream. I think Hodgson's goal is to persuade that the sorts of methods that I advocate (and more especially abstraction) face essentially the same problems as those confronting the mainstream. In other words, Hodgson seems to be working on two fronts. On the one hand he wishes to suggest that formalistic methods can be more useful than I allow. On the other hand he wishes to convey the impression that any alternative methods that I have advocated share any difficulties that can be associated with formalism.

I say this because I notice that his discussion of these matters is preceded by a commentary on my own critique of the mainstream insistence on using mathematical-deductivist methods. Specifically, Hodgson notes my criticism of the mainstream turns on their reliance on methods that presuppose a closure, in order to analyse open systems. Hodgson clearly hopes to undermine this critique. And his strategy in this entails seeking to persuade the reader that methods that I advocate also presuppose a closure. For if the latter were the case, Hodgson seems to suppose, then my critique of the mainstream would be seen to be unsuccessful, at least in the sense of my failing to propose anything better. Hodgson notes that I distinguish the method of theoretical isolation from abstraction and that I associate closures mostly with the former. At the same time I emphasise that abstraction allows us to investigate open as well as closed systems, and, specifically, that it does not force us to treat open systems as though they are closed. Thus Hodgson perceives a need to undermine the distinction between abstraction and theoretical isolation.

Now, curiously enough, if my assessment of Hodgson's motivation is correct here he does not actually need to seek to dissolve the distinction between methods of theoretical isolation and abstraction. For I do not deny that closures of some sort are often important to the (dialectical sort) of explanatory analysis that I have often emphasised. However, I do find that the closures that have proven most relevant are not of the sort that mainstream economists tend to seek. Mainstream economists are interested in *closures of causal sequence,* those (if any) in which one (set of) event(s) or state(s) of affairs stands in the causal sequence of another (for example, household disposable income standing in the causal history of household expenditure patterns). The sort I think are typically more useful to explanatory social analysis are (a subset of) *closures of concomitance* (see Lawson, 2003), and specifically those that hold when specific features of a domain of reality share a similar causal history and so can be expected to turn out roughly the same (when the price of postage stamps increases in London it also does so in Cambridge; the amount of rain that falls in a square metre of my front garden on any given day, is much the same as falls in the same area in my back garden (and in my neighbour's).

Moreover, for mainstream economists the goal of seeking closures is to predict and thereby control. In contrast, closures are of most use to the very different sorts of (dialectical) explanatory procedures I defend precisely when they unexpectedly break down, when the stretch of reality under consideration in fact ceases to be closed (to support an event regularity). There is much to be said here (see especially chapter 4 of *Reorienting Economics*). Certainly the methods I believe to be most useful to explanatory analysis are very different to the deductivist modelling efforts of the mainstream.

If Hodgson really wants to suggest otherwise it is here he ought to pitch his critique. But he does not. Instead he notes my positive exploration of the method of abstraction. As I say, I can only suppose it is because he thinks that he can somehow undermine my own explanatory emphasis by showing that abstraction and theoretical isolation cannot be easily rendered distinct that it is here he concentrates his attack. Because I believe the distinction that Hodgson's attempts to dissolve is nevertheless vital to social analysis (if not actually the one Hodgson best needs to attack) I must now defend it anyway. This is the focus of the discussion that follows.

Abstraction and Theoretical Isolation

Before turning to the details of Hodgson's criticism I should recall what I take the distinction between abstraction and theoretical isolation to be.

I interpret abstraction, here as always, according to its traditional meaning of focusing upon certain aspects of something to the (momentary) neglect of others. It is a process of focusing on some feature(s) of something(s) while others remain in the background. For example, in considering the ability of copper to conduct electricity well I may focus upon its atomic structure and thereby abstract from its colour, texture, malleability, and so on. It follows that there is always something that is abstracted *from*. That which is abstracted from is the *concrete*. One significant purpose of abstraction is to individuate one or more aspects, components, or attributes and their relationships in order to understand them better.

Hodgson notes that in referring to the method of theoretical isolation I draw on the work of Uskali Mäki. This is correct; the method is not one I myself defend or have sought to develop. Let me then restate the conception of it I set out in *Economics and Reality*, which makes reference to Maki's (1992) conception, and seems to be Hodgson's notion. There I write of it:

> "According to Mäki, [the method of theoretical isolation] is a method 'whereby a set of elements is theoretically removed from the influence of other elements in a given situation' (1992: 318). Mäki adds that 'In an *isolation*, something, a set X of entities, is "sealed off" from the involvement or influence of everything else, a set of Y entities; together X and Y comprise the universe" (ibid.: 321). Of course, even in experimentation no such isolation occurs literally, only physical re-arrangement. But the aspect of all this that I find most problematic is Mäki's notion of *theoretical* or *ideal isolation*, an apparently 'traditional forceful procedure ... in economics' wherein no material re-arrangement is involved at all. Rather 'a system, relation, process, or feature ... is closed from the involvement or impact of some other features of the situation' by way of 'an intellectual operation in constructing a concept, model or theory' (ibid: 325). In fact, Mäki goes further and distinguishes 'internal' and 'external isolation': 'In an *internal isolation*, one isolates a system from influences coming from within the system, while *external isolation* closes a system from influences that have sources which are external to the system'. Mäki adds that both 'internal and external isolation are relevant in economics' (ibid.: 326)" (Lawson, 1997, pp. 131-2)

The distinction before us is easy enough to grasp. To abstract is to focus on aspects of something whilst *not* assuming the non-existence, or non-impact, of features not focussed explicitly upon (that are abstracted from). To theoretically isolate is precisely to treat those aspects not focussed upon as non-existent, or at least as sealed off, as having no systematic influence.

The difference between the two is easily demonstrated if we consider an aspect of some team game, say football or hockey, on television. Suppose, we see on the television screen a player, say a footballer, running with the ball down the side of the pitch, towards the end at which the opponents goal is situated. If we are abstracting we will be interpreting this footballer's actions in a manner that takes into account the fact that supporting players on the same side will be moving in the same direction, and defenders of the opponents' team will be facing up to make a challenge. If instead we treat these other players as somehow sealed off from the action, as momentarily non-existent, then we are using the method of theoretical isolation. Clearly this creates a different world to the actual one addressed via abstraction. In the isolationist's world as described, if scoring is the player's objective all he or she has to do is take the ball to the goal and kick it between the two posts. There will be no opposition, because all other players not focussed upon are assumed to have no impact.

A sports example such as this is useful because the game is clearly an internally related whole. The parts, the various movements of individual players, only get their meaning from the whole, so that any attempt to interpret one bit whilst ignoring the rest must fail if it is meaningful at all. I throw in the TV screen just to get the abstractionist and the isolationist to focus on the same part.

Clearly abstraction, but not theoretical isolation, will be relevant wherever the whole is not just the mechanical sum of parts. Composers, surgeons, artists as well as social theorists deal with internally related wholes. As such abstraction, not theoretical isolation, will be the appropriate method of analysis.

Now it should be clear, although it is worth emphasising it anyway (not least because this may be a source of the confusion), that these two methods, *though distinct*, are not strict alternatives. Because the world in which we live is so complex, abstraction is always involved. It will be so even where an isolationist approach is adopted (indicating that the presumption of 'isolation', or being sealed off, can only ever be a relative one). For example, in considering the sports game above, in the scenarios of *both* the abstractionist and the theoretical isolationist, the causal force of gravity is presupposed, but abstracted from. That is, in each case gravity is not mentioned or analysed but accepted as acting; it is not assumed to be sealed off or inoperative; in neither case are the players treated as being propelled into outer space[19].

In contrast the method of isolation has very restricted conditions under which it is relevant or anyway useful. The paradigm case is provided by a situation of controlled laboratory experiment. Here a mechanism is physically insulated from countervailing

[19] In other words, in cases where isolationist methods are adopted, and it is assumed that some factor X operates in isolation, it will be clear from the context of analysis that some (and which) factors are being treated as operative but unmentioned as opposed to being sealed off.

features in order to be empirically identified: an event regularity is generated correlating the triggering of the mechanism and its unimpeded effects. Once more abstraction will be involved. We may momentarily concentrate on the 'isolating' of the mechanism, then on the triggering of it, then on some of its effects. We may at all times abstract from, colour, smell, sound, cost of apparatus, and so forth; but then again we may not: it all depends on the context.

Of course, the controlled experiment represents a physical not a theoretical isolation. But a theoretical isolation is a process of imagining what would occur if a physical isolation could be achieved. A theoretical isolation is indeed a thought experiment. And where physical conditions are such as to inhibit in principle (as opposed to providing practical difficulties for) the physical isolation of certain features, then it seems that the method of theoretical isolation is without utility.

For example the interconnectedness and mutual constitution of the numerous different features of social reality are such that it is impossible to experimentally isolate individual components, such as money, firms, or markets, and examine how they so operate when isolated from each other and from everything else. Equally, it is meaningless to *theorise* these features as if sealed off from the influence of each other and everything else. Of course, stated explicitly, this all seems obvious. Nevertheless such isolationist procedure dominates the specific methodological practices of modern economics.

If it seems clear enough that abstraction and theoretical isolation are indeed rather distinct (albeit not strict alternatives), I must now turn and examine how Hodgson argues to the contrary. There are essentially three strands to Hodgson's endeavour. First, seemingly accepting that closure is presupposed by the employment of methods of theoretical isolation, Hodgson seeks to associate closure with abstraction too. Second, drawing on Schelling's (1969) analysis of 'racial' segregation, Hodgson seeks to show that the use of heuristics, clearly presupposing the method of isolation, is equivalent to the process I describe under the heading of abstraction. Third Hodgson claims that the distinction I draw between the methods of abstraction and isolation is too vague to be of practical import. Let me run through each of these charges in turn indicating why I believe each to be unsustainable.

1) Abstraction and closure

How first does Hodgson argue that abstraction presupposes closure? In truth, he does not; that is, he does not provide an argument. Rather he asks the question:

> "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?

And he gets Stephen Nash to answer it for him:

> "Stephen Nash (2004) has recently argued in the affirmative, suggesting that Lawson too must assume conditions or forms of closure"

Hodgson adds nothing to this, though he perhaps believes that his case is bolstered through his suggesting that I use the distinction (between abstraction and theoretical isolation) as a form of protection:

> "[Lawson] 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"

Now whatever else Stephen Nash (2004) does or does not do, as far as I can see he nowhere mentions abstraction, let alone argues that it presupposes a closure. Nash does suggest that the explanatory method I advance presupposes closures (something I have already acknowledged above), but fails to draw a distinction between closures of concomitance and closures of causal connection and so to appreciate that it is the former variety that are significant for the explanatory approach I defend. He further fails to note that (perceived) closures are significant precisely when they beak down. But this is all beside the point. The topic of abstraction is not addressed.

If there is no argument advanced either by Hodgson or by Nash, there is seemingly none (here) to which I must determine a response. And to the point it should be very clear from all that has gone before that of course abstraction does not imply closure. At least it does not if by abstraction we mean focussing on a part of a whole whilst leaving the rest of the whole momentarily out of focus, and if by closure we mean, as I do throughout, a system supporting an event regularity. Clearly, abstraction can be applied to all types of systems, to those that support strict event regularities, to those that support partial ones and equally to those seemingly not supporting any. It can be applied to matters that are real or fictitious. If I talk only about the horn (or white colour, or billy-goat beard, or lion's tail, or cloven hoofs) of a unicorn, I am abstracting in the context of discussing a fiction. To say of the social system, or of any specific part of it, that it is fundamentally open is to abstract. To suggest that abstraction presupposes closure is simply to misunderstand one or other or both of the two terms.

2) The use of heuristics is equivalent to the method of abstraction

A second point of difference between us concerns the way in which Hodgson believes that formalistic models might serve as heuristic devices. Once more Hodgson is concerned to connect abstraction to theoretical isolation. The latter method is closely connected to the notion of heuristics as used in economics, and this seems to account for Hodgson's concern to relate the latter in turn to abstraction. Here the analysis gets more interesting. Hodgson writes:

> "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"

It is this belief that appears to ground Hodgson's suggestion, which I am disputing, that the use of heuristics and abstraction are closely related if not equivalent, that "heuristics relate to the very process of abstraction that Lawson himself highlights"

Hodgson's contention that the 'purpose of a heuristic is to identify possible causal mechanisms', is, I believe, simply wrong (at least if I am interpreting him correctly, a matter to which I return in due course). It may well be the case that heuristics can be useful for some yet-to-be-explicated purposes 'without necessarily making adequate predictions or closely matching existing data'. But their contribution is not 'to establish a plausible segment of a causal story'. Rather any usefulness they posses, or so I shall argue, can stem only from the fact that a plausible segment of a causal story has already been established[20].

However, I realise that this last claim is likely to be contentious; that others too will likely be uncomfortable with it. So let me defend it at length. It is in the context of suggesting that heuristics achieve the same outcome as abstraction that Hodgson refers to Schelling's (1969) model of 'racial' segregation (to illustrate his argument). This latter focus is useful in that, unlike many economic contributions, Schelling's analysis does indeed seem to carry insight and be somewhat persuasive. This, no doubt, is why Hodgson draws upon it. So eventually below I run through my argument referencing Hodgson's example of Schelling as is relevant.

[20] In other words, I am suggesting that any insight attached to a formal model is typically not the result of the modelling or heuristic exercise itself but derived first in a different context. For a mainstream economist, the overriding objective is to produce a mathematical model. Obviously, modellers are uncomfortable with the charge of irrelevance, so attempts will be made to render models as realistic as possible; real insights will be tagged on wherever feasible. But as I say, I believe the real insights are typically independent of, and indeed achieved prior to, the construction of the mathematical model.

Heuristic and the method of successive approximation

First, though, I should establish the meaning of the term heuristic. Of course, specific categories can be made to mean anything we want them to. But all have a context, and most have a history. The term heuristic originally meant something like “serving to find out”. How the latter is interpreted does vary according to context. In education it usually relates to a system in which pupils are trained to find out things for themselves. In philosophy and science it most commonly means a rule of thumb that has been found to be useful in making progress towards solving a problem. A heuristic computer program is one that begins with only an approximate method of solving a problem within the context of some goal, and then uses feedback from the effects of the solution to improve its own performance.

It is worth emphasising that in all such cases, the heuristic (the method, system or whatever) is observed to work; it is found successfully to serve some process of finding out. If something new is proposed as a heuristic device there is presumably an expectation at least that it will serve its intended purpose.

The way the term heuristic is usually employed in economic methodology is bound up with developing theories by way of relying on assumptions or conceptions believed to be false[21]. Heuristic assumptions are often said to be those that are used to simplify the analysis as a first step with the expectation (or perhaps, with hindsight, with a knowledge) that the picture is to be (or has been) rendered more realistic through complicating it at a later stage.

Such a process of gradually complicating the picture with the aim of making it more realistic is presumably what Hodgson has in mind when he notes that “heuristic models […] are literally unrealistic” and he writes that Schelling’s “model is simply a heuristic step along the road towards that more complete end”. In a later version of his paper (presented at the Cambridge Realist workshop in November 2005) Hodgson also refers approvingly to Musgrave (1981) who in fact provides the classic statement of the step-wise procedure in terms of heuristic devices or assumptions:

> “[A scientist] may wish to *develop* ... a theory in two stages: in the first stage he takes no account of factor F, or ‘assumes’ that it is negligible; in the second stage he takes account of it and says what difference it makes to his results. Here the

[21] This is the conception shared by others who work in this field. For example, Steve Keen recently considers heuristics emphasising clearly that “A heuristic assumption is one which is known to be false, but which is made as a first step towards a more general theory”

'assumption' that factor F is negligible is merely a heuristic device, a way of simplifying the logical development of the theory. Let us call such assumptions *heuristic assumptions*" (1981, p. 383)

It is thus easy to see how the method of theoretical isolation "whereby a set of elements is theoretically removed from the influence of other elements in a given situation" (Maki, 1992, p. 318) relates to the making of heuristic assumptions in economics. For both advanced knowing that significant causal influences (and perhaps other significant features) are omitted.

The stepwise approach of moving from initial conceptions of isolated features to a more realistic or complete theory is often referred to as the method of successive approximation, particularly when there are (possibly many) more than two steps involved. It is something I discuss explicitly in *Economics and Reality*, where I look at the practice of accepting certain sorts of believed-to-be-false assumptions as a "heuristic device in a step-wise process of moving from simplified or ideal conceptions to others of greater complexity and so, it is supposed, realisticness" (Lawson 1997, p. 127). What is required here is some analysis of heuristic assumptions interpreted as unrealistic claims, and how they serve to facilitate theory development.

Hodgson himself does not give any analysis of why, or when, a conception regarded as unrealistic might be illuminating. He does, though, appear to suggest that it might be provided by Robert Sugden's (2000) account of 'credible worlds', which also makes reference to Schelling's (1969) contribution. Sugden's idea is that if a model captures a 'credible counterfactual world' we apparently have some inductive warrant for its relevance to our world. Hodgson writes:

> "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 on 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"

Now inductive inferences concerning states of affairs are problematic at the best of times. But even overlooking this, we are entitled to ask: what does it mean to talk of 'credible counterfactual worlds'? It appears to be accepted that worlds such as that described by Schelling could not come about. So in what sense are they credible? We surely need to know this if Hodgson's reference to Sugden is to help us understand when or why some conceptions that are acknowledged as unrealistic remain useful.

It so happens, as I say, that in *Economics and Reality* I set out an analysis of possible heuristics and related issues that can make sense of all this. This is something to which I now need to return. Let me give a brief summary.

The method of successive approximation and heuristic assumptions

In *Economics and Reality*, in fact, I argue that two conditions are essential (though by no means sufficient) to the success of this method of successive approximation (relying on heuristic assumptions as interpreted by Musgrave and others). These are:

1) "that the factors considered in 'isolation' be real causal factors, structures and/or transfactually acting mechanisms or tendencies; and

2) that the effects of the factors so considered in 'isolation' combine or interact mechanically" (Lawson, 1997, p. 129)

These conditions, and the need to satisfy them, are easily grasped. Basically the idea is to move from an understanding of a part of a causal story to an understanding of more, or of all, of it.

This requires, first, that the features (treated as) isolated in thought be causal mechanisms and that the nature of the causal parts, or individuals, viewed in isolation as a first step, are not knowingly portrayed incorrectly. In other words, it is essential that the intentional fictionising concerns *not* the manner of acting of the parts considered separately (in isolation) but only the (heuristic) assumption of their acting in isolation from (some) other factors affecting the total outcome.

Of course, this requirement, in its turn, presupposes that the way a causal mechanism operates as it is found in reality is the same as it would operate in 'isolation from (some) other factors affecting the total outcome' (or under the conditions assumed in the heuristic exercise).

Notice too, that this requirement rules out the vast majority of theoretical conceptions that are bound up with modern mainstream formalistic modelling.

A second condition that must typically be satisfied if we are successfully to utilise the method of successive approximation (or heuristics), that is if we are to achieve an understanding of the whole by way of considering the workings of parts considered in isolation, is that the effects of the different parts or causal elements can be aggregated, that is combined additively or mechanistically. In contrast, if there are, say, emergent powers of the more complex entity or whole irreducible to those of its parts considered separately, then it is not clear that it is especially useful to proceed by seeking to understand (or speculating about) how each part might act in isolation.

Notice too, that to make sense of the first condition (that the way a causal mechanism operates as it is found in reality is the same as it would operate in isolation from (some) other factors affecting the total outcome) we need the notion of transfactuality. Something is said to be acting transfactually when it is having its effect *whatever the actual outcome*. Gravity is pulling my computer keyboard to the floor even as my desk acts to counteract this force and leave the keyboard at rest (relative to myself) in front of me. In other words, the keyboard does not need to be dropped in an experimental vacuum for gravity to have its effect. Similarly, the aspirin acts to offset my headache even if my noisy environment and heavy drinking countervail its effects and leave my head in a worse state. Such factors as gravity and the aspirin, when triggered, act not counterfactually but transfactually. We can talk not just of how they would (counterfactually) act in different non-actual but ideal circumstances, but also of how, when triggered, they are continually transfactually acting, whatever other forces are in play. The category of a tendency is reserved for the effects of forces that are acting transfactually, i.e., whatever the actual outcome[22]. It is when a mechanism is insulated from countervailing factors that (as in a well controlled experiment) its tendencies and the outcome produced coincide.

Although advocates of the method of successive approximation, or the making of heuristic assumptions, typically do not identify the two noted conditions for the method of successive approximation to be successful, they can be seen to be built into their illustrations. Consider the classic exposition of Alan Musgrave, concerning Newton's analysis of inter-planetary motion:

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[22] Notice that the point of a controlled experiment is to insulate some real causal factor (in order to better empirically identify, or, just as commonly, to empirically verify [or not], the manner in which a mechanism works). Significantly, however well the mechanism is insulated (and insulation will rarely, if ever, be perfect) knowledge of the mechanism's workings will often allow us to say that, and how, it will operate outside the experiment, in the open system of complex interacting reinforcing and countervailing forces. Clearly it is because many mechanisms act transfactually that we can successfully apply knowledge achieved in the (controlled) experiments, where event regularities are produced, to conditions where event regularities are absent; for the result achieved apply first and foremost to transfactual tendencies not to highly restricted and rare event regularities.

> "When Newton sought to discover what his theory predicted about the solar system, he first neglected inter-planetary gravitational forces by 'assuming' that there was only one planet orbiting the sun. He proved that, if his theory was correct, the planet would move in an ellipse with the sun at one of its foci. This assumption was not a negligibility assumption: Newton knew that planets would sometimes have detectable gravitational effects on one another. Nor was it a domain assumption: Newton was not saying that his theory only applied to one-planet solar systems. You miss the point if you object that Newton's assumption is false, because our system has more than one planet. You also miss the point, though less obviously, if you object that the *consequence* of Newton's assumption was false, because planets do not move exactly in ellipses. The consequences drawn from heuristic assumptions do not represent the precise predictions of the theory in question; rather, they are steps towards such precise predictions" (Musgrave, 1981, p. 383).

Clearly, the method of successive approximation is found to be successful in the case of Newton's analysis of inter-planetary motion, both because it is real (gravitational) tendencies that are so considered (in isolation), tendencies that seemingly operate transfactually whatever else is going on, and also because gravitational tendencies do appear to combine mechanically.

Equally clearly, however, the conditions in question, and in particular the requirement that causes combine mechanically, do not hold in general. Perhaps their lack of universality is most readily apparent if we think of chemical reactions and combinations. But mechanistic combining is hardly typical of social phenomena either. For example the network of social relations so central to social life cannot meaningfully be broken down into parts with some bits treated as though existing in isolation before others are eventually added back in. It makes no sense at all to treat any feature in isolation from another to which it is essentially related. In studying family behaviour, say, it is clearly quite irrelevant to study conceptions of parental mechanisms apart from conceptions of the nature, including needs, of children or the mutual relationship in which parents and children stand. Equally, it is incoherent to consider the situation of landlords/ladies in isolation from (conceptions of) tenants, or conceptions of employers in isolation from those of employees, and nor does it make sense to consider capitalist firms, markets and money in isolation from each other, and so on.

Pure and Applied Explanation

There is a further point I want to pull out from all this, one that can perhaps be made in the clearest way if I first distinguish *pure* from *applied* modes of explanation. Briefly put, pure explanation is concerned with identifying and understanding causal mechanisms; applied explanation is concerned with working out how already known mechanisms conspired to bring about some concrete real world event or state of affairs (see e.g., Lawson, 1997, chapter15). For example meteorologists pretty much know the separate causal mechanisms that govern weather patterns; much of the pure explanatory component has been done. Each day, though, at least in the UK (and probably everywhere else after some novel weather pattern has been experienced) an applied explanatory endeavour is initiated: the object becomes to explain the pattern of behaviour just experienced utilising an understanding of causal mechanisms already available (and drawn upon in explaining every other day's weather patterns).

Now the point of drawing attention to this latter distinction here is to emphasise that, to the extent that the method of successive approximation, or the making of heuristic assumptions as understood here, have relevance at all in scientific methodology, it is as a component of applied, not pure, explanation. The method in question can be put to work only after the pure-explanatory work has been done. For example, the heuristic assumption that only causal mechanism X is in play (or need be considered), albeit as a first step, presupposes that causal mechanism X has already been uncovered.

Thus in Musgrave's analysis, Newton's goal is not to construct a theory of gravitational forces. This he already possessed. Rather the purpose (we are informed[23]) was to show in a stepwise manner how his theory could ultimately correctly predict inter-planetary movement.

Grounding counterfactuals

The reason I draw attention to this distinction is to indicate that it is only *after* a causal hypothesis of interest is obtained, that is after the pure-explanatory stage, that it may be legitimate for counterfactual insights concerning it to be generated. In other words, a statement about a tendency that is transfactually in play will often license a subjunctive conditional about what would have happened at the level of the course of events if the system had been insulated from the activities of (some) other actually operative mechanisms, or indeed if it had been in any other state.

[23] Actually, I am not so sure of Musgrave's explanation of Newton's intentions (see below), even if, as in this case, the conditions for method of successive approximation to be successful appear to be satisfied.

How we obtain our understanding (or hypotheses) of causal mechanisms of interest is itself a complex process that I discuss at length elsewhere (see especially, Lawson 2003, chapter 4; but also see my responses to Caldwell and Ruccio in this volume). Contrast explanation will often be important (operating under a logic of analogy and metaphor amongst other things). But it may be merely that we abduct insights obtained in other spheres (as I think may be the case of Schelling's analysis - see below).

Now I think it is this overall framework or understanding that Sugden is edging towards with his notion of "credible counterfactual worlds". If a mechanism does act transfactually it licences a hypothetical statement about how things might be in a different world. Descriptions of the different worlds to our own may be more or less realistic. There is a sense in which alternative worlds seem more credible if the only unrealistic aspect is the absence of actual mechanisms, as opposed to claims about how identified mechanisms do, or could, actually work. This, I suspect, is the basis for Sugden regarding such a counterfactual world as in some sense credible, despite never being expected to occur.

But if it is, notice that it is not the fact of " 'credible counterfactual worlds' that give 'some warrant for making inductive inferences from model to the real world", as Hodgson, following Sugden, puts it. Rather it is a prior understanding of the real world that licences subsequent claims about certain counterfactual 'worlds' appearing credible.

Notice too, that no matter how insightful a counterfactual analysis of this sort might be, a comprehensive understanding of the causal process under study cannot typically be captured or conveyed in this manner. Rather, to obtain a full (and indeed ultimately practically useful) understanding of the situation, tendency statements must be interpreted as categorical and indicative, to the effect that, if triggered, a mechanism is really in play whether or not its effects are fully manifest.

In other words, to focus only on actual outcomes in counterfactual scenarios (real or impossible) is typically to miss the main insights available. For if a mechanism licensing a focus on 'credible counterfactual worlds' is indeed operative, then it is it is having its effects anyway (transfactually); there is a tendency continuously in play in our actual world, whatever the outcome. And this can be acted on. For example policy can consequently be devised to reinforce or countervail such a tendency as required.

The dominant-mechanism special case

Of course, if a domain of reality is such that a causal mechanism focussed upon (and treated as acting in isolation as a first theoretical step) is not only stable but dominating of other causal factors (and especially if all mechanisms in play do combine mechanistically) it is likely that event patterns that the theory predicts are recognisable in the outcomes actually achieved. But where this is so, the method fares well not because false claims are providing insight in some mystical fashion, but because the conceptions of mechanisms acting in relative insulation from counteracting others are not that unrealistic after all (and so barely qualifying as heuristics). Of course, it was in part to deal with such scenarios that I introduced the notion of demi-regularities or demi-regs[24]

In other words, isolationist methods appear most legitimate and insightful (though their ability to actually add much is questionable) precisely in situations where a causal mechanism is so dominant it is (momentarily) effectively insulated from the countervailing effects of other factors. That is, methods of theoretical isolation are legitimised and express reality in a recognisable fashion precisely on occasions where the isolation is not merely ‘theoretical’ but effectively actual. Needless to say, to suppose that such occasions licence the ubiquitous reliance on such methods is to overlook the rarity of the former.

An example of such a special case is the celestial patterning addressed by Newton and referred to by Musgrave. Indeed, I suspect that Newton was as much concerned to explain a demi-reg (the approximate ellipse traced out by the paths of many planets) as to launch a project of successive approximation (in conditions where the mathematics of dealing with the movements of more than two planets had yet to be developed, and indeed was achieved only after Newton’s death). The celestial pattern arises because of rather *peculiar* conditions that hold in the case of the planets, in that both their intrinsic states as well as the extrinsic forces acting upon them are, in relevant respects, sufficiently stable, at least over the time period with which most people are usually concerned, i.e., over human life-spans. Properly interpreted, Newtonian mechanics posits theories of how bodies (tend to) act; celestial phenomena function merely as evidence of the postulated tendencies. Thus, if the intrinsic or extrinsic states of the planets in our solar system were not so stable but were to change in some way, perhaps a massive meteor were to pass through the solar system, then such a mechanics would entail a consequent disruption of the familiar celestial phenomenal patterns.

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[24] Which in *Economics and Reality* I characterised as “a partial event regularity which *prima facie* indicates the occasional, but less than universal, actualization of a mechanism or tendency, over a definite region of time-space” (see also *Reorienting Economics*, chapter 4). In Lawson 1997 I suggested that a demi-reg:
“[…] indicates the likely effects of a causal mechanism that frequently but not uniformly are actualised over a particular region of time-space. The patterning observed will not be strict if countervailing factors sometimes dominate or frequently co-determine the outcomes in a variable manner. But where demi-regs are observed there is evidence of relatively enduring and identifiable tendencies in play”

The point is that although the celestial example is spectacular in nature, it represents a relative rarity in constituting a spontaneous demi-reg of its sort. No doubt it is precisely its spectacular nature that accounts in some part for the general failure from Laplace onwards to realise that the situation *is* relatively uncommon, to appreciate that the celestial demi-reg, or near closure supporting it, is far from being indicative of the phenomenal situation that can be expected to prevail more or less everywhere. This failure, in turn, appears to be largely responsible for the widespread, if tacit, acceptance, formerly in philosophy, and currently in the social sciences in particular, of a ubiquity of constant conjunctions of events in nature, and thus of the doctrine of the actuality of ‘causal’ laws. It no doubt also encourages the idea that methods of theoretical isolation, or of successive approximation, or of heuristics and such like, have ubiquitous relevance, when in fact conditions under which they are relevant, certainly as they are formulated in modern mathematical-deductivist economics, appear to be circumscribed indeed[25].

Schelling

We are finally in a position to interpret the contribution of Schelling. And we can now appreciate that Schelling’s analysis can be expected to be insightful if it captures a real mechanism that operates transfactually, that is, that produces tendencies towards ‘racial’ segregation, whatever else is going on.

As I understand Schelling’s (1969) analysis, the basic idea is as follows.

In a context in which

1) individuals perceive themselves as belonging to one or other of two mutually exclusive and exhaustive groups and
2) (at least a significant number of) individuals prefer not to be located in a situation where they are dominated or overwhelmed by members of the ‘other’ group, and
3) space that can be occupied is confined or restricted, or where grouping of some kind must (for whatever reason) occur

there will be a tendency towards (some) segregation.

[25] Perhaps too it a focus upon the rare (if sometimes rendered prominent) scenario, wherein some mechanism dominates others (so that that treating it as if acting in isolation is not so different from treating it realistically) that most encourages Hodgson in his view that “the distinction (between theoretical isolation and abstraction) is, at least in prominent practical instances, difficult to sustain”.

I doubt this was ever news. Indeed, do we not all experience situations in which a tendency of this sort is so dominant that it is even actualised. I certainly have, and regularly. My earliest memories include glimpses of physical education lessons in primary school where the teacher regularly asked the class of about 30 children to form four or so groups. Invariably, as I recall, the groups were wholly male or wholly female but not mixed. Today, I cannot but notice that in my workplace (Cambridge) coffee room, the 'support staff' invariably avoid sitting next to 'academic staff' except on occasions (such as outside of lecture term) when the number of academics present is much reduced (and indeed no greater than the number of support staff).

Schelling's contribution, I think, is to suggest that the mechanism in question – basically a preference not to be dominated by perceived 'others' in a confined space – is relevant to understanding racial segregation in the US, at least at a certain moment and place in history. He points out that for purposes of the US census individuals are (or were) classified as white or as black, and that many at least view themselves in this fashion. Now to the extent that a majority (and perhaps almost all) individuals prefer not to be overwhelmed by others of a different 'colour', and there is a restricted area in which some population is located, we have reason to expect a tendency to racial segregation.

Schelling suggests several more concrete claims that seem likely realistic and consistent with his basic more abstract conception. Thus he mentions mechanisms whereby "Whites may prefer to be among whites and blacks among blacks" (p.489); or "whites may prefer the company of whites, while blacks don't care" (p. 489) or whereby "Whites and blacks may not mind each other's presence, even prefer some integration, but" [where there is] […] a limit to how small a minority either colour is willing to be" (p. 489).

Schelling, though, does not develop an ontology of transfactual tendencies as set out above. Instead, he writes as though countervailing factors are absent and tendencies in play will all be realised (as outcomes or movements). For example:

> "Whites and blacks may not mind each other's presence, even prefer some integration, but, if there is a limit to how small a minority either colour is willing to be, initial mixtures more extreme than that will lose their minority members and become one colour; those who leave may move to where they constitute a majority, increasing the majority there and causing the other colour to evacuate" (p. 489)

Clearly, this passage is easily rewritten in terms of tendencies and greater contingency ('tend to lose' in place of 'lose', etc). When it is, it is this revised formulation in terms of tendencies that licenses the passage as written (not the other way round).

Of course it may be that Schelling believes that 'colour preferences' dominate all countervailing factors. If so then his formulation might be accepted as it is, as a claim intended to be realistic with respect to the real world.

Schelling and counterfactuals

Alternatively, the implicit claim that *only* 'colour preferences' are effective (or that they dominate) can be interpreted as a heuristic assumption. Proceeding on this interpretation seems legitimate, although the point of it is not clear. The language of tendencies conveys all the insights that could be so expressed (and indeed more). The aim could be to do as Musgrave suggests and add in complicating factors bit by bit. But then we would need to know that the effects of Schelling's mechanism, and of those yet to be identified, aggregate in a mechanical fashion. I see no reason to expect this and Schelling reveals no inclination to proceed in this fashion.

Instead, Schelling continues by imagining yet more concrete or detailed scenarios that conform *not at all* to the world in which we live or to one we could reasonably expect to occur. He constructs fictitious set-ups (e.g., "a line along which blacks and whites […] have been distributed in equal numbers and random order" [p. 489]), and assumes that individuals can move freely [and repeatedly move] according to fixed rules, identical for everyone, without costs or countervailing forces of any kind, and so forth.

I am not at all sure that this additional analysis provides any insight other than to the very specific properties of the very particular set-ups or 'models' considered. Any understanding concerning the real world is already contained in the analysis of tendencies. What is going on is that the concern with modelling for its own sake at this point takes over.

Interestingly enough Schelling (1969) does not actually construct any *mathematical* model as such. As a result, Hodgson's claim that Schelling's contribution is a demonstration of the benefits of "a formal heuristic" is erroneous from the outset. Still there is little doubt that the just discussed assumptions introduced by Schelling facilitate or encourage a mathematical modelling approach. And it is also clear that whatever the precise nature of Schelling's own early piece it eventually stimulated many papers of a more formalistic kind.

The point to emphasise, though, is that these later papers, or modellers, did not generate the insights we associate with Schelling, but rather they drew upon them in an attempt to legitimise their modelling endeavour. Any new insights obtained concerned merely the properties of the (increasingly complex and unrealistic) formalistic models. Assumptions are made just to get certain desired patterns or results (elegantly) to emerge[26].

If I am correct in my analysis of all this the question that clearly arises is "why bother?" Why the interest in models for their own sake? The answer, of course, is the situation I have been actively attempting to counteract throughout much of my writing: the widespread idea that mathematical modelling is in itself a necessary feature of any respectable economic theorising.

Heuristics and causal mechanisms

Let me at this stage return to Hodgson's particular critique. It will be remembered that according to Hodgson: "The purpose of a heuristic is to identify possible causal mechanisms that form part of a more complex and inevitably open system". I have shown that, to the contrary, the heuristic assumptions can go to work in economic methodology only after causal mechanisms of interest have already been identified or at least hypothesised. Schelling (1969) I have suggested illustrates just this.

After setting out his own outline of Schelling (1969) Hodgson adds:

> "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

[26] At this point I am minded once again of Frank Hahn's awakening to the nature of this sort of state of affairs (even if Hahn mistakenly supposes that those who recognise it more immediately are disposed to being anti-mathematics [as opposed to being anti- the abuse of mathematics]):
"[…] there is [...] a lesson which has only gradually been borne in on me which perhaps inclines me a little more favourably to the "anti-mathematics" group.
The great virtue of mathematical reasoning in economics is that by its precise account of assumptions it becomes crystal clear that applications to the "real" world could at best be provisional. When a mathematical economist assumes that there is a three good economy lasting two periods, or that agents are infinitely lived […] everyone can see that we are not dealing with any actual economy. The assumptions are there to enable certain results to emerge and not because they are to be taken descriptively" (Hahn, 1994, p. 246).

> (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"

Here Hodgson seems to make the same mistake as before. Let us be clear. Where a causal mechanism is in play we can focus momentarily upon it, and leave countervailing factors and so forth aside. If the causal mechanism is of a sort that it will have these effects whatever the context, i.e., whatever the relations in which it stands to other causal structures, then clearly it follows that the way it would operate in isolation is equivalent to the way it will operate as we find it in the real world. In such a scenario, it is not illegitimate, and there may be some utility, if, as a first step to an analysis, we adopt the heuristic assumption that the causal factors left out of focus do not exist, or are sealed off. This will clearly be especially the case if the omitted factors appear insignificant compared to the factor on which we focus.

But to describe this situation with the words: "heuristics are appropriate if they successfully abstract an important causal mechanism in reality" is to get things the wrong way round. A correct statement is rather of the sort that "heuristics are appropriate (if at all) where an isolatable causal mechanism has already been abstracted". And from this it does not quite follow, as Hodgson would have it, that "Accordingly, heuristics relate to the very process of abstraction that Lawson himself highlights". Rather it follows only that the appropriate use of heuristics *depends* (like everything else) on the very process of abstraction that I highlight.

The heuristic, in the given context, is the assumption that factors out of focus have no effects, that they can be treated as sealed off or non- existent, that the mechanism in focus is acting in (relative) isolation. It is thus indeed the case that "heuristics are isolations rather than abstractions".

In the second part of the passage Hodgson writes that although Schelling ignores factors such as 'bigoted racists' Schelling's "model is extremely and worryingly persuasive"

I myself am not sure how useful it is to treat Schelling's mechanism as a heuristic first step, as I have already indicated. But whether or not it is useful to do so, any insight to Schelling's analysis arises only because it first captures a case (albeit a special one) of a mechanism that can be regarded as realistic. Why any persuasiveness thereby imparted should be worrying I do not know. Moreover, the consequences of some individuals holding racist views are likely incorporated anyway; certainly

Schelling does not examine or distinguish the various grounds for preferences regarding racial segregation, so there is no reason to suppose that bigotry is excluded.

Now I am well aware that I have only provided an interpretation of what is going on and that it is open to contestation. But I do believe I have set out a defence of a framework that can render coherent the various issues before us. Certainly, I do not find much that is coherent in Hodgson's few criticisms; and nor actually am I aware of an equally coherent alternative framework provided elsewhere. Hodgson is very wide of the mark indeed when he suggests that I do not consider the possible heuristic value of formal models, and that I ignore the role of context. At the very least, I think we must accept that if there is insight to be gained from treating formal models as heuristic devises the case for this has yet to be made. If Hodgson feels compelled to establish this point I think he must first do a bit more work. In any case there is nothing here to encourage the view that abstraction is the same as theoretical isolation.

An alternative interpretation

Let me, though, add a qualification to all this. For many reasons it is vital to be charitable in debate, and perhaps there is a more charitable reading of what Hodgson is saying. The preceding discussion does seem to me to be the most accurate reading of Hodgson, in that he writes that the "*purpose* of a heuristic is to *identify* possible causal mechanisms", and that the "*purpose* [of heuristics] 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", and so forth. And I did want to respond to what seems to be the most comprehensive as well as accurate interpretation of Hodgson's position. But it is possible that Hodgson is simply suggesting that when we make heuristic assumptions and consider features as if in isolation it is important that those features treated as isolated be real causal mechanisms. That is, it is feasible that Hodgson is *not* at all suggesting that heuristic is somehow bound up with the process of *identifying* a causal mechanism but rather that heuristic (to be successful) needs to be is employed in an analysis in which a mechanism has *already been identified*.

If this is the correct interpretation of his position, Hodgson has seemingly expressed himself rather misleadingly, but at least this would be a claim on Hodgson's part that seems sustainable.

However, the nature of my response is still much the same (albeit I could perhaps have made it significantly shorter). Specifically, it remains the case that it is an achieved understanding of the actions of a transfactually-active causal mechanism that licences the subjunctive conditional or counterfactual, that sets the boundaries of, and illuminates, ‘credible counterfactual worlds’; it is not the other way round. And fundamentally, it still does not follow that theoretical isolation or “heuristics relate to the very process of abstraction” in the sense of being much the same thing.

For sure, to consider a transfactually active mechanism as if it were isolated from countervailing factors involves abstraction. But to assume thereby that this method of isolation, or the use of heuristics involved, is the same as abstraction (even if the focus is a scenario of a single dominant mechanism in operation), is simply a mistake. Abstraction, as earlier noted, is involved in theorising both the real and the fictional, the open and the closed and equally both the isolated and non isolated; indeed it is a part of all forms of conceptualisation or theorising. To conclude that because abstraction is involved in some special case it thereby reduces to, or is somehow intrinsically bound up with, that special case is a conflation that simply does not bear considering further.

So, in short, we can see that, whichever way Hodgson is arguing it, the distinction between abstraction and theoretical isolation cannot be dissolved and in fact (especially given the apparent rarity of the conditions in which the latter is likely useful) remains vital to successful explanatory endeavour.

3) The distinction drawn between abstraction and theoretical isolation is insufficiently precise.

Hodgson’s third charge is that the distinction I draw between abstraction and theoretical isolation is insufficiently precise. I hope, though, that is by now clear that this is not so, and that I have said enough to demonstrate that the two methods are indeed irreducible one to the other. Still let me go through Hodgson’s last strand of argument to illustrate this one more time.

Hodgson considers a case in which some factor X (trade with other nations) is ignored (in an analysis of the workings of a national economy), and questions whether ignored meaning being-not-mentioned (abstraction) is not the same as ignored meaning treated-as-having-no-influence or not-existing (theoretical isolation)

He writes of the former (abstractionist) scenario:

> “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"

Actually, I think it is typically only in the case of theoretical isolation that mention of specific omissions is warranted. Analysis never starts from complete ignorance; much is always taken for granted but remains unstated. As I noted earlier, most social theorists take gravity as given (they abstract from it) in social analysis, but rarely acknowledge or explain that omission. If in contrast they wanted to assume its effects were absent or sealed off, this would most certainly warrant a mention, and an explanation. For under such a heuristic assumption our planet (if indeed there was one at all) would be very different indeed.

In similar fashion, when an economist discusses aspects of an economy such as the UK, its trade with other nations might not get a mention (be abstracted from); but it would nevertheless be presupposed. The focus may be on the workplace, say on improving work security, or on gender mainstreaming in employment strategy. In each such case trade may not be mentioned (it may be abstracted from); because most social theorists take for granted the fact that the UK is a trading nation. But if trade were assumed away, rather than treated as a background causal factor, this once more would certainly warrant a mention and an explanation. For life in the UK, under such an assumption, would be very different indeed to life in the UK as we currently know it[27]. There is no support here for the thesis that abstraction and theoretical isolation cannot be distinguished.

Hodgson continues:

> "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 case, 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"

[27] Any resulting analysis would be extremely different. This absence would mark most of the UK's internal economic institutions, as well as the sorts of political activities undertaken. All goods, including technology, would be home produced. Competition would presumably be internally generated. International pressures affecting security at work policies would be absent. The economy would not (need to) be part of trading blocks. Presumably factors like the European Employment Strategy would have no impact. There would be no scope for policies like export-led growth or import quotas. There is no obvious reason why movements in world commodity prices would have any impact, etc, etc.

The first two sentences here are surely correct. But it does not at all follow that to not mention a causal linkage is thereby, of necessity, to treat it as not existing, or as being sealed off. As I say, it is not at all the case that a social analysis that neglects to mention the gravitation is necessarily operating thereby under the assumption that gravity does not exist. That is one reason abstraction is so useful.

In short, it is clear that there is nothing in all this that in any way threatens the distinction between abstraction and theoretical isolation or renders it 'highly problematic'. It may well be that the method of isolation is found to have little utility. But the distinction in question, between the practice of not focusing on something and assuming its effects are somehow absent or sealed off, remains as clear and vital to social theorising as ever.

Concluding Comments

Despite the emphasis that mainstream economists place on methods of formalistic modelling (or perhaps because of it), they make relatively few attempts to justify their orientation. This is surely a significant absence leading in and of itself to an impoverishment of the discipline, whatever else might be going on. Hodgson's spirited intervention to make a substantial defence of forms of formalistic modelling, and/or the manner in which mathematical-economic models might fruitfully be interpreted or utilised, is thus to be welcomed. He performs an important service. For reasons given, however, I am not convinced by Hodgson's efforts so far. But knowing Hodgson, I suspect he will persevere further in this, and I am confident we will all be the better for it, whatever may be the conclusions that are reached along the way.

Addendum

At the end of 2006, after I had completed the foregoing response to his *Post-Autistic Economic Review* piece, Geoffrey Hodgson kindly presented me with a copy of his new book *Economics in the Shadows of Darwin and Marx*. It contains a revised longer version of his *PAE Review* paper. In this longer version, Hodgson expands some of his arguments and adjusts some of his rhetoric. But I find nothing in it to give me reason to revise or qualify anything written above.

However, Hodgson does introduce one seemingly new line of argument in his later text. There is an extra section tagged on asking whether economists should be allowed to embrace false assumptions. This contains an additional criticism of me:

> “Lawson (2002, p. 76) warns against false assumptions, proposing that if they are allowed ‘it is clearly possible to derive any conclusions whatsoever – true ones or false ones – simply by deductive logic’. The argument is wrong. If I make the (false) assumption that ‘the economy consists of just two goods’ then there is no way we can deduce from that proposition alone that the economy consists instead of four, five or a million” (Hodgson, 2006, p. 128).

But my argument is *not* wrong. What is wrong is Hodgson’s representation of it. The first sentence in this passage makes reference to the claim (by me) that a specific suspect *practice* (allowing false assumptions) makes it feasible for the modeller to generate any conclusions. The contention ridiculed in the third sentence is that any specific suspect *model* can be used to generate any conclusions. The mistake, Hodgson’s not mine, is to suppose that the latter contention follows from the former.

Let me be clear. I do point out that if false assumptions are allowed, then for *any* conclusion X, I can always find some allowable set of assumptions Y, from which X can be deduced. For example if (to use Hodgson’s construction) the desired conclusion X is “the economy consists […] of four, five or a million [goods]”, then I can assume 1) the number of goods in the economy is the same as the number of days in a week; and also 2) the number of days in a week is four, five or a million. The desired result follows by deduction. But if the suspect *practice* in question (of allowing assumptions thought to be false) allows me to generate any result, it does not at all follow that once I specify some particular *model* H (e.g. ‘the economy consists of just two goods’) that any conclusion (such as X) can be drawn it. Hodgson’s charge simply rests on a non-sequitor.

In Lawson (2002), I do give illustrations. In fact, at the point where Hodgson paraphrases me I write:

> “Now if, and once, falsity (in this broad sense) of assumptions is allowed, it is clearly possible to derive any conclusions whatsoever -- true ones or false ones -- simply by deductive logic. Thus suppose I want to deduce the (apparently true) proposition that ‘all ravens are black’. One way I might do it is by including in my assumptions, the propositions: "all ravens are vegetables" and "all vegetables are black". Clearly my desired conclusion follows by deductive logic. In similar fashion if I want to deduce that "agent X does Y", I need only assume (i) that a situation prevails in which it is rational (in a specific sense) for a situated agent X to do Y, along with the assumption (ii) that X always acts rationally in the

specified sense. The assumptions about the situation and human capabilities and their exercise need not be realistic, merely facilitating of mathematical modelling tractability. What could be more trivial and more pointless?"

Of course, my focus in the paper in question is specifically on strategies of mathematical-deductivist modelling. It is in this context that I look at the consequences of the practice of allowing assumptions considered by everyone to be false. The reason I do so, it should be clear, is precisely that, given the nature of social reality, the mainstream insistence on a formalistic modelling orientation regularly necessitates the reliance on such accepted-as-false assumptions. My point is that because the construction of such assumptions is considered legitimate *practice* (i.e., the restriction of being realistic is removed), the mainstream 'theorist' or modeller can always find some way of generating a particular desired result[28].

In any case, I hope it is now clear enough that in Lawson (2002) I am, at the relevant stage of the discussion, not at all concerned with the properties of any specific model[29]. As I say, the mainstream insistence on a formalistic modelling strategy in economics requires that a reliance upon thought-to-be-false assumptions be an allowed practice. I am merely pointing out how easy it is to generate any desired conclusions once such a practice (that Hodgson seems to be encouraging) is sanctioned, and questioning the value of such activity[30].

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[28] The specific challenge for formalistic modellers in economics is to start from 'results' widely accepted as plausible and to work back to assumptions relating in effect to a closed world of isolated atoms (that such models presuppose) that would allow the desired results to be deduced. Clearly, if the reliance upon thought-to-be false assumptions were discouraged, this would prove very debilitating for any project insisting on employing methods of formalistic modelling in an attempt to illuminate social reality.

[29] I feel sure, though, that this is, and always was, entirely obvious. What is happening in all this, I fear, is that Hodgson is being less than charitable in the way he is choosing to interpret and/or represent me (an orientation with which he seems to conclude the relevant chapter of his book). I only hope that this does not overly detract from matters where there is real substantive or philosophical disagreement.

[30] I might add that one reason I appreciated Frank Hahn's presence at 'theory' seminars in Cambridge is that, when modellers started their papers setting out their axioms and assumptions, Hahn regularly jumped in saying 'you are only assuming x, y and z to allow conclusion c to be drawn'. At this point, of course, the seminar was effectively over (although those speakers tended laboriously to make their way to the anticipated conclusion c anyway).

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