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Central Fallacies of Modern Economics

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Abstract

Although it is widely recognised that the modern discipline of economics is short on explanatory successes, there is little sign that ongoing critical assessments of the situation are leading to any improvements. The reason for this lack of progress, it will be argued, is a prevalence of a set of fallacies maintained very often by mainstream practitioners and heterodox critics alike. These tend to take the form of presuppositions that underpin more explicit beliefs and accepted practices. Mostly they remain implicit and largely unnoticed. When noticed, they are regarded as so obviously correct they are rarely critically examined. Here I do examine them both explicitly and critically.

Zusammenfassung

Obwohl es weithin anerkannt ist, dass die moderne Wirtschaftswissenschaft nur geringen Erklärungserfolg vorweisen kann, gibt es nur wenig Anzeichen dafür, dass kritische Einschätzungen dieser Situation zu irgendwelchen Verbesserungen führen. Der Grund für diesen Mangel an Fortschritt liegt, wie hier argumentiert wird, in der Vorherrschaft einer Reihe von Irrtümern, die häufig gleichermaßen von Mainstream-Praktikern wie heterodoxen Kritikern aufrecht erhalten werden. Dieses findet gemeinhin in der Form von Vorfestlegungen statt, die explizitere Überzeugungen und akzeptierte Praktiken untermauern. In den meisten Fällen blieben diese Vorfestlegungen implizit und werden deshalb nicht erkannt. Und wenn sie erkannt werden, werden sie so offensichtlich korrekt angesehen, dass sie selten kritisch reflektiert werden. Hier werden sie einer expliziten und kritischen Untersuchung unterzogen.

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> When you are criticising the philosophy of an epoch do not chiefly direct your attention to those intellectual positions which its exponents feel it necessary explicitly to defend. There will be some fundamental assumptions which adherents of all the variant systems within the epoch unconsciously presuppose. Such assumptions appear so obvious that people do not know what they are assuming because no other way of putting things has ever occurred to them. With these assumptions a certain limited number of types of philosophic systems are possible, and this group of systems constitutes the philosophy of the epoch.
>
> Alfred North Whitehead (1926, 61).

1. Introduction

Intellectually speaking, the modern discipline of economics is in some disarray, short on explanatory successes, largely detached from its subject-matter, and seemingly without clear objectives or sense of direction. This is especially true of the hugely dominant mainstream project whose protagonists insist that mathematical modelling is the only proper or serious or 'scientific' way of doing economics (see Lawson, 1997, 2003, 2015a).

The latter project in particular has attracted much criticism of course, especially in recent years. Nevertheless it has doggedly survived almost unscathed and remains as dominant as ever. Why or how has this been possible given the continuing explanatory failures and indeed persistently unrealistic formulations?

One significant reason this mainstream project is able to persist in its unhappy state and yet simultaneously remain hugely dominant and influential, is the continuous impact of a set of fallacies, many and perhaps most of which are accepted at least as much by heterodox critics of the mainstream as by the latter's own protagonists.

These myths and fallacies usually take the form of presuppositions that underpin more explicit beliefs and accepted practices, and as such they tend very often to go unnoticed, or, if noticed, they are rarely critically examined. It is because the (often unwitting) acceptance of such fallacies is so harmful for the discipline that here I focus on formulating the more fundamental ones explicitly and in somewhat stark terms, placing them upfront, and indicating in an equally stark manner why indeed they are erroneous.

There are likely various reasons that fallacies of the sort that I have in mind do not receive greater critical examination. The most significant explanation though is simply that modern economists, not least those who set themselves

up as media commentators, policy advisors / analysts and the like, mostly (there are important exceptions) reveal themselves to be unwilling to do the philosophical legwork necessary to get to the nub of the issues involved.

Most of these individuals unhesitatingly presume that the recourse of criticising substantive claims (typically modelling assumptions) of others and thereafter substituting (equally questionable) alternatives of their own is always sufficient and proper procedure (the sort of error that I am here seeking to dispel). To the extent that a few of the individuals in question do reveal some awareness of somewhat philosophically oriented critiques, the resort typically is to avoid the effort of engaging by instead displaying overtly dismissive postures, suggesting for example that their formulators know no economics, have hidden agendas, cannot do the mathematics, are 'economic flat-earthers', merely hide behind terms ending in 'ism' and 'ology', and so forth. The inevitable consequence is that discussions of the state of the modern discipline remain largely superficial, criticism is mostly misdirected and overly tame, and supposed / proposed alternative approaches or projects (some of which receive significant financial backing) end up, in the main, being essentially more of the same.

It is thus with the aim of counterbalancing tendencies of the noted sort that in the current paper I take the opportunity to focus solely, and in an explicit and sustained fashion, on those fallacies that, in my assessment, individually or collectively, serve most to obstruct serious attempts to transform modern academic economics into a more relevant, open-minded, serious and pluralistic discipline. I proceed, as I say, by way of formulating a number of them in simple and stark terms and indicating briefly why I take the presupposition in question to be erroneous. I also provide references to textual sources where the relevant argument is developed at greater length.

The fallacies I have in mind run as follows:

2. Twenty Fallacies of Modern Economics

2.1 Concerning the Nature and Problems of the Discipline

1) The widely observed crisis of the modern economics discipline turns on problems that originate at the level of economic theory and / or policy.

It does not. The basic problems mostly originate at the level of methodology, and in particular with the current emphasis on methods of mathematical modelling. The latter emphasis is an error given the lack of match of the methods in question to the conditions in which they are applied. So long as the critical focus remains only, or even mainly or centrally, at the level of substantive economic theory and / or policy matters, then no amount of alternative text books, popular monographs, introductory pocketbooks, journal and magazine articles,

newspaper columns, blogs, *even student protests, petitions, and 'reclaiming economics' campaigns and events*, new institutes and/or centres, alternative programmes, conferences, workshops, plenary speeches, videos, comic strips, or whatever, are going to get at the nub of the problems and so have the wherewithal to help make economics a sufficiently relevant discipline. It is the methods and the manner of their usage that are the basic problem.

The point is simply that all methods are appropriate under some conditions but not others. Hammers and pens have their uses. But if the task at hand is, say, to mow the lawn neither a hammer nor a pen is likely to be up to the job. Similarly the sorts of mathematical methods economists insist upon have their uses. But social analysis is not one of them. This is because the methods in questions, to be successful, require closed systems, i.e. those in which correlations occur, where the guaranteeing of the latter closures require worlds of isolated atoms. By an atom I just mean a factor that always acts with the same, separate and independent effect, whatever the context. By isolated I merely mean that the economic atoms act in conditions in which there is nothing to interfere with their workings and so to prevent their effects from being deducible/predictable.

It is easy enough to demonstrate that social reality is not like this. Rather social reality is generally open, with everything (from social structures to embodied personalities) in process, being transformed through human practice (thus undermining atomism), with all aspects constituted in relation to (and not merely linked to and certainly not organised independently of) each other (thus undermining any requirement of isolationism). For lengthy discussions see for example Lawson, 1997, 2003, 2015a.

2) The failings of modern economics emerged only with the recent economic crisis.

This is simply false. Economics has been in an intellectually sorry state for the last 50 years or so. The output of the discipline has long been explanatorily a failure, plagued with unrealistic assumptions, and produced by those with no real idea where the project is going. This has been the case indeed ever since the significant take-up of methods of mathematical modelling in economics.

This sorry situation, moreover, has often been noted even by certain prominent mainstream spokespeople, not least *Nobel Memorial Prize Winners in Economics*, at least when adopting a reflective mode (in presidential speeches and such like). Thus for example back in 1982 Wassily Leontief was concerned that:

> "Page after page of professional economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions … Year after year economic theorists continue to produce scores of mathematical models and to explore in great detail their formal properties; and the econometricians fit algebraic

functions of all possible shapes to essentially the same sets of data without being able to advance, in any perceptible way, a systematic understanding of the structure and the operations of a real economic system" (Leontief, 1982, 104).

Staying in the last century we find Milton Friedman lamenting the fact that:

"economics has become increasingly an arcane branch of mathematics rather than dealing with real economic problems" (Friedman, 1999, 137).

And Ronald Coase notes that;

"Existing economics is a theoretical system which floats in the air and which bears little relation to what happens in the real world" (Coase, 1999, 2).

An outline of the history of the failure of the discipline, as well as additional prominent acknowledgements of the latter's dire state can be found in, amongst other places, Lawson, 2003 chapters 1, and 10; 2015a, chapters 1, 5, 6, 7 and 11).

3) It is a failing of modern economics not to have predicted the timing of the recent crisis (and given that so few did so, those who were successful should be lauded).

Not really. Much has been made of the failure of economists to predict the fact, and precise timing, of the recent crisis. Even the British Queen has gotten in on the act in criticising economists for this perceived failure. Unfortunately all such criticism has only encouraged economists in the idea that event prediction is the legitimate goal to pursue, i.e., that the only serious economics is one (a form of mathematical modelling) that devotes resources to the aim of achieving this.

This is all quite misguided. Social reality including the future is open, so that successful event prediction is typically not much more creditable than winning a lottery. This is not to deny that we can understand many of the various tendencies in play at any time, not least those that are unsustainable. In the latter case we all know that something somewhere must 'give' in some way sooner or later (and the insight that capitalism will experience repeated crises, along with analyses as to why, has been provided by Marx and others a while ago). But when and how a specific manifestation happens is usually highly contingent.

Of course, as with all forms of betting it is very often the case with economic forecasting that, at each point in time, all possible outcomes are covered by the totality of forecasts made. Thus, at any given point in time there are usually some modellers that can claim (with at least some subset of their projections) to have got it 'right' (in the sense that official figures or 'measurements' fall with the assumed-to-be-appropriate bounds of error attached to these forecasts) whatever the actual outcome (although these 'official figures' are frequently revised with time, albeit perhaps after reputations have been established). But like any other gambles, those forecasts interpreted as successful are usually not followed by similar successes the next time around.

In any case, successful event prediction, if mostly infeasible in the social realm, is not only unnecessary but mostly unwanted anyway. This is so because we can still successfully identify explanatory social causes of phenomena, and the impossibility of systematic event prediction is consistent with our ability to use explanatory insights achieved to knowledgeably transform social reality to make the world a better place. In other words, it is better that we can knowledgeably (and preferably with wisdom) make our own history than are confined to merely watching it unfold, if albeit in a predictable manner (see Lawson, 2015a, chapters 6 and especially 7).

4) The economics taught in modern universities is driven by right-wing or neo-liberal ideology.

It is not. It is driven by a methodological ideology: that mathematical modelling is the only sound way to do economics. Throughout the modern academy the latter is widely accepted uncritically as 'common sense'. Most academic economists, in my experience, have little idea what neo-liberalism even means; nor do they care (see Lawson, 2015a, chapters 3 and especially 7).

5) Dismissing the substantive theory and/or policy contributions of opponents with the label 'neoclassical' is helpful in pinpointing the problems of the discipline.

It is not. Rarely is the term defined. And far from pinpointing or facilitating an understanding of any fundamental problem of the discipline it almost always detracts from doing so, through giving the impression that the problem is self-evidentially essentially a matter of poorly, but freely constructed, theory or policy – that which is dismissed as neoclassical. This merely encourages lazy dismissals in the place of sustained explanatory analysis and critique (see Lawson, 2013a; 2015a, chapter 4). Not much better is the strategy of supposing that any approach labelled heterodox or Post Keynesian or institutional or whatever, especially if it is a form of mathematical modelling, is necessarily any better *just because* it is so labelled.

To transform the discipline in a constructive way it is necessary first to identify its problems; this is not achieved merely by signalling opposition or support for particular contributions through the use of unexplained labels (see Lawson, 2013a or 2015a, chapter 4 [where it is argued that there actually is no neoclassical economic theory or policy]).

6) Whatever may be the source of the discipline's problems we can always make progress by highlighting and chipping away at the lack of realisticness of prominent substantive assumptions (such as the familiar claims made about rationality, preferences, beliefs and states of the economy).

I doubt it. To render mathematical modelling exercises tractable (to guarantee that embedded hypotheses are consistent with event correlations) economists must, as already noted, turn the 'agents' of their analyses into metaphorical

atoms and situate them in isolated systems quite unlike those in which we actually live (see Lawson, 1997, 2003, 2015a). This endeavour (given the actual nature of human beings and the social world) necessitates the making of assumptions that are inevitably mostly unrealistic. It is specifically absurd formulations of rationality and the like that serve the purpose of 'atomisation', of reducing the 'subjects' of their analyses to preprogrammed (human) atoms.

Significantly however, although the cause of this lack of realisticness (i.e., the very use of inappropriate methods) goes unappreciated, *the fact of this lack of realisticness* of assumptions is usually acknowledge by everyone, including those who continually employ them, and frequently with regret; very often the assumptions made are interpreted as temporary devices that are expected to be improved upon in due course[1]. Certainly, few if any modellers or mainstream 'theorists' have defended their rationality assumptions as realistic, and when pushed, do often substitute other (fixed) behavioural assumptions or formulations (e.g., alternative specifications of rationality; or those of limited rational-

[1] Of course, the regret is not always admitted. Some contributors even seek to turn a limitation into a virtue. Lucas for example writes:

> "To observe that economics is based on a superficial view of individual and social behaviour does not seem to me to be much of an insight. I think it is exactly this superficiality that gives economics much of the power that it has: its ability to predict human behaviour without knowing very much about the makeup and lives of the people whose behaviour we are trying to understand" (Lucas, 1986, 425).

Mathematical models can certainly generate predictions (just as the latter can be plucked from the air). The feature they manifestly lack the ability to achieve (in an open system such as the social world in which we actually live) is systematically to *predict at all accurately or successfully*. The point, though, is that even those as misguided as Lucas recognise the unrealisticness or "superficiality" of the assumptions made.

Lucas's focus is models employing the rational expectations hypothesis of course. Frank Hahn understands the possibilities for mathematical modelling better, explicitly rejecting the goal of successful prediction and observing instead that

> "What a rational expectations theory provides is an understanding of an imagined economy which satisfies the assumption." (Hahn, 1985, 11, 12).

Indeed, Hahn is often dismissive (to say the least) of Lucas and other who think such models might inform policy analysis:

> "When policy conclusions are drawn from such models, it is time to reach for one's gun" (Hahn, 1982, 29).

Rather, Hahn recognises that the goal can only be to see where unrealistic assumptions lead, or to enable particular outcomes to be generated:

> "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, 246).

Hahn, if clear that mathematical modelling is merely about exploring where unrealistic assumptions lead, never actually tells us why this activity is interesting or useful. The point here, though, is simply that it is evident that little difference/progress is going to be made by critics merely indicating that various assumptions regularly employed by mathematical modellers are not realistic. This is unlikely to be received as news, or as insightful, by anyone.

ity; or those even of irrationality [as for example proposed by neuro-economists and the like]) that serve similarly to 'atomise' the subjects of the analysis.

In the same fashion, modern accounts of human beliefs (e.g. rational expectations) and economic states (e.g. equilibrium) work to facilitate model tractability or achieve various model consistency properties, and are rarely held by their formulators to be realistic. The former assumption merely renders the model in the head of posited agents consistent with the model in which the agents are situated by the economic modeller (see Lawson, 1995). The latter equilibrium notion is just a solution concept of a system of (unrealistic and irrelevant) equations Lawson, 2015a, chapter 8). There is rarely a pretence that any of this is expresses reality (see Lawson, 2015a, especially chapters 4, 8 and 9).

In consequence, it is more or less futile for critics to think that inroads can be made by noting specific cases of assumptions that are unrealistic. Any lack of realisticness is rarely news; very often, as I say, it is regretted.

The insight that does seem like news, that does appear to go unrecognised by most, is that it is the emphasis on methods of mathematical modelling that is responsible for this persistent lack of realisticness, and that in an open complex social reality the production of unrealistic formulations is not a temporary contingent state but inevitable. Much better then to focus the critique on the modelling emphasis *per se* (see Lawson, 2015a, especially 4, 8 and 9).

7) The project of seeking to mathematise the economics discipline is a relatively modern one, and its dominance has been achieved through the project's significant explanatory successes.

Not at all. It is a project that has been underway for over 200 years (see Lawson, 2003 chapter 10). And the current dominance of the mathematical modelling endeavour within the academic discipline owes nothing to explanatory successes, something to the manner in which mathematics was reinterpreted by mathematicians themselves in the early years of the last century, and much to politics, especially following World War II.

For much of the history of attempts to mathematise economics, the aim was to do so in a manner that was analogous to the model of mechanics that dominated the non-social sciences. Economics was notably poor in this endeavour. However, in the early years of the twentieth century, a transformation took place in the manner that mathematics itself was interpreted. Basically the classical reductionist programme in physics fell into disarray as developments in relativity theory and especially quantum theory caused the image of nature as continuous to be re-examined in particular. A result was that the role of infinitesimal calculus, which had previously been regarded as having almost ubiquitous relevance within physics, came to be re-examined even within that domain.

One result, facilitated by the contributions of Hilbert (1990) in particular, was that mathematics became interpreted as (no longer an attempt to apply the physics model, and specifically the mechanics metaphor, but rather as) a discipline properly concerned with providing a pool of frameworks for *possible realities*. Mathematics was now viewed *not* as the language of (non-social) nature, abstracted from the study of the latter, but as a practice concerned with formulating systems comprising sets of axioms and their deductive consequences, with these systems in effect taking on a life of their own. The task of finding applications was henceforth regarded as being of secondary importance at best, and not of immediate concern.

This emergence of the axiomatic method removed many of the most severe constraints facing those who would seek to get mathematical practices accepted amongst the collective practices of the discipline of economics. For the time being, at least, researchers involved with mathematical projects in economics could postpone the day of interpreting their preferred axioms and assumptions. At this point mathematical modelling practices were included and able to compete amongst the accepted (collective) research practices of the discipline without the need to be formulated in ways that were analogous to the methods of mechanics, etc. Mathematical models indeed were created supposedly without carrying any necessary interpretation[2].

This development in the interpretation of mathematics moreover fed into an arguably even more significant environmental shift, this time concerning the set of political practices. The latter include both the onset of World War II and most especially the post-war emergence of McCarthyite witch-hunts in the US the face of the ongoing Cold War.

Initially, following the changes in the non-social sciences, the project of mathematising economics received its greatest stimulus in Austria and Germany, where the new physics, a revised conception of the role of mathematics

[2] Consider for example Debreu's eventual (1959) axiomatic treatment of (the existence and uniqueness) of general equilibrium, a contribution that gained its author the Nobel Memorial Prize in economic science. Even today the language and symbolism of Debreu's *Theory of Value* is found in many axiomatic papers. Debreu's contribution rests for its legitimacy precisely on the claim that axioms are not in need of any interpretation. As Debreu expresses these matters himself:

> "*Allegiance to rigor dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations*. In order to bring out fully this disconnectedness, all definitions, all the hypotheses, and the main results of the theory, in the strict sense, are distinguished by italics; moreover, the transition from the informal discussion of interpretations to the formal construction of the theory is often marked by one of the expressions: "in the language of the theory," "for the sake of the theory," "formally." Such a dichotomy reveals all the assumptions and the logical structure of the analysis. It also makes possible immediate extensions of that analysis without modification of the theory by simple reinterpretation of the concepts; […]" (Debreu, 1959, x, emphasis added).

and a specific emphasis upon axiomatic mathematics, had originated and came to flourish. In particular, it was here that von Neumann, Wald, Morgenstern and other mathematicians made their initial contributions. Approaches such as those of Wald and von Neumann were different in kind. But they were later reconciled in the US, where many of the early contributors emigrated under the Nazi threat.

So with attempts to mathematise economics freed up from the burden of having to fit with reality, it is significant that many of the practitioners of this approach ended up being located in the US in the post war period. This is so for two reasons especially. First it turned out that the US had the resources to dominate the post-world war II international academic scene in economics (as indeed it has done so in so many other disciplines). Second, this emphasis on an economics without any necessary interpretation proved to be an attractive proposition in the context of the McCarthyite witch-hunts in the face of the Cold War.

At that time the nature of the output of economics faculties – traditionally a form of political economy, a field that attracted those who sought a more humane system than capitalism – became a particularly sensitive matter. In such a context the project of mathematising economics proved to be especially attractive in that it carried scientific pretensions whilst being significantly devoid of any necessary empirical content.

The group most feared and mistrusted by the McCarthyites were the intellectuals (Reinert, 2000). The mathematising project with its detached technicist emphasis, often to the exclusion of almost any critical or reflexive orientation, was clearly extremely attractive to those caught up in the situation. For the option of supporting an economics taking the form of such a project was extremely convenient not just to insecure or fearful university administrators but also to the funding agencies of US social scientific research (who were especially important in this period – see for example, Coats, 1992; Goodwin, 1998; Yonay, 1998). Clearly by allocating the funds to the mathematising project there was little risk to these bodies of being accused of supporting those who wanted to transform the economic system; for by everyone's account the mathematising project had no obvious bearing on social reality.

It was in this way that the mathematising project (first allowed to compete openly following the change in the way mathematics itself was interpreted) came to dominate the discipline, initially in the US, and eventually worldwide, despite having never been found to be explanatorily very successful (on all this see especially Lawson 2003, chapter 10; also Lawson, 2015a, chapters 1, 4, 5, 7 and 11).

2.2 Concerning Theoretical/Philosophical Issues

8) Economics can and should avoid philosophy.

It is this belief I am suggesting that most serves to prevent the discipline from identifying the obstacles that lie in the path of an emancipated economics. Most immediately, the widespread opposition to methodology serves to prevent criticism of the mainstream mathematical modelling emphasis, as well the development of alternatives.

But perhaps even more fundamentally, the neglect of philosophy in the form of ontology is an even greater obstacle to progress, allowing fallacies of the sort here being criticised to prevail almost unchallenged. Ontology is the study of the nature of being, as well as our implicit preconceptions about the latter. As such it is an inherently critical activity casting a spotlight on the presuppositions of all our activities.

This activity, though, is not limited to grounding critiques of standard practices. Like all forms of philosophy, ontology plays a systematic ground clearing role for science. As a result scientists, whatever their domain of study, need repeatedly to engage in it at significant moments in the advancing of causal, and indeed all other forms, of knowledge. Many physicists for example concern themselves with investigating the basic material of reality when they inquire into the *nature* of quantum fields, 'dark matter', particles and waves, mass, curved space-time, quantum gravity, black holes, etc., all issues in ontology.

Economics too has its more basic concerns. These include such matters as social relations, collective practices, social positions, community, capitalism, money, corporations, technology, gender, rights, obligations, human nature, care, trust, crises, economy, and so forth. Yet most economists, if inevitably occasionally referencing such categories, do rarely investigate their nature. However, it is impossible to provide much insight without at least some understanding of the nature of both social being in general and also the specific social phenomena being 'theorised'. These issues, all concerns of social ontological analysis, are easily shown to constitute part of the subject-matter of any would-be serious social science (see Lawson, 2012a, 2012b, 2015a, 2015b, 2015c, 2015e, 2015f; also see Archer (ed.) 2013, 2014, 2015; Fulbrook (ed.) 2009; Lawson/Latsis/Martin's (ed.) 2007; Mäki (ed.) 2001; Pratten (ed.) 2015; or Searle, 1995, 2006, 2010).

9) The division between modern mainstream economics and heterodox alternatives rests fundamentally on competing substantive and policy claims.

It does not. The division rests ultimately on very different ontological presuppositions (preconceptions, often implicit and unexamined, about the nature

of social reality) combined with the fact that heterodox, but not mainstream, economists embrace pluralistic stances at the level of method.

Whilst the implicit ontological presuppositions of the reliance upon mathematical methods of the sort on which economists often insist are closed systems / worlds of isolated atoms, it is easy enough to demonstrate that a far more explanatorily successful ontological conception finds social reality to be characterised by a prevalence of open systems marked by (internal-)relations, process, emergent totalities, meaning, value, etc. A concern with each of these latter features is found to characterise the various heterodox groups. Thus Post Keynesians concern themselves with openness is stressing the fact of fundamental uncertainty; feminist economists focus on relationality in their concern with care, gender, oppression, discrimination, emancipation, and so on; (old / original) institutionalists concern themselves with process (and forms of stability) in their analyses of social evolution, institutions, habit, technology, etc; Marxian economists are concerned with that emergent internally-related totality-in-motion that is capitalism; and so on.

It is true that although this alternative ontology does not provide conditions for the generalised successful application of methods of mathematical modelling, many heterodox economists seemingly fail to recognise this and consequently allocate far more resources to experimenting with mathematical methods than appears reasonable. However heterodox mathematical modellers remain distinct from their mainstream counterparts in both not insisting that all economists employs such methods, and accepting the value of insights obtained by other means (on all this see Lawson, 2015a, especially chapter 3, but also chapters 4 and 9).

10) To criticise / oppose the current mathematical modelling emphasis is to adopt an anti-mathematics stance.

It is not. It is simply to point out that various tools (methods of mathematical modelling) are being used, and more or less exclusively so, in conditions (social reality) where these tools are generally inappropriate and more useful alternatives are available.

One of the many features that I find striking in the scene captured in Gustave Courbet's 1849 painting *The Stonebreakers*, depicted on the cover of my 2015 book, *The Nature and State of Modern Economics*, is the seeming incongruity between the wielding of relatively light-weight hand-held hammers and the task at hand – which is to break up rocks and stones in the process of creating a road through a hill or mountain. The use of mathematical modelling methods to address economic phenomena turns out to be at least as incongruous, and perhaps significantly more so. For when Courbet was painting, the hammers were conceivably used in the absence or unavailability of alternatives tools that were more appropriate to the allotted task; or at least this was likely the situation facing the older man and young boy depicted (individuals who in them-

selves seem equally ill-suited to the sorts of tasks undertaken). However, it is not clear that mathematical models, interpreted as tools for providing insight to social reality, have ever been found to be useful to this allotted task; and there have always been better more productive/fruitful alternatives readily available (as well as people skilled in their application).

The point is that if close study of the practices of the modern economics academy reveals a situation that is every bit as marked by monotony and (albeit in a different sense) impoverishment as Courbet's scene (and its portrayal is similarly irritating to the powers that be) the rendering of this situation in a realistic if somewhat stark manner is *not* to oppose (or be 'anti') the tasks or the tools. Rather it is to criticise the generalised matching of each to the other along with the societal causal conditions that underpin the continuing insistence/conviction that the identified tasks be addressed or tackled only in the depicted ways (see Lawson, 2003, 2009a, 2015a).

11) To criticise/oppose the current mathematical modelling emphasis is to adopt an anti-science stance.

It is not. Mathematics is not essential (or inessential) to science; science involves using tools that are appropriate to the given task. A science of economics is perfectly feasible, and the current emphasis on mathematical modelling in economics serves, given the nature of social reality, mostly to prevent that potential from being realised (see Lawson, 2015a, chapters 1 and 9).

12) To criticise/oppose the current mathematical modelling emphasis is to adopt an anti-pluralist stance.

It is not. Pluralism, I take it, is an orientation of support for variety at all levels, as well as of tolerance and respect for, and willingness to listen to, and to engage with, others. To criticise the current emphasis on mathematics is not to argue for keeping the approach of mathematical modelling out of the toolbox, or to refuse to engage its users. Rather is to resist the dogma that *only* mathematical modelling methods should be, and be unquestioningly, utilised (and utilised however unrealistic the assumptions and explanatorily unsuccessful the whole endeavour; and despite the availability of more appropriate alternatives). It is, in other words, to resist one particular denial of pluralism, the version that currently dominates the discipline of economics (see Lawson, 2015a, chapters 6 and especially 10).

13) The mathematical models of modern economics can be shown to generate insights about aspects of the real world, once or if these models are appropriately, albeit super-cautiously, interpreted.

This claim, where it is not totally banal, or a mere expression of hope and/or faith, is almost always based on a failure to recognise that, in most cases certainly, any insight attributed to the modelling endeavour was never actually a result of the latter, but rather achieved prior to model construction and incorpo-

rated into the modelling process. Mathematical models in the main are incapable of providing new insights about social reality; rather insights about the latter are tagged onto (bits of) the former where feasible in an attempt to make the models constructed appear in some way credible (see especially Lawson, 2009a).

14) Methods of mathematical modelling are, even if unnecessary, used in a neutral fashion, serving as just another language or heuristic device.

They are not used in a neutral fashion. They are tools. And like all tools they are appropriate for some tasks and conditions and not others. In certain contexts the inappropriate use of tools can be positively harmful. This has been (and is usually) the case with the application of mathematical methods in economics. It has forced the discipline into irrelevancy at best, whilst diverting resources away from potentially insightful alternative projects and applications. The claim that the mathematical methods adopted by economists are, or might conceivably be, employed as useful heuristic devices, serves, in the main, merely as an apology for this unhappy affair (see Lawson, 1997, 2003 and especially 2009a).

15) Thought-to-be false assumptions and questionable modelling methods are justified and so useable if/where they generate agreeable conclusions, or anyway conclusions held to be true.

This is incorrect, though seemingly widely believed even, or perhaps especially, amongst heterodox economists critical of the mainstream. That is, heterodox economists frequently suppose that although their modelling assumptions are (necessarily) false, their models are better (than those of their opponents) because the conclusions generated are held to be true. It may be true that 'all polar bears are white'. But if this apparent truth is deductively generated from the assumptions that 'all polar bears eat snow' and 'all snow-eaters are white', we have added nothing to our understanding of polar bears, snow or whiteness; and nor have we provided explanatory support for the proposition that 'all polar bears are white'. All deductive exercises that are so based on known absurd fictions, and this inevitably includes almost all mathematical modelling exercises in modern economics, are just as pointless. Certainly they add little to our understanding of social reality (see Lawson, 2015a, chapter 5 and 6).

2.3 Concerning Proposals for Constructively Transforming the Discipline

16) The solution to making modern economics more relevant lies either in revising certain assumptions of mathematical models, or in a turn to more complex (in particular non-linear) forms of mathematical modelling, perhaps in the form of simulation analysis.

It does not. This fallacy is based on a failure to see that the worldview presupposed by a reliance on these revised methods and forms is just as unrealistic (and indeed in essence is much the same) as that presupposed by more traditional ways of mathematical modelling. There is little reason to suppose that any of the novel modelling assumptions, modelling forms, model applications, or model estimation techniques currently on offer are of much use in the endeavour of rendering the discipline more relevant. The problem in all cases remains a mismatch of method and the conditions of application. A continuing inability to recognise, or reluctance to accept, this fact of the situation explains the failure of the more recent 'alternative' projects interpreted as critical and 'new'. *The Institute for New Economic Thinking* (INET) sponsored by George Soros is a particular example. Although INET no doubt sponsors a few projects that do avoid the noted problems, in the main, and despite Soros' own best intentions (see Lawson, 2015a, chapter 9), the enterprise mostly fails to address the discipline's more fundamental problems, and indeed risks constituting an enormous waste of resources and opportunity (see Lawson, 2015a, chapters 5, 6 and 7; see also Morgan, 2015).

17) If conditions of experimental control do not hold in the social realm, then not only is science impossible, but all methods must be inadequate and not just mathematical modelling techniques. Thus in seeking to improve the discipline we might as well stick with the current emphasis on mathematical modelling.

This is another view that is both pervasive and wrong. A social science can fruitfully concern it itself not only with much needed social ontological elaboration but also and especially with identifying unknown causes of significant phenomena, amongst other things.

Especially important here are dialectical methods such as contrast explanation. The latter proceeds by seeking to explain *not* some single outcome Y but a (surprising) contrast along the lines of "Y rather than X", in conditions (a contrast space) where background knowledge leads us to expect all outcomes to be similar (to be X if this emerges as the typical and so expected outcome). For example why are these cows (with symptoms we now associate with mad cow disease) behaving differently to the rest? Why is the crop yield twice the average at the end of the field? Why do I feel so much worse today than usual? Why are house prices increasing far faster in one area of a city than the rest? And so on.

By asking such a question in a situation where we find a surprising contrast, we are effectively standardising for all the factors common to the cases being contrasted in the hope of identifying the causal factor responsible for the difference. This is the prion in the case of mad cow disease; perhaps a passing river in the case of crop yield; perhaps over-drinking of alcohol in the case of the individual not feeling too good; perhaps a train station in the case of an area of faster-rising houses prices, marked by a recent opening of a fast rail link to London; etc. (on all this see especially Lawson, 2009 b, 2012a, 2012b). Why is

contrast explanation a dialectical method? Simply because the explanatory exercise starts (stage 2) with an apparent contradiction to (i.e., a surprising contrast in the context of) some sort of already achieved (stage 1) background understanding (all cows behave in similar sorts of way; the crop seed allocated, soil composition, and weather etc., is the same throughout a give field; etc.), where the eventual the outcome attained (stage 3) is an advance in understanding (achieved by resolving or accounting for the apparent contradiction). (On all this see Lawson, 2009 b, 2012a, 2012b; or Morgan, 2013).

18) Economics, including any transformed discipline, can and should avoid matters of ethics/morality.

Both parts of this claim are false. Ethics and moral argument are unavoidable, so it is better and indeed vital to address moral and ethical concerns in an explicit, systematic and sustained fashion (see Lawson, 2013b, 2015d). Even to contend that economists should avoid making judgements for purposes of practice/policy is to engage in ethical/moral argumentation. And to argue that almost all resources in the discipline be allocated to the practice of mathematical modelling is morally highly questionable (and indeed indefensible – see Lawson, 2015d). Ethics, and specifically an ethics grounded in ontological analyses of such matters as human nature, care, the nature of social organisation, and the possibilities for flourishing (of human and other living beings), is essential for any suitably transformed more pluralistic, emancipated, economics (see especially Lawson, 2015d).

19) Economics appropriately conceived is basically descriptive common sense, and this must be the basis of a transformed economics.

This is not so. This mistaken view is unhelpfully widely promoted by various heterodox economists especially. Although mainstream and heterodox economists disagree on the value of descriptive common sense, the two are united in presuming that the latter is the only real alternative to methods of mathematical modelling (the latter being a methodological common sense to the mainstream – see Lawson, 2015a, chapter 6). This shared presumption is simply wrong. In fact, we all need seriously to raise our game and move way beyond common sense in all its forms; and in this both causal analysis and explicit, systematic and sustained projects in social ontology are likely essential. That is, instead of simple and lazy common sense (and a naïve rush to forming simplistic policy recommendations) we need to return to pursuing economics in the manner prosecuted by the likes of Smith, Ricardo, Marx, Marshall, Veblen, Keynes, Schumpeter, Hayek and others, who dedicated themselves to explicit systematic and sustained programmes of uncovering the workings of the social system in which we actually live (see Lawson, 2015a; and also see chapters in Pratten, 2015).

20) The improving of economic teaching inevitably requires a good deal of prolonged, collective, formal discussion and debate over issues of substantive theory and policy.

This may be so, but I doubt it. Such an assessment seems to be underpinned by the idea that current academic economists, in constructing syllabuses, either conform to the uncaring atoms of their own theories or are ideologically biased at the level of theory and/or policy. I find neither to be typically the case. Rather, most modern economists are ideologically blinkered primarily by the idea that economics must be mathematical if it is to make a contribution. Even the lack of pluralism that so characterises the modern economics academy, stems mostly from a belief that to allow other methods into the toolbox will lead to a dumbing down of the discipline and a waste of resources.

Once the methodological error underpinning all this is revealed, or rather fully recognised, the potential is there in principle for the skills and energy of all the various participants in the economics academy to be harnessed to help fashion a more relevant discipline. Of course, new skills will likely need to be acquired by many. And serious research requires sustained effort and critical reflection. But we always must start from 'here'. In a process of successfully emancipating the discipline, the emphasis in the beginning will doubtless be as much upon supplementing, as upon replacing, existing courses, albeit likely involving the conversion of various currently compulsory courses into options (and also a change in styles of teaching [with the latter likely being rendered more interactive]). However, I do not anticipate that, with methodological blinkers removed, the task of providing relevant sets of courses in economics would be significantly more difficult (given time and sustained critical activity) than it is in any other open-minded, confident and successful discipline, where balancing acts regarding content taught are always to be performed (all disciplines must cope with issues of change in subject matter, competing interpretations and interests, and limited resources).

The pedagogical balancing act in an emancipated discipline of economics would presumably always be one of combining insights of the discipline regarded (albeit provisionally) as the more 'foundational' with any ongoing (possibly widely contested) advances regarded as contemporarily exciting and/or novel, taking into account local research and teaching expertise, skills and interests as well as the concerns of students; a transformed balancing act to that currently in play, but no less (or more) an inevitable balancing act.

The current problems, as I say, do not, in my assessment, derive in the main from an incapacity to care, or the sway of political ideology, or even an inability to find solutions. Rather, to repeat one last time, they stem from a pervasive and uncritical, indeed blinkered, belief in, and insistence upon, a simple and understandable, if ultimately mistaken, methodological dictum: that mathematical modelling is essential to any serious contribution to economics. Once that fallacy is transcended, the discipline can hopefully recommence its journey as a fruitful form of academic enquiry.

3. Conclusion

It is the declared goal of many observers both inside and outside the academy to improve the state of the current discipline of economics. My objective with the current paper is merely to point out that all such effort is almost worthless, certainly hopeless, if carried out without first emancipating ourselves from the noted methodological blinkers that render a (continuing) irrelevant economics inevitable. On a more positive note, it seems reasonable to suppose that once such blinkers are indeed removed, the obstacles that remain to achieving an emancipated and relevant discipline will be of the challenging and interesting sort that confront most other branches of academic endeavour. Of course there will remain various (political and other) differences no doubt, as in any science. But still achieving relevance and purposeful debate (if not agreement) about the state of the world has to be a very major step forward.

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