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Realism and critique in economics: an interview with Lars P. Syll

Lars P. Syll and Jamie Morgan [Malmö University, Sweden; Leeds Becket University, UK]

Lars Pålsson Syll is Professor of Social Science, Malmö University, Sweden. He holds two PhDs awarded in the 1990s. One in economic history and another in economics. His regular blog postings have made his name familiar to *Real-World Economics* readers. In 2018 his blog was recognized by Focus Economics (their list of bloggers also included Steve Keen and Michael Hudson).[1] His postings and publications have drawn on and drawn attention to the continued relevance of Keynes, the work of post-Keynesians, and the scope and potential of modern monetary theory. However, he is perhaps best known for his criticism of the methodological foundations of mainstream economics. His book *On the use and misuse of theories and models in mainstream economics (2016)* is published by WEA.

His work can be accessed at: https://larspsyll.wordpress.com

He is interviewed by Jamie Morgan for *RWER* ….

Jamie: Lars, perhaps a useful place to start would be with some introductory comment on what informs your reasoning. Whilst your postings range across many subjects you regularly return to a common theme. Specifically, the use economists make of mathematics to express theory and of analytical statistical techniques to conduct research. What are the key problems you see here and in what sense can or should matters of methodology provide a common thread to this common theme?

LPS: Well, I think the main problem here when it comes to applying mathematics and inferential statistics to economics is that mainstream economists usually do not start by asking themselves if the ontology – real-world economies and societies – is constituted in a way that makes it possible to explain, understand or forecast our economies and societies with the kind of models and theories that mathematics and inferential statistics supply. The basic fault with modern mainstream economics, in my view, is that the concepts and models it uses – often borrowed from mathematics, physics, and statistics – are incompatible with the very objects of economic study. The analytical instruments borrowed from the natural sciences and mathematics were constructed and used for totally different issues and problems. This has fundamentally contributed to the non-correspondence between the structure of economic science and the structure of real-world economies. And I think it may also be one of the main reasons why economists so often have come up with doubtful – and sometimes harmful – oversimplifications and generalizations.

Using simplifying tractability assumptions – rational expectations, common knowledge, linearity, ergodicity, etc. – because otherwise one cannot "manipulate" the models or come up with rigorous and precise predictions and explanations, does not really exempt economists from having to justify their modelling choices. Being able to manipulate things in models cannot be enough to warrant a methodological choice. Take, for example, the discussion on

[1] https://www.focus-economics.com/blog/top-economics-finance-blogs-of-2018

rational expectations as a modelling assumption. Those who want to build macroeconomics on microfoundations usually maintain that the only robust policies are those based on rational expectations and representative actor models. As I tried to show in my book *On the use and misuse of theories and models in mainstream economics* (2016) there is really no support for this conviction at all. If microfounded macroeconomics has nothing to say about the real world and the economic problems out there, why should we care about it? The final court of appeal for economic models should not be if we – once the tractability assumptions are made – can manipulate them. As long as no convincing justification is put forward for how the inferential bridging is made, mainstream model building is little more than hand-waving.

Mathematics can be an excellent tool for constructing models. But – it should never become a goal in itself. Most mainstream economists construct mathematical theories and models for the purpose of being able to deliver purportedly rigorous deductions that may somehow be exportable to the real world. By analysing a few causal factors in their "laboratories" they hope they can perform thought experiments and observe how these factors operate on their own and without impediments or confounders. But it does not – at least not as far as I can see – work. Causes have to be set in a contextual structure to be able to operate. Instead of incorporating structures that are true to the real world, the settings made in economic models are standardly based on mathematical tractability. In the models, they often appear as unrealistic tractability assumptions, usually playing a decisive role in getting the deductive machinery to deliver precise and rigorous results. This, of course, makes exporting to the real-world problematic, since these models are thought to deliver general and far-reaching conclusions that are externally valid. But how can we be sure the lessons learned in these theories and models have external validity when based on highly specific and unrealistic assumptions? As a rule, the more specific and concrete the structures, the less generalizable the results. Admitting that we in principle can move from falsehoods in theories and models to truth in the real world does not take us very far unless a thorough explanation of the relation between theory, model and the real world is made – and to just have a deductive warrant for things happening in a mathematical model is no guarantee for them being preserved when applied to the real world (see for example, Freedman, 2010).

In my view, what is wrong with mainstream economics is not that it employs models. What is wrong is that it employs poor models. They – and the mathematical-logical tractability assumptions on which they to a large extent build – are poor because they do not bridge to the real world in which we live.

Now, in mathematics, the deductive-axiomatic method has worked just fine. But science is not mathematics. As far as I can see, conflating those two domains of knowledge has been one of the most fundamental mistakes made in modern economics. It has made economics both narrow and hopelessly irrelevant.

Let me just round off this already far too long answer to your question with some remarks more specifically on the use of inferential statistics in economics.

As a critical realist, I must confess to not being surprised to find that an approach that – like econometrics – presupposes a closed system, fails when it is applied to essentially open systems such as real-world economies. Although the mathematical-statistical theory upon which it builds presupposes the existence of stable parametric relations, the identified relations are almost always unstable. Simply assuming that you can model social and economic relations with mathematical-statistical functions, is not a recipe for being able to contribute explanations of real-world phenomena and structures.

Limiting model assumptions in science always have to be closely examined since if we are going to be able to show that the mechanisms or causes that we isolate and handle in our models are stable in the sense that they do not change when we export them to our "target systems", we have to be able to show that they do not only hold under *ceteris paribus* conditions and so are of only limited value to our understanding of the real world. Now, I would maintain, the kinds of laws and relations that econometrics has established, are laws and relations about entities in models that presuppose causal mechanisms that are atomistic and additive. When causal mechanisms operate in the real-world they, however, only do it in ever-changing and unstable combinations where the whole is more than a mechanical sum of parts. If economic regularities obtain they do so, as a rule, only because we engineered them for that purpose. Outside man-made "nomological machines" they are rare, or even non-existent. Unfortunately, that also makes most of the achievements of econometrics – as most of the contemporary endeavours of mainstream economic theoretical modelling – rather useless.

In my view, economics should be a science in the true knowledge business, and so I remain a sceptic of the pretenses and aspirations of econometrics. Its ever-higher technical sophistication in no way makes up for the lack of serious under-labouring of its deeper philosophical and methodological foundations. Economists who consider it "fruitful to believe" in the possibility of treating unique economic data as the observable results of random drawings from an imaginary sampling of an imaginary population are skating on thin ice.

Jamie: As you say, a ("long") thorough answer. However, it does create something of an issue that begs answers that are as much sociological as they are methodological. You state:

> "This, of course, makes exporting to the real-world problematic, since these models are thought to deliver general and far-reaching conclusions that are externally valid. But how can we be sure the lessons learned in these theories and models have external validity when based on highly specific and unrealistic assumptions? As a rule, the more specific and concrete the structures, the less generalizable the results. Admitting that we in principle can move from falsehoods in theories and models to truth in the real world do not take us very far unless a thorough explanation of the relation between theory, model and the real world is made…"

Your main point focuses on mismatch between mathematical and statistical forms and the reality investigated – inducing failure of adequate description and of explanation and prediction (if prediction is ever possible). Consider, a member of the public may well in the modern world be sceptical of "expertise" but this applies to all purveyors of knowledge that have influence. A great deal is made of post-truth etc. At the same time, in general it makes no sense to deny good faith (good intentions) to experts. A mainstream economist, just like you, has a sense of self, an identity. It would be unreasonable to assume they collectively do not care about the *status* of what it is they produce. Like any other field, the majority have integrity and would not be able to do what they do if they did not in some sense consider it a contribution to "knowledge". Moreover, as we are all aware, mainstream economists come in many forms, with a range of socio-political affiliations. They are not mere ideologists who all conform to the same political position in the same way, which then provides some reason to deform or subvert the use (misuse) of theory. *And*, mainstream economists have and continue to have (criticisms not withstanding) great power and influence in the world (a valued skillset that in most countries makes them amongst the highest paid university graduates). To a member of the public it must seem weird that it is possible to state, as you do, such fundamental criticism of an entire field of study. The perplexing issue from a third party point of view is how do we reconcile good intention (or at least legitimate sense of self as a scholar), and power and influence in the world with error, failure and falsity in some primary sense; given that the primary problem is methodological, the issues seem to extend in different ways from Milton Friedman to Robert Lucas Jr, from Paul Krugman to Joseph Stiglitz. Do such observations give you pause? My question (invitation) I suppose, is how does one reconcile (explain or account for) the direction of travel of mainstream economics: the degree of commonality identified in relation to its otherwise diverse parts, the glaring problems of that commonality – as identified and stated by you and many other critics?

LPS: When politically "radical" economists like Krugman, Wren-Lewis or Stiglitz confront the critique of mainstream economics from people like me, they usually have the attitude that if the critique isn't formulated in a well-specified mathematical model it isn't worth taking seriously. To me that only shows that, despite all their radical rhetoric, these economists – just like Milton Friedman, Robert Lucas Jr or Greg Mankiw – are nothing but die-hard defenders of mainstream economics. The only economic analysis acceptable to these people is the one that takes place within the analytic-formalistic modelling strategy that makes up the core of mainstream economics. Models and theories that do not live up to the precepts of the mainstream methodological canon are considered "cheap talk". If you do not follow this particular mathematical-deductive analytical formalism you're not even considered to be doing economics.

So, even though we, as you formulate it, can identify many "diverse parts" of modern mainstream economics, the "degree of commonality identified" comes from the non-negotiable demand that the proliferation of models has to take place as a kind of axiomatic variation within the standard neoclassical model. But to me, and I guess most other heterodox economists, no matter how many thousands of "technical working papers" or models mainstream economists come up with, as long as they are just "wildly inconsistent" axiomatic variations of the same old mathematical-deductive ilk, they will not take us one single inch closer to giving us relevant and usable means to further our understanding and explanation of real economies.

The kind of "diversity" you asked me about, is perhaps even better to get a perspective on, by considering someone like Dani Rodrik, who a couple of years ago wrote a book on economics and its modelling strategies – *Economics Rules* (2015) – that attracted much attention among economists in the academic world. Just like Krugman and the other politically "radical" mainstream economists, Rodrik shares the view that there is nothing basically wrong with standard theory. As long as policymakers and economists stick to standard economic analysis everything is fine. Economics is just a method that makes us "think straight" and "reach correct answers". Similar to Krugman, Rodrik likes to present himself as a kind of pluralist anti-establishment economics iconoclast, but when it really counts, he shows what he is – a mainstream economist fanatically defending the relevance of standard economic modelling strategies. In other words – no heterodoxy where it *would really count*. In my view, this isn't pluralism. It's a methodological reductionist strait-jacket.

To me this also shows – to answer another part of your question – that the relationship between political/ideological views of economists and theory are not of the one-to-one kind. Leon Walras was a socialist. Knut Wicksell a social democrat. Paul Krugman is a political "radical". But that's not the point. A lot of my economics teachers at university in the 1970s and 1980s were Left party members, but they still preached neoclassical general equilibrium theory as if it were a gospel that had to be learned without question. To me, the political affiliations of these people were totally uninteresting. I am sure most of them had, as you put it, "good intentions" and looked upon themselves as "scholars". But I fiercely criticized them then – as I do now – not because of their political/ideological views, but because they taught irrelevant mathematical-formalist theories and models that had nothing to do with real-life. The ideology that I, as an economist focusing on science-theoretical and philosophical aspects of economics, am interested in is not of a political kind, but rather of a methodological kind. And that ideology is pervasive in economics!

Today we debate diversity a lot in economics in terms of what some call an "empirical revolution" that is said to have taken place within economics the last couple of decades (for example, Angrist and Pischke, 2010; Starr, 2014). It is often seen as a new kind of paradigm where economists in rigorous ways now try to test theories against reality, and where we can see a shift to empiricism away from philosophy with the use of imaginative empirical methods – such as natural experiments, field experiments, lab experiments, and RCTs.

I don't share that view. Why? Well, because these new methods face the same basic problem as theoretical models. They too are built on rather artificial conditions and have difficulties with external validity. If we see experiments as tests of theories or models that ultimately aspire to say something about the real "target system", then the problem of external validity is central. By this, I do not mean to say that empirical methods are so problematic that they can never be used. On the contrary. I am basically, though not without reservations, in favour of the increased use of experiments within economics. Not least as an alternative to completely barren "bridge-less" axiomatic-deductive theory models. My criticism is more about aspiration levels and what we believe we can achieve with these tools and methods in the social sciences. Making appropriate extrapolations from experiments to different settings, populations or target systems, is not easy. "It works there" is no evidence for "it will work here". Causes deduced in an experimental setting still have to show that they come with an export-warrant. The causal background assumptions made have to be justified, and without license to export, the value of "rigorous" methods and "on-average-knowledge" is despairingly small.

I find it hard to share the uncritical enthusiasm and optimism on the value of experiments and all the statistical-econometric machinery that comes with it. Although different empirical approaches have been – more or less – integrated into mainstream economics, I would argue there is still a long way to go before economics has become a truly empirical science. Sure, the "empirical turn" has made mainstream economics more diverse, but the "commonality" you referred to in your question still rules the roost. It is still mostly diversity within the mainstream methodological straightjacket!

So why do they do what they do then, these mainstream economists? And why do they still have so much "power and influence" in the world?

Most academic economists probably do what they do because that is what they have been taught to do and believe in. They have – almost exclusively nowadays – been trained to learn how to construct and use mathematical models, and preferably without asking if these models are appropriate or not for the problems at hand. Most economists today are brought up within the same neoclassical tradition, a tradition that looks upon economics as a kind of social physics and applying the same kind of methods that are used in the natural sciences. Many of them look upon their science as “the queen of social science” also for that reason. Mathematics is conceived of as being somehow “objective” and “neutral” and hence contributing to the scientific image of economics. And most of them are happy with just continuing to play along with the inherited smorgasbord of mathematical tools and models. They usually take for granted that being scientific means that you unquestionably have to use mathematics. The fruitfulness of mathematics is taken as an article of faith, and if you want to do serious economics, you simply have to express your ideas and theories in mathematical form. Mathematising physics and the natural sciences turned out to be a success in the 18th and 19th centuries, so it just has to (it is argued) be a success to do the same with social sciences and economics.

To me, all this is, however, disheartening nonsense. Why? Because these guys have never sat down and earnestly asked themselves the one fundamental question every scientist has to ask herself: is the “thing” we study really of the sort that makes the methods used feasible? Instead, they are so eager to appear “scientific” and just take for granted – or pretend – that the preferred mathematical-formalist methods they use are appropriate. In my eyes, this is nothing but incomprehensibly and outrageously unscientific! Before any epistemological elaborations on models and theories are made, it should be imperative that this ontological question has to be posed and answered. Since modern mainstream economists almost without exception do not (dare to) do so, they happily go on doing what they have always done. But – neglected ontology returns with a vengeance! The kind of “laws” and regularities they come up with have no export license to the real world. The quest for “rigour” and “precision” in the end only turns out to be obtained at the cost of losing contact with the real world. The (pretended) scientific “rigour” evaporates when confronted with real-life. Economics has to be more than a simple intellectual exercise! Sure, you can always SAY that it is possible to learn things of significance about our world from constructing these kinds of models. But the proof of the pudding is in the eating, and I have still not seen a single convincing demonstration of HOW this is done! And for those of us who go for reliable knowledge and want to use economics for understanding and explaining things in the world in which we live, that means these “as if” model results are nothing but a useless and irrelevant waste of time! The (in my view perverted) allegiance to “rigour” and to a focus of scientific endeavour on proving things in mathematical-formal models is a gross misapprehension of what economics is or ought to be. The total lack of explanatory success of mainstream economics when it comes to things like (real-world) unemployment, structural change, financialization, and economic crises is a testament to the futility of building theories without solid ontological under-labouring! A relevant and realist economics should never give up on the real world and content itself with proving things about thought up worlds in “fables”, “parables”, “stories”, “fictions”, “narratives” or what have you.

On your question of “power and influence” of economists, I do actually think mainstream economics has lost much of it – at least to “members of the public” – during the last decade. Very few people outside economics departments – rightly – take the kind of analyses that mainstream economists come up with seriously. And I think it has much to do with methodology. Mainstream economists still embrace simplistic theoretical assumptions and almost religious faith in mathematical techniques where real-world applicability isn’t even on the agenda. And even if they expand the smorgasbord of analytical models with new empirical studies, that isn't enough. The crisis of 2007-08 showed more than anything else that mainstream economics with all its fancy mathematical and econometric models and theories had no answers to the questions that the UK Queen and others posed to understand what happened to our economies.

Today, when university students all over the world are increasingly beginning to question if the kind of economics they are taught – mainstream economics – is of any value, and some even question if economics really is a science at all, it is all-important to ask ourselves how we are going to proceed if we want to re-establish credence and trust in economics as a science (see, for example, Rethinking Economics & The New Weather Institute, 2017).

For a start, I think we should stop pretending that we have exact and rigorous answers on everything. Because we don't! Mainstream economists build models and theories and tell people that they can calculate and foresee the future. But they do this based on mathematical and statistical assumptions that often have little or nothing to do with reality. Then I think one should really reconsider the use of mathematics in economics since the kind of formalism that mathematics instantiates is perhaps the deepest source of the irrelevance and uselessness of modern economics. Mathematics gives exact answers to exact questions. But the relevant and interesting questions we face in the economic realm are rarely of that kind. Instead of a fundamentally misplaced reliance on abstract mathematical-deductive-axiomatic models having anything of substance to contribute to our knowledge of real economies, it would be far better if we pursued "thicker" models and relevant empirical studies and observations. Models that we already know are nothing but absurd fictions, are not the stuff that real science is made of! And – finally – we should end treating other social sciences as poor relations. We have to be more open-minded and incorporate knowledge and perspectives from other disciplines. Economics has long suffered from hubris, and a more broad-minded and multifarious science would definitely enrich today's altogether still too introverted economics.

Jamie: To follow up on some of what you say here and to make some additional sense of what I was getting at by "sociological"; if we grant that mainstream economics persists with theorems, methods, models and applications that are rooted in fictions and that mainstream economists make claims to precision or relevance that are ultimately falsely premised, this does not make them irrelevant nor does it imply they are without consequence. Future facts can be created by the (sometimes unintended) consequences of past fictions (and those fictions can be unrecognized falsities inaccurately considered true when false or more diffusely simply convenient or necessary points of departure or building blocks of model forms). Economics, it seems worth emphasising, has long been caught in the tension between representing itself as a science that describes a fundamental economic reality that can then be manipulated *and* producing a form of theory that (inadvertently) influences the nature of that economic reality (it is not fundamental in a fixed sense) *and* advocating a form or forms for that economic reality. It is perhaps because of the resistance to taking ontology seriously that the role of economics in social reality has remained so problematic for economists and for societies. Mainstream economics is not without norms or advocacy, but does tend to assimilate these on the basis of its own (implicit) ontology and methodological commitments – the role of *normativity* itself as a constituting part of social (economic) reality is rarely at issue; instead norms become predicates or hypotheses to test.

In any case, social (economic) reality is not a product of what is true, but rather (arguably) a consequence of what we do based on the complex structuring of activity that in part has depended on the influence of mainstream economics. RCTs, for example, have had profound effects on development policy (as Martin Ravallion and others note), modern macroeconomics and financial theory have, as you note, played a role in financialization and financial crises. Here, there are various points one might make that speak to your argument regarding economics as "science". You note: "The fruitfulness of mathematics is taken as an article of faith, and if you want to do serious economics, you simply have to express your ideas and theories in mathematical form." One might elaborate that the "empirical turn" (including the claimed "credibility revolution", Angrist and Pischke, 2010) has not to any great degree reversed a general tendency in mainstream economics to *not* apply standard scientific practice. There is a significant difference between adopting methods that one thinks are social science equivalents of natural science and making use of them in the same way they are made use of in natural science (and this is an additional point to whether in fact the adoption is appropriate or the understanding of natural science they are posed within is adequate – as Phil Mirowski or Tony Lawson or Edward Fullbrook might note; for example, Fullbrook, 2016). By this I mean that mainstream economics has placed increasing emphasis on empirical work but still pays considerably less to duplication, replication or confirmation of results (a matter one should not conflate with the issue of whether in fact there are problems of philosophy of science with falsification, confirmation etc. if thinking about positivism or empiricism). The *American Economic Review,* for example, published two sections on this problem recently (both May 2017, 107(5), for example, Hoffler, 2017), albeit without this making any great difference to the direction of travel of the mainstream (a point one might also make about the turn towards an ethical code for economists that the AEA has sponsored).[2] Sociologically, surely this lack of focus on replication of results is one (not the only) way in which the status of the mainstream based on its current commitments is able to persist?

More broadly, there is a complex socialisation process that might account for economists' self- image and that might account for the nature of their influence in the world and how it persists (scepticism notwithstanding regarding expertise). Fourcade and various collaborators (for example, Fourcade et al, 2015) have done a lot of interesting work on this, as I am sure you are aware (as has, over the years, David Colander, and before his demise Fred Lee; for example, Colander, 2005; Colander and Klamer, 1987; Colander et al., 2005). This brings me to something else that perhaps you can usefully clarify, not least because it tends to confuse the status and consistency of critique, and that is use of the term neoclassical. Throughout your comments you seem to refer to neoclassical and mainstream as though the two are synonymous. There are those who consider this confusing for various reasons: the term had a particular meaning when coined by Veblen, the term was taken up by Stigler and others subsequently and became synonymous with Chicago School and associated thought and the term is often used more diffusely as a general term for mainstream economics and to imply an orthodoxy that is little changed (and concomitantly is sometimes used as a pejorative term by critics and sometimes used as a self-identification by advocates – though less today than in the past; see, for example, Arnsperger and Varoufakis, 2006; Morgan, 2016). I am sure I am not telling you anything you don't know here since you have a deep background in the history of economic thought as well as an interest in philosophy of economics. But your main purpose has always been constructively focused on argument intended as a contribution to transforming economics. With this in mind and on reflection, how do you see the term neoclassical and your use of it? You might also want to place this in the context of pluralism –

[2] The May 2017 issue of *American Economic Review* also contains an article on abduction and one might think this too is significant in terms of changing face of economics; however, the term is used for an iterative approach to multiple modeling and hypotheses – a highly limiting understanding of what abduction allows.

what scope do you see for a more pluralistic economics and how do you see current tendencies in mainstream economics (in so far as instantiated in schools of thought) as part of that pluralism?

LPS: A couple of years ago I was interviewed by a public radio journalist and we were discussing the monumental failures of the prediction-and-forecasting-business. But – the journalist asked – if these cocksure economists with their "rigorous" and "precise" mathematical-statistical-econometric models are so wrong again and again – "why do they persist wasting time on it?" Yes, indeed, why do they? Do we want to make claims about the real world in which we live, well, then we have to start using assumptions and models that we at least believe are true. Starting – as most economists do today – with assumptions that we make for mathematical tractability reasons and which no one believes are in any sense true, is a non-starter that only perpetuates endless and totally irrelevant model exercises that makes up such a big part of economics today. In my view, at least, real-world accuracy always beats model rigour and precision. We want to know *true* things about *reality*, not *consistent* things about *models*. The more I think about it, the more I wonder why any sane person should be interested in that kind of endless parade of known to be stupid models.

But maybe, as you suggest, one has to put a more sociological or psychological view on these matters. I know that people, like Marion Fourcade, have tried to explain economists' self-confidence as "intellectuals" and "scientists" from those perspectives. Economists, more than any other social scientists, concern themselves with measurable quantities, use quantitative methods and mathematics, emulate "real" sciences like physics, and so, of course, have to be considered much more "objective" and in possession of higher intellectual capabilities than the rest of the "riffraff" social sciences. I have spent forty years within the academic economics tribe, and have never been able to understand or share that inflated and self-congratulatory superiority view on our discipline.

Sure, I have met a lot of both talented and intelligent economists, but sad to say, talent and intelligence are no guarantee for delivering truly *interesting* and *relevant* knowledge. They publish a lot, are invited to conferences, have highly-paid jobs and are considered to be "experts" and "authorities" on almost everything that comes their way. Why? One important reason is that economists have been successful in selling the image of themselves as knowledgeable truth tellers and "pure" scientists equipped with scientific models and theories that are "objective", "apolitical" and "non-normative", and that politicians and business leaders can use as some kind of cooking recipes or blueprints for solving all kinds of problems they may encounter.

To me, however, this is nothing but an example of economics' pretence-of-knowledge syndrome (a term used by Hayek in his Swedish Bank Prize speech and more recently popularised by Caballero, 2010). The "econ tribe" (Leijonhufvud, 1973) has become so entranced with its own deductive-axiomatic models that it has forgotten that there is an all-important difference between the rigour and precision they manage to achieve in their models and the real-world in which policymakers and politicians have to apply these models. In the "econ" models, all uncertainty can be reduced to calculable risk, all actors have rational expectations, and they always optimize. Reality, however, is different. More complex. Genuine uncertainty is everywhere, people are not "rational", and do not always optimize. Although economists think they are in possession of relevant knowledge, trying to use those models in that real-world context is not only pointless but also many times harmful and makes a colossal muddle of things. It would indeed be better, as Keynes once said, if economists could look upon themselves as humble and competent dentists!

You did also raise a couple of terminological questions about “neoclassical” and “pluralism”, so let me say a few words about that.

Looking at what has happened with what we used to call neoclassical theory, it is obvious to everyone that it in many respects has become more diverse over time. Most mainstream economists today do not characterise themselves as neoclassical, but rather self-identify as game theorists, experimental economists, behavioural economists and so forth. But many of them share the basic core of the neoclassical tradition out of which these varieties of modern economics have emerged. Usually, that is rather unproblematic since, in the context of the given discourse, we usually know what it refers to. To me, it has, however, become natural to use the term “mainstream economics” and by that I am referring to what I consider the shared core of neoclassical economics – the methodological imperative of using mathematical-deductive-axiomatic methods. You could, of course, argue that the genealogy of the term “neoclassical” – going back to Veblen–says something else. But I think somewhere we have to accept that terms and concepts live their own lives. That is not a big problem for me, as long as you make clear to your readers in what sense you yourself use those terms and concepts. Much of what you mention – the new “empirical turn” and the “credibility revolution” – has in some ways definitely broadened the scope of economics, but I still think the new approaches and sub-disciplines are pervaded with neoclassical thinking and its inherent bias towards analytical formalism. It is also for that reason, as I said before, that I think the “empirical” transformation of economics is mischaracterized. I can’t really see that it constitutes a “paradigm shift”. I look upon it more as an extension of the field of *application* of economics.

There has long been a need for more pluralism in economics, on that most heterodox economists agree (see for example, Lee and Cronin, 2016; Jo et al, 2018). The question, however, is what *kind* of pluralism. Here my view is that what we need is not so much more of different theories and models, but rather *methodological* pluralism. That kind of pluralism would also open-up a much needed philosophical and science-theoretical awareness.

In mathematics, the deductive-axiomatic method has worked just fine. To reiterate because this bears repeating, science is not mathematics. Conflating those two domains of knowledge has been one of the most fundamental mistakes made in economics. There is no way you can relevantly analyse economic phenomena as a purely logical relation between hypothesis and evidence. In economics, we have to argue and try to substantiate our beliefs and hypotheses with reliable evidence. Deductive inferences are purely *analytical* and it is this truth-preserving nature of deduction that makes it different from all other kinds of reasoning. But it is also its limitation, since truth in the deductive context does not refer to a real-world ontology, and it is totally non-ampliative – the output of the analysis is already given by the input.

Instead of this insistence on using mathematics and the deductive kind of inference I would rather see economics orient as more of an abductive science. Using abduction we infer something based on what would be the best explanation of data given some contextual background assumptions. We start with a body of (purported) data and search for explanations that can account for the data. Having the best explanation means that you, given the context-dependent background assumptions, have a satisfactory explanation that can explain facts better than any other competing explanation – and so it is reasonable to consider the hypothesis to be true. Even if we do not have deductive certainty, our abductive reasoning gives us a license to consider our belief in the hypothesis as reasonable. This, of course, does not mean that we cannot be wrong. Abductions are fallible inferences. The premises do not logically entail the conclusion. But if the abductive arguments put forward are strong enough, they can be warranted and give us justified true belief, and hence, knowledge. As economists we sometimes – much like Sherlock Holmes and other detectives that use abductive reasoning – experience temporary delusion. We thought that we had reached a strong abductive conclusion by ruling out the alternatives in the set of contrasting explanations. But what we thought was true turned out to be false. That does not necessarily mean that we had no good reasons for believing what we believed. If we cannot live with that contingency and uncertainty, well, then we're in the wrong business. If it is deductive certainty you are after, rather than the ampliative and defeasible reasoning in abduction – well, then get into math or logic, not economics.

I know that mainstream economists do not want to make this methodological change, because then they would have to give up their dream of building a "rigorous" and "precise" science on a par with physics. They do not want to admit that there are severe limits to formalism. I am a pluralist and wouldn't dream of saying that we should have none of that. Like Keynes when he criticized Tinbergen, I say: let them go on with their modelling and methods. But there has to be an end to the insistence that you must work within the constraints of the mathematical-deductivist frame. The mathematisation of economics since more than seventy years now has made these economists more or less obsessed with their formal-deductive-axiomatic models. Confronted with the critique that they do not solve real problems, they often react as Saint-Exupéry's Great Geographer, who, in response to the questions posed by The Little Prince, says that he is too occupied with his scientific work to be able to say anything about reality. Sure, modern mainstream economics is in some sense "rigorous" – but if it's rigorously wrong, who cares? Method and theory pluralism shouldn't be an end in themselves, but instead of making formal logical argumentation based on deductive-axiomatic models the message, I think we are better served by economists who – like dentists – try to contribute to solving real problems. As John Maynard Keynes (allegedly) stated – "It is better to be vaguely right than precisely wrong."

Jamie: OK, there is a consistent theme here that underpins much of what you say and that anyone familiar with your work would recognize. And I expect most readers of RWER have great sympathy with the general framing and direction of travel of your comments. But, in the spirit of critique it is also worth considering whether a consistent theme has a consistent analogue. One might, for example, wonder how far *non-mainstream* economists exhibit the desirable characteristics you state (abductive reasoning, limited or contextual use of rigorous deductive-analytical methods, emphasis on evidence, commitment to realism, methodological pluralism, an open–minded approach to alternatives etc.). For example, to what degree would you characterize post-Keynesians as developing their work based on these kinds of characteristics, and, in so far as they do, in what sense has this provided more adequate accounts of real economies?

LPS: Well, I think it is fair to say that being a science-theoretical critical realist I find myself having a lot in common with several heterodox traditions. I've always been interested in studying the work of people like Veblen, Commons, Marx, Keynes, Kalecki, Åkerman, Davidson, Minsky and (on epistemological issues) Hayek. There's a common ontological orientation in these economists' methodological stance that I appreciate. They don't conflate model and reality.

To me, especially in times when scientific relativism is expanding, it is important to keep up the claim for not reducing science to a purely discursive level. We must maintain the Enlightenment tradition of looking upon science as in the truth business. Science is made possible by the fact that there are structures that are durable and are independent of our knowledge or beliefs about them. There exists a reality beyond our theories and concepts of it (even if social reality is also in some sense concept-dependent).

The problem with modern economics – which to a large extent is nothing but a variety of positivist social science – is not that it gives the wrong answers, but rather that in a strict sense it does not give answers at all. Its explanatory models presuppose that social reality is "closed", and since social reality is fundamentally "open", models of that kind cannot explain anything about what happens in such a universe. Mainstream economics has to postulate closed conditions to make its models operational and then – totally unrealistically – impute those closed conditions to society's real structure. But – the world itself should never be conflated with the knowledge we have of it. Science can only produce meaningful, relevant and realist knowledge if it acknowledges the divide between model and reality – and then, most importantly, earnestly tries to bridge it! Ultimately this also means my critique of mainstream economics is that it doesn't take that ontological requirement seriously.

When I read post-Keynesian economists I notice most of them share that fundamental realist view, and already by doing so actually provide more adequate accounts of real economies than does mainstream economics (for example, on methodology, Dow, 1996). Following in the footsteps of Keynes, post-Keynesians try to develop an economic theory that does not portray monetary economies as if they were barter economies; that does not reduce genuine uncertainty to calculable risk; that takes finance and instability seriously; that does not treat real historical time as if it was possible to analyse with an ergodicity postulate that more or less reduces the future to a repetition of the past.[3]

Money matters. Unemployment is to a large degree involuntary. The future is non-ergodic. To me, those views are some of the hallmarks of post-Keynesian theory – a theory that gives a far more adequate account of real-world economies than formalistic-deductive-axiomatic mainstream economics.

The basic post-Keynesian pillar is the recognition and acceptance of an ontological fact – societies and economies are permeated by genuine uncertainty. But in "modern" macroeconomics – Dynamic Stochastic General Equilibrium, New Synthesis, New Classical and "New Keynesian" – the variables used in the models are treated as if drawn from a known "data-generating process" that unfolds over time. "Modern" macroeconomics obviously did not anticipate the enormity of the problems that unregulated "efficient" financial markets created. Why? Because it builds on the myth of us knowing the "data-generating process" and that we can describe the variables of our evolving economies as drawn from an urn containing stochastic probability functions with known means and variances. Some macroeconomists, however, still want to be able to use this tool (their "hammer"). So, they decide to pretend that the world looks like a nail, and pretend that uncertainty can be reduced to risk. They construct their mathematical models on that assumption. The result: financial crises and economic

[3] Note from Jamie: for a recent post-Keynesian collection on these issues see Dow et al. (2018).

havoc. The most basic lecture post-Keynesian economists – like Minsky and Davidson – have taught us is this: trying to cope with an unknown economic future in a way similar to playing at the roulette wheel, is a sure recipe for only one thing – economic disaster.

Nowadays there is a lot of discussions about Modern Monetary Theory (MMT). To me – an old student of Minsky – that is also a sign of heterodox economics contributing to developing economics in the right – realist and relevant – direction.[4] MMT rejects the traditional Phillips curve inflation-unemployment trade-off and has a less positive evaluation of traditional policy measures to reach full employment. Instead of a general increase in aggregate demand, it usually prefers more "structural" and directed demand measures with less risk of producing increased inflation. At full employment deficit spending will often be inflationary, but that is not what should decide the fiscal position of the government. The size of public debt and deficits is not – as already Abba Lerner argued with his "functional finance" theory in the 1940s – a policy objective. The size of public debt and deficits are what they are when we try to fulfil our basic economic objectives – full employment and price stability. That government can spend whatever amount of money they want is a fact. That does not mean that MMT says they *ought* to – that's something our politicians have to decide. No MMTer denies that too much of government spending can be inflationary. What is questioned is that government deficits are *necessarily* inflationary.

Take Sweden, for example. In my country, as in so many other countries, neoliberal "norm politics" invaded the economy in the 1980s and 1990s. The mantra was that it was high time for Sweden to follow in the footsteps of Thatcher and Reagan. Deregulate the economy – especially the financial markets – and make the central bank independent, so that one could concentrate economic policies on inflation-targeting rather than on low unemployment, then Sweden would prosper. Today we have a Finance Minister that still keeps on talking about how necessary it is to balance the budget. And that in a situation where the deficit is at its lowest in 40 years and still falling![5]

What MMT shows, is how harmful this penny pinching really is. The Swedish experience illustrates how a government's ability to conduct an "optimal" public debt policy may be negatively affected if public debt becomes too small. To guarantee a well-functioning secondary market in bonds it is essential that the government has access to a functioning market. If turnover and liquidity in the secondary market become too small, increased volatility and uncertainty will, in the long run, lead to an increase in borrowing costs. Ultimately there's even a risk that market makers would disappear, leaving bond market trading to be operated solely through brokered deals. As a kind of precautionary measure against this eventuality, it may be argued – especially in times of financial turmoil and crises – that it is necessary to increase government borrowing and debt to ensure – in the longer run – good borrowing preparedness and a sustained bond market.

Jamie: We've travelled some distance here and touched on a lot of subjects that we could probably discus at much greater length. For example, following themes central to post-Keynesian work on money economies, the significance of how the majority of money is created and what it is actually created through and for. That is, bank money generated from

[4] Noting, of course, that there is debate within post-Keynesian and structural Keynesian circles regarding the originality and adequacy of MMT – Paul Davidson and Thomas Palley are to different degree sceptics. For example, contrast Davidson (2017) and Wray (2015).
[5] Note from Jamie: Belfrage and Kallifatides (2018) provides an interesting contemporary analysis of some of the issues.

borrowing, principally for the production and trading of financial assets rather than as a by-product of primary productive investment (see McLeay et al, 2014; Kumhof and Zoltan, 2015). It strikes me that post-Keynesians have had far more insightful things to say regarding this, financialisation, credit cycles etc. than most others. Still, there remains some obscurity regarding what money "is", if one wanted to explore this at the most basic level of conceptualization via ontology. For example, is it appropriate to advocate a "credit theory of money" or is this more accurately phrased as a "theory of credit money"? Is it that *all* money must be credit or is it that money as we know it is typically positioned as a credit relation but need not be so? This is a question I have been thinking about recently having read Tony Lawson in debate with Geoff Ingham on this (see Lawson, 2019; Ingham, 2018). Lawson is one of the if not *the* prime mover in rehabilitating philosophy (as ontology) in and for contemporary economics (see Fullbrook, 2009; Morgan and Patomäki, 2017). He seems to be someone you have a lot of time for. As a way to sign off, how would you place your work and influences in terms of his and other methodologists?

LPS: I think it is natural for someone like me – a critical realist – to embrace post-Keynesianism. But there are, of course, different varieties of realism and different views on how to relate to the object of study. If you take Tony Lawson and, for example, Uskali Mäki, they obviously share a common interest in analysing the ontological assumptions – explicitly or implicitly – made in the modelling strategies used by economists (see for example, Lawson, 2015; Mäki, 2013, 2001). But where Mäki is mostly focused on performing a traditional, rather "detached" academic analysis, Lawson also – like myself – is more openly critical of the state of "modern" economics and wants to actively contribute to change the direction of economics.

Lawson and Mäki are both highly influential contemporary proponents of economic methodology and philosophy. Next to Nancy Cartwright and Kevin Hoover, I guess they are those contemporary methodologists I have learned most from. Although to a certain degree, probably also a question of "temperament", I find Lawson's critique of mainstream economic theories and models deeper and more convincing than Mäki's more "distanced" and less critical approach. Mäki's "detached" style probably reflects the fact that though he is trained in economics he works as a philosopher with an interest in economics, rather than as an economist (whilst Lawson remained in an economics department at Cambridge). Being an economist myself it is easier to see the relevance of Lawson's ambitious and far-reaching critique of mainstream economics than it is to value Mäki's sometimes rather arduous application of the analytic-philosophical tool-kit, typically less ambitiously aiming for mostly conceptual and terminological "clarifications."

Just to round off this interview a little, let me say some words about the future of economics.

Contrary to people like Dani Rodrik – who totally dismisses calls for methodological pluralism in economics and think that, just because the "smorgasbord" has grown with the (alleged) "empirical turn" in economics, we have had a tremendous paradigm-shift in economics – I would rather argue that this continuing insistence on using only a deductive mathematical framework for approaching economic issues is a strong sign of how limited the change in mainstream economics really has been. From a methodological point of view, the message is still "business as usual."

From my own point of view, I think it is safe to say that if economics is going to be a relevant and useful project in the future it will have to redirect its present underlying methodology and philosophy. Although very much in favour of the quest from economics students for more pluralism and the need for more and different theories and models, I don't think that is enough. The cut has to go deeper! Economics has to get back to being – as in the 19th century – more than just an "intellectual exercise" and reorient itself into being a real-world science. It has to become aware of and accept, the limits of analysis set by ontological facts. The world is, to a large extent, a complex, open, evolving, genuinely uncertain, emergent, non-ergodic, nonhomogeneous, and organic totality. Mainstream economics has refused to earnestly reflect on what these impregnable facts do to our possibilities of making relevant models and analyses. Instead, they have contented themselves with building toy models of ideal non-existent worlds. Going on just refining that project will not constitute a real advance. To progress, economics has to totally re-evaluate the basic premises of that modelling strategy. If not, economics will remain a useless "intellectual exercise."

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