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Entrevista

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1. Epistemology matters? For whom? Why?

Even when restricted to economics and economic methodology, there is no simple answer to this question. Epistemology matters to particular individuals (and groups) in many different ways, but it is always with us. When one practices economic science – theory or empirical work – or even when one is just thinking informally about current economic events and policies, one is always employing some conception of economic knowledge. Which economic theories provide the best insights into and/or most knowledge about the theoretical questions we are examining? What data and empirical techniques provide the most reliable knowledge about the empirical relations we seek to understand? What is reasonable to suppose about the causal forces behind individual behavior? Who, and what theories, should we trust and why? There is no epistemic tabula rasa; we must start from some conception of what our knowledge of the economic world is, can, and should be, before we can engage in any systematic thinking (formal or informal) about the economy or economic policy. The more controversial question is whether experts in the epistemology of economics can improve the quantity and quality of our economic knowledge. I certainly think that is the case, but it is a more controversial issue than whether epistemology matters.

2. Pluralism is a vital demand in many communities engaged in heterodox economics including Feminist Economics and many others. Do you think it is a relevant subject to be philosophically addressed because of its consequences for the advance of the discipline or is it just an ideological and political demand

on the part of marginal groups struggling for better conditions?

Although pluralism is a topic discussed within the heterodox literature, there are arguments for pluralism that are quite consistent with standard textbook economics. Every since Adam Smith's invisible hand, the virtues of competition and competitive behavior have long held a revered position within economic theory. And yet, what is competition without variation? The evolutionary argument for competition and the survival of the fittest does not get off the ground without variation. Evolution is based on random variation and selective retention – and without variation within the set of things to be selected over, there is no potential gain from selective retention. This is the same in economics. For example, this is the way that entry and exit work in the theory of the competitive firm; the competitive process selects among the most efficient firms but without some variation among the individual firms there would be nothing to select over and no way to achieve efficiency gains. Similarly well-known arguments about the gains from trade only apply when there is some variation among various countries (technology, endowments, etc.). This idea manifests itself not only in the invisible hand, the theory of the firm, and gains from trade, but also in things like portfolio diversification and a number of other ideas in standard economic theory.

Of course as many philosophers of science have argued over the years – Philip Kitcher, Nicholas Rescher, Paul Feyerabend, a number of Popperians, defenders of evolutionary epistemology, and others – the growth of scientific knowledge can be seen to work in the same way. Given a sufficiently wide variation of scientific ideas (i.e. sufficient cognitive diversity) and the proper selection mechanism – either from community norms and institutions, or proper individual behavior (or both) – a properly functioning scientific community will selectively retain the ideas that make the greatest contribution to the growth of scientific knowledge. This seems to be the view of how our scientific knowledge grows that is most consistent with the way that economists generally think about diversity and the competitive process. Perhaps this is the way that feminist and other heterodox schools of thought should approach the defense of pluralism within economics. The demand for pluralism may be an "ideological and political demand on the part of marginal groups struggling for better conditions," but it need not be exclusively this. There are serious philosophical arguments – arguments that are consistent with well-accepted ideas within standard economics – that support increased theoretical pluralism within economic science.

Given this connection, it has always struck me that one of the great ironies of the economics profession is that it is a discipline that has always emphasized the efficiency-enhancing properties of competition and diversification and yet in terms of its own social and institutional structure seems to be relatively anti-pluralist and thus anti-competitive. Economists tend to see competition as a good thing everywhere except within their own profession (or at least not at the level of core theoretical foundations). At one point earlier in my career I worked a bit on the economics of economics; I thought it would be a "natural" for economists. If economists believe that the outcomes we observe in human life are the unintended consequences of rational self-interested action, then why not think of the outcomes of the economics profession in the same way? And if the right institutional arrangements can harness that self-interested action into economic efficiency, then why can't the right institutional arrangements within the economics profession harness the rational self-interested behavior of individual economists into the proper cognitive division of labor and level of epistemic efficiency?

Although I still think these are very interesting questions, my experience has been that most economists do not seem to be very comfortable with such arguments. They apparently think in terms of what might be called "economics exceptionalism": economics gives insight into rational individual behavior and the emergence of social efficiency from such behavior, except in the case of economics itself. As an example of this, John Davis and I recently organized a workshop on the question of the epistemic consequences of herding within the economics profession (the impact on economic knowledge of all economists employing the same theoretical framework and/or techniques). Although most of the attendees were economists, the only people who dealt with the question in terms of the impact of herding on the cognitive/epistemic division of labor were non-economists! We were expecting some of the economists to tell a standard textbook story about how herding into a single monopoly research program could produce cognitive inefficiency and epistemic risk to both the economy and the economics profession, but evidently that is not the way economists think about such things.

As a final point I would add that the discipline seems to have become more pluralist over the last few decades. This is a position that John Davis and a number of others within the methodological community have been defending and it clearly seems to be correct. New fields of research have sprung up – experimental economics, behavioral economics, and neuroeconomics – and economists working within more traditional fields increasingly employ a wider range of theoretical and empirical tools. Until very recently almost every major contributor to the field of economic methodology – from John Stuart Mill, to Lionel Robbins, to Milton Friedman – started from the position that experiments could not be done in economics, and motivated the need for economic methodology on these grounds: as needed to explain how economics could still be scientific even though it was not experimental. Now it is becoming experimental. In addition, the computer has widened the scope of how theory can be done and has increased the range of quantitative tools available to economists. On the empirical side, consider the vast array of empirical work that now goes on within applied economics and the wide range of sources of empirical data that are now considered professionally acceptable compared to even a few years ago: laboratory experiments, field experiments, survey data, contingent valuation, and on and on. Deirdre McCloskey once said that during the 1970s she asked an economics graduate student what the word "empirical" meant and the student replied "linear regression using government data." Those days now seem to be long gone. This may not be the pluralism that many heterodox economists were looking for, but it is increased pluralism and from my own methodological perspective it is a good thing.

3. Behavioral Economics is now an established field of research. And contributions from the social sciences and the role of ethical issues have been growing in the practice of economics. How do you see the prospects of interdisciplinary work in the field of economics?

My answer to the previous question also applies to the rise of behavioral economics and the revival of ethical concerns within economics. I think these are both positive recent developments within the discipline. In general, the prospects for interdisciplinary research and application have improved significantly over the last few decades: both those which combine various social sciences and also those combining the social and natural sciences (particularly biology). Fields that were long viewed as substitutes are increasingly being viewed as potential complements.

This said, there are some specific things to be noted about behavioral economics and ethics. Behavioral economics has in many ways been the canary in the coal mine for the new interdisciplinarity, and I think its survival – despite repeated well-organized efforts to attack it on methodological grounds – is a very good sign that the air within the economics profession is a bit easier to breathe. This said, there are a number of very important and hotly contested areas of controversy in, and surrounding, behavioral economics. One issue is the relationship between behavioral economics as a positive – predictive and explanatory – scientific theory of behavior that challenges standard rational choice theory on scientific grounds, and behavioral economics as a normative theory of how rational agents ought to behave and how they could be nudged back into behaving rationally when they fail to do so. As Floris Heukelom and others have argued, although the variety of choice anomalies uncovered by behavioral economists seriously challenges the adequacy of rational choice theory on positive scientific grounds, when it comes to normative issues most behavioral economists are quite traditional; they simply accept rational choice theory's normative characterization of what agents ought to do in order to be rational and focus on how to correct the public's frequent failure to exhibit such rationality. This is only one of many important, but quite complex, questions raised by recent behavioral economics. Similar complexities are involved in the recent revival of ethics within the professional discussion of economists. Daniel Kahneman and others have endorsed a return to the hedonistic utilitarianism of Jeremy Bentham, Amartya Sen has challenged utilitarian-based welfare economics with his capabilities approach, and there has been a mini-revival of Adam Smith's moral sentiments and impartial spectator. I think all of this work is healthy, but at this point it is too early to tell exactly how either behavioral economics or the revival of ethics will ultimately impact standard practice within the profession. In any case these developments provide those working in the philosophy of economics an opportunity to help shape the eventual impact.

4. If you had to pick out five new promising issues in philosophy of economics, which ones would you choose?

This is a tough question since there are so many more than five. But here are five (in no particular order).

1) The first one is noted in the previous paragraph, but the topic has much wider implications than merely the connection to behavioral economics. It is the ongoing re-examination of the subtle and shifting grounds between positive (explanatory and/or predictive) economics and normative economics (ethically or rationally normative). Economists held on to the bright-line distinction between positive/scientific economics and normative economics long after such distinctions had become blurred in other social sciences and in philosophy. The blurring is now starting to take place within economics and it presents a very fertile field for investigation by scholars in the philosophy of economics.

2) Like the positive/normative distinction, rationality and choice theory are currently under serious re-examination. The old workhorse of well-ordered preferences and constrained optimization is still the baseline for economic rationality, but there are also many other ways of characterizing rationality and choice at work within contemporary economics. My own research has focused primarily on contemporary revealed preference theory, but there are also various versions of bounded rationality; the ecological rationality of Gerd Gigerenzer, Vernon Smith, and others; as well as prospect theory and a number of other ideas from within experimental psychology and behavioral economics. How all these views relate to each other and how they all work, or fail to work, in both positive and normative economics is an extremely exciting area of inquiry. I would note that a lot of recent work has been done in this area – by Daniel Hausman, Don Ross, and many others – but there is still important work to do.

3) What economic models are, how they function, and how their use in economics differs from the way they are used in other sciences remains a fertile area for research. Over the last few years the work of Marcel Boumans, Mary Morgan, Uskali Mäki and others has greatly improved our understanding of economic models, but given the importance of, and variation among, models within economics there is much more to understand.

4) One field within economics that I think is ripe for investigation by scholars within the philosophy of economics is financial economics. The recent financial crisis has initiated a wide-ranging critique of the epistemological foundations of the efficient market hypothesis, the capital asset pricing model, and many other key aspects of modern financial econom-

ics. A substantial amount of research has appeared during the last few years – including Roman Frydman and Michael Goldberg's imperfect knowledge economics, the material culture sociology of Donald Mackenzie and his colleagues, research on the philosophical foundations of econophysics, and even critical literature aimed at a broader audience like the work of Nassim Taleb and George Soros's writings on reflexivity – but there are many more areas to explore. The financial system of the entire world now rests on a vast network of derivatives and other recently constructed financial assets; an investigation of the knowledge on which these things rest seems to be long overdue.

5) Finally, and related to what I said about financial economics, there is macroeconomics. The recent crisis has also produced a variety of criticisms of the reigning dynamic stochastic general equilibrium (DSGE) theorizing that dominated macroeconomics during the decade or so before the crisis. It seems to me that while DSGE certainly deserves much of the criticism it has received, I would also note that we have a way to go before the majority of the methodological literature on macroeconomics will come up to the same high standards of the recent methodological literature in fields like experimental economics or behavioral economics. So far it seems that most of the commentary amounts to little more than a rehash of older methodological debates between Keynesian macroeconomics and monetarism, or between Keynes and modern versions of classical theory. Although certain aspects of these earlier controversies may be important, it would also be desirable to have some new approaches to questions about the philosophical foundations of macroeconomics. If I were recommending a field of research that is particularly ripe for original new ideas by young scholars in philosophy of economics I would say macro. The field, like the macroeconomy, is ready for a bounce.

5. Naturalism is now firmly established in current philosophy of economics. Don't you think that it jeopardizes its critical approach? What would it mean to be critical of theoretical economic practice for a discipline that is consciously engaged in recovering the rationality of such a practice?

The question of how a philosophical inquiry into a particular science can be naturalistic and at the same time provide normative advice – possibly critical advice – about how the science ought to be done is an old one, and I am afraid that I do not have any magic key to unlock the solution to this problem. The issue is of course just a version of the more general question of the relationship between the positive and normative: "is" does not imply an epistemic "ought" any more than it implies an ethical "ought."

This said, and recognizing there is no magic key, some useful insights can be gained from the relationship between how we think about ethically normative theories/views about human behavior and how humans in fact are/behave. "Is" does not imply an ethical "ought," and certainly an ethical "ought" does not tell us what "is" the case, but an ethical "ought" does imply "can"; that which one ought to do is something which one can in fact do. No ethical system – from either ethical philosophy or religion – would make it immoral for someone to not flap their arms and fly around the room. People cannot – as a factual matter – flap their arms and fly around the room and therefore it cannot be immoral for them not to do so; ought implies can. So too for epistemic oughts. What one ought to do in order to do good science, needs to be something that is possible (as a factual matter) for scientists or the scientific community to do. But many of the constraints on what scientists can, and cannot, in fact do are not universal; they are discipline, field, or sub-field, specific – and thus situational and context-dependent. This means that the philosopher of science or economic methodologist needs to understand the field or subfield in detail to understand the relevant constraints, and this is the case for those who come to criticize disciplinary practice as much as for those who come to praise it. This is one way to read Herbert Simon's criticism of the methodology of rational choice theory. The theory says that economic agents act rationally by maximizing well-ordered preferences, and the associated normative methodology says that economists ought to explain the behavior of economic agents in this, and only this, way. Simon's point was that real economic agents cannot do what the theory requires them to do; they do not have the information or computational power to maximize in the way the theory demands. It is a critique of rational choice theory and the associated normative methodology based on the fact of the matter about human decision making. This is just one example, but the point is that it is possible for our best available factual knowledge to play a direct role in our normative methodological recommendations (even critical recommendations).

6. Related to the current economic practice, could you mention a couple of papers published in top economic journals

that are questionable on epistemological or philosophical grounds? And what would that criticism be?

Rather than picking out a couple of "questionable" papers and trying to criticize them, I will discuss some of my own recent work where I have been critical of a particular body of economic literature and explain the approach I have taken. A good example of the relevant literature is the 2008 paper by Faruk Gul and Wolfgang Pesendorfer defending and extending contemporary revealed preference theory. In recent and forthcoming papers I have criticized this work on the grounds that it cannot do all of the things that defenders claim it can do. My general approach has been critical, but critical in a way that exemplifies the way that I believe a methodological paper can be critical in an effective way. My research does not criticize the various specific empirical applications of contemporary revealed preference theory. It is a body of empirical research that may end up being either a great success or a dismal failure, but which it turns out to be will ultimately be decided by those working within the field of applied choice theory: those who know the data, know the constraints faced by the various empirical techniques available, and understand the practical issues associated with the application of such techniques. One needs inside knowledge to pass such local judgments and they need to come from those with hands-on experience within the field. What I can question, even without such local knowledge, is that many advocates of contemporary revealed preference theory – Gul and Pesendorfer in particular – make a number of bold methodological claims about the applicability of this approach to a wide range of other areas within economics; they assert that it is what most economists should and must do all the time. I find many of these claims to be internally inconsistent and/or resting on weak philosophical foundations and I try to provide an effective methodological critique on these grounds. Readers will need to decide for themselves whether I am successful or not, but the point here is that I think this research offers a nice example of how an economic methodologist – even one without practical experience in the specific area of economics – can effectively make critical methodological arguments. There may of course be other ways, and for those with local experience/knowledge there are even more methodological opportunities.

7. Or if you prefer to put it in another way, supposing that the distinction between orthodox and heterodox economics is clear enough, which piece of orthodox economics deserves

philosophical criticism? And what would that criticism be?

The problem is that I do not think that these days "the distinction between orthodox and heterodox economics is clear enough," and given this, it is not entirely clear what one means by "orthodox economics." Is behavioral economics orthodox? Or neuroeconomics? Or going backward in time, is the once-orthodox IS-LM Keynesian macroeconomic model still orthodox? Historical or philosophical analysis of economics based on these two categories seems to be frozen in Cold War amber and I do not think these two labels cut up contemporary economic thinking in any particularly useful way. As I argued in Reflection Without Rules years ago, I do not think there is a single 3"x5" card philosophy of science (to borrow a phrase from Deirdre McCloskey) that one can apply to various economic research programs to decide if they are good or bad science. One is certainly free to engage in such exercises, but practicing economists will not pay much attention, and I would suggest that these days most of the people writing in economic methodology will not pay much attention either; one will be dancing alone. But what one can do as a methodological commentator is either i) to immerse oneself in the context and constraints of a particular field of economic inquiry and advise from within, or ii) do what I discussed in the previous section – focus one's attention on questions about the consistency of the methodological recommendations (implicit or explicit) of those within the field.

8. Regarding the recent turn from a theory-centered to a model-centered epistemology, do you think it's a really promising avenue for the field or just a fad?

As I noted in my response to question #4, I think the recent work on economic models has greatly improved our understanding of what economic models are, how they work, and how they differ from models in other sciences, and that it is an important area for future research. That said, I am not suggesting that philosophers of economics give up thinking about economic theory (or empirical practice, or the institutions of the profession, or any other traditional topic). We know a lot more about models in economics, but we need to know still more. Given their importance, economic models were under-investigated for a long time and it is good to see the increased attention they received during recent years. I most certainly think that it is not just a fad and I encourage young scholars to consider working in this area; that said, there is more to economics than models.

9. What is the relationship between economic models and reality? What can they tell us about real systems and how they do?

Well I guess it is reasonable to save the really big question for last, but those wanting a grand finale answer will probably be disappointed. I do not think that one can put the relationship between economic models and reality on a 3"x5" card any more than one can put the rules of economic methodology on such a card. One of the things we have learned from the recent work on models by Morgan, Boumans, and others is that there is not just one way that models work – give us knowledge or something else we find useful – in economics; economic models tell us important things about real economic systems and issues – from environmental regulations to monetary policy – but they do not do so in one single way for all fields and sub-fields, or for all times and contexts. But of course that is a good thing for those of us in working in the field of the philosophy of economics since it means there remains a significant amount of interesting and important work for us to do.