Economic Methodology in the Twenty-First Century (So Far): Some Post-*Reflection* Reflections

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Abstract

This paper provides my personal reflections on the development of economic methodology during the 21st century as well as a discussion of the methodological literature immediately preceding it. It is based on my experience – both as an editor and researcher within the field – and to some extent it reflects my own interests and concerns. It provides an interpretative history of the field and the various forces at work within it – doing so with a fairly broad brush, but at times focusing in and being much more specific.

I. Introduction

This essay contains my reflections on the developments that have taken place within the field of economic methodology since the beginning of the twenty-first century as well as some of the background relevant to these developments. It is motivated in part by the fact that after fifteen years of editing the *Journal of Economic Methodology* (JEM), John Davis and I stepped down at the end of 2019. There have been many changes in the field during our years as editors and this seems to be a very good time to reflect on those developments. The subtitle "Post-*Reflection* Reflections" alludes to the fact that my *Reflection Without Rules* was written during the last years of the twentieth century and was published in 2001. Not only was *Reflection* my attempt to "write an interpretive survey of recent work in economic methodology and the various developments within contemporary science theory that are relevant to it" (ix), it was, unlike much methodological writing, less an attempt to persuade the reader about how scientific economics ought to be done, than an historical investigation of the various forces that had helped move economic methodology to where it was at the time. Since I argued that the field was in the midst of a significant, but generally unrecognized, change, this also seems like a good time to assess my argument in light of recent developments.

This paper can be read as a companion piece to Daniel Hausman's recent retrospective reflection on the philosophy of economics published in this journal (Hausman 2018a). Hausman of course had his own particular interests and concerns – as do I – but the two papers are similar in spirit and I think quite complementary. There are topics that I will not spend much time on because Hausman covered them so well, which gives me the opportunity to spend more time reflecting on my own experiences. I will paint with a rather broad brush, emphasizing not only popular ideas and topics, but also relevant changes in cognate fields such as economics, philosophy of science, and the history of economic thought. There will also be some discussion of the changing institutional structure of the field and shifts in disciplinary identification and attention. My focus will be historical, but it is light-touch reflective history, concerned with the general trends and tendencies rather than thick historical detail. In section V I will note some of the topics that have received quite a bit of attention in recent years, but it will not be a comprehensive survey. I will focus on only a few of those topics, and my choices are motivated in part by my own interests and research. I would like to note in advance that this type of brief interpretive examination of such a large body of literature will undoubtedly leave out the work of many different authors and many important ideas.

II. What's in a Name?

It seems like a useful place to start is with the name: economic methodology. Why have I used the term economic methodology, rather than economic method, or philosophy of economics, or economic philosophy, or some of the many other terms used to identify closely related areas of inquiry? How are these various areas of research defined? How are they, or are they, different?

One might try to answer these questions historically, with the definition of economic methodology in classics like Mill (1874), Robbins (1932), and Friedman (1953). This seems promising since these authors all wanted to defend the scientific credentials of the mainstream economics of the day and they all held basically the same view about why economic methodology was needed. All three argued that economics was a science, but unlike the natural sciences, one could not do controlled experiments in economics, thus there was the need for a specific *economic methodology*: an explanation of how exactly it is that economics can produce legitimate scientific knowledge even though experiments were not possible (Mill, 1874, p. 47; Robbins, 1935, pp. 74-75; Friedman, 1953, p. 10). The main difficulty with using this motivation today is the significant empirical and experimental turn that has taken place within economics during the last few decades (e.g., Backhouse and Cherrier, 2017; Biddle and Hamermesh, 2017). Given this recent turn, this traditional definition suggests that economic methodology is no longer needed.

Perhaps a better way of answering these questions is to start by focusing on distinctions where there is a fairly clear consensus. One such case is the difference between *methodology* and *method*. This distinction is clearly spelled out in the "aims and scope" section of the JEM website: "The Journal distinguishes between methodology (which concerns the relationship between economics and broad questions about scientific knowledge) and methods (which involve particular techniques relevant to practitioners in a specific field of economics) and reserves the pages of the Journal for authors and readers with broader epistemic interests." This seems pretty clear: methodology is about broad philosophical questions about the relationship between economics and scientific knowledge, while method concerns more specific techniques used by, and primarily of interest to, practitioners in various fields of economics.[1]

Similarly, it is fairly easy to distinguish research in *economic philosophy* from *economic methodology*. Economic philosophy has recently been defined as "the study of fundamental values and principles of economic theories, the study of the structures, the meanings, the impact and the limits of rationality in action, ontology, methodology, and epistemology" (Campagnolo, 2019). Given this definition, economic methodology is a (proper) subset of economic philosophy; economic methodology is concerned with the relationship between economics and the philosophical understanding of scientific

[1] This is complicated a bit by the fact that the organization that sponsors JEM is the International Network for Economic Method (INEM).

knowledge, primarily philosophy of science, while economic philosophy is a much broader field of involving the connection between economics and many different branches of philosophical inquiry.

The most difficult distinction is between *economic methodology* and *philosophy of economics*. Historically, there was a time, during the heyday of the logical empiricist Received View (Suppe 1977), when the philosophy of economics, like the philosophy of physics, was considered to be the fairly narrow application of logical empiricist philosophy of science to a specific field of scientific inquiry. Given this definition, the field of economic methodology actually had broader concerns than the philosophy of economics. But with the breakdown of the Received View, this definition of the philosophy of economics no longer seems to be descriptively useful. There is also a more empirical way of characterizing the difference between economic methodology and philosophy of economics and that is by the profession of the relevant author(s); research written by economists is statistically more likely to be called economic methodology while work written by philosophers is more likely to be called philosophy of economics. But even this statistical relationship has been changing.

So given all this, what is this paper about? First, let me say that the topic is neither the very narrow "method" nor the very broad "economic philosophy." But second, I do not think the question of whether the paper is about economic methodology or the philosophy of economics needs to be answered in general. Different authors define these terms in somewhat different ways, but both involve philosophical ideas about the nature and character of scientific knowledge in the field of economics, e.g, bringing together philosophy of science – now more broadly defined than during the middle of the twentieth century – and economics. Given this, and my own background, I will use the term economic methodology, but in most cases the term philosophy of economics would work just as well. The one exception will be in section V where some topics will be discussed – idealization and explanation in economics for example – where I will switch to the term philosophy of economics because much of the scholarly interest in these topics has come from trying to draw *general* philosophical lessons about scientific modeling from the use of models in economics, rather than from traditional methodological questions about the specific characteristics of economic science.

III. A Little Historical Stage-Setting

My first publication in economic methodology (Hands 1979) was a review article on Spiro Latsis' *Method and Appraisal In Economics* (1976). This was during the important period (1975-1985) that Hausman (2018a, p. 187) calls the "beginnings" of the modern literature on the philosophy of economics. This was the period where the influence of logical empiricist-based philosophy of science began to fade and be replaced by a more disparate set of ideas associated with Karl Popper (1959, 1965), Imre Lakatos (1970), Thomas Kuhn (1970), and other post-positivist philosophical (and in some cases historical and sociological) ideas. Since the literature of this period has been examined

in detail in the existing literature – in surveys of economic methodology such as Blaug (1980, 1992), Boumans and Davis (2016), Caldwell (1994), and Hands (2001) – I will not recount the main story line here. What I will do is to point out two characteristics of this literature that I think are particularly significant because they are so different from most of the research during the last few decades.

The first characteristic is what I called the "shelf of scientific philosophy" in Hands (1994). This was the idea that those writing on economic methodology would simply take particular accounts of scientific knowledge directly off of the shelf of scientific philosophy and apply it directly to economics, with little or no reflection or reconfiguration. Of course the views of those writing about economic methodology have always been influenced by the philosophical ideas of the time, but the way that philosophical ideas were involved in some of this particular research was more than this traditional influence. Rather than simply making arguments about economics that were influenced by philosophy, much of this work was more narrow and rules-based in the way it was applied. If Popper said scientists needed to make bold conjectures and attempt to falsify those conjectures with severe empirical tests, then economics, in order to be legitimate science, had to do exactly that; or if Lakatos said that in order to be scientific, research programs needed to possess a distinct hard core, an evolving protective belt of positive and negative heuristics, and consistently generate novel empirical facts, then economic science needed to do precisely those things as well. This was the period Deirdre McCloskey (1994) called the 3" x 5" card philosophy of science approach to economic methodology. Certainly not all of the methodological literature of the period was this way – in particular, the tendency was actually more pronounced among economists than philosophers – but it was clearly a prominent feature of the literature (a literature that I must admit, I participated in myself).

The second, but related, feature of this literature concerns the target and the goal of the methodological analysis. The methodological target was often very general theoretical frameworks such as neoclassical economics, Keynesian economics, or Marxian economics; and the goal of these assessments was generally demarcation: arguing that such theorizing either was, or was not, scientific. The outcome of these studies was binary: either the general theoretical framework in question (neoclassical, Keynesian, Ricardian, etc.) followed the methodological rules laid down by the relevant philosophy of science, or it didn't, and thus, either it was real science, or it was not real science. As a result this research was often more concerned with labeling – science or non-science – than with providing advice about how a particular body of economic theory or practice could be improved. Although this tendency was most pronounced in the literature applying Popperian falsificationism to economics, it was also prevalent in research based on other philosophical approaches. This said, I do not want to overstate the case here – there were authors writing on methodological topics in many other ways during this period. This said, the demarcation-of-general-theories literature was substantial and it stands in stark contrast to later methodological research. Although this literature is quite different from most twenty-first century methodological research, it did, as

Hausman explains (2018a, pp. 187-190), help initiate a general revival of the field of economic methodology and influenced, in numerous ways, that which came later.

IV. The Changing Face of Economic Methodology

In this section I want to discuss some of the broad changes that have taken place in the transition between the self-of-scientific philosophy literature of the 1980s and 1990s, and the literature of the last two decades. This will be followed by section V which focuses more narrowly by providing a few specific examples of popular topics within the recent methodological literature. Some of the changes discussed in these two sections initiate from within philosophy, some from within economics, and others from elsewhere on the intellectual landscape. I will again be painting with a fairly broad brush and highlighting only a few key influences.

•Changes in the Philosophy of Science: One of the important forces in end-of-the-century philosophy of science was a turn toward naturalism and a more practice- and historically-sensitive approach to scientific philosophy than had been associated with either logical empiricism or Popperian falsificationism. The work of Kuhn and many others had a fairly profound impact on philosophy of science, moving it sharply away from armchair philosophizing about what science ought (epistemically or logically) to be/do, to a more naturalistically-inclined philosophical inquiry which pays close attention to what actually happens in successful science and uses it to help guide philosophizing about the relevant normative standards. Simultaneously with this impulse toward disciplining philosophy of science with the historical facts and current practice of science, there was also a turn away from an emphasis on universal scientific theories. Increasingly philosophers of science turned toward smaller units of scientific theory and practice: one particular subfield, one laboratory, one interpretation of key data, one key experiment, and away from 3" x 5" philosophy of science. Judgments about scientific adequacy, epistemic warrant, and cognitive significance were typically still the goal, but conditioned much more closely by the actual practice of science.

This naturalism and local-focus spilled over into economic methodology. There was less shelf-of-scientific-philosophy-based methodology discussing grand economic theories and more research on particular modelling strategies or specific applications; the messy complexity of empirical research was examined in detail rather than talking about "testing" economic theories in some fairly abstract way; interdisciplinary influences from psychology and other fields were examined rather than just one economic theory being assessed by one particular characterization of the rules of good science; and on and on. These changes came hand in hand with a turn away from the demarcationist emphasis of the previous period; increasingly the focus was less on certifying that economic theory was, or was not, scientific and more on how a particular small piece of economic science could be improved, and improved with recognition of the various forces constraining those engaged in the relevant research.

I think one good example of this contemporary approach to methodological research is the discussion of randomized controlled trials in economics, particularly in development economics. This literature is extensive and expanding, but probably the most influential text promoting this approach is Banerjee and Duflo (2011). The idea is simple, and it is the same basic idea as randomized trials in other fields such as medicine: randomly divide the population into two groups, one that receives a particular treatment and one that does not, and then compare the outcomes of the two groups. The presumption is that by randomization, the two groups will be identical with respect to factors that might influence the outcome other than the treatment. These experiments are generally double-blind and the most discussed application in development economics is the provision of mosquito bed nets. The claim is often made that such randomized controlled trials are the "gold standard" for empirical research in economics. There is an extensive philosophical literature critical of randomized controlled trials, some criticizing randomized experiments even in medicine (Cartwright 2009, González-Moreno, Saborido, and Teira 2015, Worrall 2007a, 2007b), but the literature has expanded greatly with the extension of these techniques to economic policy (Deaton and Cartwright 2018, Cartwright and Marcelles 2014, Favereau 2016, Harrison 2013, Reiss 2013, 2015, and many others). I will not try to list all of the methodological concerns that have been raised, but I will note a few in passing: i) randomization does not guarantee that the control group and the treatment group are identical prior to treatment, ii) in the social sciences, randomization can itself introduce new variables, iii) blinding is very hard to control in social science (e.g., it is easy to see who gets the bed nets), and iv) the emphasis is (self-consciously) away from the relevant causal forces thus creating problems about external validity (Reiss, 2013, pp. 202-206).

My point is not to defend these methodological critics – although I will admit to agreeing with much of what they say – but rather to point out how much this methodological literature differs from the shelf-of-philosophy approach from the previous period. First, the target is a relatively localized body of economic practice, not grand theorizing like neoclassical or Marxian economics. Second, the concern is not demarcation. The critics are not arguing that randomized controlled trials are not scientific. Rather, they are trying to understand exactly how such experiments work, when they might and might not be effective, how they can be improved, and how other empirical approaches may serve as either substitutes for, or complements to, randomization. And finally, the criticisms are grounded in contemporary philosophy of science which is more pragmatic and more sensitive to the broad range of constraints facing the scientific practitioners in question (i.e. one needs to be careful talking about one scientific approach being the gold standard).

•Changes in Disciplinary Affiliation and Institutional Structure: Concerning disciplinary affiliation, it is useful to note that although many philosophers, including Daniel Hausman, Alexander Rosenberg, Uskali Mäki and others, made extremely important contributions to the methodological literature during the 1980s and 1990s, the

field at that time was predominantly one populated by economists. There were many philosophers writing on decision-theory, but those writing on economic methodology, even when it involved the direct application of ideas from the philosophy of science, were generally economists. In recent years the number of scholars writing in the field that were trained as philosophers and working in philosophy departments and/or institutes, has increased at a fairly steady rate.

One reason for this change concerns some changes in the philosophy of science that were not mentioned in the above discussion. One is philosopher's increased attention to scientific models, and the corresponding decreased attention to scientific theories. This model-centered conception has itself been related to the movement away from the logical empiricist-inspired syntactic (or statement, or law-based) view of scientific theories toward various versions of semantic (non-statement, model-based) views. There are many reason for this change, including the naturalist turn noted above, but certainly the breakdown of the strict theory versus observation distinction played an important role. For the logical empiricist tradition the theoretical aspects of a theory were one thing and the observational aspects were something quite different, and for successful science the two needed to be connected by clearly specified correspondence rules connecting theory with observation. As it became increasing clear that these tight categories could not be maintained – either philosophically or in the practice of successful science – attention turned to re-thinking the theory-observation connection and the complexity of the possible relationships between theory and observation. As Patrick Suppes noted during the 1970s:

> Consider the classical philosophical theses that an absolute causal account can be given of phenomena, that ultimate laws … can be gleamed from natural phenomena, and that some rockbed of perceptual certainty is necessary to gain a firm knowledge of the world. All three of these theses are false and hopelessly out of date in terms of the kinds of theories now coming to dominate science. In ordinary talk and ordinary affairs, such certainty and absolutism are not necessary and are in fact deleterious to the exercise of good sense." (Suppes, 1974, p. 283)

Although there was fairly general agreement about the problematic nature of the traditional theory-observation relationship, different philosophers and different schools of philosophical thought took different approaches on how exactly to alleviate these problems. Suppes provided an influential approach that focused on the key role of models. Models were representations that mediated between high-level theory and lower-level observation, but in a much more nuanced and complex way than the rigid correspondence rules of logical empiricism. Theoretical models on one side, and empirical models (or models of data) on the other side, created a "a hierarchy of theories, models, and problems that arise at each level to harass the scientist" (Suppes, 1962, p. 258). This emphasis on models opened up the theory-observation relationship and accommodated new conceptions of scientific representation:

> Now, models (and not correspondence rules) provide the interpretation for the axioms; hence the passage from the theory to reality is not direct but is mediated by models. The philosopher of science does not need anymore to look for a correspondence between what the theory says and how the world is in order to evaluate the adequacy of a scientific theory. Now, since any theory requires an interpretation in order not to remain just a formal system, this interpretation is a much more complex process but, at the same time, it offers the nuances able to depict a more sophisticated way in which a theory may *represent* reality. (Ferrario and Schiaffonati, 2012, p. 72)

This emphasis on models also characterized many other semantically-oriented approaches: pragmatic versions (e.g. Giere 1999), various structuralisms (e.g. French 2014, Sneed 1971, Stegmüller 1979), more empiricist (e.g. van Fraassen 1980) and more realist views (e.g. Cartwright 1983, 1989), more naturalist or practice-focused accounts (e.g. Morgan and Morrison 1999, Morrison 2015), and some specifically concerned with economics (Hausman 1992). Of course economics is, and in many ways has always been a *modelling science* (Morgan 2012), and this has increasingly motivated philosophers to look toward economics as a place to help them better understand how scientific modeling works in general (Morgan and Knuuttila 2012). Add to this the fact that i) philosophers of science have turned more in the direction of biology and interdisciplinary fields like climate change in recent years, and ii) many highly idealized economic models look much like the idealized models in biology and related fields, and you have at least a partial explanation of why philosophers of science exhibit far more interest in economic science than they did only a few decades ago. Finally, one additional incentive for the increased philosophical interest in economics, is the movement within economics away from the traditional emphasis on (particularly competitive) markets, prices, and outcomes, and more toward decision theory and questions of agency, rationality, and normative evaluation. The rise of behavioral economics has been highly correlated with philosopher's increased interest in economics.

This increased interest in economics on the part of philosophers of science, has generated a demand for research in the philosophy of economics. Twenty years ago papers on economics were very rare in philosophy of science journals like the *British Journal for the Philosophy of Science, Philosophy of Science,* or *Synthese* – and the ones that did appear almost never introduced economic theorizing as a case study to help with general questions within the philosophy of natural science – now it is rather commonplace. This is true for both regular submission (individual papers submitted to the journal by the author or authors) and symposia on various special topics. Given that there has generally not been an increased discussion of philosophical or methodological topics within economics journals – particularly North American economics journals – this increased attention from philosophy of science journals pulls research papers,

symposia and conferences, and even students, into philosophy of science as the pathway for publication, communication, and perhaps even careers working on the economics-philosophy nexus rather than economics.

•Changes in Economics: Mainstream economics has changed in recent decades as well. The Walrasian and econometric normal science that characterized the second half of the twentieth century gave way to a much more diverse set of scientific practices in recent years: game theory, behavioral economics, behavioral welfare economics, new empirical techniques and evidence-based economics of various forms, agent-based and complexity economics, and others. As David Colander, Richard Holt, and Barkley Rosser put it: "economics is currently at a turning point; it is moving away from a strict adherence to the holy trinity – rationality, greed, and equilibrium – to a more eclectic trinity of purposeful behavior, enlightened self-interest, and sustainability." (Colander, Holt, and Rosser, 2004, p. 1). Many of these new developments have become solidly mainstream – game theory and behavioral economics in particular – but at the same time they are, in many ways, also critical of the earlier literature. Although traditional heterodox research programs still exist, they are no longer attracting as many followers as was once the case. On the other hand, a number of alternative approaches to specific areas of economics – ecological economics and econophysics to name just two – have developed that are critical of mainstream theory, although with a more narrow focus than traditional heterodox theory.

As noted above, economics has clearly taken an empirical turn in recent decades. Some of this research is self-consciously a-theoretical, but most of it is not opposed to theory in general, just the extremely idealized theorizing associated with things like Arrow-Debreu general equilibrium theory and DSGE macroeconomics. As a result, the economics that methodologists typically focus on is more empirical as well. Experimental economics, behavioral economics, and various versions of evidenced-based economics are some of the most discussed topics within the recent methodological literature. Again scholars writing about the methodological issues associated with these fields are much more likely to be examining smaller divisions within economics and doing so with a more practical approach than was the case in the previous generation.

•Changes in the Relationship Between History of Economic Thought and Economic Methodology: The fact is that many of those writing in the field of economic methodology during the 1980s and 1990s – Mark Blaug, Terence Hutchison, being two influential examples – were not only economists, but also influential historians of economic thought. Mark Blaug for example, authored definitive textbooks in both history of economic thought and methodology: Blaug (1978) and (1980). During that time much of the important methodological research was published in history of economic thought journals such as *History of Political Economy*. Some of this was undoubtedly due to the fact that there were so few outlets for methodological research. *Economics and Philosophy* didn't appear until 1985 and JEM not until 1994 (although its

predecessor *Methodus* appeared in 1989) – and as noted earlier, papers on the philosophy of economics were much less welcome in philosophy of science journals than the are today – but it wasn't entirely practical expediency. There were also a number of intellectual affinities between the history and methodology of economics. For instance, this was the heyday of growth of knowledge theories which straddled history and philosophy of science. Kuhn was a historian of science who had a profound impact on philosophy of science and received a lot of attention from economists, while at the same time Lakatos was both popular among economists and famous for his meta-methodology for judging alternative methodologies in terms of how many of the "best gambits" (Lakatos, 1971, p. 111) of science each methodology could rationalize. There are undoubtedly many other forces at work in the relationship between the history of economic thought and economic methodology, but for these and other reasons the two fields seemed to be quite closely aligned at the end of the twentieth century.

It seems clear to me that the previously close relationship between the history and philosophy of economics has weakened during the last two decades. Purely methodological papers seldom appear in dedicated history of economic thought journals, and (speaking as a JEM editor) historical papers now need to demonstrate a more clear and explicit methodological relevance in to be published, or even refereed, in methodology journals. Assuming the separation is in fact as pronounced as it seems to me, then I suspect that much of it has to do the proliferation of journals in both fields. *History of Political Economy* was the first dedicated history of economic thought journal when it appeared in 1969, but now there are a large number of English language, peer-reviewed, international, journals in the history of economic thought and many more regional journals. This is a bit less the case in economic methodology and philosophy of economics, but there has still been substantial expansion such journals as well as space for research on economics opening up in philosophy of science journals. One does not need Adam Smith to recognize that an increase in the extent of the market can lead to increased specialization and division of labor. There has also been an expansion in the number of economists' archives available to scholars, as well as increased digital access to many archives and other sources of historical information, and these also contribute to specialization among historians of economic thought. Some of the tendencies toward specialization among philosophers of economics have already been discussed.

V. Some Topics of Current Methodological Interest

In this section I want to focus specifically on a few of the methodological topics that have received quite a bit of scholarly attention during the last two decades. Since there are many more such topics than can be discussed, my presentation will be selective and driven in part by my own interests.

Surprisingly I will start by noting a change that would seem to be very important, but is difficult to discuss in any serious way: the massive increase in the amount and variety of empirical data now available and the technology used to analyze it. We are awash in

data – various kinds of experimental data, individual choice data, market data, field data, survey data, calibration data, bibliometric data, biometric data, data from the neurosciences, data from agent-based models, and many other sources – and it seems undeniable that these changes have had, and will continue to have, a significant impact on the way economics is done. As Raj Chetty noted in his 2015 Richard T. Ely lecture:

> … the empirical applications discussed in this article are all examples of recent studies in applied microeconomics that use administrative datasets with millions of observations. This 'big data' approach often leads researchers to identify empirical regularities that are unrelated to their initial hypotheses and sometimes do not match neoclassical predictions, making it useful to draw on insights from behavioral economics. As economics becomes an increasingly empirical science, economic theories will be shaped more directly by evidence, and the pragmatic approach to behavioral economics described here may become even more prevalent and useful. (Chetty, 2015, p. 5)

Such changes certainly raise many interesting and important methodological concerns. One issue, on first gloss a seemingly innocent issue, concerns the changes that analysis using finite data might have on microeconomic theory long steeped in differential calculus and continuous functions. After all, the neoclassical revolution of the 1870s was a calculus-based marginal utility revolution. While philosophers of economics do not seem to have been drawn into the discussion of these mathematical issues[2], there is an on-going debate about such topics within the philosophy science: the literature on ineliminable mathematical idealizations and indirect representation (e.g., Batterman 2010; Knuuttila and Loettgers 2017; Rice 2019) and the literature on distinctive mathematical explanations (e.g., Lange 2013, 2018; Craver and Povich 2017). Although economics is often alluded to but never really discussed in this literature, a few philosophers have made passing comments. Suppes for instance, saw the big data revolution as something that would eventually move economics out of its calculus-real variables shackles:

> I … agree that in any serious sense the data are finite, but the use of the calculus is really only accidental, as we see now so commonly in physics, where continuous theoretical quantities are mainly computed as numerically discrete, to meet the requirements of the computer programs during the calculations … The ever refined approximations of discrete data and computations now dominate physics, and so it will soon be in economics, as economists learn how to use all of the massive data that are available to them. Surely, economics is bound to become one of the 'big data' sciences in the future." (Suppes, 2014, p. 257)

[2] On exception is Hardt (2017).

Even though I have noted some potential methodological issues raised by big data and the associated technologies, it is not possible to discuss different accounts because at this point there really isn't a literature about these issues within economic methodology. That said, it certainly seems like it will become increasingly important in the methodological literature going forward; it is a major change in the way we do economics, and for that matter all science.

So with this brief excursion into what I will not discuss here, let me now turn to the literatures that will be considered in this section. I will discuss three topics, but two of them involve behavioral economics, so I will begin with a brief discussion of behavioral economics, and then move on to the two specific topics: the so-called normative turn in rational choice theory and it relationship to behavioral economics, and behavioral welfare economics (particularly libertarian paternalism). The third and final topic is more traditional: the nature and character of highly idealized models in economics, particularly the relationship between idealization and explanation.

Behavioral economics, particularly the "heuristics and biases" (HB) program evolving out of the work of Daniel Kahneman and Amos Tversky,[3] has been a major, and probably *the* major, topic in methodological literature during the last few decades. It started with attempts to answer fairly straightforward methodological questions around whether behavioral economics or rational choice theory was a better scientific theory of individual behavior, but it quickly expanded into other areas of methodological inquiry.

The main result of behavioral economics research has been that real human decision-makers often behave in ways that are inconsistent with rational choice theory; they fail to act rationally and their decision-making exhibits systematic *mistakes*. Associated with these mistakes, behavioral economists have identified a vast number of empirical *anomalies*: including loss-aversion, anchoring effects, constructed preferences, social preferences, hyperbolic discounting, and many others. These behavioral anomalies present a serious challenge to rational choice theory. As Daniel McFadden put it many years ago:

> This research paradigm has been the focus of Amos Tversky and Danny Kahneman on experimental study of *cognitive anomalies*: circumstances in which individuals exhibit surprising departures from rationality. This work has both fascinated and dismayed economists: it has been like watching master carpenters construct the scaffold for your hanging. (McFadden, 1999, p. 79)

[3] This literature is sufficiently well-established that references may not be needed, but perhaps noting a few of the most influential writings is useful: Camerer and Loewenstein (2004), Kahneman (2003), Kahneman and Tversky (1979, 2000), and Thaler (1980, 2000).

These experimental results obviously raise many methodological questions: about testing, about explanation versus prediction, about external validity, about aggregation in markets, about unification, about the causal mechanisms behind such cognitive anomalies, and on and on. A prodigious amount of methodological literature has been written on these topics and the consensus within this literature has clearly been that behavioral anomalies and the related research should be taken very seriously. That said, there has been surprisingly little consensus that goes beyond recognition of the importance of the topic.

With this brief introduction to behavioral economics I will now turn to two recent methodological debates involving the behavioral literature. The first one is the so-called *normative turn* in rational choice theory.

Va. The Normative Turn in Rational Choice Theory

Although this may be changing, most economists consider rational choice theory to be a positive scientific theory of individual behavior. Of course some economists consider it to be a good theory, while others consider it to be a poor theory, but it in either case it is viewed as a scientific theory designed to describe, predict, and/or explain the behavior of individual human beings. On the other hand, most economists consider, although again this may be changing, *normative* questions/concerns to be exclusively concerned with ethics. As Milton Friedman put it in a 1955 paper on utility theory: "Science is science and ethics is ethics; it takes both to make a whole man; but only confusion, discord and misunderstanding can come from not keeping them separate and distinct, from trying to impose the absolutes of ethics on the relatives of science" (p. 405). By the second half of the twentieth century this characterization of normative economics – both parts: the "normative is, and should be, strictly separated from the scientific" and the "normative = ethical" – were widely accepted within mainstream economics and had become canonized in the introductory chapter of nearly every economics textbook.

But of course outside of economics, "normative" does not necessarily mean "ethical." Norms involve rules and action-guiding principles; they are prescriptive, but not all prescriptions prescribe that which is ethical. When someone is told they "ought to get more exercise," their actual behavior is being compared to a norm or ideal, but it is a norm about health, not an ethical norm. More relevant to economic methodology, when Mark Blaug criticized economists because they do not subject their economic theories to severe empirical tests, he was accusing them of violating a norm, but it was methodological norm, not an ethical norm.

One way to interpret rational choice theory is as a *normative theory of rationality*: a theory that describes what one *ought to do in order to be rational*. The relevant notion of rationality is quite specific – rational goals plus acting in an instrumentally rational way given those goals – but the theory tells us what ought to be done in order to behave consistently with this notion of rationality. While economists have not traditionally

thought about rational choice theory in this way, it is how almost all behavioral economists characterize rational choice. Rational choice theory fails to predict or explain individual behavior because people are subject to various heuristics and biases which lead to mistakes in rational decsion-making, but rational choice can still serve as a normative standard for proper rational action. Although this interpretation of rational choice theory is dominant among behavioral economist (and seems to be spreading to economists more generally), it did not originate within the behavioral economic literature; it is a normative conception that is present in the work of the behavioral decision theorists who influenced Kahneman and Tversky (Heukelom 2014)[4].

> The modern theory of decision making under risk emerged from a logical analysis of games of chance rather than from a psychological analysis of risk and value. The theory was conceived as a normative model of an idealized decision maker, not as descriptive of the behavior of real people … the logic of choice does not provide an adequate foundation for a descriptive theory of decision making. We argue that the deviations of actual behavior from the normative model are too widespread to be ignored, too systematic to be dismissed as random error, and too fundamental to be accommodated by relaxing the normative system. (Kahneman & Tversky, 1986, pp. 251-52)

But this normative interpretation of rationality is not a view unique to behavioral economists and a particular group of psychologists; it is also the view of many philosophers and decision theorists. As Robert Nozick noted even before the behavioral turn in economics:

> An elaborate theory of rational decision has been developed by economists and statisticians, and put to widespread use in theoretical and policy studies. This is a powerful, mathematically precise, and tractable theory. Although its adequacy as a description of actual behavior has been widely questioned, it stands as the dominant view of the conditions that a rational decision should satisfy: it is the dominant normative theory. (Nozick, 1993, p. 41)[5]

So how does the normative interpretation of rational choice theory connect up to the discussion of behavioral economics in the recent methodological literature? It certainly seems that empirical evidence from behavioral economics would (perhaps should) be a serious challenge to *homo economicus* – and in some sense, with respect to positive economics, it actually has: economists have moved to expand rational choice in ways

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[4] There is also historical work on the social and political context of this conception of rationality (Heyck 2015) and also on the early seeds of this normative turn (Herfeld 2018).

[5] Also see Nozick's Ph.D. thesis (Nozick 1963). This view was fairly standard in the work of many other philosophers and decision theorists during the third quarter of the twentieth century: Leonard Savage (1972), Donald Davidson and Pat Suppes (1959), and others.

that can accommodate many of these anomalies – but the normative aspect has actually worked in the opposite direction. It has re-framed rational choice theory in a way that seems to preserve, rather than undermine, the role of *homo economicus* in economics.

The first response of economists was to challenge the empirical results of psychologists and behavioral economists (Plott 1996 and others), but more recently, the response has been to re-frame rational choice theory as a normative theory of rationality rather than a descriptive theory of what agents actually do. This is rather surprising. One does not need to be a strict falsificationist to think that if you have a theory of behavior that has been shown to be empirically inadequate, it would be epistemologically questionable to redefine the theory as a normative theory. Such a change could certainly be interpreted as an ad hoc theory-saving move. Through most methodological lenses, the proper response to empirical anomalies is more and better empirical work, not simply deciding that the theory is about what people ought to do, not what they actually do. This methodological issue is sometimes called the *normative retreat story* (or strategy, or problem) and it has received some attention within the methodological literature: Guala (2000), Hands (2015), Heukelom (2014), Jallais, Pradier, and Teira (2008), Malecka (2019), Moscati (2016), Starmer (2009), and others. It is a very intriguing issue, although I would also note that it is hard to make the case that it was purely a defensive move since the normative view of rational choice theory was common within psychology and philosophy long before the behavioral anomalies appeared in recent decades. My point in discussing the normative retreat story is not that something nefarious was/is definitely going on, but simply to note that it is an interesting and important methodological question that has received some, but from my point of view not enough, attention in the recent methodological literature.

Vb. <u>Behavioral Welfare Economics (Particularly Libertarian Paternalism)</u>

Behavioral economics and the normative interpretation of rational choice theory have certainly been the subject of much methodological debate, but these disputes have recently spilled over into an often more heated debate about the impact of behavioral economics on welfare economics (and thus into the methodological foundations of microeconomic policy). It has led to what is called the reconciliation problem and to methodological disputes about various approaches to what is now called *behavioral welfare economics*.

> For at least the last three quarters of a century, both descriptive and normative economics have been based on assumptions about individual rationality … however, there have been increasingly evident signs that economics might be changing direction, … There has been an accumulation of work which tests rationality assumptions, often in controlled experiments, and finds systemic 'anomalies' (that is, deviations from received theory) … These developments pose severe problems for normative economics – most obviously, the fundamental

> theorems of welfare economics … The problem of how to reconcile normative and behavioural economics – the *reconciliation problem* – is only just beginning to be recognized. (McQuillin and Sugden, 2012, pp. 553-554)

A vast literature has been generated in behavioral welfare economics in recent years, some of it theoretical, some of it empirical, some of it philosophical-methodological, and some of it quite practical. There are at least five, often intertwined, but to some degree separable, such literatures:

- Libertarian Paternalism, Asymmetric Paternalism, and Related Nudging (Sunstein and Thaler 2003, Thaler and Sunstein 2003, 2009, …)
- The Economics of Happiness (Frey and Stutzer 2002, Layard 2006, Oswald 1997, …)
- Neo-Hedonism and Experienced Utility (Kahneman and Thaler 2006, Kahneman, Wakker, and Sarin 1997, …)
- Choice-Theoretic or Revealed Preference-Based (Bernheim 2016, Bernheim and Rangel 2009, …)
- Neuroeconomic and Neurophysiology-Based (Bernheim 2009, Fehr and Rangel 2011, …)

All of these versions of behavioral welfare economics have received a significant amount of attention in the methodological literature, but the one where the reconciliation problem is perhaps most easy to see, is libertarian paternalism. It is also the approach to behavioral economics-based policy that has "gained public attention through being presented in books aimed at the general audience."

Libertarian Paternalism (LP) was first introduced by Richard Thaler, Cass Sunstein, and others in 2003, but Thaler and Sunstein's *Nudge: Improving Decisions About Health, Wealth and Happiness* (2009) is the popular version of the approach. LP begins from the behavioral economics position that that individuals make *mistakes*, but then seeks to find ways to nudge these individuals back to more rational choices *without using coercion* or *incentive-based economic tools*. Since the choice context matters to outcomes, it is argued that the individual's choice environment can often be changed in ways that will nudge individuals into making more rational choices.

> In our understanding, a policy is "paternalistic" if it tries to influence choices in a way that will make choosers better off, *as judged by themselves*. Drawing on some well-established finding in social science, we show that in many cases, individuals make pretty bad decisions – decisions they would not have made if they had paid full attention and possessed complete information, unlimited cognitive ability, and complete self-control. (Thaler and Sunstein, 2009, pp. 5-6)

The idea is to change the choice architecture in such a way that individuals who are prone to HB-type mistakes will be nudged into more rational choices, while those who are not prone to such mistakes will not change their behavior. Thaler and Sunstein call those who do not make mistakes *Econs* (the *Homo economicus* of standard economic theory) and those to who do make such mistakes *Humans* (Thaler and Sunstein, 2009, p. 7).

An extensive methodological/philosophical literature on LP has developed in recent years. Although there does seem to be a consensus that LP is problematic, there does not seem to be any consensus about what it is exactly that makes it problematic or what to do about it. As a result, all I will do here is to list a few of the many questions and criticisms that have been raised.

i) How do we know that nudging and shaping decisions only involves changing choices for given preferences and does not change preferences? This has been called the assumption of the inner rational agent (Infante, Lecouteux, and Sugden, 2016). This means, among other things, that LP is blind to constructed preferences (Lichtenstein and Slovic 2006) and other important insights of behavioral economics.

ii) LP nudging preserves the *normative* roll for rational choice theory. Nudging involves making people do what rational choice theory says they should do in order to be rational. Nudging assumes "the singular and universal supremacy of rational choice axioms as the proper normative benchmarks against which virtually all forms of behavior are to be measured" (Berg and Gigerenzer, 2010, p. 148) and even though behavioral economics challenges positive rational choice theory "behavioral paternalism maintains those axioms as normative standards to which agents ought to conform" (Whitman and Rizzo, 2015, p. 410).

iii) The social planner (choice architect) has an epistemic problem. The point is to nudge people into doing what Econ do, but what Econs do is act rationally on the basis of well-ordered preferences. But of course we do not even know that people have well-ordered preferences, and even if they do, how could the choice architect know them? Versions of this criticism have been raised by many different authors (e.g. Berg and Gigerenzer 2010; Gigerenzer 2015; Grüne-Yanoff and Hertwig 2016; Guala and Mittone 2015; Hausman 2016; Infante, Lecouteux, and Sugden 2016; Rebonato 2012; Rizzo and Whitman 2009; Sugden 2008, 2015, 2017, 2018; and others).

iv) *Autonomy* problems are generally associated with issues in moral and political philosophy: freedom, power, liberal values, manipulation, etc. How do we know the nudges are designed to make Humans better off as judged by themselves (rather than as judged by the choice architect)? Does being nudged into more rational behavior make people less able to act rationally on their own? And on and on … There is an extensive literature on these wide-ranging concerns (e.g. Barton and Grüne-Yanoff 2015;

Berg 2018; Fumagalli 2016; Grüne-Yanoff 2012; Hausman 2018b; Hausman and Welch 2010; Heilmann 2014; and others).

v) The politically-charged character and general fuzziness of LP rhetoric: there is often no specification of necessary and sufficient conditions for successful nudging, the need for nudging, the alternatives of nudging, and (the big one) what exactly is being assumed about the agents: either Humans or Econs. The emphasis is on "obviously better outcomes" rather than on foundations when the obviousness is contested. See for example Hagman, Andersson, Västfjäll, and Tinghög 2015; Lepenies and Malecka 2018; Rebonato 2012; Sugden 2018; and others.

Vc. The Problem of Idealized Models in Economics

The last issue I will discuss is the issue of false assumptions and highly idealized models, which has been one of the longest-running debates in economic methodology. It was a major locus of contention even during the early 1800s in the debate over free trade and the Corn Laws, where the relevance of Ricardian economics was aggressively challenged because of its deductive form and empirically questionable assumptions. But these concerns became even more relevant during the middle of the twentieth century during the heyday of the so-called formalist revolution of Arrow-Debreu general equilibrium theory. Even though the wave of this formalist revolution seem to have broken in economics, the core issues are still with us. Mathematical economic models typically contain large numbers of highly idealized assumptions – assumptions that are not even approximately true and known to be false even by those building and using the models – and yet these idealizations are often necessary for the derivation of the model's main results. This raises serious questions about how such models can provide adequate scientific explanations or achieve other epistemic goals.

The problem of idealization has been framed in many different ways, depending on the class of models that the author is concerned with, the scientific goals that are presumed to be most important (explanation, prediction, unification, etc.), the deeper philosophical commitments of the author in question, and many other factors. I will just present a few quotes that I think demonstrate both the general spirit of these concerns as well as the variety of different forms these concerns take within the contemporary philosophy of science literature:

> Why should theorists who are primarily interested in studying what is actual try to understand what isn't actual? The answer to this question cuts deep into the heart of theoretical practice. (Weisberg, 2013, p. 47)

> Models contain all sorts of falsehoods, from omissions, abstractions, and idealizations to outright fictions. One of the most controversial issues in model explanations is whether these falsehoods, which are inherent in

> the modeling practice, are compatible with the explanatory aims of science. (Bokulich, 2017, p. 108)
>
> Highly abstract and simplified theoretical models have an important role in many sciences, for example, in evolutionary biology and economics. Although both scientists and philosophers have expressed doubts about the epistemic import of these idealized models, many scientists believe that they provide explanatory insight into real-world phenomena. Understanding the epistemic value of these abstract representations is one of the key challenges for philosophers of science who attempt to make sense of scientific modeling … (Ylikoski and Aydinonat, 2014, p. 19)

Such questions about idealized models are some of the most important topics in contemporary philosophy of economics, and in particular they are the questions that constitute the strongest link between the philosophy of economics literature and the more general concerns of philosophers of science. The problem of trying to discuss them here is that there is such a vast amount of approaches, accounts, and answers to the various question raised by idealization (in economics and elsewhere) that it is not possible to examine the literature in any detail. So all I will do here in closing this section is to provide a taste of the vast and rapidly growing literature on idealized economic (and other scientific) models and their relationship to explanation, understanding, knowledge, etc.

- Idealized models provide scientific understanding although not scientific explanations (Ylikoski and Aydinonat 2014, …)
- Idealized models provide scientific understanding and possibly explanation, although the two are different (Verreault-Julien 2019, …)
- Idealized models provide learning, but not explanation (Grüne-Yanoff 2009, 2013, …)
- The Isolation Account: "false idealizations often serve an important purpose, that of theoretically isolating causally significant fragments of the complex reality" (Mäki 2009a, p. 71; also see Mäki 2009b, 2011, 2013, …)
- Fictionalism: idealized models involve fictions and should be thought of in terms of truth in fiction (Bokulich 2012, 2016; Frigg and Nguyen 2016, …)
- The Models as Mediators Account: de-emphasizes the importance of a tight connection between models and targets by characterizing models as flexible and context-sensitive mediators between theory and phenomena (Morgan 2012, Morrison and Morgan 1999, …)
- How-Possibly Explanations: an extensive (and diverse) literature suggesting that idealized models provide some version of how-possibly, rather than how-actually, explanations (Grüne-Yanoff 2009; Reutlinger, Hangeliter, and Hartman 2018; Verreault-Julien 2017, 2019, …)

- Various strategies such as derivational robustness can increase the epistemic credibility of even highly idealized models (Hands 2016; Kuorikoski, Lehtinen, and Marchionni 2012, Schupbach 2018, …)
- The explanation paradox: The three seemingly acceptable statements – i) Economic models are false, ii) Economics models are explanatory, and iii) only truth can explain – is a paradox (Reiss 2012).

V. Some Brief Closing Thoughts

This paper has provided my personal reflections on the development of economic methodology during the 21st century as well as a discussion of the methodological literature immediately preceding it. It was based on my experience – both as an editor and researcher within the field – and to some extent it reflects my own interests and concerns. It provides an interpretative history of the field and the various forces at work within it – doing so with a fairly broad brush, but at times focusing in and being much more specific.

In closing I would note that while I have tried to be a fair and impartial participant-observer, no doubt there were times when the participant's voice overpowered the observer's. I am pleased to say that while *Reflection* certainly did not predict the specific developments of the last two decades, my general argument that methodology was becoming more naturalistic, less universal (with respect to both science and philosophy), and more practical, did in fact play out as I suggested. I will make no such predictions here, but hopefully the reader has a good idea what the major changes have been as well as the various forces contributing to those changes.

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