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# RESEARCH IN THE HISTORY OF ECONOMIC THOUGHT AND METHODOLOGY

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A RESEARCH ANNUAL

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# STIGUM'S ECONOMETRICS AND THE PHILOSOPHY OF ECONOMICS WORLDS APART?

Kevin D. Hoover

Review essay on Stigum, B. (2003), *Econometrics and the philosophy of economics: Theory-data confrontations in economics*. Princeton, NJ: Princeton University Press. xxii + 768 pp. ISBN: 9780691113005. \$105.

The long delay between the publication of Bernt Stigum's *magnum opus* and this review owes not only in part to personal reasons of no interest to anyone but the reviewer, but also to the sheer heft and density of the book itself. It is a long (768 pages) and difficult book. In fact, it is really two books: the first is Stigum's treatise on the philosophy of econometrics and the second is an anthology of contributions, constituting 8 of its 27 chapters, from a distinguished group of 16 econometricians, including two Nobel Prize winners (Granger and McFadden). The anthology sits somewhat uneasily alongside the treatise. The inclusion criteria seem to be either that the topic is one that Stigum thought ought to be covered or one that illustrates his larger points. Yet, it is unclear that the contributors fully subscribe to Stigum's analysis or that their contributions do not rather obscure than clarify his own position. The work would have been stronger and more readable had Stigum chosen to publish the treatise and the anthology separately. Even broken up in this way, Stigum's own 329 page contribution would be a formidable and erudite work. Although it is rare enough to find a scholar who is comfortable in mixing such disparate thinkers as Aristotle, Carol Gilligan, and E.E. Evans-Pritchard in the same

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work, it is, I am sure, unprecedented when that work is principally concerned with econometrics.

According to Stigum, his book is about a riddle: "How is it possible to gain insight into the social reality with data concerning a socially constructed world of ideas?" (p. 3). He gives this riddle a philosophical gloss, explicitly drawing the parallel with Kant's question: "How is pure natural science possible?" (p. 4). Kant turned this into a more abstract question: How is the synthetic a priori possible? Although Stigum says that he differs from Kant in important ways, the Kantian influence is a genuine one. Stigum believes that there is no substantial economic knowledge without an a priori interpretive framework and that there are real gulfs between theory and data that are bridgeable only through a priori assumptions. The *Ding an sich* of economic reality is truly unknowable.

It is, therefore, somewhat surprising to find Stigum claiming in "a final remark on the riddle" that "I have tried to write a book that gives an econometrician the idea that he or she should 'get on with the job' while keeping" the problem of the gulfs between theory and data and data and the world in mind (p. 7). Had he not said it, I do not believe any reader would have taken such a pragmatic attitude to be a central one in the book. Pragmatism requires a different approach. Just before beginning to compose this chapter, I happened to be glancing through a wonderfully succinct, deeply insightful, and unfortunately neglected econometrics textbook from the late 1950s. In discussing how hypotheses are chosen, its author, Stefan Valavanis (1959), writes:

The economist, geneticist, or other investigator usually begins with (1) prejudices instilled from previous study, (2) vague impressions, (3) data, (4) some vague hypotheses.

He then casts a preliminary look at the data and informally rejects some ... He uses the remaining data informally to throw out some of his hypotheses, from among those that are relatively vague and not too firmly grounded in prejudice.

At this stage he may prefer to scan the data mechanically ... rather than impressionistically ... Logically, of course, any mechanical method is an implicit blend of theory and estimating criteria; but, psychologically, it has the appearance of objectivity. The good researcher knows this, but he is too overwhelmed by the illusion that mechanisms are objective.

Having done all this, the investigator at long last comes to specification ... he then estimates, accepts, rejects, or samples again.

This stage-by-stage procedure is logically wrong, but economically efficient, psychologically appealing, and practically harmless in the hands of a skilled researcher with a feel for his area of study. (pp. 154-155)

Valavanis has truly described "getting on with the job." Nothing in Stigum's book compares to such a robust pragmatism, and it is difficult to see how Stigum's highly formal, axiomatized approach could actually aid in the practice of work-a-day econometrics.

Yet, there are other goals than furthering practical econometrics. Stigum's book might be seen as a case study in the philosophical analysis of an applied discipline. Much of the philosophy of science has traditionally been somewhat abstracted from the sciences that are supposed to be its subject. Though highly formalized approaches have become less popular in the philosophy of science in recent years, even in their heyday, there were few attempts to move beyond the schematic to a concrete implementation. Stigum does just that, and it is hard not to admire the intellectual scope and energy that allows him to write confidently about economic theory, econometrics, and philosophy. Practical economists often act as if the line between theory and data were direct; Stigum's *tour de force* brilliantly characterizes the structure of the relationships between economic theory, data, and the world in a way that makes clear that the connection is very indirect indeed.

The puzzle posed in Stigum's riddle arises from his view that economic theory is not really about the economic world that we live in. Rather he regards all theoretical models as toys - models that are true about a separate theoretical world that cannot, in principle, directly attach to the data. The data themselves also belong to a realm that is separate from the real world and are equally characterized by models. In a move that goes back to the logical positivists, Stigum invokes bridge principles as a means of connecting theory to data to the world. The worlds of theory and of data, and the bridge principles as well, are presented as axiomatized systems. Philosophers have distinguished between the syntactic view and the semantic view of theories. The syntactic view treats theories as formal systems, whose terms are uninterpreted and whose important properties are logical deductions. On the syntactic view, theories attach to the world by being assigned a relevant interpretation. On the semantic view theories are instead collections of models, where in the classic (strong) interpretation a "model" means a consistent interpretation of a formal system. Stigum rightly recognizes that the difference between the syntactic and the strong semantic view of theories is vanishingly small. Both trade in axiomatized formal systems, and Stigum sees no reason to commit to one view over the other. His rhetoric may, to some extent, favor the semantic view, since he recognizes a great deal of flexibility about the contents of the worlds addressed by economic theories, so that it is convenient to stress the multiplicity of models.

The most characteristic point is not any preference for one view over the other, but a clear preference for formal systems. This has the practical implication that Stigum's approach appears to be top-down: we start with axioms and work out implications. "Top-down" should not be thought to imply a priority of theory over data, since for Stigum even data is characterized axiomatically. Stigum's vision of the theory-data confrontation is one in which theory, data, and bridge principles are all characterized as formal systems, which are tested by asking whether the different systems are logically consistent. This vision shortchanges the observational and investigative aspects of science and gives no real account of how loose insights get refined into theory. Though Stigum does not believe that the work-a-day econometrician must actually employ an axiomatic approach to get on with his research, he nevertheless manages to step on that message through his own relentless formalism, which is poles apart from Valavanis's messy pragmatism.

Stigum could have adopted a weaker semantic approach – one in which a theory is seen as a group of interpreted models linked by Wittgensteinian family resemblance, a view familiar from Paul Teller (2001) and Ronald Giere (2006) among others. A scientific architecture in which theory and data are worlds apart is quite consistent with the weaker semantic view. Nancy Cartwright (1999, chap. 2) has, for instance, argued in a manner parallel to Stigum that the truths of models are exact in their own world. On her view, models are fables as interpreted by the German romantic dramatist and critic Georg Lessing. A fable tells a genuine truth about its subject; but our interest in a fable is in its positive analogy with something that we want to understand in the world. This fits nicely with Stigum's understanding of theoretical models as having positive and negative analogies with data and the world. The application of models draws on the positive analogies while steering clear of the negative ones.

Stigum maintains that (1) when we claim that, say, an economic agent maximizes utility, we mean that literally about the theoretical world in which people are economic agents; (2) despite negative analogies, we should not think of the application of positive analogies as approximations; and (3) "[o]ne's understanding of the theory and the data one possesses determine [*sic*] what kind of questions about social reality one can answer in a theory-data confrontation" (p. 11; see also p. 118). Points (1) and (3) together rule out an instrumentalism (which Stigum attributes to Milton Friedman) in which theories are successful if the world acts *as if* they were true; for Stigum maintains that there is no "as if" – theories are literally true in their world.<sup>1</sup>

He takes points (2) and (3) as distancing himself from John Stuart Mill's notion that economic theories express tendency laws. He does not further amplify this position. Whether it is reasonable depends largely on how one reads the notion of a tendency law. Stigum seems to read it as an approximation. But Mill, who was an *a priori*ist with respect to economic theory, is open to another reading, one that is consistent with Cartwright's (1989) notion of causal capacities. A capacity for Cartwright always acts to express itself appropriately and fully in the circumstances. But different capacities combine together in various ways; so that what we see in the world is a complex interaction that may quite obscure the unimpeded action of the capacity. A *tendency* then would be the product of the interaction of some set of capacities without the interference of other capacities. Just as for Stigum, this is not an approximation, but a truth (for Mill a deductive truth) about a world that we do not actually observe. If interfering causes (that is, the actions of capacities that are not in the set that defines the world of the model) are not too potent, a regularity (of various degrees of exactness) might be produced. We might even say that such a regularity is approximately true, but the *law* is not this regularity but the outcome of the interaction of the capacities in a world without interference.

Stigum rejects scientific realism on the ground that the terms in economic theories "pertain to matters of fact in a toy economy and nothing else." This position is closely related to his notion that that theory and data occupy separate worlds and that the standard of resolution for confrontations – both theory-data and theory-theory – is consistency of complete formal axiomatizations of these worlds. "Toy" is a pejorative – or, at the least, distancing – adjective. Toy models are not models of the world, it would seem, because the world is vastly more complex than any model, which must be simple enough to analyze formally if logical consistency is to be the principal virtue. But there are other sorts of realism. Cartwright, for instance, is a realist about capacities. Stigum cannot follow her because, in effect, he subscribes to what Teller (2001) refers to as the "perfect model model" – the idea that a realistic model of the actual world must be *complete* and *consistent*. Here again, Stigum's adherence to the strong semantic approach does not serve him well. A weak semantic approach allows a realism, like Cartwright's, in which realistic models are less than universal in scope.

As I have already observed, Stigum pays lip service to data analysis, but even his treatment of actual data is top-down and axiomatic, driven by the view that data do not speak for themselves. His vision of scientific investigation is explicitly anti-Popperian. The goal of the scientist is not to propose strong hypotheses and to let data falsify them. It is rather more like

solving a complex crossword puzzle, where some parts are known and others have to be filled in a consistent manner. It is an attractively constructive view. But it puts a great deal of weight on what the investigator himself brings to the table (Kantianism again) with little attention to how one might reasonably modify these prior commitments. The result is arbitrariness and an extraordinary view of investigatorial privilege

It is quite clear that two researchers can have different opinions about the meaning of a given set of observations. It is equally clear that the same two researchers may differ in the ways they relate the given observations to variables in a particular theory universe. Be that as it may, for the purposes of this book, it is important that there be agreement that in any given theory-data confrontation, *it is the opinion of the researcher in charge that counts.* (p. 255; emphasis added)

Without some sort of privileged view, there are so many degrees of slippage among the different levels of modeling that it is a single, unequivocal understanding of a theory–data confrontation seems unlikely – the Duhem-Quine problem. Yet, Stigum offers a procedure that “may enable the econometrician to circumvent the Duhem trap” (p. 256 and chapters 10, 11, and 17). Try as I might have not been able to grasp Stigum’s solution to the Duhem-Quine problem.

I have dwelled perhaps too much on the shortcomings of a remarkably ambitious and largely unprecedented analysis of the philosophical foundations of econometrics. Econometrics as defined in the founding documents of the Econometrics Society was the field that considered “economic theory in its relation to statistics and mathematics” and whose object was the “unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems” (Frisch, 1933, p. 1). Most econometrics texts today are long on statistics and rather short on economic theory. Stigum has made a bold attempt to understand the enterprise of econometrics in its original formulation. It is not only a difficult book, but also a rich and provocative one. No other such work exists. It sets the standard against which any future contribution to the philosophical foundations of econometrics will be judged.

## NOTE

1. That Friedman is actually an instrumentalist is widely but not universally accepted (see Mäki, 1992; Hoover, 2009).

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