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Theory, generalisations from cases and methodological maxims in evidence-based economics: Responses to the reviews by DiNardo, Guala and Kincaid

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First, I wish to thank John DiNardo, Francesco Guala and Harold Kincaid for their very thoughtful and provocative reviews of *Error in Economics*. I do not want to hide my contentment with there being much agreement with its main claims and the overall stance it takes. In so far as they criticise ideas presented in the book, the criticisms pertain to details and I'm grateful to be given the chance to respond to them here.

A theme that runs through two of the three reviews is that *Error in Economics* exaggerates its disapproval of economic theory. Thus Guala writes, 'Reiss blames experimenters' theory-centrism for biasing the design process' (p. 85), whereas in fact 'the role of experimenters in the FCC enterprise was limited' (p. 85); and Kincaid, 'On his view, economics is too theory driven' (p. 80) and 'I do not know if this is true of most economics – that would require a rather Herculean survey'.

I now too think that I was to some extent mistaken about the enemy. It is not economic theory as such that is what is wrong with economics, but rather a certain form of reasoning that resembles theorising but is in fact far from it. I take genuine scientific theories such as Newton's, Darwin's or Einstein's (or even Marx's or Freud's, if we suppress empirical doubts for the moment) not only to provide a conceptual framework within which to think about problems of interest, but also a small number of explanatory hypotheses that can be used over and over again in the explanation of a wide range of phenomena. Genuine theories are surprising and counter-intuitive in which they show that phenomena, once thought to be unrelated, in fact share a common origin. Modern mainstream economics does nothing of this kind.

It is not an accident that prominent economists such as Steven Levitt speak of economics as a set of tools rather than a substantive theory (Levitt and Dubner 2005, p. 11).[1] The utility framework indeed gives us a way to structure research problems of interest. This is one function of a scientific theory to provide a language and modelling techniques. But when it comes to the second function, explaining economic phenomena (normally achieved through a system of substantive hypotheses), mainstream economics is almost entirely *ad hoc*. There is a 'theory' (say, Carl Menger's) that explains the origin of money. There is another (say, Irving Fisher's) that explains the interest rate. Yet another (say, Milton Friedman's) explains savings and therefore (household) debt. Yet another (say, Robert Solow's) explains growth. Yet another (say, Finn Kydland and Edward Prescott's) explains the business cycle, and so on.

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Any genuine economic theory worth its name would unify all or at least a substantial number of the mentioned phenomena. It would show that these seemingly disparate socio-economic practices have a common explanation. No such thing is true of modern mainstream economics. (Similar things could be said about micro economics.[2])

I now think that it is probably true that having a genuine theory at hand would help with many of the book's projects such as drawing causal conclusions from observational data using instrumental variables or extrapolating results from one setting to another. But this does not make its criticism of current practice less pertinent. Indeed, the reviewers agree with my detailed criticisms but they seem unhappy with certain inferences I draw, or seem to be drawing.

A point I made in Chapter 5 is that auction (or, more generally, mechanism) design may be biased when done through the lenses of economic theory. In the particular case, the FCC auctions, a plurality of goals set by Congress was replaced with the goal of maximising government revenue. In this context, the difference between 'theory as a language and set of tools' and 'theory as a body of explanatory hypotheses' is especially relevant. Viewed as what it is – language and set of tools – economic theory can in principle model any set of goals. A utility function does not care whether it takes monetary gain, (hedonist) happiness or number of armed conflicts in the world as arguments. Practising economists, however, tend to pretend that people care only about monetary gain. It is important to be clear that this assumption is *not* part of a system of substantial hypotheses of economic theory (and good thing too – as it is false for the most part).

Nevertheless, because practising economists have historically tended to make the assumption in many existing models, within and outside of auction theory, embody it. This way, I argued, the goals of Congress were misinterpreted.

Guala knows much more about the actual history of the FCC case than I do, so I trust his report of the order of events. But the aims of the chapter did not include apportioning blame for a failed policy to a specific set of people. I actually said so quite literally: 'It is hard to blame (only?) the consulting economists for the performance of the auctions for a variety of reasons. Most notably, an economist consultant is a specialised expert, and a specialist will always frame a problem in terms of his discipline' (p. 103).

Instead, one of its aims was to discuss the methodological maxim that policy advice should be made on the basis of the goals of the advice seeker, not the advice giver, in the context of a concrete case. Perhaps some passages could be read as attempts to apportion blame but to do so would misconstrue the book's overall goals, which are methodological throughout.

Another point the chapter made was about the method of analysis and synthesis. The chapter argued that it cannot be assumed that economic factors (such as monetary incentives, experience and information) make a contribution to the outcomes that is stable across a wide range of circumstances. In his review, Kincaid grants this point but cautions, 'to think either (1) they must or (2) most experiments in economics have such problems is asserting much more sweeping claims which require a correspondingly greater level of argument' (p. 80).

I fully agree with Kincaid's caution. But so did the book. It says for instance, 'it is hard to think of factors in these experiments *in general* to constitute persistent factors that continue operating whether or not other factors intervene . . . Some factors, without doubt, exhibit a certain degree of stability. [. . .] But other factors do not follow this model' (pp. 95–96; original emphasis). The point was that it is an empirical fact whether or not an economic factor does contribute stably across different circumstances, and therefore that extrapolations from one set of circumstances to another require the appropriate evidence.

The point was exactly to advise researchers not to make claims that are more sweeping than is warranted by the evidence.

DiNardo similarly worries that what the book says about one case (e.g. the Boskin report; Card and Krueger 1994) might not quite carry over to another case (e.g. the NAS report; other work by Card and Krueger and others on minimum wages). Again, I agree. Let me say a few things in response, one general, and some more specific about the cases.

The general point is that the case-based approach the book took tries to strike a balance between the abstract and universalist attitude of the (traditional) analytic philosopher of science and the highly particularist attitude of the historian. It aims to make methodological points that are motivated by and embedded in concrete cases but that are at the same time to some extent generalisable. The cases it discusses were selected having, in part, this generalisability in view. What does not matter so much from the point of view of the book's main goals is whether or not any given case constitutes the best work or the last word on a given issue. Methodology is at the forefront, not criticism of bits of practice or groups of economists.

When DiNardo says that 'Many of the flaws that Reiss points to in the Boskin report are not present (or less evident) in the NAS report' (p. 88).[3] I could happily agree except that I had focused the discussion on the latter, many of the methodological points could simply not have been made or only made less forcefully. And given the prominence of the Boskin report and the impact it had, I could hardly be charged with cherry picking.

One such point concerns the question whether the concept behind the consumer price index should be a cost-of-living index (COLI). Chapter 2 asks the more technical question whether the measurement procedure associated with the index is appropriate *given* the COLI framework. Chapter 3 addresses the question of the appropriateness of the framework itself and argues that value judgements must be called upon to answer it.

Now, the fact that not everyone thinks that the COLI framework is appropriate, including Angus Deaton (whose paper was also discussed in the chapter), it doesn't show that the project of Chapter 2 is not valuable. In the large majority of literature on the topic I had the chance to study, the COLI framework is presupposed without question, and indeed, the Bureau of Labor Statistics implemented it quite some time ago. Deaton's was one of the dissenting voices in the NAS report. Moreover, since the COLI framework is closer to the kind of economic theorising much of the book takes as foil than other frameworks, I could make some points that I think are true of parts of economics that have nothing to do with price measurement.

My response to DiNardo's discussion of the Card and Krueger case is differently pitched. Chapter 7 regards their work as exemplary for the identification of what I call a 'causal instrumental variable'. Chapter 9 addresses the difficult problem of external validity. One of its main claims is that even very reliable empirical methods in economics – such as causal instrumental variables – are good at best to establish internal but not external validity. Extrapolating a result from one internally valid study to a new setting requires a different kind of evidence. According to some very recent contributions, essential evidence concerns the mechanism(s) by which a certain outcome is produced (e.g. Steel 2008), for instance, the mechanism(s) that connect minimum wages and employment. To my knowledge, what these mechanisms might be is still highly controversial (e.g. Neumark and Wascher 2008). It is no surprise, then, that the chapter cautions not to apply results from natural experiments such as the one in New Jersey's fast-food industry too hastily elsewhere.

All these differences are, to my mind, differences of emphasis and presentation. At the end of the day, I hope that many readers will agree with DiNardo when he says that 'actual practice is an extremely fertile place to put on some "philosophical glasses"'.

Notes

1. He also thinks that the principle 'people respond to incentives' is at the heart of economics, and one could regard it as substantive. Even if so, since that principle neither determines what the incentives are to which people allegedly respond nor how they respond, my point about explanatory hypothesis stands.
2. The paradigm case is game 'theory'. In its case it has been pointed out that the system of ideas is in fact a branch of applied mathematics – thus, a language and set of tools – rather than a body of substantive hypotheses.
3. The report is actually cited in both Chapters 2 and 3.

References

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