

which runs the risk of replicating the phenomenon of what Sunstein terms “group polarisation” in populations, whereby opposing ideological groupings refuse to give ground, thus generating more “heat” than “light”.

One final lesson from Sunstein’s book struck me, in that in writing this review I have of course focused upon the arguments presented by Sunstein himself, and thus there is a danger that I have overlooked the thoughts and views of others. Am I not therefore suffering from the very phenomenon that Sunstein warns against? The reader perhaps ought to be fearful that I am.

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The Methodology of Experimental Economics, by Francesco Guala. Cambridge University Press, 2005, xi+286 pages

This is a book that sorely needed to be written, and the experimental economics community should be grateful that Guala was the one to do it. In recent times the debates over methodology and the “scientific status” of experimental economics has become so integral to understanding the actual results of experiments, that many conversations which begin about the causes of, say, preference reversal, will end up being about the philosophy of science. Unfortunately, most scholars who know about the former will know little about the latter, which means that question and answer sessions at presentations of results can often be object lessons in how to talk at cross-purposes. I am happy to say that Guala is an exception to this general finding, and his book should be widely read to help improve the quality of such discussions. Debates between behavioural economists/constructed preference theorists and more neoclassical experimental economists/discovered preference theorists have become both more heated and more public, as the Nobel Prize being awarded to Daniel Kahneman and Vernon Smith brought such questions to a more mainstream audience (I will use the discovered-constructed preference terminology to refer to the two “sides”). The discovered preference theorists accuse the constructed preference theorists

of using “dirty test tubes” (the phrase is Binmore’s; see Guala, 246 *passim*). The constructed preference theorist replies that the discovered preference theorist is sticking her head in the sand by denying that the axioms of expected utility theory are truly refuted by solid laboratory experimentation. Guala’s book charts an admirable and sensible middle ground between these two positions.

Guala’s trick, as it were, is to present an outline of the state of play in the philosophy of science, but to illustrate his points with the example of the science of experimental economics. Guala denies that his book is a handbook of experimental economics or a methodological handbook (6–7). He is also at pains to emphasize, throughout the first part of his book on induction and inferences within experiments, that nor is the book a primer in the philosophy of science. What it is instead is a careful, cautious and calm argument about how *real* laboratory experiments work (in both the natural and the social sciences), and then how such results are philosophically justified in providing support or refutation for hypotheses and theories. As someone who always thought that the discovered-constructed preference debate was a load of spilled ink over some very simple questions, I couldn’t welcome this book more. However, it is here that the book has a drawback, to me at least: I found very little in the book that I thought to be truly original or exciting. I struggled to find one argument in the book that I thought wrong, and I just couldn’t. Guala clearly, and helpfully, sets out eight “themes that recur” throughout the book, which I found so immediately sensible that I would have been tempted to merely have the book on my shelf rather than read it. But I am glad I read it, and there are those who will take issue with even the basic themes or principles that I find so agreeable. Being “too correct” may seem like an odd criticism, but I myself often think the best books are those which are hugely original, challenging, and sometimes wrong (Nozick would be the classic example of this type of author), but at least they’ve stuck their neck out and their doing so has made me think. I fear that my review will therefore merely outline the basic structure of Guala’s book, as I can find so little to criticize. I will briefly entertain an objection to Guala’s dismissal of subjective Bayesianism in epistemology and philosophy of science. But lest I be misinterpreted, let me make a qualification: when so much ink has been spilled in a debate, and the participants in the debate are so often at cross-purposes, it is obvious that clarity and not originality is the intellectual virtue of choice. It is for this reason that I recommend Guala’s book so highly and why it is so welcome.

I would like to provide a brief and informal introduction to the discovered-constructed preference debate, as it is this area where the most heated methodological exchanges in experimental economics take place, and from which Guala has drawn the most inspiration, especially

in the second part of his book. The debate goes as follows¹: an anomaly in expected utility theory is found, say the preference reversal phenomenon, where in a choice task between A and B subjects generally choose A, but in a monetary valuation task a higher monetary value is generally given to B. The constructed preference theorists say “ha, so the axioms of EUT aren’t robust to empirical tests. Individual preferences are clearly dependent upon the elicitation measure, and so preferences are constructed by elicitation measures”. The discovered preference theorist replies: “These results are not subject to the discipline of a real market, which provide proper incentives, feedback on choices, and repetition of those choices. When these conditions are met, preferences become stable. The preferences are really in there all along, we just needed to discover them”. Among other replies, the constructed preference theorist replies, “but real markets aren’t always like you describe them, for instance, the most important purchase most people ever make, that of buying a house, is usually made only once at most. This is a huge domain of economic activity that your market disciplining hypothesis has nothing to say about!” To which the discovered preference theorist replies, “that’s as may be, but at least my theory explains how people are able to buy milk every week”. Or something like this.

Guala’s book seeks to rectify this stand-off by delineating various strands in the argument. The first, and the topic of the first part of the book, is “how do experiments confirm or disconfirm hypotheses and theories anyway?” In chapter two, Guala begins where all good philosophy begins, with an example, and interestingly, he uses the example of an experiment he himself conducted on public goods. In the course of the chapter we are walked, slowly and in great detail, through what happens in real experiments in economics. We are shown how various constraints, of time, resources, money, recruiting subjects, securing anonymity, shyness of subjects, computer programming to name but a few of his examples, get in the way of an experimenter’s best intentions. The aim of the chapter is both to show the vagaries of methodology, but also to show the importance of replication and of the phenomenon of interesting failures (38).

The third chapter concerns hypothesis testing, or what experiments are supposed to do for us as scientists. Here we are guided skilfully through the hypothetico-deductive model, Popper’s view of the importance

¹ I am here especially borrowing from work by Robin Cubitt, Chris Starmer and Robert Sugden and their collaborators; see (Cubitt et al. 2001; Loomes et al. 2003; Braga and Starmer 2005), and Cubitt (2005). I must here also declare a bias: I come from the same “stable” that Guala had his experimental economic training in (xiii), namely, the Economics Department at the University of East Anglia where I was a fellow for three years, and was indoctrinated in much the same way as Guala, namely by training under Sugden and conversing with Cubitt and Starmer, so it is perhaps unsurprising that I find so little to disagree with in his book.

of refutation and confirmation of hypotheses through evidence, and the Duhem–Quine problem. The latter is the problem that “deduction is insufficient for science, because positive inferences to a theoretical hypothesis are always *underdetermined* by the evidence” (59; emphasis in original. Or conversely it is common to say that facts are always overdetermined by theory). Fortunately for experimenters of all stripes, Guala does not see the Duhem–Quine problem as insurmountable for the hypothetico-deductive (HD) approach to hypothesis testing, instead “it is (only) an insurmountable problem for a very specific version of the HD model, the ultra-deductivist one” (ibid). Even better news for experimental economists is that, following John Hey, Guala argues that experimental economics is better able to deal with deductive problems because experimental control and theory choice are more directly up to the experimenter.

Chapter four concerns causation and experimental control, and is quite straightforward. Philosophers of science (indeed, anyone with much understanding of the philosophical foundations of statistics) should be warned that they are likely to learn very little from this and the last chapter. However, as a refresher course or introduction for an experimental economist, they do a fine job.

More interesting to both philosophers and economists will be the discussion of the role of prediction in chapter five. This is largely owing to Friedman’s famous defence of an instrumentalist philosophy of science in his 1953 essay on methodology. The chapter is not so much one where Guala sticks his neck out, but where he at least claims some territory as his own. Guala’s concern is with *epistemology* and not *ontology*, i.e., he is concerned with that part of philosophy of science which asks how and what we can *know*, not what there *is*. In this way his concern cross-cuts that of instrumentalism versus realism in philosophy of science: the aim is for us to understand how we might come to know how certain phenomena are out there, not whether we are merely predicting what those phenomena will be or what those phenomena “really are”. This chapter also sets up the concern of the second part of the book that draws from Friedman’s instrumentalism: namely, that what we do in the lab and in theorizing hypotheses had better have something to do with the world beyond our laboratories and armchairs. Again, there is not much in the chapter which is new or contentious, but nor is anything mistaken.

The last chapter of the first part of the book concerns how to eliminate sources of error by experimental control. The chapter covers material familiar to anyone who has studied Lakatosian philosophy of science, and hence the Duhem–Quine problem is reappearing. In summary of this chapter, and indeed the first part of the book, I must contest Guala’s perhaps modest defence that he is not writing a primer on philosophy

of science. I think the first part of the book is a very clear and highly readable introduction to current philosophy of science. Indeed, it would make a particularly good introduction, especially to the philosophy of *social* science, because of his continuous and judicious use of examples from experimental economics.

So on to the second part of the book, which concerns how one should move from inferences within experiments to inferences about the world. Any experimenter will recognize this problem, known as the problem of external (sometimes ecological) validity, only too well. It occurs when one's laboratory work is described to someone and they reply, "well that's very interesting, but it doesn't tell us anything about the *real world*, does it?" In chapter seven, Guala does not so much provide a philosophical solution to the problem of external validity, as reject it as a philosophical problem. He instead very sensibly claims that "the external validity problem is empirical in character and must be solved by appropriately combining field and laboratory evidence" (160). That is, we should apply what we learn from the lab in the world and see if it makes a difference. If not, as the phrase has it, it's "back to the lab".

The next two chapters of the book (eight and nine) look at various examples of interplay between the real world and the lab, focusing primarily on the famous FCC auctions designed by Klemperer, tested by Binmore, and then implemented by the British Government for use in auctioning off mobile phone airwaves (and a similar case in the USA). Guala thoroughly discusses the theoretical axioms which inform auction theory, the laboratory results of auction experiments, and their implementation. Again, a quite high level but very clear introduction to an area (auction design) is lurking in here, but Guala's modesty prevents him from pointing this out. In general all goes well: the Becker-DeGroot-Marshak mechanism elicits more consistent preferences; theories accurately represent auxiliary conditions and/or hypotheses; we amble our way towards external validity; and we eventually apply sensible public policies based on our best understanding of the previous facts. I felt that subjective Bayesianism could be helpful in discriminating among hypotheses more vigorously here, but Guala felt this proposition not worth the further testing.

Chapter ten develops the two themes of the second part of the book: "the first one is that we have no reason to believe a priori that an experimental result applies (or does not apply) to nonexperimental circumstances ... second ... successful external validity arguments are empirical and can be constructed only by appropriately combining experimental and field evidence" (203). (Guala could be more clear in his terminology: experiments take place in the lab and in the field, indeed, anywhere where variables can be controlled and post-test results measured. But substitute "the laboratory" for "experimental" in the phrase

and it becomes clear.) To this end he introduces the idea, also developed by Sugden, that experiments act as mediators between our theories and the real world, and this correct, but uncontentious, argument is well represented in his diagrams in figures 10.2 and 10.3. The argument is essentially that one starts with a description of a phenomenon, one develops a model of that phenomenon, and conducts an experiment around that model and examines the results (the latter is known as internal validity, when it works). The next stage is to take the results of the experiment and see if they can be replicated in the real world (the “target system” in Guala’s terms). This is known, again if it works, as external validity.

It is all very sensible stuff, but here the lack of novelty in his argument is somewhat irritating, for the diagrams basically replicate other diagrams which describe induction, deduction, theorizing, hypothesis testing, observing the real world, etc. A familiar example is in Martin Hollis’ (1994: 60) *The Philosophy of Social Science*, and indeed from his earlier (1977: 46) *Models of Man*, which Hollis himself plundered from an anthology in sociological theory. Indeed there is a long tradition in sociology of formal model building, testing and theory construction that deals with external validity, albeit not in the analytic-philosophical tradition that is Guala’s trade (cf. Abell, Blalock, Fararo and more recently Hedstrom and others). This irritates slightly not only because of a lack of novelty, but also because Guala must surely be familiar with this tradition as a philosopher of social science. So, irritating? Slightly. Novel? No. Incorrect? Absolutely not.

Guala closes the book with a chapter which should probably have been an appendix, on monetary incentives. Here, Guala, again with utter reasonableness, if not boldness, says: sometimes you really must have monetary incentives, sometimes you don’t. He discusses good reasons for both views.

As will have become clear by now, I find little to fault with the book. Originality and contentiousness can certainly be overrated intellectual virtues. And, as I indicated earlier, the virtue of clarity is especially virtuous when there is so much disagreement about. In the end, I can hardly imagine a better exercise in intellectual ground clearing for debates in experimental and behavioural economics than Guala’s book, and I recommend it highly.

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