

1. Introduction: useful philosophy of economics

Harold Kincaid and Don Ross

This collection of commissioned essays embodies a particular stance toward philosophy of science generally, and toward philosophy of economics in particular. We sketch this stance in this introduction, and say why and how we think the volume reflects it. We briefly indicate why we think that it represents the best way for philosophy of economics to develop.

We commissioned the chapters from a naturalist perspective that holds that, among other things, philosophy of economics ought to be close to and useful for the practice of economics. We briefly explain our version of naturalism and then suggest how the chapters of the volume embody it and how we hope they may prove useful for understanding and improving actual economic research, modelling and policy design.

Naturalism has multiple, only partially overlapping, connotations. Simple formulations commit to the proposition that everything, including humans and their behaviours and thoughts, constitutes a single natural world, and then usually add that everything in this world is amenable to study using scientific principles. As is common in grand debates, a view is often helpfully understood by seeing what it denies. Here naturalism denies ghosts, gods and other non-measurable entities and forces. In appealing to scientific principles, naturalism as a philosophical position denies that there are special philosophical methods, or a priori truths that can be known by conceptual analysis and intuition.¹ So, just as the parts of ontology constructed by humans are continuous with the parts of ontology that resist our wilful construction, so philosophy – or at least philosophy of science – is continuous with scientific investigation itself.

As described so far, naturalism is still nebulous in ways that might let all kinds of camel noses into its tent. Many philosophers describe their own work as scientifically and empirically based, despite its having zero relevance to practising scientists or to scientific progress. That is not the kind of naturalism we support.

There is, of course, a continuum of sorts running from abstract conceptual and theoretical work through into fine-grained empirical investigation of

highly localised phenomena. However, it is important not to be seduced by what Wilson (2006; 2017) calls a ‘fuzzy holism’ in which everything we believe is interconnected, and unconstrained metaphysical speculation and empirical investigation are inseparable. The continuum we defend between philosophy of science and science has much tighter constraints, as partly articulated in Ladyman and Ross (2007), demanding that worthwhile philosophy have clear relevance to empirical research programmes in which scientists actually engage, or clearly will engage as soon as they break through contingent barriers to epistemic access.

For example, conceptual analysis and clarification can be two different things: (1) proposed essentialist definitions tested against philosophers’ semantic intuitions and skill at generating counterexamples, or (2) Carnap-style explications of important scientific concepts and practices. Our naturalism favours the latter and eschews the former.

To give an important example, work on causation provides a nice illustration of what useful conceptual clarification looks like. Philosophers have for millennia tried to give an account of causation. Over the last several centuries that has generally meant trying to define individually necessary and jointly sufficient conditions for ‘*x* is the cause of *y*’ which make no use of other causal terms. Definitions are proposed, counterexamples appealing to what ‘we’ are comfortable calling a cause are identified, and then proposed definitions are refined or replaced. Largely independently of this form of conceptual debate, a group of biologists (Wright 1921), economists (Haavelmo 1944), computer scientists (Pearl 2000 [2009]), biostatisticians (Robins 2001; Shipley 2016), and philosophers of science (Spirtes, Glymour & Scheines 2001) have been analysing causation in a very different way. Rather than trying to find a definition in noncausal terms, they have analysed and proposed logics of causal inference. What, they have asked, are the interconnections between causal assertions and concepts of interventions, dependency, probability, and so on as they are used in ongoing empirical practice in various sciences? The result has been a clarification and unification of various empirical approaches, contributing significantly to scientific progress (see Kincaid, Chapter 3 in this volume). This kind of activity aimed at clarifying and improving actual, and not merely hypothetical, empirical investigation exemplifies the sort of naturalism we advocate and hope to advance with the present collection of essays.

Philosophy of science has changed significantly over the last 50 years, with implications for philosophy of economics. One way to mark those changes, which usefully simplifies the real, richer history, is based on conceptualising the history of philosophy of science in terms of three stages: positivist, Kuhnian and post-Kuhnian.

We can take the first stage to be original Vienna-style positivism. Those ideas were in fact much more complex, subtle and diversified than its most

famous critics realised (Cartwright et al. 1996; Richardson 1998; Friedman 1999; Richardson & Uebel 2007). However, two central aspects were a focus on theory, especially as formalisable deductive systems, and a search for universal rules for evaluating evidence and explanation. Kuhn (1962), in criticism, argued that there was much more to science than formal theory and that logics of evidence and explanation are either not obtainable or do much less work in science than positivists supposed. The central organising idea of ‘paradigms’ tried to bring in everything Kuhn thought was missing. Among the neglected elements were experimental practices, skills and heuristics for applying abstract theories, scientific norms and social influences. Kuhn at times flirted with drastically relativist conclusions and others after him pursued these ideas to a full-blown scepticism. Post-Kuhnian philosophy of science, which this volume promotes, seeks to retain insights from Kuhnian approaches while rejecting exaggerated relativism, and to preserve core inspirations from positivism – for example, commitment to clarification of theoretical commitments and evidential relations – without boiling the conception of science down to something so abstract and thin that the real practice of science becomes invisible.

Above all, the post-Kuhnian perspective emphasises that philosophy of science should describe and provide useful analyses of science on the ground. This attitude has led to the development of distinct ‘philosophies of ...’ the various disciplines – of physics (Batterman 2017), biology (Sterelny & Griffiths 1999), social science (Kincaid 1996), medicine (Solomon, Simon & Kincaid 2017), economics (Mäki 2012) and most recently, political science (Kincaid & Van Bouwel 2021). Within these, a plethora of different questions about how each science works have been pursued. Historical, formal, sociological, conceptual and empirical approaches have been combined. General questions about evidence and explanation are not ignored, but they are embodied in much more concrete issues within specific sciences and indeed within specific research trajectories inside those sciences.

At its best, current philosophy of science contributes in direct ways to clarification and progress within the sciences themselves. This need not require surrendering critical stances or entirely giving up philosophy for empirical work (Ross 2013). It does mean a considerable shift away from the pole of high-level abstract analyses of science in general, or even of whole disciplines in general, toward the pole of clarifying and contributing to actual empirical investigation.

So, what, then, does our naturalist stance imply for philosophy of economics? We outlined the basic answer more than ten years ago in *The Oxford Handbook of Philosophy of Economics* (Kincaid & Ross 2009). Early philosophy of economics asked very general questions about the field, sometimes giving answers that reflected earlier positivist approaches – for example,

‘What are the necessary and sufficient conditions for being a law and do economic generalisations meet them?’ – or what we have called Kuhnian philosophy of science, looking at such questions as whether neoclassical economics is a progressive or degenerating research programme. For the *Oxford Handbook* we commissioned chapters with a view to advancing the post-Kuhnian naturalist stance by encouraging a philosophy of economics closer to the practice of economic research. The present volume aims to turn this ratchet another cycle tighter. The chapters here are, for the most part, still more closely embedded in methodological and conceptual issues with which working economists engage out of practical necessity and not only intellectual curiosity. One indicator of this is that ten of the fifteen chapters are by authors based in economics departments, with the remaining five being by scholars who mainly identify as philosophers of science. A more effective indicator is that, as per the briefs we provided to the invited contributors, none are general surveys of broad areas of the discipline. All tackle specific problems in an argumentative way that reflect the actual, engaged commitments of authors and that arise from their own ongoing research.

We cite two examples to illustrate what we mean. We asked Nathaniel Wilcox to write about utility theory. This is, of course, a mansion of many rooms, and a topic that has preoccupied philosophers of economics for a century and a half. But Wilcox does not attempt to view this vast space from an altitude of 46,000 feet, or even 10,000. He focuses directly on a challenge that he confronts recurrently in his experimental investigations of choice under risk and uncertainty, how to reconcile the ambitions for testability that motivate the revealed preference conception of utility with the fact that actual people, both in the field and in the lab, exhibit apparent inconsistency that interferes with any straightforward identification of utility optima. To cite another example, Guilhem Lecouteux writes about behavioural welfare economics, and considers three approaches to understanding empirically applicable approaches to welfare assessment that feature in work currently produced in leading behavioural labs. But his objective is not to comprehensively survey the foundations of these approaches. Rather, it is to offer practical advice to economists about which approach to deploy depending on the nature of the choice problems with which laboratory subjects are confronted.

When we commissioned the chapters, we did *not* begin by reviewing topics that have traditionally taken pride of place in classic philosophy of economics anthologies such as Hausman (2008). Such books are extremely valuable for grasping the history of the field, but history is not our focus here. The question around which we selected topics was instead: ‘Which controversies that recurrently arise in contemporary economics seminars and panel debates have important philosophical aspects?’ The topics featured ahead reflect our answers to this question.

The logic of the book's organisation is loosely micro then macro, both with respect to the way in which that distinction is reflected in economic theory, and with respect to the scale of policy applications.

We discussed Nathaniel Wilcox's chapter above. It is followed by Harold Kincaid on causal modelling and inference in economics. Though Kincaid begins by reviewing some recent work in philosophy of science, this is not motivated by a concern with analysis of the *concept* of causation. Rather, Kincaid finds some analytical tools that philosophers of science and kindred spirits in computer science have developed for supporting causal inference about data that do not require randomised experiments or use of instrumental variables in model estimation. These tools have yet to significantly penetrate economic practice, encouraging the widespread view among economists that without experiments or quasi-experiments nothing can be said about causal influences.

Of course, experiments *are* important, and increasingly so, in economics (a development that Kincaid in no way gainsays). The book features two chapters on them. First, Glenn Harrison raises grounds for doubt about the prospect that, outside the laboratory where all that is at stake is subjects' time (for which they and experimenters can agree on an exchange rate for monetary compensation) and how much of the experimenter's money subjects can extract, the welfare consequences of experiments can be assessed by the economist's mere common sense. When economists borrow experimental design principles from other engineering disciplines such as medicine, they should implement the same ethical caution that medical and technology researchers apply to both their investigative methods and the policy implications on which they pronounce. Then Sean Muller focuses more specifically on the form of randomised control trial (RCT) methodology that has lately become dominant in both development microeconomics and micro-policy mechanism design. He pulls few punches in identifying dubious assumptions and overly casual justifications that are used to motivate these methods. Particularly in light of Harrison's conclusions about potential for harm from inadequately examined assumptions, we support Muller's rhetorical frankness as being fit to the stakes.

Critics of mainstream economics have often worried about 'economics imperialism', sometimes stridently so (e.g. Fine & Milonakis 2009). Associated with, though not identical to, this concern is widespread exasperation among many non-economists and heterodox opponents of the mainstream over the extent to which economists tend to regard their practices and technical foundations as being superior to those emphasised in other social science disciplines. Such critics seldom carefully consider evidence for or against the proposition that this alleged arrogance constitutes a problem for the epistemic good health of economics itself. In his chapter, Jack Wright attends to this gap. His general finding is that even if the critics are ultimately right that economists'

wilful 'splendid isolation' undermines the pursuit of economic knowledge, they have yet to furnish the kind of evidence that could establish their case. So whereas Wilcox, Harrison and Muller press hard against methodological complacency among some economists, Wright confounds complacency among their kibitzers.

On the other hand, many economists might be guilty of a different kind of arrogance about which we *do* have evidence for alleging epistemic self-sabotage. The arrogance in question is implicitly assuming the superiority of a cluster of values associated with the cultural and historical construct of masculinity. For example, few economists feel slighted if they are characterised as 'tough-minded', which is usually interpreted as meaning 'not distracted from the truth by sentimentality and empathy'. Julie Nelson considers evidence from important economics literature that this represents self-serving bias in a discipline still demographically dominated by men, bias which, like most bias, obscures some otherwise open paths to knowledge and to best policy advice. Furthermore, by creating a special cost for women who consider entering the profession, it is profligate in wastage of human capital. In keeping with the general approach of the book, Nelson identifies specific areas of economic application in which biases of male perspective have hindered recognition of potentially improved models and policies.

Few topics have generated more heated methodological controversy among economists over the past two decades than the question of how the discipline's boundary with psychology should be drawn and managed. Behavioural economics as we now understand it was born in a call by some economists, particularly Thaler (1992), for resort to psychological data and hypotheses to rescue microeconomic theory from alleged 'anomalies'. And the surge of enthusiasm about potential insights from new non-invasive observational technology in neuroscience at the dawn of the current century captured the imaginations of some prominent economists (Camerer, Loewenstein & Prelec 2005), though confidence in the prospects for an overtly reductive neuro-economics seem more recently to have faded. Gul and Pesendorfer (2008) were motivated by these developments to produce something contemporary economists generally disdain, a sweeping methodological jeremiad urging the autonomy of economics from both psychology and neuroscience. They pushed this asserted separateness to an extent that almost no commentators have found plausible, but which usefully marks out the features of a purist's corner. And then the explosion of interest in 'nudging' (Sunstein & Thaler 2003; Thaler & Sunstein 2008), fuelled by the welcome that its rhetoric and scale of policy attention almost instantly drew from governments around the world, has brought contestation over the role of psychology in economics into countless highly salient policy debates. Andreas Ortmann's chapter in our book turns up the temperature in this already hot room. Gul and Pesendorfer's argument for

keeping psychology firmly apart from economics is purely defensive; they do not contend that the psychology of value uses unsound methods even on its home turf. By contrast, Ortmann goes over to the offensive. The crisis of replication in psychology, he argues, makes it a potentially poisonous influence on economic methodology and theory, even in the applied policy domains where almost everyone genuflects to the importance of interdisciplinarity. As in Muller's chapter, punches are not pulled. We expect that Ortmann's chapter will draw plenty of critical attention from philosophers of science, who are generally better versed in assessment of the epistemologies of whole disciplines than other specialists. Consider just a couple of the under-explored philosophical questions that Ortmann's mordent review invites. Are economists superior to psychologists in their ability to extract knowledge from statistics? Kincaid's chapter casts doubt on this in one sense; yet the techniques for causal inference that he describes seem as absent from experimental psychology of value as they are from economics. And Ortmann characterises the model of self-interest optimisation as a principle to which economists should be methodologically committed for Popperian reasons, but to which they are not empirically committed. That is not a new idea in economic methodology, but it has not been explicitly defended for some years. Responses by economists to Ortmann's resuscitation of it here may indicate whether it has been regarded by them as defeated, or merely as having been pushed underground.

If the most closely inspected (and fought over) borderland of economics is with psychology, its boundary with sociology, where in the days of Max Weber the *methodenstreit* raged, has lately been more placid. However, as Michiru Nagatsu's chapter shows, there has been a lot of traffic across it. According to Nagatsu, it is the sort of traffic of which economists approve: not armies seeking conquest, but trade that shifts both intensive and extensive efficiency frontiers outward. Of all the chapters in the book, Nagatsu's comes closest to being a balanced survey. In keeping with our ambition to reflect the most contemporary currents in meta-economics, this topic seems the one where such an approach is most in order: we are confident that the volume of high-profile work in social economics is in the early stages of an explosion that will run for at least a couple of decades. And Nagatsu's emphasis in his conclusion is as practically focused as most of the rest of the volume. He sees two complementary future paths ahead for social economics: a shift from theoretical highlands to applied empirical work, and the fostering of what he calls 'the ethics of institutional design'. In the world in which we write, characterised by catastrophic failures of institutional confidence and responsibility in one country and industry after another, we warmly hope that Nagatsu's expectation here is right.

We briefly discussed Guilhem Lecouteux's chapter on behavioural welfare economics above. The problem of reconciling normative economics with

descriptively accurate models of human behaviour is recognised as a wickedly hard problem by almost everyone, regardless of the extent to which they think that humans approximate some standard of ‘rationality’. Part of the problem is that there is nothing like a consensus on what ‘rationality’ requires, with the long-running debate between followers of Kahneman and Gigerenzer, respectively, being only the most salient battle in a conflict of multiple factions. Binmore (2009), Bernheim (2016) and Sugden (2018) offer other recent, sharply divergent, approaches. In a reversal of previous roles, confidence that welfare can be assessed against a general and explicit, as opposed to a loose and pragmatic, standard of rationality is more likely to be found among some philosophers than among economists. Buchak (2013) provides an outstanding example. Lecouteux’s chapter wades into this ferment with a confidently presented and original proposal for separating the combatants and understanding each of the leading approaches as complementary. He does this in part by following an exemplary methodological principle: consult Savage (1954).

Another area of economics that is a festival of cross-cutting controversies is development theory and practice. As discussed earlier in connection with Muller’s chapter, the currently most highly charged rail is the value and role of RCTs in guiding policy. But the attention recently lavished on this debate can obscure the fact that the policies that exert the most causal influence on development outcomes – i.e. on the lives of people in poor countries – are motivated from macro-scale perspectives that involve no input from RCTs. Since there is no such thing as global macroeconomic policy, the appropriate scale of focus is national. For the book we therefore commissioned a case study on methodological undercurrents in development policy in the country we know best, South Africa (SA). This choice was not simply a matter of our parochial convenience. SA’s transition from exploitative minority rule to mass democracy 30 years ago provides an unusually clear ‘year zero’ for assessment, which coincided with a historical moment of self-awareness about the challenges of achieving coherence in development policy against the constraints of politics. An impressively large system of welfare transfers was built up almost from scratch. Efforts to improve growth and export competitiveness enjoyed some early success, but faltered badly after 2008. We asked Julian May, a South African economist who has been closely involved in policy formation, to document the main debates and assess their implications for more general questions about how development policies should be promoted and assessed in light of assumptions about what economists can and can’t know. Arguably, the kind of economics it is most important to do well for good development policy is sophisticated design and analysis of surveys rather than abstract theory.

Most anthologies in the philosophy of economics feature heavy emphasis on themes and issues from microeconomics and give relatively little attention to macroeconomics. But it is the latter that constitutes the primary shop front of

the discipline as far as the general public is concerned. Our book includes three chapters, by Aki Lehtinen, Alex Rosenberg and Edward Leamer and Sumit Shinde, on problems in the core modelling methodology of macroeconomics. All are highly critical of standard practice, but at progressively less abstract levels of analysis. Lehtinen first explores, from the deliberately and usefully naïve perspective of a philosopher of science, why a discipline that studies what are obviously highly complex phenomena should institutionally enforce strong expectations for a single core model. His inquiry does not dissolve his scepticism about the plausibility of this ambition, but he is alert and sensitive to the practical considerations, given the demands exerted on macroeconomists by monetary policy managers, that drive macroeconomists away from methodological pluralism. Rosenberg then narrows the critical focus more sharply to assess the epistemological standing of the core model in question, Dynamic Stochastic General Equilibrium (DSGE). The main virtue claimed for DSGE over its historical rivals is its alleged adherence to coherent microfoundations. However, as Rosenberg documents, what ‘microfoundations’ means in the DSGE context is strikingly different from the conceptions found in other sciences that study complex systems, such as the biological disciplines. The primary target of his chapter is diagnosis of this difference. His conclusion is that macroeconomics is not mainly in the business of *explaining* phenomena, but of *regulating* them to serve normative goals anchored in a certain conception of efficiency. This accounts, he argues, for the fact that microfoundations are not actual, discovered mechanisms that constrain and refine theory, but normative constructs driven by the policy goals that macroeconomists impute to monetary and fiscal policy-makers. He refrains from pronouncing on the extent to which this aim is successfully achieved, except to note that scepticism is widespread, including among experts.

Leamer and Shinde are such experts. Their chapter extends and amplifies famous critical work Leamer has contributed on the soundness and coherence of econometric estimation (Leamer 1978; 1983), here as applied to identification problems with macroeconomic models in general (i.e. not only DSGE models). This is integrated with his previously articulated conception of best economic methodology as being application of an experienced craftsperson’s judgment, about both the value and limitations of formal modelling, to artfully selected and transparently represented empirically observed patterns (Leamer 2009; 2012). Most abstract macroeconomic modelling, Leamer and Shinde argue, is economically empty mathematics. There are some recurrent patterns in investment, spending, and savings behaviour, at least at the scale of an individual large country such as the USA, but to isolate them requires a trained and cautious eye, because important differences between observed episodes generate many singularities. And even when patterns are successfully detected, this falls short of identifying the causal mechanisms that support them.

A question readers may want to think about is the extent to which Leamer and Shinde's scepticism about economists' capacities for sound causal inference from modelled observational (i.e. not experimental) data might be leavened if economists take up the newly available tools discussed in Kincaid's chapter. Kincaid shares Leamer and Shinde's modest view about how much we can reasonably ask of econometrics where causal inference is concerned. But perhaps there are other, complementary devices that can extend our explanatory and forecasting power.

Philosophers of economics resemble other kinds of philosophers in relying very heavily on bodies of explicit scientific theory to understand the epistemologies and working ontologies of the discipline they study. Decades ago in the general philosophy of science and in philosophy of physics there was a prominent wave of criticism (Cartwright 1983; Hacking 1983; Ihde 1991) to the effect that philosophers had exaggerated both the importance of theory in driving experiments, and the extent to which practising scientists take theory literally as opposed to using it mainly as a heuristic and pedagogical tool. Given the proportion of economists who at least rhetorically endorse Milton Friedman's methodology, according to which economic theory does not even aim to state empirically true or explanatory propositions, there is some irony in the fact that a parallel correction to what happened in philosophy of physics has seeped into philosophy of economics only recently and gradually. But seeped it has. Leamer (2012) and Rubinstein (2012) engagingly urge such a perspective, and it is the dominant recent theme in the extensive methodological corpus of David Colander (Colander & Su 2018; Colander & Freedman 2019). It must be emphasised that none of these authors argue that theory is *dispensable*. There is no contradiction in arguing, as Ross (2014) does, both that economic theory by itself gives a distorted view of the empirical and ontological beliefs of economists, *and* that much behavioural economics, along with the work of the 'randomistas' in development economics, undermines the methodological strength of the discipline as a whole by trying to get by with far too little economic theory in designing experiments and interpreting their results. Ross (2017) argues that the perennial debate over whether economists are entitled to be regarded as 'scientists' is another instance of a traditional philosophy of economics preoccupation that is largely a distraction from useful inquiry. Physics, the paradigmatic 'science', is an evolving, dense weave of essential theory and what Leamer calls 'craft', the tacit knowledge of how to design experiments, how to select data for modelling and how to build models that successfully forecast out of sample. So science *includes* craft. Thus arguing for the importance of craft in economics should not be regarded as undermining the status of economics as a science.

The nature of the 'dense weave' to which we just referred is best illustrated, as Leamer, Rubinstein and Colander all urge, by illustrative case studies

that show the interanimation of theory and situational judgment at a fine level of resolution. The final chapter in the book, by Don Ross and Matthew Townshend, is intended as an example of this. In reviewing the methodology they apply in their work on the economics of road project prioritisation in South Africa, Ross and Townshend also exemplify another theme stressed by Colander and his co-authors, as well as by Sugden (2018): policy advice that is intended for actual influence on specific agents – as opposed to the policy advice to a nonexistent benign dictator that is a standard part of the rhetorical template for most journal articles – cannot be literally derived from theory, but relies on situation-specific empirical knowledge *and* normative awareness. The latter term refers partly to sensitivity to local norms that constrain real policy contexts, but can, in addition, as in Ross and Townshend's case study, draw on resources from professional moral philosophy.

We summarise by saying that we will regard our project in editing this volume as having been successful to the extent that readers take it to demonstrate that philosophical and methodological reflection, undertaken with an attitude of modesty about its own scope and basis, can not only help economists achieve improved professional self-awareness, but is an element of best practice in real economic modelling, research, and policy development.

NOTE

1. The only challenges to this view that we acknowledge to be difficult concern mathematics. But naturalism about mathematics does find strong and rigorous defences (e.g. Kitcher 1983; Maddy 1997).

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