Economic Methodology in 2020: Looking Forward, Looking Back

Don Ross

School of Society, Politics, and Ethics, University College Cork; School of Economics, University of Cape Town; Center for Economic Analysis of Risk, Robinson College of Business, Georgia State University

don.ross931@gmail.com

ORCID: 0000-0003-1813-3111

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Abstract: I appraise some areas of recent achievement in economic methodology by identifying four topics on which there will likely be heavy exogenously generated demand for methodological innovation over coming years, and asking what foundations have been set for this work. The topics in question are economists' role in policy formation, macroeconomic management, causal and structural modeling of economic processes, and welfare non-standard and dynamic utility.

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One way to assess the collective accomplishments of economic methodologists over the past two decades is to ask how well they have prepared us for emerging preoccupations. An advantage of that approach is that it can be used to downweight the valuation of debates that have consumed attention in methodological literature but have degenerated into in-house conversations. At least, the approach will have that effect if one believes, as I do, that inward-focused problems tend to end in cul-de-sacs, whereas methodological work that truly advances knowledge is typically generated mainly by exogenous demand. Methodologists famously complain that other economists don't pay much attention to them. Whatever set of possible mechanisms might better draw external audiences to methodological work, it seems clear that emphasis on topics that methodologists didn't largely construct among themselves should be among them.

I will identify four broad work areas that I expect economists to be increasingly engaged with over the coming twenty years, and where economists' existing analytical tools are likely to require substantial innovation and revision. To illustrate the point by a negative example, even though I expect to see major continuing in interest in dynamic pricing modeling by microeconomists, as digitally mediated consumer markets finally swamp transactions that require moving human bodies around, I see no reason to think that existing economic methods of analysis aren't adequate to the task. Thus the demand for novel and critical methodology is likely to be limited in this area. By contrast, the four topics on which I will focus involve current methodological limitations that will need to be (and therefore will be, at least to some extent) overcome.

For each topic area I will (i) indicate why the area in question is or will be hot; (ii) explain why economists currently lack a canonical set of tools for engaging with it; and (iii) assess the extent to which recent advances have equipped methodologists to propose useful innovations for application to it.

(1) Economists' role in democratic policy formation

An over-arching context that ties my four topics together is the evident collapse of the late twentieth-century strategy and institutional structures for regulating economies. This basically involved coordinating national-scale policy control hubs by common reference to aligned international measures and benchmarks. The latter have been negotiated under close guidance from economists, financial modelers, and corporate lawyers. This allowed contestable value judgments – that human well-being is well proxied by welfare in the economist's special sense, that convergence of poorer economies to wealthier ones should be achieved through growth-financed debt, and that global corporate financial institutions should be allowed to limit policy space – to be packaged as technical ones (Kennedy 2018). This approach has fallen into systemic crisis because the hyper-democratic rhetoric in which technical management of deep social challenges came to be packaged involved a level of bullshit, in Frankfurt's (1986) sense, that eroded public confidence in expert authority to an extent that could not survive the genuine democratization of public communications brought about by the internet. Perhaps the most important element of the bullshit was the implication that if *potential* compensation for losers from policy choices was available, the implied transfers could in fact be effected without major political reforms that technicians lack leverage to bring about (Boix 2019, Blinder 2018). Politicians as a class do not constitute a dynamically coherent locus of agency; technicians do. Those with a coherent identity, ironically, are easier to pin down for attribution of responsibility, fairly or not. Consequently, new political entrepreneurs are always available to exploit resentment of technicians' influence, and to consequently mask political failures as failures of experts.

Various aspects of standard economic methodology have been consistently favored precisely because they allowed economists to avoid contentious ethical commitments and to set up soluble technical problems within ethical frameworks – such as welfarism, convergence, and Kaldor-Hicks efficiency – that are not deep but are relatively easy to defend. It is simplistic – basically false – to claim that prevailing social crises are attributable to poorly chosen methods of economic analysis. The relevant point here is rather that economists have until recently been able to methodologically factor out some influences on economic dynamics that they now need to consider more rigorously in order to address policy problems from within a less sheltered niche in the grand institutional ecosystem. Drazen (2002) made this point two decades ago, and offered careful methodological proposals in light of it.

An example of improved application of standard economic methods to the problem of economic policy guidance is provided by Dixit (1996). He combines a Coasean transaction-costs analysis with non-cooperative game theory to effectively represent politically mediated policy choice as an efficiency problem that economists can (in principle) solve. This represents a considerable advance on the older public choice approach that reduces governance to competition over rents and thereby falsely implies that governments are intrinsically sources of impediments to efficient market operations. Even if we set aside the tendency of the traditional approach to buttress crude ideological pre-commitments to 'free' markets, that standpoint implicitly changes the subject from 'how should economists try to influence policy?' to 'which kinds of constitutions should economists prefer?'. Dixit instead shows us how to model the *political* achievement of improved *political* efficiency.

A problem with Dixit's analysis, as he recognizes, is that it is not clear which agents are in positions to act on his advice, or how. He provides economists with additional considerations they should take into account when restricting policy alternatives to feasible sets. This is useful for both practicing policy consultants and economists who aim to assess the relative efficiencies of actual institutional decision processes. More recently, Sugden (2018) and Colander and Su (2018) have forcefully reminded us that it is not particularly helpful for economists to identify the solutions to policy problems that they would promote if they were advising dictators who cherish economic efficiency just as economists do, since no such agents exist. The tradition of concluding economic articles with 'policy recommendations' that are addressed to no feasible agents and abstract away from any specific political context has, Sugden, Colander, and Su argue, encouraged an illusory sense of practical relevance into the culture of the discipline.

We have here, as of 2020, a problem that is fundamentally methodological – how can we address economic policy challenges using analyses that are fully politically alert but don't simply collapse into endorsing whatever the most powerful interests would compromise with one another on? – but institutionally pressing. We will not get good economic policy, except by sheer accident, if rigorous economic analysis doesn't identify it. The methodological research community has done enough work to frame this as a problem about *where* economists should insert themselves into policy formation and which elements of theory can be put to practical use in these varying consultancy settings (Duflo 2017; Levy 2014; Ross and Townshend 2021). But we are very far from being able to see general solutions, or even whether there are any such things to be had.

(2) Macroeconomic management

It has become routine rhetoric in many quarters that macroeconomics, with respect to both theory and policy design, is in a state of crisis. This is often attributed to macroeconomists' alleged failure to predict the financial crisis that began in late 2008 (Palley 2012). However, there is high variance of opinion concerning *which* features of standard macroeconomic theory might be at fault. For years before the crisis there had already been widespread criticism (e.g. Hoover 1988) of what Hall, as attributed by Gordon (1989), called 'freshwater' macroeconomics that emphasizes maximum-strength versions of rational expectations and assumes away heterogeneity of agents by modeling an economy as optimizing the utility function of a single representative agent. Mankiw (2006) pronounced the freshwater / saltwater dichotomy to have already been synthesized away, Hegel-style, before the financial crisis occurred. However, Krugman (2009) prominently blamed the crisis, to at least a considerable extent, on 'freshwater' theorists. More recently Romer (2016) posted a scathing but clearly argued criticism of the general class of dynamic stochastic general equilibrium (DSGE) models that combine the freshwater New Classical paradigm with a 'New Keynesian' concession that nominal wages resist adjustment to new equlibria. As DSGE models provide the standard design template for the forecasting models used by central banks and other important policy agents, the wide, but certainly not unanimous, acceptance of Romer's criticisms imply that major methodological reform is in order in macroeconomics.

One of the features of DSGE models that is widely cited as explaining their blindness to the looming financial crisis before 2008 is that they focus exclusively on 'real' consumption and production and ignore fluctuations in the supply and cost of finance.[1] This criticism seems to have formed the basis for a revised consensus, since new DSGE work now nearly always includes a mechanism to 'bolt on' effects of financial fluctuations.

That this has become conventional is itself methodologically interesting. A pragmatic defense of DSGE models without financial effects before 2006 was that, with the widespread abandonment by governments of fiscal policy as an instrument for short-run stabilization after the 1970s, the domain of macroeconomics had shrunk to relationships amongst variables over which monetary policy authorities – so, mainly central banks – then had leverage. The mission of central banks was conceived as – and sometimes legislated to be (du Plessis 2009) – keeping price inflation within boundaries that were in turn favored on the basis of microfoundations models. What central bankers needed macroeconomic models to predict to do this job were rates of medium-term growth and ratios of aggregate investment to aggregate savings and aggregate consumption. DSGE models are built to do this. Romer argues that they fail because the most important causal factors, particularly technological innovations, are under-theorized as exogenous shocks. But regardless of whether someone agrees with Romer about this, the deeper methodological lesson concerns the extent to which the pre-crisis defense conceded

[1] The absence of transmission channels from financial markets to the real economy in high-church macroeconomics by no means amounted to their absence from economic theory generally. Holmström and Tirole (1997) modeled the effects that were dramatically observed in 2008 and 2009 years in advance, and their model was generally known to economists. Nor was this buried away from the sight of non-specialists. Several leaders in *The Economist* between 2005 and 2008, warning of trouble ahead, reflected the model.

that macroeconomics is a policy science (Leamer 2012), a basis for engineering. In response to the crisis, first the US Federal Reserve and then other central banks with power over large enough currencies, invented quantitative easing as a mechanism for fiscal intervention. That might well be the explanation for the ease with which DSGE modelers accepted financial variables into their domain: the job description of central banks has expanded to include them as instruments.

In other respects orthodox approaches remain resilient. There are widespread proposed programmes for radically different policies (e.g. Jacobs and Mazzucato 2016), but these tend to argue purely consequentially (and, often, ethically), rather than seek to *discover* new policy prospects through fundamental methodological innovation. There have been some exceptions. Frydman and Goldberg (2007) develop foundations for formal modeling of economies that, they argue, trade off implausibly exact predictions for greater power in identifying qualitative trends. A crucial innovation in this work is abandonment of even moderate versions of rational expectations. Given the explicitness of the modeling template, these ideas merit more attention from methodologists than they have received. By contrast, Colander and Kupers (2014) are skeptical about the idea of a new monolithic analytical engine on the scale of 'grand' DSGE (as opposed to locally anchored models inspired by the checklist of relationships expressed in standard DSGE equations). This skepticism is based on the widespread view that a modern economy is a complex dynamical system that has neither a natural nor an engineerable equilibrium. Leamer (2009) argues that macroeconomic insights can nevertheless be achieved through causal stories disciplined by graphical expression of mechanisms and identification of quantitative tests. We should take seriously the prospect that the best methodology for macroeconomics is nothing more definite than pragmatic empiricism combined with rigorously critical statistics. The proposal is by no means to abjure theory. But theory would be about causal relationships, specified by graphs, and principles for their identification in data sets. This is roughly how biological ecology is done, without ecologists regarding their epistemic situation as futile or 'unscientific'.

(3) Structural and causal modeling

I suggested that major challenges in both public economics and macroeconomics direct attention to improved methods for identifying causal influences (which will sometimes, but not always, be mechanisms in the sense of Machamer, Darden, and Craver [2000]). DSGE models follow a venerable tradition in economics of avoiding reference to causal transmission by including instantaneous general or partial equilibrium transformations. At least as prominent in the history of economics is restriction to proxies for causation as modeled in time-series econometrics. However, contemporary methodologists have been strongly influenced by forceful cases pressed by Cartwright (1989, 2007) and other philosophers of science to the effect that Granger causation is not a statistically tractable 'form' of causation; it is, rather, an unfortunately named form of correlation.[2]

Perhaps the single most significant area of advance in economic methodology over the past twenty years has been the development of analytical technology for modeling causal relationships. This work has not been contained within economic methodology, but its leading contributors, Pearl (2009) and Spirtes, Glymour and Scheines (20000), have consistently focused on economic applications. This is partly because they recognize that identifying economic causation is of high practical importance but fiendishly complicated, and partly because a natural method for the work and its critical assessment consists in examining ways in which causal modeling deepens, and often reveals limitations in, structural econometric model estimation. Kincaid (2021) provides detailed and illuminating diagnosis and examples of complementarities between Pearl's Directed Acyclic Graphs (DAGs) and associated structural equation modeling (SEM), on the one hand, and structural econometrics, on the other hand, for cases of hypothesized sufficient causes of measurable effects. This is a satisfying start, but as Kincaid recognizes, economists are often interested in *enabling* conditions that are necessary but not sufficient causes of outcomes, and in factors that *constrain* causal influences beyond thresholds. Extension of analysis based on DAGs and SEMs to these varieties of causal influence merits strong attention from economic methodologists in the coming years.

This effort will be assisted by recent advances in the methodology of structural econometric estimation of choice data. The ability of modelers to control for selection effects, applying the string of innovations launched with Heckman (1976, 1979) has produced steadily increasing yield of more reliable population-scale measurement over the past few decades. Looking forward, methodologists can and should aim to bring structural sampling analysis properly to bear on work by conductors of randomized control trials (RCTs), as part of a general effort to overcome the misguided hyper-empiricism and aversion to strong theory in much of the RCT literature (Leamer 2010, Keane 2010, Harrison 2011).[3] Another recent contribution to structural estimation methodology that merits deeper integration into the larger methodological literature is use of maximum-likelihood mixture models to frame different behavioral theories as models of distinct data-generating processes that might co-exist in a data set, including in data derived from a single experiment (Werner 1999; Wang and Fischbeck 2004; Harrison and Rutström 2009). This procedure makes unequivocal – by building it into a statistical methodology – the important point that economists typically do not interpret theories as conjectured empirical generalisations, but rather as filters for

[2] See Hicks (1979) for an ideal example of the Humean understanding of causation that is common among economists, untroubled by any hint of awareness of critical alternatives.

[3] For a more general criticism of overzealous empiricism in economics, Wolpin (2013) is a major recent methodological contribution.

constraining identification and estimation of models of observations. In my view, this drives the nail into the coffin of Popperian philosophies that dominated an earlier generation of economic methodology. This is progress. Increased use of full-information maximum likelihood modeling of experimental data can help economists to resist the kind of non-accumulating hypothesis testing that has generated the crisis of reproducibility in psychology, and in parts of behavioral economics beset by over-credulity about the methodological good health of that discipline.

(4) Welfare economics with non-standard and dynamic utility

The limits of classic welfare analysis were clearly articulated many decades ago (Graaff 1957). However, welfare economics has never died, because population-scale normative assessments are unavoidable in public policy assessment, and obviously should be at least informed by, though not exclusively driven by, economic theory and measurement (Fleurbaey and Mongin 2005). Welfarism, the thesis that optimized preference satisfaction is equivalent to well-being, has become a nearly extinct doctrine, for reasons surveyed in Tiberius and Plakias (2010). But this philosophical opinion is compatible with the idea that optimizing aggregate preference satisfaction is the most appropriate ideal function for makers of public policy who serve liberal states that do not presume to impose any particular conception of good living. Arguably, mainstream normative economics remains as committed to liberalism in this sense as it has always been, though Sugden (2018) argues persuasively that advocates of 'nudges' (Sunstein and Thaler 2003) in fact undermine this commitment in ways that their rhetoric disavows.

The nudge agenda has been driven by claims from behavioral economists that their experiments have revealed that most people's choices are systematically inconsistent with rational maximization of subjective expected utility. The interpretation of experimental evidence as revealing outright general irrationality, to an extent that undermines inferences of welfare from observed choice, is highly contested and clearly not conceded by most economists. On the other hand, it has ceased to be effectively controversial that revealed-preference analysis must allow for error and stochasticity that are generated by more complex processes than mere trembling hands. A crucial source of this is that the bulk of information about human choices comes from situations involving risk or uncertainty, and there is no rationally best general attitude to risk. Buchak (2013) and Harrison and Ross (2018) agree, on the basis of different but complementary arguments, that it is not a viable criterion of practical rationality that people choose in accordance with expected utility theory (EUT). Mixture models of controlled, incentivized risky choice data show that people more commonly choose in conformity to rank-dependent utility theory (Harrison and Ross 2016) (though much EUT-consistent choice is also observed), and this pattern can at least often, perhaps even typically, be rationalized. Lecouteux (2021) critically reviews this literature and constructs a synthesis between it and the non-preference-based approach of Sugden (2018). According to Lecouteux's argument, which of the different welfare assessment methods is appropriate to a scenario is partly conditional on whether the choice contexts are large or small worlds as per Savage (1954). These ideas set the stage for continuing debate about models of welfare that promise to be fundamentally different from those of earlier decades. Along with admitting more structurally complex and heterogenous utility functions, new welfare economics will formally acknowledge *dynamical* utility functions, in which preference change strategically spreads through networks (Kuran 1995; Stirling 2016), and neither descriptive nor normative aggregation take atomistic individualism for granted (Davis 2010; Ross 2014).

Conclusion

If we agree with Leamer (2012) the economics is fundamentally a policy science, then it is hardly surprising that welfare theory is an enduring central topic of both first-order work and methodology. I forecast, however, an upsurge in the urgency and extent of it as the reality and perception of global crises, particularly institutional failure caused by epistemic fracturing, and by climate change and biodiversity collapse that will entail mass population migrations, deepen. Modern welfare states achieved relative stability that partly depended on settled institutions for macroeonomic management of cycles, and systems of rules for governing markets that reflected microeconomists' understanding of incentive compatibility.[4] This naturally fostered stability in economists' institutional roles and their habitual methods, which in turn explains why methodologists retreated into preoccupation with highly abstract problems such as the analysis of ideal practical rationality and were viewed as irrelevant by most economists. The great challenges of the 21st century almost certainly make the familiar welfare state, as self-sustaining within secure national borders, unviable. Substantial unconditional basic income (UBI) guarantees will likely be needed to protect people from asymmetry in the rates of change in productive technology and accessible, effective education. If UBI is politically blocked from evolving, as well it might be, the capitalist epoch might give way to a kind of technologically advanced neo-feudalism. In either case revolutionary changes in economic methods, generated by revolutionary changes in what economists will be called on to understand, are probable.

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[4] I contend that, in general, microeconomists' influence has been much less visible than macroeconomists', and consequentially more pervasive and more effective.

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