Journal of Economic Methodology

ISSN: (Print) (Online) Journal homepage: https://www.tandfonline.com/loi/rjec20

Economic methodology for policy guidance

How economics should be done, by David Colander and Huei-chun Su, Edward Elgar, 2018, $40.00, pp 270+ xxvi, Cheltenham UK, ISBN 978-178897992-4Where economics went wrong, by David Colander and Craig Freedman, Princeton University Press, 2019, $27.95, pp 267 + xii, Princeton NJ, ISBN 978-0-691-17920-9

Don Ross

To cite this article: Don Ross (2021): Economic methodology for policy guidance, Journal of Economic Methodology, DOI: 10.1080/1350178X.2021.1893979

To link to this article: https://doi.org/10.1080/1350178X.2021.1893979

Published online: 03 Mar 2021.

JOURNAL OF ECONOMIC METHODOLOGY

Economic methodology for policy guidance

How economics should be done, by David Colander and Huei-chun Su, Edward Elgar, 2018, $40.00, pp 270+ xxvi, Cheltenham UK, ISBN 978-178897992-4

Where economics went wrong, by David Colander and Craig Freedman, Princeton University Press, 2019, $27.95, pp 267 + xii, Princeton NJ, ISBN 978-0-691-17920-9

Before reading two recent books of which David Colander is first author, I had thought of him as a unique gadfly who has been the best promoter of three loosely connected strands of work. He has done insider-informed sociology of economic ideas that is happily unencumbered by ponderous sociological theory. He has empirically studied, and kept the profession well briefed on, patterns in the professional and intellectual formation and attitudes of apprentice economists. And he has promoted the application of complexity theory in macroeconomics. Now, thanks to these new books, I appreciate that these three activities are unified by a distinctive methodological perspective, and that, importantly, my description of the third strand above isn't quite accurate. Henceforth I'll say: Colander is the best example of an economic methodologist who seriously aims to help economists increase the effectiveness of their influence on society and policy. What I mean by 'best' here is that he is the one most likely to actually enjoy some success in this ambition. This is for two main reasons. First, he is sympathetic to the aims that most economists actually have, instead of wishing they would try to do something else. Second, he does not ask economists to think with imported concepts they would have to struggle to integrate into their standard thinking kit.

Though unified in their themes and implications, the two books are very different from one another, and, for reasons I will explain, in their sophistication and depth. The Colander and Su volume is a collection of Colander's papers, some co-authored, published in various journals and collections between 1987 and 2016. Su curated them into topic clusters and provides an introduction that ties them together. None of the older essays have become out-dated, which partly reflects how little other methodological literature has addressed Colander's issues of concern. The Colander and Freedman book is a sketch of one strand of the history of what they regard as a major mis-step in the evolution of economic method, the same mis-step that is the recurrent focus of attention in the essays in Colander and Su. As I will explain later, the historical strand in question presents the reader with a distractingly oblique view of the main problem. I will argue that it might lead readers who do not acquaint themselves with the much broader vision presented in the Elgar volume to underestimate the significance and scope of Colander's critique. There are likely to be many such readers, because the Princeton volume is shorter, written in simple, non-technical language, and distributed by a publisher with wider reach than Elgar. I likely would have had a somewhat negative view of it had I not read the Colander and Su collection first.

I will focus first on the deeper book. Colander's core methodological thesis is stated clearly in Su's introduction, and re-stated in several of the papers in the collection. This reiteration is useful, serving to make clear what is foundational and what is derived in Colander's portfolio of opinions. So I will summarise the foundation.

According to Colander, economic science seeks to discover universal, or at least highly persistent and pervasive, relationships among economic states and processes. It does this by means of mathematical modelling. For optimal progress, the more sophisticated the math the better, and so theoretical economists should be students of new developments in mathematics – and also, for the sake of econometrics, in statistics. But because of its universalistic ambitions and mathematical method, economic science is necessarily highly abstract. This equips it poorly as a tool for generating policy advice, because policies are always implemented under special local circumstances in which major causal effects are generated by (i) infrequent contingencies and correlations amongst economic variables themselves, (ii) exogenous political, social, cultural, geographic, demographic, technological, and institutional peculiarities that resist specification and identification of tight priors, and (iii) strategic feedback from agents with imperfect knowledge, heterogeneous utility functions, and limited information-processing resources, especially time. Policy is typically centrally concerned with active short-run coordination, in time and space, of choices that do not need such deliberate management in the long-run modelled by abstract theory. To give an example of my own: quantitative easing (QE) by central banks after 2008 should have no effect on asset values in the long run because sophisticated traders who can use derivatives to spread risk across the time horizon are at least as capable of forecasting bond yields as the monetary authorities, and have the informational advantage of knowing their own coefficients of intertemporal risk aversion. But QE, first in the US and a few years later in the EU, probably twice prevented a world depression because most actual businesses cannot survive temporary demand crashes or credit stops on the basis of expected future liquidity.

Colander's heroes in the history of economic methodology, who are called 'Classical' in Colander and Su and 'Classical Liberal' in Colander and Freedman (about which more later) knew that economic science is not designed to directly yield policy advice. They resisted the 'vice' of the errant outlier in their own ranks, Ricardo. A bit awkwardly for Colander's history, two of the heroes, J.S. Mill and Nassau Senior, failed to follow their own advice when they took real-time positions on responses to the Irish famine. It is true, however, that Mill recognised, at least when he was philosophising instead of trying to improve the work ethics of the Irish, the limited practical power of abstract theory. And Colander's most regularly quoted wise man, John Neville Keynes, was explicit that economic policy formation is an 'art', meaning that its success depends on discerning ethical and political judgment and accumulated weight of experience, rather than a 'science' in the sense of boiling down to mathematical generalisations that can be relied upon regardless of circumstances. Keynes's son, the most influential producer of policies among economists after Smith and Marx, was equally trenchant and more rhetorically inventive in stressing the same point. In this clarity, Keynes the younger also benefited from the influence of his teacher Marshall, who favoured modelling 'one thing at a time' to general equilibrium reasoning because, to actual business strategists, the former is a sensible choice input and the latter isn't. Colander's final frequently quoted advocate of a firewall between economic science and the art of economic policy guidance is Lionel Robbins.

What disrupted this sound picture, according to Colander, was the rise of technical welfare economics and the emphasis on optimal control specification. This body of theory directly drew foundations for normative economics from general equilibrium theory. Colander emphasises that the hubris incorporated in and encouraged by this ambition was smelled out quite quickly by economists themselves. He approvingly cites Graaff's (1957) conclusion, at the end of his elegant consolidation of formal welfare theory, that the 'possibility of building a useful and interesting theory of welfare economics … is exceedingly small' (p. 169). Over the six decades since Graaff's book, economists have tolerated a kind of institutional schizophrenia about welfare theory. On the one hand, endorsement of sentiments such as Graaff's have been widespread and frequent, and on occasions the sub-field has been pronounced 'dead' (on grounds critically reviewed by Fleurbaey & Mongin, 2005). On the other hand, the core theory has been continuously retrofitted and refined within the many branches of applied policy economics, such as international trade theory, development, transport, health, and labour economics. Defense of this state of affairs typically appeals to pragmatism: policy choices must be made, and if they are not to be driven by ideologies, then even flawed economic theory is surely a better basis than nothing. As Colander and many others have noted, this somewhat forlorn justification is not consistent with the levels of confidence with which economists typically offer normative advice.

Colander points out that what is meant by 'advice' here itself needs scrutiny. As Sugden (2018) has also recently argued, the 'policy recommendations' sections dutifully constructed near the ends of journal articles are seldom addressed to actual agents who commissioned the advice, or who could possibly implement it. I will give an example from my immediate professional experience in developing economically sound prioritisation of the South African roads budget (Ross & Townshend, 2021). Most work on this topic by other economists has aimed to identify the optimal proportions of national investment and expenditure for roads. I have no evidence that this work has had any influence on policy choice, because in South Africa as in all democracies there is no agent who has discretion to allocate the national budget on the basis of effects on the national economy conceived through general equilibrium representation. Budgets result from political horse trading shaped by the relative power of cabinet departments and interest group lobbies. Actual decision makers, roads authorities at various administrative levels, have occasionally sought and used economists' advice on how to invest the budgets they're given, and there is currently a nascent attempt underway to generalise such local-level consultation by Townshend and me, under the coordinating guidance of the National Roads Agency. Crucially, this effort is premised on the fact that our models are not abstract, but take account of relationships between idiosyncratic local conditions and types of pavement surfaces at a granular scale, i.e. discriminating not just between sealed or gravel, but among 11 different seals. Our task is to prioritise roads for resealing, constrained by a set of policy targets. Colander refers to this sort of role for the economist as 'general contracting'. It does not involve *neglect* of economic theory. If it did, why would an economist be consulted in the first place? But theory is used for building checklists of economic relations to be studied, and for identifying opportunity costs and variables that accountants and engineers aren't trained to estimate, such as local shadow prices of unskilled labour for maintenance of such surfaces as don't require high-tech methods.

Among recent commentators on methodology, this theme is not unique to Colander and his co-authors. Eight decades ago J.M. Keynes (1936) urged economists to refrain from promoting sweeping normative programmes and to adopt the posture of 'humble, competent people, on a level with dentists' (p. 373). Leamer (2012) argues that because economists' collective allocation of effort to problems has its primary source in policy challenges, economics is a 'craft' rather than a 'science'. His main focus of attention is on how the Heckscher-Ohlin (H–O) family of international trade models, which clearly does not literally describe actual trade flows between any countries, can be used in assessing prospective trade agreements for risks of Stolper-Samuelson effects on domestic wages. This depends, Leamer argues, on the economist's crafty judgment, honed from experience, about the H–O model's pragmatic diagnostics, as well as about effect sizes that cannot be econometrically estimated on the basis of a model that isn't 'true'. Finally Duflo (2017) sketches a role for development economists as 'plumbers'. By this she refers to the value of attention to local-scale features that accommodate or resist *installation* of a policy, as opposed to just its intended inputs and desired outputs. This plumbing role corresponds particularly closely to Colander's 'general contracting'. However, Duflo does not follow Colander – or Keynes – in urging humbleness upon economists, because she thinks that while some economists engage in plumbing, *other* economists ('engineers') should be entrusted with formal mechanism design and still others ('scientists') should develop and test relevant generalisations. Duflo's conception of economic science emphasises a specific form of model-free experimentation. As with other promoters of randomised control trials who think that this methodology is recommended because it 'lets the data speak', she attributes less value to structural economic theory than Colander does. Thus full positioning of the 'randomistas' within Colander's critical framework is complicated, and warrants special attention elsewhere.

I turn now to another topic that looms large in the volumes under review, special problems around macroeconomics. Colander and his co-authors share the widespread disenchantment with macroeconomics that accords dynamic stochastic general equilibrium (DSGE) the status of core model. The strongest arguments for dethroning it echo Romer's (2016) critique, which is cited several times in the more recent of the papers in Colander and Su. The main sources of rot in

DSGE according to Romer are that it stuffs what matters most to macroeconomic *causality* into the black box of 'exogenous shocks' (see Leamer and Shinde [2021] for a more extended discussion of this general problem), and relies for apparent empirical sensitivity on ultimately spurious 'calibration' procedures. Citations to Romer notwithstanding, however, Colander mainly quarrels with DSGE-based macro as a specific example of his more general target, overly abstract modelling used to directly derive policy selection. DSGE is particularly vulnerable to this attack because, unlike avowedly foundational theories such as Arrow-Debreu GE, the intended point of DSGE is to isolate effects of variables that can be manipulated by monetary policy authorities. The strongest available *defenses* of DSGE concede this point. The basic DSGE model is invariably customised to reflect country-specific factors and histories by central banks that use it for forecasting and guidance, notwithstanding the obvious consequent risk of over-fitting. By similar logic, DSGE modellers have responded to the failure of most macroeconomists to anticipate the 2008 financial crisis by bolting effects of financial fluctuations onto the core model that assigns no role to money. This response can be defended against charges of being ad hoc precisely by emphasising the status of macroeconomics as a form of policy engineering rather than disinterested science. Arguably, prior to the Great Recession central banks saw their objective as being largely restricted to control of inflation. Within this restriction, what macroeconomic models were asked to forecast were rates of medium-term growth and ratios of aggregate investment to aggregate savings and aggregate consumption. A case can be made, based on trends measured before the COVID-19 singularity, that they were doing this successfully even in 2007. (Of course, these variables moved far from their trend lines in the short run, prior to recovery after 2011.) Then, the defense continues, central banks extended their mandates into fiscal policy by engaging in quantitative easing; so the scope of DSGE-based modelling was likewise extended to incorporate traditional fiscal policy variables. Thus modelling choices followed derived demand for policy advice. Whether someone finds this response persuasive or not, its logic positively embraces the kind of direct theory to policy relationship that Colander attacks.

Colander and co-authors don't engage with these sorts of details on how DSGE modelling is institutionally implemented. They implicitly assume that the policy goal of all macroeconomics must be reducing business cycle amplitude. Of course this assumption is fully warranted where the origins of macroeconomics are concerned, and Colander's astute paper on the nature of economists' contribution to pre-2008 policy and forecasting failure, which is among the best chapters in Colander and Su, takes a long historical approach. However, when Colander goes on in the next chapter to promote cointegrated vector auto regression (CVAR) as a superior and distinctively 'European' alternative to DSGE, the absence of attention, at least within the covers of the book, to the *details* of DSGE-inspired policy guidance leaves him vulnerable to the following line of argument. Overly general DSGE models, without situational bolt-ons and incorporating maximum-strength rational expectations, are indeed inferior to carefully constructed CVAR modelling as policy tools. Arguably, however, 'standard' CVAR models have been insufficiently sensitive to modest interpretations of rational expectations of the kind discussed by Ragot (2012). Colander complains that European macroeconomists have been failing to adequately hold their line and have shown increasing signs of surrendering to the hegemony of DSGE. But as applied DSGE modellers abandon purity and bolt on financial and contextual sidecars, they effectively converge their approach with those CVAR modellers who recognise that the Lucas critique may be over-stated but should not be ignored. There is tension in praising CVAR for its flexibility, on the one hand, and worrying that its 'true spirit' is being diluted, on the other hand.

I referred in opening this review to my somewhat mistaken prior image of Colander as an advocate of applying complexity theory in economics. There are many such advocates (e.g. Albin, 1998; Anderson et al., 1988; Arthur et al., 1997; Blume & Durlauf, 2006; Ormerod, 1998), and few mainstream economists regard their contributions as fringe heterodoxy. (Arrow's presence among the complexity advocates would make such dismissal implausible all by itself.) However, as Colander acknowledges, their impact on economic theory *and* on policy has been slight. Many commentators explain this with rhetoric about 'early days' of a new technology. But the earliest source I cited above was published thirty-three years ago. Colander emphasises a different view, grounded in his general methodological critique. Economics based on complexity theory is still *theory*, and as such should not be directly applied to policy. One might think that a complex-systems model of an economic process is *closer* to the context-sensitive level where Colander thinks that normative economists should live if it is based on simulation of that specific process and either incorporates or represents its institutional idiosyncrasies. But this is not always or necessarily true – standard applied economics abounds with country-specific and firm-specific models – and Colander does not lay much stress on the point. What he more interestingly argues is that it is the economy *itself* that is complex, in the sense of being dynamical and non-ergodic, and this interferes with the direct applicability of *any* theory, including theory about complex systems, for normative guidance. Of course Colander believes that models which themselves include dynamics and avoid assuming ergodicity are likely to better fit data, all else being equal, than models that don't. So he is strongly sympathetic to increased investment, especially by younger economists, in complexity-based economic theory and in multi-agent modelling that breaks as radically as possible with representative agent restrictions. But he also recognises the validity in objections that generality is among the aims of good theory, whereas methodology based on simulation struggles to achieve this. Colander's advocacy of a strong 'firewall' (his recurrent phrase) between theoretical and artful economics forms a barrier in *both* directions, thus blocking simple and sweeping inferences such as 'good policy for a complex system should best be informed by theoretical models that are themselves complex'. On the other hand, it is hard to argue with the idea that if the macroeconomy is complex and dynamical then a model that doesn't represent the complexity and permits only comparative-statics analysis is likely to miss important patterns, no matter how clever and innovative the econometric compensations.

My own opinion is that economists should study complex-systems models as a search method for finding potentially interesting patterns, but should not base forecasts on them in advance of having much better confidence than is currently warranted about which elements that could be simulated are most informative, conditional on identified structural variables such as relative elasticities in factor markets. Colander might, for all I can tell, agree with this, subject to the caveat that we should not use theory alone to generate forecasts for policy guidance in the first place. And I think he is right about that, his central point. Where macroeconomics is concerned, I came to Colander and Su already persuaded of their conclusion by the example of Leamer (2009), the most satisfying book on macroeconomic policy I know.

I said at the outset that the Colander and Freedman volume is a less nutritious intellectual meal than Colander and Su, and furthermore that I would advise no student to read Colander and Freedman unless they'd read Colander and Su first. I'll now explain these judgments.

Colander and Freedman is a historical narrative intended to explain, in part, why and how economists forgot the good advice of Mill, Marshall, Robbins, and the two Keyneses. Its greatest value lies in the fact that it is based partly on interviews with participants in key debates. Annoyingly, there is no list of these interviewees, but based on the text they at least included Gary Becker, Ronald Coase, Aaron Director, Milton Friedman, Rose Friedman, James Kindahl, Sherwin Rosen, and Paul Samuelson. Of course, given the timelines of these individuals, most of the interviews occurred some years ago. The reader will note the dominance of Chicago economists in this list, and that reflects the primary conceit of the narrative, which may strike many readers as eccentric.

Here is the story plot, with apologies to Colander and Freedman for the inevitability of making it look less nuanced and informative than it really is by boiling it down to bare bones. The play has three main acts.

In Act I, at the dawn of the post-war world, leading economic theorists inherited a dominant tradition with two features: it had been 'Classical Liberal' and it had been skeptical about deriving policy from technical analysis, and especially from mathematical analysis (as opposed to insight from diagrams and technical stories). Colander and Freedman's point in referring to their heroes gallery, the same wise historical methodologists who are honoured throughout Colander and Su, as 'Classical Liberals' is that although they did not deny that governments should play active roles in fostering and maintaining civil society, they thought that social efficiency was generally best served by letting markets allocate most resources based on price signals. As Marshall argued explicitly, a greater burden of argument rests with a proponent of a new public intervention to regulate commerce. So they were economic liberals.

Act II stars Samuelson. After both theoretical and policy anticipations in the 1930s, the Classical Liberalism of Act I came under pressure as theorists, primarily those associated with the Cowles Commission and leading East Coast schools, especially Harvard and MIT, developed the technical economics of optimal control. This involved stapling together, though without genuine connecting foundations, the model of a boundlessly rational agent (an individual, a firm, or a household) with Hicksian (IS/LM) macroeconomics. The great Arrow-Debreu result was viewed as confirmation of the efficiency of markets, but in principle efficient general equilibrium allocations could be brought about by a planner with power to effect lump-sum transfers at least as well as by independent participants coordinated only by prices. The logic of control implied direct determination of policy from theory; and because the content of the theory in question had to be precise, all proposed elements of it had to be represented mathematically. An implication was that makers of economic policy should have no room in their toolkits for imprecise concepts from qualitative social theories or everyday politics and culture.

In Act III the Economics Department at Chicago, following a period of ambivalence in the immediate postwar years, acquired a distinctive intellectual mission under the leadership of Director, Stigler, and Friedman to be the bastion of resistance against 'Saltwater' indifference to the avowedly essential role of free markets in promoting efficiency and prosperity. Colander and Freedman argue that the Chicago School might have – and should have – stemmed the tide by preserving the Classical Liberalism exemplified by their leader in the previous generation, Frank Knight. Indeed, Colander and Freedman further maintain, they could have reinvigorated Classical Liberalism in a distinctive way had they brought Buchanan's understanding of public choice, and especially Coase's view of transaction costs as unavoidable barriers to general equilibrium reasoning as a policy recipe, into their tent. But none of these things happened. Buchanan's recognition that government officials are agents within the economy rather than its external choreographers was latched, under Tullock's influence, to axiomatic rational choice theory. Coase's insight was turned upside down by Stigler and marketed to economists as Coase's Theorem, stating that where transaction costs are removed, as they are in general equilibrium, market regulation is pointless since agents will bargain to efficiency. Most importantly, Friedman (1953) in the most influential methodological essay of the postwar era, argued that apparent policy disagreements among economists mainly stemmed from confusions about the role of counterfactual assumptions in economic theory, and that if this mistake was avoided then the content of economics could consist entirely of a positive science of efficiency. According to Chicago, the central message of that science for policy makers is that they should protect legal property entitlements and otherwise leave almost all other domestic policy questions to be settled by market dynamics. One could of course fill a decent-sized library with criticism of this libertarian (or 'neo-liberal') mantra. Colander and Freedman's fire is directed instead at the methodological context in which Friedman and his colleagues grounded its defense: the view that economic science can be used to directly resolve policy controversies.

Colander and Freedman don't explain why they chose to defend their Classical Liberal view by focusing on those who they think should have saved it from eclipse, the Chicago gang, rather than on those who played the principal role in displacing it to begin with, the 'saltwater' economists of optimal control. (Compare, by contrast, a recent book that has much in common with Colander and Freedman's critique, but which I found more enlightening, Mirowski and Nik-Khah [2017].) One possible reason is that it was indeed Friedman, not Samuelson (and certainly not Arrow), who went to the trouble of explicitly arguing, in exact contraposition to Colander and Freedman's central thesis, that normative policy disagreements can and should be directly resolved by appeal to results from positive economic theory. But then the reader might reasonably want to know from the historians what Samuelson, Arrow, Baumol, Phelps, and other more representative voices on methodology thought about Friedman's, 1953 essay. Colander and Freedman's subtitle is *Where Economics Went Wrong*. If the literal intended reference of this phrase is to Chicago, this suggests acceptance of that School's conceit that it represented the main core of the discipline. The Chicago economists were remarkable advertisers, and managed to convince much of the policy establishment that their views were the default principles of mainstream economists. But this was never true.

In this context it is also worth remembering that the majority of person hours that academic economists (as opposed to non-academic economists working for financial companies or governments or public agencies) devote to policy consulting are microeconomists' hours. Much of this consulting builds real, functioning mechanisms. I am thinking here of unbiquitous features of the fixed economic environment such as dynamic pricing algorithms, public asset and service auctions, tax compliance incentives, matching algorithms, and public health queuing rules. The majority of this work is applied game theory. When Colander and Freedman occasionally mention game-theoretic modelling – for example, in their invocation of Ariel Rubinstein and Alvin Roth as contemporary expressers of the Classical Liberal ethos – they suggest that it avoids their central criticism. As reference to Rubinstein and Roth suggests, Colander and Freedman also seem to exclude experimental economists from inclusion among those who have 'gone wrong'. In my experience, experimental labs tend far more to derive the problems that inspire their designs from policy challenges than from abstract theory. And what about mechanism designers? What about Duflo's 'plumbers'? Coyle (2007) devotes most of her book on the contemporary 'soul of economics' to these activities. In general, it seems that where Colander and his co-authors talk about 'economics' gone adrift, they really mean: applied general-equilibrium economics and macroeconomics.

I have concentrated on criticisms. This should not obscure the point that the Colander and Su volume is enlightening and engrossing, is for the most part persuasive and sensible, and is richer than the sum of its parts. Colander and Freedman is a useful though somewhat slight extension to the historical dimension of Colander's critique.

I have long thought that economic methodologists and philosophers of economics have neglected the consulting side – consulting under real commissions, that is – of the profession. Closer attention to this would allow us to give more closely informed answers to questions that Colander and his co-authors raise. How does use of theoretical models vary when one passes from client-directed to open-ended policy framing? To what extent does consulting experience reflect back into economists' more curiosity-driven modelling? As far as I know these things haven't been examined. It is a mark of methodological reflection far from the herd, such as Colander's, that new matters for investigation come into focus.

References

Albin, P. (1998). *Barriers and bounds to rationality*. Princeton University Press.

Anderson, P., Arrow, K., & Pines, D. (Eds.). (1988). *The economy as an evolving complex system*. Perseus.

Arthur, B., Durlauf, S., & Lane, D. (Eds.). (1997). *The economy as an evolving complex system II*. Addison-Wesley.

Blume, L. & Durlauf, S. (Eds.). (2006). *The economy as an evolving complex system III*. Oxford University Press.

Coyle, D. (2007). *The soulful science*. Princeton University Press.

Duflo, E. (2017). The economist as plumber. *American Economic Review*, *107*(5), 1–26. https://doi.org/10.1257/aer.p20171153

Fleurbaey, M., & Mongin, P. (2005). The news of the death of welfare economics is greatly exaggerated. *Social Choice and Welfare*, *25*(2-3), 381–418. https://doi.org/10.1007/s00355-005-0010-1

Friedman, M. (1953). *Essays in positive economics*. University of Chicago Press.

Graaff, J. d. V. (1957). *Theoretical welfare economics*. Cambridge University Press.

Keynes, J. M. (1936). Economic possibilities for our grand-children. Re-printed in Keynes, *Essays in Persuasion*, pp. 358–373. Norton (1963).

Leamer, E. (2009). *Macroeconomic patterns and stories*. Springer.

Leamer, E. (2012). *The craft of economics*. MIT Press.

Leamer, E., & Shinde, S. (2021). Theory and evidence as drivers of economists' opinions regarding the impact of fiscal stimulus. In H. Kincaid & D. Ross (Eds.), *Modern guide to philosophy of economics*. Edward Elgar. forthcoming.

Mirowski, P., & Nik-Khah, E. (2017). *The knowledge we have lost in information*. Oxford University Press.

Ormerod, P. (1998). *Butterfly economics*. Faber & Faber.

Ragot, X. (2012). The economics of the laboratory mouse: Where do we go from here? In R. Solow & J.-P. Touffut (Eds.), *What's right with macroeconomics?* (pp. 181–194). Edward Elgar.

Romer, P. (2016). The trouble with macroeconomics. https://www.law.yale.edu/system/files/area/workshop/leo/leo16_romer.pdf.

Ross, D., & Townshend, M. (2021). Everyday economics. In H. Kincaid & D. Ross (Eds.), *Modern guide to philosophy of economics*. Edward Elgar, forthcoming.

Sugden, R. (2018). *The community of advantage*. Oxford University Press.

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

https://doi.org/10.1080/1350178X.2021.1893979