



Second thoughts on economics rules*

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When I was a graduate student at Princeton I used to hang out with a group of fellow students who were economic theorists. They were good at both thinking hard and drinking hard! After a couple of beers, I would start bugging them about the models we were being taught. Why do we focus so much on the perfectly rational individual even though real people do not seem to behave quite that way? (This was before the behavioral revolution obviously.) Shouldn't we be paying a wee bit more attention to disequilibrium, in addition to equilibrium? Why do our models exclude social and institutional features without which markets could not work? Why do we emphasize math so much and disregard things that cannot be easily quantified?

My meta-complaint beyond these and other specific questions was that this extremely bright group of doctoral students, who each went on to become a distinguished economist, had little interest in exploring the validity of economic methods. I simply could not engage them in a discussion on methodology. One day an exasperated theorist turned to me and explained:

Look, it is all about the division of labor. We do economics as it is currently practiced. If someday a philosopher of economics or a specialist in economic methodology comes up with a better idea, then somebody will tell us about it and we will know.

Economists' lack of curiosity on the philosophy of science is reflected in the fact that informed discussion on these questions is relegated to specialized journals such as this one. More damagingly, it is reflected in the lack of training in graduate school in methodology. I may have been a tad more curious about the subject than most. But that does not mean I had a proper training or had more than an amateur's knowledge of it.

So what a pleasure it is to be taken seriously by scholars who have thought long and hard about the philosophy of models – the real grownups in the room! Of course, *Economics Rules* is not a treatise on economic methodology. It does not aim to provide a systematic account grounded in the rich literature that exists (and in which I barely dipped my toes while writing the book). It is rather a practitioners' attempt to explain (to himself and others) how economics works, when it does, and how it fails, as it often does. It is gratifying that bona fide specialists in the philosophy of economic science have found something of value in the book.

Economists' models and the need for 'model commentary'

My take on economic models in the book differs from the standard one. Ask an economist what purpose an economic model serves, and the typical answer you will get is something like this:

A model is an abstract, simplified setup that sheds light on the economy's workings, by clarifying the relationship among exogenous determinants, endogenous effects, and intermediating processes. Economic science advances by testing these models against reality, keeping those that do a good job and discarding the rest.

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I have little difficulty with the first sentence, but I think the second one does not describe accurately how economists work. Models rarely get rejected in economics. This is not just because we lack all the needed data. Nor is it because our empirical methods are imperfect at discriminating among models. More fundamentally, it is because of the malleability of the social world implies no single model can do justice to reality everywhere and at all times. A model that works here and now will fail later or elsewhere. A model that works poorly here and now may prove quite useful later. The falsificationist ideal to which many economists aspire is a poor guide to how economics generates knowledge or how it progresses.

This does not make economic models less useful. We still need models to clarify our thinking and understand how different causal channels work. The fact that we have newer models of how markets work or fail does not make older generation models less important or relevant. Models of monopoly or oligopoly do not displace the traditional perfectly competitive model; they increase the range of circumstances that becomes amenable to systematic economic analysis. Incomplete information models do not supplant perfect information models. But the fact that economic knowledge expands horizontally – through additional models – rather than vertically – by discarding older models – means we have to have a healthy skepticism about the applicability of our models to different contexts. Economists have to be syncretic, holding a variety of different models together in their minds.

Economists do not sufficiently appreciate the importance of what Uskali Mäki calls in this symposium ‘model commentary’ – informed reflection about the intended ‘targets, purposes, audiences, issues of relevant resemblance’ that should be attached to any model but rarely is. They need to think more carefully than they typically do about how to navigate their way among alternative models. The idea that economists are often deficient in their ‘model commentary’ is a nice way of articulating many of the criticisms I lay at the profession’s feet.

As usual, Keynes put it best. He defined economics as the ‘science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world.’ As he explained, ‘unlike the typical natural science, the material to which [economics] is applied is, in too many respects, not homogeneous through time.’ Furthermore, ‘good economists are scarce because the gift for using “vigilant observation” to choose good models, although it does not require a highly specialized intellectual technique, appears to be a very rare one.’¹ Here in three sentences is the core of my argument in *Economics Rules*. Had I been familiar with this quote from Keynes before I wrote the book, I might have chosen not to spend the effort!

Combining versus selecting models

‘It’s *a* model, not *the* model.’ This is the main message of the book. (N. Emrah Aydinonat calls it the book’s motto.) But how does this actually work? How many models should we have? What are the relationships among them? And how does the diversity of models actually help with explanation? In my book, I barely scratched the surface of such questions. The papers in the symposium bring useful refinements and elaborations – as well as reservations.

Mäki rightly points out that economics can be viewed as having core models that unify distinct branches. A model then is a particular version that moves towards greater specificity in one or other of two dimensions – either by bringing greater concreteness or by adding or subtracting certain aspects. We can take the generic perfectly competitive market model and turn it into a model for apples. Or we can transform it into a model with asymmetric information or oligopoly or behavioral elements. Such core models are often called the canonical models of the field – as in the canonical ‘lemons’ model of asymmetric information or the canonical factor-endowments models of international trade.

Aydinonat usefully fleshes out how multiple models – distinct branches within the core model – help with the explanatory process. This can happen in one of two ways. Model diversity may help because it helps identify a number of plausible causal explanations that can be stitched together into an overall account that is compelling. Or it can help by allowing us to hone in on the one

correct model for the specific context. Multiple models can be used to understand multiple channels of causation, or as a list from which one (and ultimately one) is to be drawn from. Aydinonat is right that my account in the book is slippery on my preferred version.

Is reality better understood at any point in time by combining several models, or using a single model that may turn out to be fairly complex? I guess I find myself conflicted on this question. Since the world is always complicated, the stitching together argument for model diversity takes us in the limit to an infinity of models (or at least a very large number of models) being needed. And that obviates the usefulness of models. On the other hand, as Aydinonat rightly observes, often a single model is too parsimonious to capture the full set of relevant facts we are trying to explain.

To use the example I used in the book, and which Aydinonat refers to, no single model can account for the rise of US inequality. It is by combining multiple models – based on trade, technology, and institutions – that we can get a useful and reasonably complete account. I am happy to accept this in general, though I would also put great weight on Occam's razor: use the least number of models as possible.

Aydinonat provides a useful set of guidelines for using multiple models. Start by determining the set of plausibly relevant models. Compare and contrast the predictions of these models with the facts you are trying to explain. Verify empirically, as best as you can, the match between model(s) and reality. If the match is deficient, modify the initial set of models. Repeat the process until you have a satisfactory match. A procedure like this is very much at the heart of our Growth Diagnosis exercise, as I discuss in the book. It should be at the center of good economics practice in all fields of economics. Yet it is curious that professional economists rarely externalize this process explicitly and students get very little training in it.

The contribution by Till Grüne-Yanoff and Caterina Marchionni hones in on the alternative strategy of selecting a single model. They ask: What are the conditions under which the process will yield a single model from among the multitude of candidates? In doing so, the authors formalize in propositional logic an approach that I had described only verbally, and based more on the experiences of a practitioner than on systematic and methodical thinking. As the authors underscore, my goal was to sketch a middle line between hardcore falsificationism (which gets us nowhere) and empirical nihilism (which presumes there is no there there). They write

In his description of the selection procedure, Rodrik also carves out a role for informal procedures of verification – procedures whereby theoretical models are assessed but that might not yield the kind of assurance one expects from formal empirical tests. Commentators and practitioners seem to have paid little attention to such features. The latter perhaps have the aim of holding up the pretension that economics endorses something akin to falsificationism, and the former, at least in some cases, press the charge that economics shields its models from empirical evidence altogether. In this sense, Rodrik offers a balanced and realistic picture of how economic models are used.

This seems to me a very good description of what I had in mind.

Grüne-Yanoff and Marchionni perform a service by formalizing my informal procedure and pushing it as far as it will go. This is useful insofar as we can learn something about the generalizability and/or likely success of my argument. Their formalization identifies certain gaps. For example, the criteria for model inclusion are not sufficiently specified. (I assumed these would be drawn from the current library of models economists tend to use, and it would not be controversial.) The criticality of an assumption needs further thinking, emphasizing in particular that what is critical depends on the purpose (the question being asked). Verification may need a focus on assumptions beyond the critical ones. I note here that Mäki has a very useful discussion of critical assumptions in his contribution to the symposium. He defines criticality in terms of negligibility. 'An assumption is critical if its unrealisticness (of some sort and degree) is not negligible for the conclusion drawn. And an assumption is not critical if its unrealisticness is negligible.'

As Grüne-Yanoff and Marchionni rightly point out, there is reason to worry that the selection process will not result in adequate filtering, leaving us with too many contending models in the end. I do not disagree that this is a problem. It is the reason I stressed the craft aspect of navigating

among models. Even when the process does not do a great job of filtering, though, there is an advantage which I mentioned in the book. Models provide a way of holding a reasonably scientific conversation among economists, despite the lack of convergence of views in those cases. They allow agreement on what the disagreement is really about. We see this, for example, in the model-based work of Robert Barro and Jason Furman, two economists with very different takes on the likely consequences of the most recent tax reform in the US.²

Model selection and modern macro

Jaakko Kuorikoski and Aki Lehtinen confront my book's argument with the reality of the DSGE world in macroeconomics. They find the fit to be loose at best. As they put it, 'There is definitely an art to macroeconomics, but it is the art of ad hoc modification rather than of outright model selection.' Modern macro was clearly unprepared for the global financial crisis and its consequences. There were those, such as Paul Krugman, who argued that the Keynesian model had to be resuscitated. But by and large the response of macroeconomists was to retain the benchmark DSGE model and amend it by adding more shocks and frictions, especially on the financial side. As the authors note, the core model remained the Ramsay model of intertemporal optimization.

I believe Kuorikoski and Lehtinen are right when they say macro did not move in the direction I advocate in my book. Even the critics from within seem attached to the 'one true model' school of economics. Krugman seems to believe we should give up on DSGE models and just go back to the Keynesian IS-LM model. Paul Romer, who has harshly criticized leading macroeconomists for using math to obscure rather than illuminate, writes: 'As scientists, we have to hold ourselves to a standard that requires us to reach a consensus about which model is right, and then to move on to other questions.' The debate in macro seems stuck on the question 'which is *the* right model?'³

I am not a macroeconomist, but it seems to me this is not a useful question. The new classical models with intertemporal optimization and rational expectations were developed because the conventional Keynesian model was not very helpful in the face of the supply shocks of the 1970s. They may have done OK during the 1990s. But they were weak on financial market frictions (asymmetric information, moral hazard, systemic risks) and they became less and less useful with growing financialization of the world economy. Once the crisis struck, they were largely irrelevant to a world with deficient aggregate demand. Our response should not be to jettison them. The right conclusion is that the features of the economy they emphasized – rational forward-looking behavior in the context of well-functioning labor and financial markets – became less important when the context was transformed. We should have been able to navigate among these alternative models as circumstances changed. After the crisis, the Keynesian model became relevant once again. The DSGE models surely also will do well once more.

As Kuorikoski and Lehtinen write, macroeconomists' attachment to the DSGE model rests in large part on 'the necessity of microfoundations and intertemporal maximization.' My view on microfoundations is a bit different. Every modeling convention is an abstraction, and it comes at some cost. It is not immediately clear to me that assuming a representative household with an infinite lifetime is always a better representation of individual-level behavior – i.e. appropriately 'microfounded' – than, say, assuming fixed saving propensities for different groups of consumers. The latter assumption may seem ad hoc, but so is the assumption that there is a representative individual or that she has an infinite horizon. As I discuss in the book, our choice of critical assumptions must depend on context and the questions we are trying to answer. Modern macro seems to have sacrificed relevance and usefulness to a modeling convention that is no less arbitrary than its plausible alternatives.

Economics versus economists

Mäki takes me to task for the distinction I make between economics and economists. Basically, my argument is that economics is in fine shape. The problem has been what economists have made

of it in the public domain and in public discussions. He finds this distinction difficult to sustain. If the actual practice of economics has failed (or not lived up to its billing), how is it possible that economics as a discipline is a good shape? Isn't this akin to saying something like 'guns don't kill people, people do' and recommending more guns (more models) as the solution?

What I meant to argue is that what often fails is the *public interface* of economics, rather than its practice *qua* economics. The difference here relates to the economics in the seminar room versus the economics in public debate. The problem is with 'economists' insofar as the public's view of economics is distorted by exposure to a very narrow sliver of economists, who are not necessarily representative of the profession at large.

An analogy might help. Consider the role of physicists in public debates on nuclear weapons and proliferation. Physicists get drawn in this debate because they have the requisite technical expertise, and also because they may feel a sense of public responsibility. But having good policy judgment in nuclear weapons strategy requires more than deep knowledge of particle physics. Participants in the debate need to have a broad understanding of international relations, bureaucratic politics, economic considerations, and so on. Nuclear physicists who jump in with their preferred policy – develop this program or that one – may not always make a positive contribution when they do not exercise appropriate judgment over these other matters. A similar argument can be made in the domain of climate change. It is perfectly possible for nuclear physics or climate science to be OK *qua* science, and yet for the manner in which physicists – meaning particular physicists – engage in the public debates in these areas to remain problematic.

The role of economists in public policy debates is not that dissimilar. Economists who opine on trade agreements, tax reforms, or financial regulations may be experts in the relevant economic subfields. For example, a trade economist will be familiar with all the related trade theory on gains from trade and comparative advantage. But whether the trade agreement in question is a good one or not will depend on a whole lot of other issues besides the aggregate gains from trade. Who wins, and who loses? Will the losers get compensated? What are the foreign policy implications of the trade agreement? An economist who comes to the debate equipped just with the Ricardian model, and the presumption that this is all she needs, may not make the most productive contribution.

Worse yet, economists often operate under naïve assumptions about political economy. Even though they may be aware that the world is second-best, and that many of the gains from trade may not be realized as a result, they will choose to keep the caveats to themselves for fear of providing ammunition to the barbarians. I have discussed elsewhere the damage that this attitude does to the quality of the public debate on globalization, as well as to the reputation of economists themselves (Rodrik, 2017).

One wishes that economics training would do a better job of instructing future professional economists about their responsibilities – the need for multiple models, sound political economy analysis, and the ethics of public participation. If my book provides the tiniest impetus in that direction, I will be very happy indeed.

Notes

1. The quotes are from Keynes' letter to Roy Harrod on 4 July 1938. See <http://economia.unipv.it/harrod/edition/editionstuff/rfh.346.htm>.
2. Barro and Furman (in press).
3. Romer (2015).

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