Review of Dani Rodrik's *Economics Rules*

An economist spends two years at an interdisciplinary social science institute (the School of Social Science at the Institute for Advanced Study) and writes a book entitled "Economics Rules". That economists tend to think of their discipline as superior to other social sciences is a cliché many economists would readily admit to, and that has recently been bolstered by sociological evidence (Fourcade et al. 2015). If this sense of superiority has suffered due to the criticism economists have had to endure, both from inside and outside of their discipline, in the wake of the recent global financial crisis, then part of the mission of Dani Rodrik's *Economics Rules* is to restore it. The book claims that economics is not only a science, but one that is better suited to studying the social world than other social sciences. The other meaning of "Economics Rules" suggests a more self-critical book, one that develops methodological precepts that economists should follow to avoid failures like the unpreparedness for the financial crisis we faced in 2007-2008 and the global recession that followed. It is in fact here where the book excels: In the detailed and highly readable recounting of both failures and successes of economics in recent years, and in the analysis of what went wrong and what went right in each case. The emphasis in these case studies is, however, decidedly on the failures. Indeed, the conclusions Rodrik draws about his own field are at times devastating. These case studies should be interesting reading for philosophers of economics, especially since the more general methodological lessons Rodrik draws resonate with various views on models in the philosophy of economics literature. I'd thus like to start by discussing Rodrik's rules for economics, but will conclude by returning to the question of whether economics really rules below.

The core message of *Economics Rules* is that economics should be understood as providing us not with universal theories, but with a vast variety of models, each of which is useful in some circumstances, but not all. Moreover, deciding which model best applies requires skill and sensitivity to the empirical evidence pertaining to the case at hand. This is meant to be a message for economists and non-economists alike. It is meant to clear up misunderstandings about the discipline by outsiders. And it is meant to explain good practice to economists themselves. Rodrik argues that a failure to acknowledge the picture of economics he paints explains much of what went wrong in economics in recent years: Economists either mistook a particular model for a universal theory that should apply in all circumstances, forgetting about all other models at their disposal. Or they let their model selection be guided by convention and formal elegance rather than empirical adequacy.

Chapter 1 provides an account of models and their importance in economics, according to which their simplicity and unrealistic assumptions are not necessarily problematic. According to this account, models isolate and clarify the main causal mechanisms at work in the world. They need to be simple in order to play this role. Realism of assumptions matters, but only insofar as the model's "critical assumptions" (on which more below) need to be true. Chapter 2 argues that models make economics a science. Progress in economics mostly consists in the creation of new models which add to the "library" of possible explanations. Each new model moreover provides guidance for how it can be applied and tested empirically. The library of models we end up with will contain models that support mutually inconsistent conclusions about the world. Selecting a model to apply to a particular circumstance thus becomes a crucial step in economic research. Chapter 3 explores how this should be done, while admitting that it is more a craft learned through experience than a science. Model selection should be driven by ensuring fit with observed phenomena: Economists should verify that the model's critical assumptions are

true, that the causal mechanisms the model isolates operate in the world, and that the model's implications are borne out.

Chapters 4 and 5 are mostly concerned with the failures of economics. Chapter 4 recounts episodes where economists have been too concerned with developing grand theories, such as when macroeconomics became dominated by the New Classical approach. This search for universally valid theories is doomed to failure, according to Rodrik. Given the complexity of the social world, models providing partial and contextual explanations are the best we can do. Macroeconomics, for instance, would do better retaining a variety of different models, and paying more attention to when each best applies. Chapter 5 analyses further recent failures of economics, namely the failure to foresee and accommodate the experience of the financial crisis in the most widely used models, as well as the Washington Consensus. In both cases, a consensus formed that cannot easily be squared with the diversity of models at economists' disposal. In the first case, this was a consensus on what models of financial markets should look like — roughly, that they should incorporate a version of Fama's Efficient Market Hypothesis. In the second case, it was a consensus on policy conclusions demanding market liberalisation in developing countries, irrespective of local context. In both cases, the consensus left economists wildly at odds with experience: Models of financial markets did not leave room for the kind of breakdown we witnessed. And the Washington Consensus failed many developing countries in Africa and South America. Rodrik argues that, in both cases, economists essentially misunderstood their own field. They mistook one type of model for *the* universally valid model. In both cases, the favoured type of model featured markets that work efficiently. However, economists also had at their disposal a variety of models of market failure and of second-best policies which could have been fruitfully tailored to the contexts at hand.

What explains this misguided consensus formation? Rodrik argues this has mostly to do with the psychology and sociology of the field. Fads and fashions develop about what models have to look like. Moreover, Rodrik admits that economists, on the whole, have a pro-market bias. This again can be explained by a kind of herd mentality, helped along by the relative insularity of the field. Most worryingly, Rodrik proposes that there are also strategic political reasons for economists' public advocacy of free markets. Economists suffer from what Rodrik calls the "barbarians are only on one side" syndrome: *"Those who want restrictions on markets are organized lobbyists, rent-seeking cronies, and their ilk, while those who want freer markets, even when they're wrong, have their hearts in the right place and are therefore much less dangerous. Taking up the cause of the former gives ammunition to the barbarians, while siding with the latter is, at worst, an honest mistake with no huge consequences."* (p.170)

One might think that these damning observations would make Rodrik sympathetic to critics who lament the field's ideological bias, lack of pluralism, and nonchalance about unrealistic assumptions. Yet, Chapter 6 argues that those critics, too, have misunderstood the nature of economics. Economics, Rodrik claims, is just a collection of models. Those models have a range of different policy implications. If economists fail to reflect or properly exploit this diversity, this is not a fundamental flaw in the discipline. It is only a flaw of the people that practice it. Besides, behind closed doors, there is more diversity of opinions than the public gets to see.

For philosophers of economics, the most interesting aspect of Rodrik's vision of economics is likely to be his emphasis on the plurality of models, and the activity of navigating among them. Discussions on how we can learn from economic models often focus on single models. Rodrik reminds us that there is a harder superordinate question: How do economists learn when they

have a variety of models at their disposal, each of which seems to some extent relevant, but they each point to different conclusions? Rodrik not only raises this question, but also presents interesting examples of where economists have dealt poorly or well with the plurality of models. Providing a philosophical account of learning from a plurality of models is another matter, of course. The most prominent existing accounts of economic models don't address Rodrik's question satisfactorily. It is therefore no surprise that Rodrik, who draws on them, leaves much unresolved. Since his book is not meant to be a philosophical treatise, but is rather written for a general audience, the same level of precision is neither expected nor desirable of his account of models. Some of his lack of precision, however, seems to have led Rodrik to overstate the power of simple economic models. One problem in particular I'd like to point out here is that the book moves freely between two main accounts of models, neither of which permits Rodrik to draw all the conclusions he wishes to draw.

Rodrik stresses that, although the social world is complex, economic models are and ought to be simple. In arguing for this, he characterises models as tools to isolate individual causal mechanisms from all the other causes operative in the social world. This account bears resemblance to Cartwright's causal capacities account of models (see Cartwright 2009) inspired by Mill's (1836) method *a priori*. Rodrik also claims that the only model assumptions that need to be realistic are the "critical" ones: *"an assumption is critical if its modification in an arguably more realistic direction would produce a substantive difference in the conclusion produced by the model."* (p. 27) According to this definition, isolating assumptions, those implying that various other causes active in the world are not present, will tend to be critical — unless those other causes don't affect our outcome of interest. But by their very nature, isolating assumptions are not true. There is therefore a tension between Rodrik's claim that critical assumptions need to be true and the causal isolationist language used to justify the simplicity of economic models. Granted, at times Rodrik suggests that the critical assumptions are just those that describe the causal mechanism we wish to isolate. However, on this alternative reading, we lose the explanation why we shouldn't be worried about unrealistic non-critical assumptions. Some of these non-critical assumptions may be isolating assumptions, but not all are. This is what Cartwright (2009) calls the problem of 'overconstraint'.

In fact, Rodrik's explicit definition of critical assumptions suggests a different account of models, one whereby we can only learn from models to the extent that we can show any lack of realism not to matter for the result in question (see Kuorikoski et al. 2010). Such an account would indeed answer worries about the lack of realism of assumptions — although doubts remain whether economic models are ever shown to be sufficiently robust to changes in unrealistic assumptions. However, on this account, Rodrik's justification for why economic models should be simple does not apply. Indeed, in complex social situations, a model would have to explicitly capture a lot of the complexity of the world in order for a move to greater realism in the model not to change the result in question anymore. It thus seems like either Rodrik's attachment to simplicity, or his solution to the problem of unrealistic assumptions will have to give way.

While I thus have doubts about some of Rodrik's more general methodological claims, I learned a lot from his insightful analysis of failures and successes in economics. His vision of an economics that is non-dogmatic, sensitive to context, and places a greater weight on empirical adequacy strikes me as very attractive. But in the more critical parts of the book, the reader gets the sense that this picture is more a vision of how economics ought to be, or perhaps of where it is headed, not of how it already is. Rodrik criticises his colleagues for not appreciating the diversity of economic models, and for being misguided in their model selection, with grave

consequences. His rebuttal to outside critics of economics, on the other hand, is less convincing, and I suspect that many of them will see themselves strengthened by Rodrik's own critical assessment of his discipline.

Rodrik's dismissal of most outside criticism crucially depends on defining economics narrowly as the collection of models we find in the economic corpus. Rodrik persuasively argues that models play a central role in economics, that they help make it a science, and that economic progress happens when models are added to the corpus. But that needn't mean that economics *is* a collection of models. Intuitively, when economists apply a model to a particular context, this activity and its result are just as much part of economics as the original model. Indeed, it is only through the application of models and their confrontation with empirical evidence that they could help make economics a science. After all, many of the arts could be thought of as providing collections of models, too. In addition, there are arguably economic methods that don't require modelling, unless we water down our concept of a model, and that may even be better suited to studying some social phenomena (see Northcott and Alexandrova 2015). On the ordinary, more inclusive understanding of economics, if fads and fashions and pro-market bias influence model selection, then this is actually a problem that cannot be separated from economics proper.

More importantly, agreeing to clearly separate the collection of economic models from what economists do with them should not assuage critics if Rodrik's claims about the sociology of his field are correct. For one, it would be unlikely that the same fads that influence model selection don't also influence which models are created in the first place. In macroeconomics, the time spent on building microfounded DSGE models could have been spent on developing empirically more fruitful ones instead. And, as Rodrik admits and finds unproblematic, ideal market conditions are treated as a benchmark for model building in economics: The further one's assumptions diverge from them, the harder it is to justify one's model in the community. The library of economic models may thus end up better stocked in some areas than others. Moreover, for an outsider to the field, it will be little solace to be told that, while economists may be offering policy advice and analysis based on poor or biased model selection, in fact the corpus of economics contains models that would have made possible better advice. Rodrik locates malaise in a place that is very difficult for outsiders to scrutinise. If model selection is indeed more craft than science, and requires experience, then all outsiders can do is trust the practitioners that they are doing a good job. Rodrik's remarks on the sociology of economics, however, provide little basis for trust. He does point out that economics has changed a lot in recent years, for instance by having become more empirical. Development economics is singled out as a field that has embraced the context-sensitivity of policy recommendations after the failure of the Washington Consensus. Some of the most interesting passages of the book explain Rodrik's own work on growth diagnostics. To instil trust, Rodrik would have been better advised to provide more such examples of good practice.

The book ends on the note that, given how difficult it is to learn about the social world with models, economists should show humility. Yet, and despite all the book's criticism of economics, Rodrik titled it "Economics Rules". This is not just a clever trick to get his colleagues to buy it. The book in fact frequently asserts the superiority of economics, especially in the opening and concluding chapters. It claims that economics is more meritocratic than other social sciences, that the mathematical consistency of economic models and the sophisticated empirical methods economists employ make the field more rigorous than other social sciences, and that those other sciences would do well to emulate that rigour, if they don't already do so. The sole evidence cited for the alleged lack of professional standards of some other social sciences and

the humanities is the Sokal Hoax. Rodrik declares that economists' *"exposure to diverse models of social life, capturing varieties of behavior and social outcomes, render them perhaps more alert to the possibilities of social progress than other social scientists are."* (p. 210) just a paragraph after calling for greater humility in dialogue with those other social scientists. This is transparently ironic, and it is also hard to square with much of the rest of the book, which is critical of the herd mentality of economics, provides examples of excessive focus on certain particularly elegant modelling tools, and stresses that, for economics to be useful, its internally consistent models need to be matched with economists who are skilful and context-alert navigators of a multitude of mutually inconsistent models. The rhetoric of superiority is not only unnecessary, but detracts from the interesting ideas and insightful analysis also found in the book. Philosophers of economics should nevertheless take note, especially of the middle Chapters 3-5.

References

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