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## **What Economics Models Really Say**

*A Review of Economics Rules: The Rights and Wrongs of the Dismal Science by Dani Rodrik (Norton, 2015)*

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The blurb on the jacket of *Economics Rules* says, “In this sharp, masterful book, Dani Rodrik, a leading critic from within, takes a close look at economics to examine when it falls short and when it works, to give a surprisingly upbeat account of the discipline.” I heartily agree with nearly all of this, with the exception of the “upbeat” part. As I will explain toward the end of this review, my view of economics, and, especially, of the role that economists play in public policy, is much more critical.

A central theme in the book is the role of mathematical models in economics. Formal models in economics and other social sciences are often disparaged. According to the critics (who include some economists, many other social scientists, and the overwhelming majority of historians), models oversimplify complex reality, employ unrealistic assumptions, and deny “agency” to human beings.

Rodrik rejects this critique. According to him, mathematical models—“simplifications designed to show how specific mechanisms work by isolating them from other, confounding effects”—are the true strength of economics. A simplified description of reality is not a shortcoming, it’s the essence of a good model.

My own training was in mathematical biology, and as a graduate student during the 1980s I saw the tail end of the “Math Wars” in ecology. By the 1990s the war was won, and any respectable department of ecology and evolution had to have on faculty at least one modeler. Today, the great majority of ecologists agree that a science cannot become a Science until and unless it develops a well-articulated body of mathematical theory.

In the social sciences, different disciplines made this transition at different times, with economics leading the pack and laggards, like history, undergoing this transition only now (hence cliodynamics—“history as science”; it’s worth noting that most American historians consider history not as a social science, but as one of the humanities).

I was, thus, a bit bemused to read Rodrik’s defense of mathematical models (haven’t economists resolved the Math Wars already?). But it’s an excellent

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defense—all aspiring cliodynamicists should read *Economics Rules*, if only for this reason.

The list of reasons why we need mathematical models in a scientific discipline is familiar to all who have extensive experience in modeling (and for those who don't have such experience, I suggest you read Chapters 1 and 2 of *Economics Rules*). Models clarify the logic of hypotheses, ensure that predictions indeed follow from the premises, open our eyes to counterintuitive possibilities, suggest how predictions could be tested, and enable accumulation of knowledge. The advantage of *clarity* that mathematical models offer scientists is nicely illustrated in the following quote from *Economics Rules*: “We still have endless debates today about what Karl Marx, John Maynard Keynes, or Joseph Schumpeter really meant. ... By contrast, no ink has ever been spilled over what Paul Samuelson, Joe Stiglitz, or Ken Arrow had in mind when they developed the theories that won them their Nobel.” The difference? The first three formulated their theories largely in verbal form, while the latter three developed mathematical models.

The value of the book, however, is in more than just weighing in on the usefulness of mathematical models. As Rodrik notes early in the book, “economics is by and large the only social science that remains almost entirely impenetrable to those who have not undertaken the requisite apprenticeship in graduate school.” And economics is “impenetrable” not because of mathematical models, at least not to someone trained in mathematical natural sciences (the math is universal), but because economists have developed an entirely distinct jargon that sets them apart from other disciplines and creates artificial barriers to understanding the many truly worthwhile insights from economics models.

Because I have not “undertaken the requisite apprenticeship”, I found very useful Rodrik's explanations of the insights generated by such classic models in economics as the First Fundamental Theorem of Welfare Economics, the Principle of Comparative Advantage, and the General Theory of Second Best. Particularly illuminating were the discussion of what happens to the fundamental result of a model when we start systematically relaxing various assumptions on which it depends. This part of the book, together with the references that Rodrik provides, could serve as a basis for an excellent mini-course on what economics theory *really* tells us.

And a general take-home message that emerges from this discussion is that if we want to understand Big Questions—when do markets work or fail, what makes economies grow, and what are the effects of deficit spending—there is not one fundamental model, “the Model”. Instead, we need to study an array of models, each telling a partial story.

So far so good. But Rodrik, in my opinion, goes too far in denying the value of general theory. At one point he writes, “society does not have fundamental laws—at least, not quite in the same way that nature does.” And: “the same theory of

evolution applies in both Northern and Southern Hemispheres,” but “economic models are different.”

Not really. Let’s take the theory of evolution. It’s not a single model. It’s a theoretical framework that includes hundreds, perhaps thousands of special case models, each telling only a partial story. To give an example, textbooks on evolutionary theory often start with a single-locus two-allele model (which gives us the famous Hardy-Weinberg Equilibrium). But you will need different models for haploid organisms (such as bacteria, who have a single unpaired chromosome), or for organisms reproducing asexually; and yet another set of models for phenotypic selection. Despite such diversity of modeling approaches, there is a theoretical unity in evolutionary biology. In particular, the conceptual framework of evolutionary theory provides a set of guidelines for the theoreticians on which model to use in which context.

And I don’t see how the situation is different in economics (and, more generally, social sciences). Yes, there is a multiplicity of models in economics, but you can’t just select one randomly (or worse, “cherry pick” among the results to suit your ideological agenda). There are rules for choosing appropriate models, and Rodrik devotes Chapter 3 of his book to explaining general principles of model selection in economics. In other words, theoretical frameworks are not simply compendia of models, they also include model selection rules (and a few other things).

Rodrik, thus, sells short the potential for general theory in social sciences. Naturally, economics, in particular, does not have such an elaborate, well-articulated, and empirically validated theoretical framework as evolutionary biology (and evolutionary biology, in turn, lags behind many subdisciplines of physics). But who is to say that economics will not develop to the same level in the future? We’ll see if we live long enough.

Let’s now shift gears and talk about Chapter 5, “When Economists Go Wrong.” To make the following discussion concrete, I will focus on a particular theoretical result in economics, the Principle of Comparative Advantage, and what this principle implies for trade policy. In popular press, of course, comparative advantage is always used as a justification for advocating free trade. Rodrik does an admirable job explaining why, under many conditions, free trade can lead to really negative consequences for economies and populations of countries that open themselves to international competition. For example, there is strategic behavior. A country may choose to protect its domestic industry with high tariffs and subsidize its exports in order to gain market share. Perhaps its leaders don’t understand the Principle of Comparative Advantage, not having the benefit of apprenticeship in economics. Or perhaps they care more about their country’s long-term survival in an anarchic international environment than about making immediate profit.

In one particularly revealing passage in the book, Rodrik writes,

consider how opening up trade—one of the key items of the Washington Consensus—was supposed to work. As barriers to imports were slashed, firms that were unable to compete internationally would shrink or close down, releasing their resources (workers, capital, managers) to be employed in other parts of the economy. More efficient, internationally competitive sectors, meanwhile, would expand, absorbing those resources and setting the stage for more rapid economic growth. In Latin American and African countries that adopted this strategy, the first part of this prediction largely materialized, but not the second. Manufacturing firms, previously protected by import barriers, took a big hit. But the expansion of new, export-oriented activities based on modern technologies lagged. Workers flooded less productive, informal service sectors such as petty trading instead. *Overall productivity suffered.* [italics are mine]

Washington Consensus outcomes in Latin America and Africa stand in sharp contrast with the experience of Asian countries. ... Instead of liberalizing imports early on, South Korea, Taiwan, and later China all began their export push by directly subsidizing homegrown manufacturing. ... All of them undertook industrial policies to nurture new manufacturing sectors and reduce their economies' dependence on natural resources.

As Rodrik correctly stresses, these cases do not prove that standard economics is wrong. In short, “someone who advocates free trade because it will benefit everyone probably does not understand how comparative advantage really works.”

Models that were developed for “the way markets really work—or fail to work—in low-income settings with few firms, high barriers to entry, poor information, and malfunctioning institutions, these alternative models proved indispensable”—by telling us why countries that followed the Washington Consensus failed, and those who threw it to the wind succeeded.

But then how does one explain that nearly all economists—96 percent—strongly agree with the following statement: “Free trade improves the productive efficiency and offers consumers better choices, and in the long run these gains are much larger than any effects on unemployment” ([Politicians Should Listen to Economists on Free Trade](#), by Bryan Riley, The Heritage Foundation, Feb.1, 2013; this was from a survey conducted by the University of Chicago’s Booth School of Business).

Rodrik argues that “the problem has to do more with the way economists present themselves in public than with the substance of the discipline.” “In public, the tendency is to close ranks and support free markets and free trade.”

But why is there such an enormous gulf between what economists know and what they say in public? One possible explanation is that policies, such as free trade, while often harming broad swaths of populations, tend to benefit narrow segments of economic elites. Perhaps the critics from the left (and a few “heterodox economists”) are right when they charge that economists speak what the powers-that-be want us to hear.

Whatever the explanation, I cannot agree that Rodrik’s book gives us “a surprisingly upbeat account of the discipline.” Economics may be a vibrant discipline, but most of the richness of its insights is hidden in academic publications behind the shield of specialist jargon, impenetrable to those who have not taken the requisite apprenticeship. And by closing ranks and unconditionally supporting free markets and free trade, economists have failed us, the general public. This is why we need more books like *Economics Rules*—so that we can find out what economics models really tell us.