What Models Do

Economics Rules

WHY ECONOMICS WORKS, WHEN IT FAILS, AND HOW TO TELL THE DIFFERENCE

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Chapter 1

What Models Do

The Swedish-born economist Axel Leijonhufvud published in 1973 a little article called "Life among the Econ." It was a delightful mock ethnography in which he described in great detail the prevailing practices, status relations, and taboos among economists. What defines the "Econ tribe," explained Leijonhufvud, is their obsession with what he called "modls"—a reference to the stylized mathematical models that are economists' tool of the trade. While of no apparent practical use, the more ornate and ceremonial the modl, the greater a person's status. The Econ's emphasis on modls, Leijonhufvud wrote, explains why they hold members of other tribes such as the "Sociogs" and "Polscis" in such low regard: those other tribes do not make modls.*

* Axel Leijonhufvud, "Life among the Econ," *Western Economic Journal* 11, no. 3 (September 1973): 327. Since this article was published, the use of

Leijonhufvud's words still ring true more than four decades later. Training in economics consists essentially of learning a sequence of models. Perhaps the most important determinant of the pecking order in the profession is the ability to develop new models, or use existing models in conjunction with new evidence, to shed light on some aspect of social reality. The most heated intellectual debates revolve around the relevance or applicability of this or that model. If you want to grievously wound an economist, say simply, "You don't have a model."

Models are a source of pride. Hang around economists and before long you will encounter the ubiquitous mug or T-shirt that says, "Economists do it with models." You will also get the sense that many among them would get rather more joy out of toying with those mathematical contraptions than hanging out with the runway prancers of the real world. (No sexism is intended here: my wife, also an economist, was once presented one of those mugs as a gift from her students at the end of a term.)

For critics, economists' reliance on models captures almost everything that is wrong with the profession: the reduction of the complexities of social life to a few simplistic relationships, the willingness to make patently untrue assumptions, the obsession with mathematical rigor over realism, the frequent jump from stylized abstraction to policy conclusions. They find it mind-boggling that economists move so quickly from equa-

models has become more common in other social sciences, especially in political science.

tions on the page to advocacy of, say, free trade or a tax policy of one kind or another. An alternative charge asserts that economics makes the mundane complex. Economic models dress up common sense in mathematical formalism. And among the harshest critics are economists who have chosen to part ways with the orthodoxy. The maverick economist Kenneth Boulding is supposed to have said, "Mathematics brought rigor to economics; unfortunately it also brought mortis." The Cambridge University economist Ha-Joon Chang says, "95 percent of economics is common sense—made to look difficult, with the use of jargons and mathematics."[1]

In truth, simple models of the type that economists construct are absolutely essential to understanding the workings of society. Their simplicity, formalism, and neglect of many facets of the real world are precisely what make them valuable. These are a feature, not a bug. What makes a model useful is that it captures an aspect of reality. What makes it indispensable, when used well, is that it captures *the most relevant aspect of reality in a given context.* Different contexts—different markets, social settings, countries, time periods, and so on—require different models. And this is where economists typically get into trouble. They often discard their profession's most valuable contribution—the multiplicity of models tailored to a variety of settings—in favor of the search for the one and only universal model. When models are selected judiciously, they are a source of illumination. When used dogmatically, they lead to hubris and errors in policy.

A Variety of Models

Economists build models to capture salient aspects of social interactions. Such interactions typically take place in markets for goods and services. Economists tend to have quite a broad understanding of what a market is. The buyers and sellers can be individuals, firms, or other collective entities. The goods and services in question can be almost anything, including things such as political office or status, for which no market price exists. Markets can be local, regional, national, or international; they can be organized physically, as in a bazaar, or virtually, as in long-distance commerce. Economists are traditionally preoccupied with how markets work: Do they use resources efficiently? Can they be improved, and if so, how? How are the gains from exchange distributed? Economists also use models, however, to shed light on the functioning of other institutions—schools, trade unions, governments.

But what are economic models? The easiest way to understand them is as simplifications designed to show how specific mechanisms work by isolating them from other, confounding effects. A model focuses on particular causes and seeks to show how they work their effects through the system. A modeler builds an artificial world that reveals certain types of connections among the parts of the whole—connections that might be hard to discern if you were looking at the real world in its welter of complexity. Models in economics are no different from physical models used by physicians or architects. A plastic model of the respiratory system that you might encounter in a physician's office focuses on the detail of the lungs, leaving out the rest of the human body. An architect might build one model to present the landscape around a house, and another one to display the layout of the interior of the home. Economists' models are similar, except that they are not physical constructs but operate symbolically, using words and mathematics.

The workhorse model of economics is the supply-demand model familiar to everyone who has ever taken an introductory economics course. It's the one with the cross made up of a downward-sloping demand curve and an upward-sloping supply curve, and prices and quantities on the axes.* The artificial world here is the one that economists call a "perfectly competitive market," with a large number of consumers and producers. All of them pursue their economic interests, and none have the capacity to affect the market price. The model leaves many things out: that people have other motives besides material ones, that rationality is often overshadowed by emotion or erroneous cognitive shortcuts, that some producers can

* The supply-demand diagrams, along with the cross, apparently made their first appearance in print in 1838, in a book by the French economist Antoine-Augustin Cournot. Cournot is better known today for his work on duopoly, and the cross is usually attributed to the popular 1890 textbook by Alfred Marshall. See Thomas M. Humphrey, "Marshallian Cross Diagrams and Their Uses before Alfred Marshall: The Origins of Supply and Demand Geometry," *Economic Review* (Federal Reserve Bank of Richmond), March/April 1992, 3–23.

behave monopolistically, and so on. But it does elucidate some simple workings of a real-life market economy.

Some of these are obvious. For example, a rise in production costs increases market prices and reduces quantities demanded and supplied. Or, when energy costs rise, utility bills increase and households find extra ways of saving on heating and electricity. But others are not. For example, whether a tax is imposed on the producers or consumers of a commodity—say, oil—has nothing to do with who ends up paying for it. The tax might be administered on oil companies, but it might be consumers who really pay for it through higher prices at the pump. Or the extra cost might be imposed on consumers in the form of a sales tax, but the oil companies might be forced to absorb it through lower prices. It all depends on the "price elasticities" of demand and supply. With the addition of a longish list of extra assumptions—on which, more later—this model also generates rather strong implications about how well markets work. In particular, a competitive market economy is efficient in the sense that it is impossible to improve one person's well-being without reducing somebody else's. (This is what economists call "Pareto efficiency.")

Consider now a very different model, called the "prisoners' dilemma." It has its origins in research by mathematicians, but it is a cornerstone of much contemporary work in economics. The way it is typically presented, two individuals face punishment if either of them makes a confession. Let's frame it as an economics problem. Assume that two competing firms must decide whether to have a big advertising budget. Advertising would allow one firm to steal some of the other's customers. But when they both advertise, the effects on customer demand cancel out. The firms end up having spent money needlessly.

We might expect that neither firm would choose to spend much on advertising, but the model shows that this logic is off base. When the firms make their choices independently and they care only about their own profits, each one has an incentive to advertise, regardless of what the other firm does:* When the other firm does not advertise, you can steal customers from it if you do advertise; when the other firm does advertise, you have to advertise to prevent loss of customers. So the two firms end up in a bad equilibrium in which both have to waste resources. This market, unlike the one described in the previous paragraph, is not at all efficient.

The obvious difference between the two models is that one describes a scenario with many, many market participants (the market for, say, oranges) while the other describes competition between two large firms (the interaction between airplane manufacturers Boeing and Airbus, perhaps). But it would be a mistake to think that this difference is the exclusive reason

* Strictly speaking, another assumption is also needed: the firms have no way of making credible promises to each other—that is, promises they will not have the incentive to renege on later. For example, each firm may want to promise to the other that it will not advertise. But these promises are not credible, because each firm has an interest in advertising, regardless of what the other firm does.

that one market is efficient and the other not. Other assumptions built in to each of the models play a part. Tweaking those other assumptions, often implicit, generates still other kinds of results.

Consider a third model that is agnostic on the number of market participants, but that has outcomes of a very different kind. Let's call this the coordination model. A firm (or firms; the number doesn't matter) is deciding whether to invest in shipbuilding. If it can produce at sufficiently large scale, it knows the venture will be profitable. But one key input is low-cost steel, and it must be produced nearby. The company's decision boils down to this: if there is a steel factory close by, invest in shipbuilding; otherwise, don't invest. Now consider the thinking of potential steel investors in the region. Assume that shipyards are the only potential customers of steel. Steel producers figure they'll make money if there's a shipyard to buy their steel, but not otherwise.

Now we have two possible outcomes—what economists call "multiple equilibria." There is a "good" outcome, in which both types of investments are made, and both the shipyard and the steelmakers end up profitable and happy. Equilibrium is reached. Then there is a "bad" outcome, in which neither type of investment is made. This second outcome also is an equilibrium because the decisions not to invest reinforce each other. If there is no shipyard, steelmakers won't invest, and if there is no steel, the shipyard won't be built. This result is largely unrelated to the number of potential market participants. It depends crucially instead on three other features: (1) there are economies of scale (in other words, profitable operation requires large scale); (2) steel factories and shipyards need each other; and (3) there are no alternative markets and sources of inputs (that can be provided through foreign trade, for example).

Three models, three different visions of how markets function (or don't). None of them is right or wrong. Each highlights an important mechanism that is (or could be) at work in real-world economies. Already we begin to see how selecting the "right" model, the one that best fits the setting, will be important. One conventional view of economists is that they are knee-jerk market fundamentalists: they think the answer to every problem is to let the market be free. Many economists may have that predisposition. But it is certainly not what economics teaches. The correct answer to almost any question in economics is: It depends. Different models, each equally respectable, provide different answers.

Models do more than warn us that results could go either way. They are useful because they tell us precisely *what* the likely outcomes depend on. Consider some important examples. Does the minimum wage lower or raise employment? The answer depends on whether individual employers behave competitively or not (that is, whether they can influence the going wage in their location).[2] Does capital flow into an emerging-market economy raise or lower economic growth? It depends on whether the country's growth is constrained by lack of investable funds or by poor profitability due, say, to high taxes.[3]

Does a reduction in the government's fiscal deficit hamper or stimulate economic activity? The answer depends on the state of credibility, monetary policy, and the currency regime.[4]

The answer to each question depends on some critical feature of the real-world context. Models highlight those features and show how they influence the outcome. In each case there is a standard model that produces a conventional answer: minimum wages reduce employment, capital flow increases growth, and fiscal cutbacks hamper economic activity. But these conclusions are true only to the extent that their *critical assumptions*—the features of the real world identified above—approximate reality. When they don't, we need to rely on models with different assumptions.

I will discuss critical assumptions and give more examples of economic models later. But first a couple of analogies about what models are and what they do.

Models as Fables

One way to think of economic models is as fables. These short stories often revolve around a few principal characters who live in an unnamed but generic place (a village, a forest) and whose behavior and interaction produce an outcome that serves as a lesson of sorts. The characters can be anthropomorphized animals or inanimate objects, as well as humans. A fable is simplicity itself: the context in which the story unfolds is sketched in sparse terms, and the behavior of the characters is driven by stylized motives such as greed or jealousy. A fable makes little effort to be realistic or to draw a complete picture of the life of its characters. It sacrifices realism and ambiguity for the clarity of its story line. Importantly, each fable has a transparent moral: honesty is best, he laughs best who laughs last, misery loves company, don't kick a man when he's down, and so on.

Economic models are similar. They are simple and are set in abstract environments. They make no claim to realism for many of their assumptions. While they seem to be populated by real people and firms, the behavior of the principal characters is drawn in highly stylized form. Inanimate objects ("random shocks," "exogenous parameters," "nature") often feature in the model and drive the action. The story line revolves around clear cause-and-effect, if-then relationships. And the moral—or policy implication, as economists call it—is typically quite transparent: free markets are efficient, opportunistic behavior in strategic interactions can leave everyone worse off, incentives matter, and so on.

Fables are short and to the point. They take no chance that their message will be lost. The story of the hare and the tortoise imprints on your conscious mind the importance of steady, if slow, progress. The story becomes an interpretive shortcut, to be applied in a variety of similar settings. Pairing economic models with fables may seem to denigrate their "scientific" status. But part of their appeal is that they work in exactly the same way. A student exposed to the competitive supply-demand framework is left with an enduring respect for the power of markets. Once you work through the prisoners' dilemma, you can never think of problems of cooperation in quite the same way. Even when the specific details of the models are forgotten, they remain templates for understanding and interpreting the world.

The analogy is not missed by the profession's best practitioners. In their self-reflective moments, they are ready to acknowledge that the abstract models they put to paper are essentially fables. As the distinguished economic theorist Ariel Rubinstein puts it, "The word 'model' sounds more scientific than 'fable' or 'fairy tale' [yet] I do not see much difference between them."[5] In the words of philosopher Allan Gibbard and economist Hal Varian, "[An economic] model always tells a story."[6] Nancy Cartwright, the philosopher of science, uses the term "fable" in relation to economic and physics models alike, though she thinks economic models are more like parables.[7] Unlike fables, in which the moral is clear, Cartwright says that economic models require lots of care and interpretation in drawing out the policy implication. This complexity is related to the fact that each model captures only a contextual truth, a conclusion that applies to a specific setting.

But here, too, fables offer a useful analogy. There are countless fables, and each provides a guide for action under a somewhat different set of circumstances. Taken together, they result in morals that often appear contradictory. Some fables extol the virtues of trust and cooperation, while others recommend selfreliance. Some praise prior preparation; others warn about the dangers of overplanning. Some say you should spend and enjoy the money you have; others say you should save for a rainy day. Having friends is good, but having too many friends is not so good. Each fable has a definite moral, but in totality, fables foster doubt and uncertainty.

So we need to use judgment when selecting the fable that applies to a particular situation. Economic models require the same discernment. We've already seen how different models produce different conclusions. Self-interested behavior can result in both efficiency (the perfectly competitive market model) and waste (the prisoners' dilemma model) depending on what we assume about background conditions. As with fables, good judgment is indispensable in selecting from the available menu of contending models. Luckily, evidence can provide some useful guidance for sifting across models, though the process remains more craft than science (see Chapter 3).

Models as Experiments

If the idea of models as fables does not appeal, you can think of them as lab experiments. This is perhaps a surprising analogy. If fables make models seem like simplistic fairy tales, the comparison to lab experiments risks dressing them up in excessively scientific garb. After all, in many cultures lab experiments constitute the height of scientific respectability. They are the means by which scientists in white coats arrive at the "truth" about how the world works and whether a particular hypothesis is true. Can economic models come even close?

Consider what a lab experiment really is. The lab is an artificial environment built to insulate the materials involved in the experiments from the environment of the real world. The researcher designs experimental conditions that seek to highlight a hypothesized causal chain, isolating the process from other potentially important influences. When, say, gravity exerts confounding effects, the researcher carries out the experiment in a vacuum. As the Finnish philosopher Uskali Mäki explains, the economics modeler in fact practices a similar method of insulation, isolation, and identification. The main difference is that the lab experiment purposely manipulates the physical environment to achieve the isolation needed to observe the causal effect, whereas a model does this by manipulating the assumptions that go into it.* Models build mental environments to test hypotheses.

* Uskali Mäki, "Models Are Experiments, Experiments Are Models," *Journal of Economic Methodology* 12, no. 2 (2005): 303–15. Note that isolating an effect in economic models is not as simple as it may seem. We always have to make some assumptions about other background conditions. For this reason, Nancy Cartwright argues that the effect is always the result of the joint operation of many causes and we can never truly isolate cause and effect in economics. See Cartwright, *Hunting Causes and Using Them: Approaches in Philosophy and Economics* (Cambridge: Cambridge University Press, 2007). This is true in general, but the value of having multiple models is that it enables us to alter the background conditions selectively, to ascertain which, if any, make a substantive contribution to the effect.

You may object that in a lab experiment, as artificial as its environment may be, the action still takes place in the real world. We know if it works or does not work, in at least one setting. An economic model, by contrast, is a thoroughly artificial construct that unfolds in our minds only. Yet the difference can be in degree rather than in kind. Experimental results, too, may require significant extrapolation before they can be applied to the real world. Something that worked in the lab may not work outside it. For example, a drug might fail in practice when it mixes with real-world conditions that were left out of consideration—"controlled for"—under the experimental setting.

This is the distinction that philosophers of science refer to as internal versus external validity. A well-designed experiment that successfully traces out cause and effect in a specific setting is said to have a high degree of "internal validity." But its "external validity" depends on whether its conclusion can travel successfully outside the experimental context to other settings.

So-called field experiments, carried out not in the lab but under real-world conditions, also face this challenge. Such experiments have become very popular in economics recently, and they are sometimes thought to generate knowledge that is

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Varying some background conditions may make a big difference; varying others, very little. See also my discussion on the realism of assumptions later in the chapter.

model-free; that is, they're supposed to provide insight about how the world works without the baggage of assumptions and hypothesized causal chains that comes with models. But this is not quite right. To give one example: In Colombia, the randomized distribution of private-school vouchers has significantly improved educational attainment. But this is no guarantee that similar programs would have the same outcome in the United States or in South Africa. The ultimate outcome relies on a host of factors that vary from country to country. Income levels and preferences of parents, the quality gap between private and public schools, the incentives that drive schoolteachers and administrators—all of these factors, and many other potentially important considerations, come into play.[8] Getting from "it worked there" to "it will work here" requires many additional steps.[9]

The gulf between real experiments carried out in the lab (or in the field) and the thought experiments we call "models" is less than we might have thought. Both kinds of exercises need some extrapolation before they can be applied when and where we need them. Sound extrapolation in turn requires a combination of good judgment, evidence from other sources, and structured reasoning. The power of all these types of experiments is that they teach us something about the world outside the context in which they're carried out, on account of our ability to discern similarity and draw parallels across diverse settings.

As with real experiments, the value of models resides in being able to isolate and identify specific causal mechanisms, one at a time. That these mechanisms operate in the real world alongside many others that may obfuscate their workings is a complication faced by all who attempt scientific explanations. Economic models may even have an advantage here. Contingency—dependence on specific postulated conditions—is built into them. As we'll see in Chapter 3, this lack of certainty encourages us to figure out which among multiple contending models provides a better description of the immediate reality.

Unrealistic Assumptions

Consumers are hyperrational, they are selfish, they always prefer more consumption to less, and they have a long time horizon, stretching into infinity. Economic models are typically assembled out of many such unrealistic assumptions. To be sure, many models are more realistic in one or more of these dimensions. But even in these more layered guises, other unrealistic assumptions can creep in somewhere else. Simplification and abstraction necessarily require that many elements remain counterfactual in the sense that they violate reality. What is the best way to think about this lack of realism?

Milton Friedman, one of the twentieth century's greatest economists, provided an answer in 1953 that deeply influenced the profession.[10] Friedman went beyond arguing that unrealistic assumptions were a necessary part of theorizing. He claimed that the realism of assumptions was simply irrelevant. Whether a theory made the correct predictions was all that mattered. As long as it did, the assumptions that went into the theory need not bear any resemblance to real life. While this is a crude summary of a more sophisticated argument, it does convey the gist that most readers took from Friedman's essay. As such, it was a wonderfully liberating argument, giving economists license to develop all kinds of models built on assumptions wildly at variance with actual experience.

However, it cannot be true that the realism of assumptions is entirely irrelevant. As Stanford economist Paul Pfleiderer explains, we always need to apply a "realism filter" to *critical* assumptions before a model can be treated as useful.[11] (Here's that term "critical" again. I will turn to it shortly.) The reason is that we can never be sure of a model's predictive success. Prediction, as Groucho Marx might have said, always involves the future. We can concoct an almost endless variety of models to explain a reality after the fact. But most of these models are unhelpful; they will fail to make the correct prediction in the future, when conditions change.

Suppose I have data on traffic accidents in a locality for the last five years. I notice that there are more accidents at the end of the workday, between 5:00 and 7:00 p.m. The most reasonable explanation is that more people are on the road at that time, driving home from work. But suppose a researcher comes up with an alternative story. It's John's fault, he says. John's brain emits invisible waves that affect everyone's driving. Once he is out of his office and on the street, his brain waves mess with traffic, causing more accidents. It may be a silly theory, but it does "explain" the rise in traffic accidents at the end of the workday.

We know in this case that the second model is not a useful one. If John changes his schedule or he retires, it will have no predictive value. The number of accidents will not go down when John is no longer out and about. The explanation fails because its critical assumption—that John emits traffic-disrupting brain waves—is false. For a model to be useful in the sense of tracking reality, its critical assumptions also have to track reality sufficiently closely.[12]

What exactly is a critical assumption? We can say 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. Many, if not most, assumptions are not critical in this sense. Consider the perfectly competitive market model. The answers to many questions of interest do not depend crucially on the details of that model. In his essay on methodology, Milton Friedman discussed taxes on cigarettes. We can safely predict that raising the tax rate will lead to an increase in the retail price of cigarettes, he wrote, regardless of whether there are many or few firms and whether different cigarette brands are perfect substitutes or not. Similarly, any reasonable relaxation of the requirement of perfect rationality would be unlikely to make much difference to that result. Even if firms do not make calculations to the last decimal point, we can be reasonably confident that they will notice an increase in the taxes they have to pay. These specific assumptions are not critical in view of which question is posed and how the model is used—for example, how does a tax effect the price of cigarettes? Their lack of realism therefore is not of great importance.

Suppose we were interested in a different question: the effect of imposing price controls on the cigarette industry. Now the degree of competition in the industry, which depends in part on the extent to which consumers are willing to substitute between different brands, becomes of great importance. In the perfectly competitive market model, a price control leads to firms reducing their supply. The lower price decreases their profitability, and they respond by cutting back their sales. But in a model of a market that is monopolized by a single firm, a moderate price ceiling (that is, a ceiling that is not too far below the unrestricted market price) actually induces the firm to *increase* its output. To see how this mechanism operates, a bit of simple algebra or geometry comes in handy. Intuitively, a monopolist increases profits by restricting sales and raising the market price. Price controls, which rob the monopolist of its price-setting powers, effectively blunt the incentive to under-produce. The monopolist responds by increasing sales.* Selling more cigarettes is now the only means to making more profits.

What we assume about the degree of market competition becomes critical when we want to predict the effects of price

* This is the same logic that causes an increase in employment after a (moderate) minimum wage has been imposed.

controls. The realism of this particular assumption matters, and it matters greatly. The applicability of a model depends on how closely critical assumptions approximate the real world. And what makes an assumption critical depends in part on what the model is used for. I will return to this issue later in the book, when I examine in greater detail how we select which model to apply in a given setting.

It is perfectly legitimate, and indeed necessary, to question a model's efficacy when its critical assumptions are patently counterfactual, as with John's brain waves. In such instances, we can rightly say that the modeler has oversimplified and is leading us astray. The appropriate response, however, is to construct alternative models with more fitting assumptions—not to abandon models per se. The antidote to a bad model is a good model.

Ultimately, we cannot avoid unrealism in assumptions. As Cartwright says, "Criticizing economic models for using unrealistic assumptions is like criticizing Galileo's rolling ball experiments for using a plane honed to be as frictionless as possible."[13] But just as we would not want to apply Galileo's law of acceleration to a marble dropped into a jar of honey, this is not an excuse for using models whose critical assumptions grossly violate reality.

On Math and Models

Economic models consist of clearly stated assumptions and behavioral mechanisms. As such, they lend themselves to the language of mathematics. Flip the pages of any academic journal in economics and you will encounter a nearly endless stream of equations and Greek symbols. By the standards of the physical sciences, the math that economists use is not very advanced: the rudiments of multivariate calculus and optimization are typically sufficient to follow most economic theorizing. Nevertheless, the mathematical formalism does require some investment on the part of the reader. It raises a comprehensibility barrier between economics and most other social sciences. It also heightens noneconomists' suspicions about the profession: the math makes it seem as if economists have withdrawn from the real world and live in abstractions of their own construction.

When I was a young college student, I knew I wanted to get a PhD because I loved writing and doing research. But I was interested in a wide variety of social phenomena and could not make up my mind between political science and economics. I applied to both kinds of doctoral programs, but I postponed the ultimate decision by enrolling in a multidisciplinary master's program. I remember well the experience that finally resolved my indecision. I was in the library of the Woodrow Wilson School at Princeton and picked up the latest issues of the *American Economic Review* (*AER*) and the *American Political Science Review* (*APSR*), the flagship publications of the two disciplines. Looking at them side by side, it dawned on me that I would be able to read the *APSR* with a PhD in economics, but much of the *AER* would be inaccessible to me with a PhD in political science. With hindsight, I realize this conclusion was perhaps not quite right. The political philosophy articles in the *APSR* can be as abstruse as any in the *AER*, math aside. And much of political science has since gone the way of economics in adopting mathematical formalism. Nonetheless, there was a germ of truth in my observation. To this day, 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.

The reason economists use mathematics is typically misunderstood. It has little to do with sophistication, complexity, or a claim to higher truth. Math essentially plays two roles in economics, neither of which is cause for glory: clarity and consistency. First, math ensures that the elements of a model—the assumptions, behavioral mechanisms, and main results—are stated clearly and are transparent. Once a model is stated in mathematical form, what it says or does is obvious to all who can read it. This clarity is of great value and is not adequately appreciated. We still have endless debates today about what Karl Marx, John Maynard Keynes, or Joseph Schumpeter really meant. Even though all three are giants of the economics profession, they formulated their models largely (but not exclusively) in verbal form. 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. Mathematical models require that all the *t*'s be crossed and the *i*'s be dotted.

The second virtue of mathematics is that it ensures the internal consistency of a model—simply put, that the conclusions follow from the assumptions. This is a mundane but indispensable contribution. Some arguments are simple enough that they can be self-evident. Others require greater care, especially in light of cognitive biases that draw us toward results we want to see. Sometimes a result can be plainly wrong. More often, the argument turns out to be poorly specified, with critical assumptions left out. Here, math provides a useful check. Alfred Marshall, the towering economist of the pre-Keynesian era and author of the first real economics textbook, had a good rule: use math as a shorthand language, translate into English, and then burn the math! Or as I tell my students, economists use math not because they're smart, but because they're not smart enough.

When I was still young and green as an economist, I once heard a lecture by the great development economist Sir W. Arthur Lewis, winner of the 1979 Nobel Prize in Economic Sciences. Lewis had an uncanny ability to distill complex economic relationships to their essence by using simple models. But as with many economists from an older tradition, he tended to present his argument in verbal rather than mathematical form. On this occasion his topic was the determination of poor countries' terms of trade—the relative price of their exports to their imports. When Lewis finished, one of the younger, more mathematically oriented economists in the audience stood up and scribbled a few equations on the blackboard. He pointed out that at first he had been confused by what Professor Lewis was saying. But, he continued as a bemused Lewis watched, now he could see how it worked: we have these three equations that determine these three unknowns.

So, math plays a purely instrumental role in economic models. In principle, models do not require math, and it is not the math that makes the models useful or scientific.* As the Arthur Lewis example illustrates, some stellar practitioners of the craft rarely use any math at all. Tom Schelling, who has developed some of the key concepts of contemporary game theory, such as credibility, commitment, and deterrence, won the Nobel Prize for his largely math-free work.[14] Schelling has the rare knack of laying out what are fairly complicated models of interaction among strategically minded individuals while using only words, real-world examples, and perhaps a figure at most. His writings have greatly influenced both academics and policy makers. I must admit, though, that the depth of his insights and the precise nature of the arguments became fully evident to me only after I saw them expressed more fully with mathematics.

Nonmathematical models are more common in social sciences outside of economics. You can always tell that a social

* Outside of economics, the term "rational choice" has become a synonym for an approach to social science that uses predominantly mathematical models. This use of the term conflates several things. Doing social science using models requires neither math nor, necessarily, the assumption that individuals are rational.

scientist is about to embark on a model when he or she begins, "Assume that we have . . ." or something similar, followed by an abstraction. Here, for example, is the sociologist Diego Gambetta examining the consequences of different types of beliefs about the nature of knowledge: "Imagine two ideal-type societies that differ in one respect only . . ."[15] Papers in political science are frequently peppered with references to independent and dependent variables—a sure sign that the author is mimicking models even when a clear-cut framework is lacking.

Verbal arguments that seem intuitive often collapse, or are revealed to be incomplete, under closer mathematical scrutiny. The reason is that "verbal models" can ignore nonobvious but potentially significant interactions. For example, many empirical studies have found that government intervention is negatively correlated with performance: industries that receive subsidies experience lower productivity growth than industries that don't. How do we interpret these findings? It is common, even among economists, to conclude that governments must be intervening for the wrong rather than right reasons, that they support weak industries in response to political lobbying. This may sound reasonable—too obvious even to require further analysis. Yet when we mathematically describe the behavior of a government that intervenes for the right reason—by subsidizing industries to enhance the economy's efficiency—we see that this conclusion may not be warranted. Industries that are performing poorly because markets are malfunctioning warrant greater government intervention—

but not to the extent that their disadvantages are completely offset. Therefore, the negative correlation between subsidies and performance does not tell us whether governments are intervening in desirable or undesirable ways, as both types of intervention would generate the observed correlation. Not clear? Well, you can check the math!*

At the other end of the spectrum, too many economists fall in love with the math and forget its instrumental nature. Excessive formalization—math for its own sake—is rampant in the discipline. Some branches of economics, such as mathematical economics, have come to look more like applied mathematics than like any kind of social science. Their reference point has become other mathematical models instead of the

* Dani Rodrik, "Why We Learn Nothing from Regressing Economic Growth on Policies," *Seoul Journal of Economics* 25, no. 2 (Summer 2012): 137–51. Further afield from economics, John Maynard Smith, a distinguished theorist of evolutionary biology, explains why it is important to develop the mathematics of an argument in this video: http://www.webofstories.com/play/john.maynard.smith/52;jsessionid=3636304FA6745B8E5D200253DAF409E0. Maynard describes his frustration with a verbal theory of why some animals, like the antelope, jump up and down while running, exhibiting a behavior that is called "stotting." This behavior seems inefficient because it slows the animal down. The theory is that stotting is a way of signaling potential predators that the antelope is not worth pursuing: the antelope is so fast that it can get away even with this inefficient run. Smith recollects how he tried to model this scenario mathematically and could never produce the desired result—that strotting could be efficient when used as a signal.

real world. The abstract of one paper in the field opens with this sentence: "We establish new characterizations of Walrasian expectations equilibria based on the veto mechanism in the framework of differential information economies with a complete finite measure space of agents."[16] One of the profession's leading, and most mathematically oriented, journals (*Econometrica*) imposed a moratorium at one point on "social choice" theory—abstract models of voting mechanisms—because papers in the field had become mathematically so esoteric and divorced from actual politics.[17]

Before we judge such work too harshly, it is worth noting that some of the most useful applications in economics have come out of highly mathematical, and what to outsiders would surely seem abstruse, models. The theory of auctions, drawing on abstract game theory, is virtually impenetrable even to many economists.* Yet it produced the principles used by the Federal Communications Commission to allocate the nation's telecommunications spectrum to phone companies and broadcasters as efficiently as possible, while raising more than $60 billion for the federal government.[18] Models of matching and market design, equally mathematical, are used today to assign residents to hospitals and students to public schools. In each

* For a relatively informal introduction to the theory, see Paul Milgrom, "Auctions and Bidding: A Primer," *Journal of Economic Perspectives* 3, no. 3 (Summer 1989), 3–22. A more thorough treatment can be found in Paul Klemperer, *Auctions: Theory and Practice* (Princeton, NJ: Princeton University Press, 2004).

case, models that seemed to be highly abstract and to have few connections with the real world turned out to have useful applications many years later.

The good news is that, contrary to common perception, math for its own sake does not get you far in the economics profession. What's valued is "smarts": the ability to shed new light on an old topic, make an intractable problem soluble, or devise an ingenious new empirical approach to a substantive question. In fact, the emphasis on mathematical methods in economics is long past its peak. Today, models that are empirically oriented or policy relevant are greatly preferred in top journals over purely theoretical, mathematical exercises. The profession's stars and most heavily cited economists are those who have shed light on important public problems, such as poverty, public finance, economic growth, and financial crises—not its mathematical wizards.

Simplicity versus Complexity

Despite the math, economic models tend to be simple. For the most part, they can be solved using pen and paper. It's one reason why they have to leave out many aspects of the real world. But as we've seen, lack of realism is not a good criticism on its own. To use an example from Milton Friedman again, a model that included the eye color of the businesspeople competing against each other would be more realistic, but it would not be a better one.[19] Still, whether some influences matter or not depends on what is assumed at the outset. Perhaps blue-eyed businessmen are more dim-witted and systematically underprice their products. The strategic simplifications of the modeler, made for reasons of tractability, can have important implications for substantive outcomes.

Wouldn't it be better to opt for complexity over simplicity? Two related developments in recent years have made this question more pertinent. First, the stupendous increase in computing power and the attendant sharp fall in its cost have made it easier to run large-scale computational models. These are models with thousands of equations, containing nonlinearities and complex interactions. Computers can solve them, even if the human brain cannot. Climate models are a well-known example. Large-scale computational models are not unknown in economics, even though they are rarely as big. Most central banks use multiequation models to forecast the economy and predict the effects of monetary and fiscal policy.

The second development is the arrival of "big data," and the evolution of statistical and computational techniques that distill patterns and regularities from them. "Big data" refers to the humongous amount of quantitative information that is generated by our use of the Internet and social media—an almost complete and continuous record of where we are and what we do, moment by moment. Perhaps we have reached, or soon will reach, the stage where we can rely on the patterns revealed in this data to uncover the mysteries of our social relations. "Big data gives us a chance to view society in all its complexity," writes one of the leading proponents of this view.[20] This would send our traditional economic models the way of the horse and buggy.

Certainly, complexity has great surface appeal. Who could possibly deny that society and the economy are complex systems? "Nobody really agrees on what makes a complex system 'complex,'" writes Duncan Watts, a mathematician and sociologist, "but it's generally accepted that complexity arises out of many interdependent components interacting in nonlinear ways." Interestingly, the immediate example that Watts deploys is the economy: "The U.S. economy, for example, is the product of the individual actions of millions of people, as well as hundreds of thousands of firms, thousands of government agencies, and countless other external and internal factors, ranging from the weather in Texas to interest rates in China."[21] As Watts notes, disturbances in one part of the economy—say, in mortgage finance—can be amplified and produce major shocks for the entire economy, as in the "butterfly effect" from chaos theory.

It is interesting that Watts would point to the economy, since efforts to construct large-scale economic models have been singularly unproductive to date. To put it even more strongly, I cannot think of an important economic insight that has come out of such models. In fact, they have often led us astray. Overconfidence in the prevailing macroeconomic orthodoxy of the day resulted in the construction of several large-scale simulation models of the US economy in the 1960s and 1970s built on Keynesian foundations. These models performed rather badly in the stagflationary environment of the late 1970s and 1980s. They were subsequently jettisoned in favor of "new classical" approaches with rational expectations and price flexibility. Instead of relying on such models, it would have been far better to carry several small models in our heads simultaneously, of both Keynesian and new classical varieties, and know when to switch from one to the other.

Without these smaller, more transparent models, large-scale computational models are, in fact, unintelligible. I mean this in two senses. First, the assumptions and behavioral relations that are built into the large models must come from somewhere. Depending on whether you believe in the Keynesian model or the new classical model, you will develop a different large-scale model. If you think economic relationships are highly nonlinear or exhibit discontinuities, you will build a different model than if you think they are linear and "smooth." These prior understandings do not derive from complexity itself; they must come from some first-level theorizing.

Second, and alternatively, suppose we can build large-scale models relatively theory-free, using big-data techniques based on observed empirical regularities such as consumer spending patterns. Such models can deliver predictions, like weather models do, but never knowledge on their own. For they are like a black box: we can see what is coming out, but not the operative mechanism inside. To eke out knowledge from these models, we need to figure out and scrutinize the underlying causal mechanisms that produce specific results. In effect, we need to construct a small-scale version of the larger model. Only then can we say that we understand what's going on. Moreover, when we evaluate the predictions of the complex model—it predicted this recession, but will it predict the next one?—our judgment will depend on the nature of these underlying causal mechanisms. If they are plausible and reasonable, by the same standards we apply to small-scale models, we may have reason for confidence. Not otherwise.

Consider the large-scale computational models that are common in the analysis of international trade agreements among nations. These agreements change import and export policies in hundreds of industries that are linked through markets for labor, capital, and other productive inputs. A change in one industry affects all the others, and vice versa. If we want to understand the economy-wide consequences of trade agreements, we need a model that tracks all these interactions. In principle, that is what the so-called computable general equilibrium (CGE) models do. They are constructed partly on the basis of the prevalent models of trade, and partly on ad hoc assumptions meant to replicate observed economic regularities (such as the share of national output that is traded internationally). When pundits in the media report, say, that the Transatlantic Trade and Investment Partnership (TTIP) between the United States and Europe will create so many billions of dollars of exports and income, they are citing results from these models.

Without doubt, models of this sort can provide a sense of the orders of magnitude involved in a decision. But ultimately, they are credible only to the extent that their results can be motivated and justified by much smaller, pen-and-paper models. Unless the underlying explanation is transparent and intuitive—unless there exists a simpler model that generates a similar result—complexity on its own buys us nothing other than perhaps a bit more detail.

What about some of the specific insights arising out of models that emphasize complexity, such as tipping points, complementarities, multiple equilibria, or path dependence? It is true that such "nonstandard" outcomes emphasized by complexity theorists stand in sharp contrast to the more linear, smooth behavior of economists' workhorse models. It is also certainly true that real-world outcomes are sometimes better described in those spikier ways. However, not only can these kinds of outcomes be generated in smaller, simpler models, but they actually originate in them. Tipping-point models, referring to a sudden change in aggregate behavior after a sufficient number of individuals make a switch, were first developed and applied to different social settings by Tom Schelling. His paradigmatic example, developed in the 1970s, was the collapse of mixed neighborhoods into complete segregation once a critical threshold of white flight is reached. The potential for multiple equilibria has long been known and studied by economists, often in the context of highly stylized models. I gave an example (our shipbuilder and the coordination game) at the beginning of the chapter. Path dependence is a feature of a large class of dynamic economic models. And so on.

A critic might argue that economists treat such models as exceptions to the "normal" cases covered by the workhorse competitive market model. And the critic would have a point. Economists tend to fixate too much on certain standard models at the expense of others. In some settings, a simple model can be, well, too simple. We may need more detail. The trick is to isolate just the interactions that are hypothesized to matter, but no more. As the preceding examples suggest, models can do this and still remain simple. One model is not always better than another. Remember: it is *a* model, not *the* model.

Simplicity, Realism, and Reality

In his exceptionally brief—one paragraph, to be exact—short story called "On Exactitude in Science," the Argentine novelist Jorge Luis Borges describes a mythical empire in the distant past in which cartographers took their craft very seriously and strived for perfection. In their quest to capture as much detail as possible, they drew ever-bigger maps. The map of a province expanded to the size of a city; a map of the empire occupied a whole province. In time, even this level of detail became insufficient and the cartographers' guild drew a map of the empire on a 1:1 scale the size of the empire itself. But future generations, less enamored by the art of cartography and more interested in help with navigation, would find no use for these maps. They discarded them and left them to rot in the desert.[22]

As Borges's story illustrates, the argument that models need to be made more complex to make them more useful gets it backward. Economic models are relevant and teach us about the world *because* they are simple. Relevance does not require complexity, and complexity may impede relevance. Simple models—in the plural—are indispensable. Models are never true; but there is truth in models.[23] We can understand the world only by simplifying it.