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Author(s): Robert M. Solow

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Robert M. Solow

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MY EXPOSURE TO ECONOMICS as a discipline began in September 1940 when I enrolled as a freshman in the elementary economics course at Harvard College. I will try in this essay to make sense of the evolution of economics over a span of more than fifty years.

An analogy that comes to mind is from *The Boston Globe.* The Sunday edition occasionally publishes pairs of photographs of urban landscapes. They are taken from the same spot, looking in the same direction, but are at least thirty, forty, or fifty years apart. One shows a corner of the city as it looked then and the other as it looks now. Some buildings have disappeared, some new ones have been built, and some of the old ones are still there but with altered facades. This description is also true of the landscape and structure of economics, and I would like to provide a few then-and-now snapshots. The difference, however, is that with economics something more is called for; the pictures have to be connected. I would like to tell a story about how and why the architecture of economics changed. It will be a sort of Whig history but without the smugness.

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There were three textbooks that were used in the 1940 economics course at Harvard. One was a standard principles text by Frederic

Robert M. Solow is Institute Professor Emeritus at the Massachusetts Institute of Technology.

Garver and Alvin Hansen. Hansen had been at Minnesota with Garver but by 1940 was a professor at Harvard and—although we freshmen had no inkling—the leading figure in bringing the ideas of John Maynard Keynes's *General Theory of Employment, Interest and Money*[1] into American economics. The second text was a large introductory book called *Modern Economic Society* by Sumner Slichter,[2] also a member of the Harvard faculty and usually referred to as the dean of American labor economists. The book was more about economic institutions and their functioning than about theory. The third text was a little green volume by Luthringer, Chandler, and Cline about money and banking, one of a series of little green books. (Lester Chandler of Princeton was the only one of the authors whose name we ever heard again.) It was a pretty boring text, as I remember, but fortunately we only had to read bits of it. This is actually an important point, and I will come back to it later.

Even a quick physical comparison of a good contemporary elementary text with Garver and Hansen and Slichter tells us something. Leaving aside the typographical changes—color, wider margins, larger type—the modern text is sprinkled with diagrams, tables, even simple equations, whereas the older ones present page after page of unbroken prose. In some seven hundred pages, Garver and Hansen have fewer than forty tables or figures. Some of them represent the working-out of numerical examples of simple propositions, and the rest, maybe half, contain data about the US economy. Similarly, there are fifty-five graphs, again divided between a small number of analytical diagrams and a larger number of graphical presentations of actual data. Slichter is not radically different in his nine hundred pages.

The modern counterpart, while no more intellectually demanding for the student (perhaps even less so), is full of diagrams, tables, and equations. The use of analytical diagrams is probably ten times as intense, and the volume of real-world data presented is correspondingly greater. Propositions are often stated in the form of equations, but these are almost always simple statements (i.e., two intuitively understandable quantities must be equal); there is not a lot of heavy mathematics in these texts. (The older books mention one equation, the Quantity Equation.) The numerical example, hallowed in economics since the days of David Ricardo, is still in use, but it is no longer the analytical workhorse.

The older books are long on classifications—kinds of goods, kinds of industries, kinds of labor—and on descriptions of public and private institutions. The first 260 pages in Slichter's text are exclusively descriptive of the US economy as it then was. I would guess that fewer than one hundred of the next six hundred pages are devoted to the development of analysis or to the application of analysis. Most provide more institutional descriptions, very sensible discussions of economic policy, and serious looks at recent history as it would be seen by an economist. No one should underestimate the value of these historical reflections. They are, in a way, the application of analytical ideas. But there is a not-so-subtle difference. The modern textbook presents and uses economic analysis as a tool to be directly applied to contemporary or historical situations. The student is shown how to map real events into the categories that appear on the axes of the diagrams or the terms in the equations. The older texts are simply more discursive. The underlying ideas are treated more like categories that resonate to this or that bit of history or policy; the authors ruminate more than they analyze.

One sees this clearly in the way these two books present the idea of supply and demand. This is the one piece of analysis that gets careful treatment. Characteristically, however, Garver and Hansen are very good on how one should think about different kinds of commodities—perishable or not, bought frequently or seldom, standardized or not—but the student is not encouraged to make literal use of the apparatus of supply and demand curves. Both books spend time discussing monopolistic elements in real-world markets, but most of the discussion is institutional. There is, of course, no serious treatment of monopoly price because there was very little known at the time.

I do not want to be misunderstood. Garver and Hansen and Slichter were serious people. Their reflections on the workings of the economy are worth reading. They inspire bursts of nostalgia; words like "civilized" came to mind. The point is that the modern text takes a different approach. Of course it explains more; the intervening sixty years of economic research have not been wasted. But it is the tone that I want to emphasize. The modern text treats economics as a collection of analytical tools to be applied quite directly to observable situations.

It is plain from this comparison that there was a significant change between 1940 and 1990 in economics as a discipline and also in the way it sees itself. Perhaps this sea change deserves to be called a transformation. One way to describe it is to say that economics became a self-consciously technical subject, no longer a fit occupation for the gentleman-scholar. And I mean that literally: nowadays economists arrive at their conclusions by using an evolving collection of analytical techniques, most of them non-intuitive, the sort that have to be learned laboriously. The shift of the center of gravity from Great Britain to the United States (and to the G.I. Bill veterans at that) may have helped the process along. Judicious discussion is no longer the way serious economics is carried out. Of course, that is not all that happened in fifty years. A lot of new knowledge was acquired, most of it by virtue of those analytical techniques. New branches of economics appeared, some of them because new facts and institutions emerged, some of them for internal intellectual reasons. Not many subfields seem to have disappeared, though there was some rearrangement as a more unified macroeconomics absorbed segments like "business cycles." At the most general level, however, the change in tone was as I have described it.

Many outside observers and some critics from within the profession have interpreted this development as a sweeping victory for "formalism" in economics. The intended implication is that economics has lost touch with everyday life, that it has become more self-involved and less relevant to social concerns as it became more formal (and more mathematical). I think that this view of the discipline rests on a misconception about the change in the way mainstream economists go about their work. Barking may well be justified, but not up the wrong tree.

If "formalist economics" means anything, it must mean economic theory constructed more or less after the model of Euclid's geometry. One starts with a few axioms, as close to "self-evident" as they can be—although this is harder to do when the subject matter is more complicated than points and lines in a plane—and then tries to work out all the logical implications of those axioms. Formalist economics starts with a small number of assumptions about the behavior of individual economic agents, and a few more about their interactions with each other, and goes on to study what can then be said about the resulting economic system.

The past fifty years have indeed seen formalist economics grow and prosper. But it has not grown very much. Only a small minority within the profession practices economic theory in this style. To tell the truth, not many more pay any attention at all to formalist theory. Generally speaking, formalists write for one another. The formalist school contains some extraordinarily able people, and of course it attracts economists who not only are talented at mathematics of a certain kind but enjoy it. It is not surprising, therefore, that outsiders think that there is a lot of formalism in economics, just as half a cup of blood spread around a bathroom can make it look like a scene from *Psycho*. Nevertheless, it is an illusion. Modern mainstream economics is not all that formal.

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What the outsider really sees is *model-building,* which is an altogether different sort of activity. In college classrooms in the 1940s, whole semesters could go by without anyone talking about building or testing a model. Today, if you ask a mainstream economist a question about almost any aspect of economic life, the response will be: suppose we model that situation and see what happens. It is important, then, to understand what a model is and what it is not.

A model is a deliberately simplified representation of a much more complicated situation. (I have no reference for this, but I think I remember that the philosopher J. L. Austin wrote somewhere that "one would be tempted to describe oversimplification as the occupational disease of philosophers if it were not their occupation." Exactly.)

The idea is to focus on one or two causal or conditioning factors, exclude everything else, and hope to understand how just these aspects of reality work and interact. There are thousands of examples; the point is that modern mainstream economics consists of little else but examples of this process.

What follows are three of them, described in the sketchiest terms. Suppose we are interested in the effects of taxation on the willingness to work. (God knows that is a reasonable thing to be interested in.) The usual approach goes something like this: Imagine a typical person of working age who enjoys both consumer goods and leisure, and whose tastes for them can be described in a simple and well-behaved way. This person has a certain amount of nonwage income, from property or from transfer payments of various kinds. He has the option of working any number of hours at a wage rate determined by the market. Part of his income is taxed away according to some known schedule. We have to assume that this person does the best he can to satisfy his tastes for leisure and for the goods that his after-tax income can buy.

We now ask the question that led to this model in the first place. How will he respond to higher tax rates—by working more or fewer hours? If he makes no adjustment, he will have the same amount of leisure time but have fewer goods. That may suggest that he work longer hours, giving up some leisure time for more goods. With the higher tax rates, however, each hour worked brings less in the way of goods, suggesting that work has become less attractive. He may choose, therefore, to work fewer hours. It may make a difference whether the tax system imposes different rates on wage and nonwage income. Perhaps it depends on the details of his preferences; not every person need react in the same way. This model asks for some deeper analysis, which it gets.

Notice all the casual oversimplifications. Not everyone can choose how many hours to work. People do not buy "consumer goods" in general; they buy hundreds of different things, some of which go particularly well with leisure. Some people, but not others, have some control over the intensity with which they work. There are customs and norms that affect the behavior of different groups. All of this sort of talk is cheap. The point of the exercise is to simplify and see where it leads. Alternative simplifications are possible, and making those choices is the art of the model-builder. How do we judge success? It is a good question, and I will return to it soon.

Here is a different type of example. Anyone who has looked at the history of business cycles knows that net investment in inventories by businesses is highly volatile and can easily account for most of the top-to-bottom change in production during a recession. It is therefore a matter of some importance that we understand the nature of inventory fluctuations. There are plenty of reasons for firms to hold inventories and to change the amount of inventories they hold. Production schedules are efficient when they are smooth, but sales can fluctuate unpredictably (or predictably, as from season to season). Inventories of finished goods provide a buffer, enabling firms to meet a fluctuating demand with smooth production. Inventories of goods-in-process and, to a lesser extent, raw materials and components may be tied fairly closely to current production. Some firms build up inventories in anticipation of future sales, or they may try to run their inventories down if they expect sales to be slack. Inventories of raw materials may provide a way to speculate on the prices of raw materials, buying more than needed when the price is low and using up the surplus when the current price is high. Finally, firms may find themselves with inventories that are lower or higher than they actually want: higher because sales have been disappointing, lower if sales have been unexpectedly strong. Even this list is not a complete inventory of reasons for holding and changing inventories. And there are potentially important conditioning factors that have been completely left out: relations with suppliers and customers and financial constraints, for instance.

Modeling inventory fluctuations is a matter of finding a way to represent some or all of these motives so that they can be weighed against one another in much the same way that a profit-seeking firm will have to weigh them as it decides what to do. Notice that last month's unintended inventory fluctuations will have an effect on this month's plans, so that the behavior to be described has a dynamics of its own. How do we judge success? Good question, and I will come to it soon.

Lastly, I give yet a third example because it illustrates a quite different point. Ten years ago, Elhanan Helpman modeled a group of countries trading with one another under very special circumstances. Each country specialized completely in producing a single variety of good. In the eyes of consumers, each country's "own" variety served as a symmetrically imperfect substitute for each other country's variety. Consumers, however, all had the same set of tastes, no matter where they lived. Under these restrictive assumptions and a few others, he showed that there would be a simple formula relating the volume of a country's trade to its size. In reality, countries do not specialize in producing a single good, and consumers do not have the same tastes wherever they are. Nevertheless, Helpman's formula seemed to work quite well for a group of OECD (i.e., advanced) countries. The moral might be that, in reality, production patterns are a lot more specialized than tastes.

Recently, however, other economists tried out the Helpman formula on a group of non-OECD countries, including some in Latin America and Africa. It seemed to work pretty well for them too. Paradoxically, perhaps that success casts some doubt on the Helpman model: one would not expect the less advanced countries to exhibit the same specialization in production and commonality of tastes that is plausible for OECD countries. After all, there may be quite different models that imply a similar relation between the size of a country and the volume of its trade. It appears that measuring success may not be a simple notion.

A good model makes the right strategic simplifications. In fact, a really good model is one that generates a lot of understanding from focusing on a very small number of causal arrows. Model-building is not a mechanical process. Some people are better at this sort of thing than others. Economic models are usually stated mathematically, but they do not have to be. They can be described in words, as I have been doing, or in diagrammatic form, or in computer flow charts for that matter. But mathematics turns out to be a very efficient way to express the structure of a simplified model and it is, of course, a marvelous tool for discovering the implications of a particular model. That is probably why outsiders tend to think of model-building as just more formalism. That is a mistake. The mere use of mathematics does not constitute formalism. Maybe the sharpest way to make this point is to say that the mathematics in these models is almost never deep. There are exceptions, of course. Nevertheless I venture the estimate (safe because it is unverifiable) that there is little or no correlation in fact between the difficulty or mathematical depth of an economic model and its value as science. God is in the details, or perhaps in the absence of details. There is something to be said for both.

The interesting question is *why* economics stopped being clubbable and became technical sometime in the 1940s and 1950s, and *why* model-building took over as the standard intellectual exercise. I think one has to allow for the possibility that it is, after all, the best way to do economics, and we are just seeing the survival of the fittest in another context. That would be Whig history with a vengeance. I confess to some sympathy with that view, but only within limits. I would add that the model-building approach is peculiarly vulnerable to unproductive controversy of a particular kind. I will discuss this later when I talk about measuring success (one model at a time).

I have a different hypothesis to suggest—that technique and model-building came along with the expanding availability of data, and each reinforces the other. Each new piece of information about the economy, especially if it is quantitative information, practically sits there and begs for explanation. Someone will eventually be clever enough to see that it is now feasible to construct a model. Reciprocally, alternative models have to compete on some basis. They are not usually fancy enough to compete on the basis of elegance or depth or the intellectual equivalent of pectoral development. They compete on the basis of their ability to give a satisfying account of some facts. Facts ask for explanations, and explanations ask for new facts.

There is another partner in this evolutionary spiral: the development of new methods of data analysis and statistical inference. The other highly visible change in the style of academic economics since 1940 has been the explosion of econometrics from an esoteric minority taste to an essential part of a Ph.D. education—at least one chapter in most of the dissertations produced in a major department. I will say a little about this vertex of the triangle later.

The spread of model-building coincided in time with the development and diffusion of Keynesian economics. This was an accident, but an accident with consequences: the heyday of Keynesian economics provides a wonderful example of the interplay among theory, the availability of data, and the econometric method. *The General Theory* dates from 1936; Simon Kuznets's book on national income accounting appeared in 1938.[3] Both were no doubt related to the depression of the 1930s, but that is just history. The point is that Keynesian theory needed the national income and product accounts to make contact with reality, and the availability of national income and product accounts made Keynesian macroeconomics fruitful (and helped to shape it).

When I mentioned at the very beginning that my freshman textbook of money and banking was a bore, this is what I had in mind. It was the only vestige of macroeconomics that we were taught—although the unemployment rate in 1940 was still about 14 percent!—and it consisted of a few details about the fractional-reserve banking system and the way it provides credit and generates cash. There was such a thing as business-cycle theory, but it was regarded as a sort of special topic. The textbook writers before 1940 had neither the theory nor the data required to give a coherent account of macroeconomics as part of the core of the subject.

Keynes more or less invented macroeconomics. He was not much of a model-builder himself, but he opened up a gold mine for those who came after. Suddenly there were models of aggregate consumption and aggregate investment, small but complete models of aggregate output and employment, and data against which they could be tested and perhaps improved. Econometricians had new problems of statistical inference to solve. And it all seemed so *important.*

The General Theory was and is a very difficult book to read. It contains several distinct lines of thought that are never quite made mutually consistent. It was an extraordinarily influential book for my generation of students (along with John Hicks's *Value and Capital*[4] and Paul Samuelson's *Foundations of Economic Analysis*[5]), but we learned not as much from it—it was, as I said, almost unreadable—as from a number of explanatory articles that appeared on all our graduate-school reading lists. These articles reduced one or two of those trains of thought to an intelligible *model,* which for us became "Keynesian economics." The most important of those articles were by John Hicks and Oskar Lange, but there was a whole series of them, by Brian Reddaway, David Champernowne, and others. This story provides a different sort of illustration of the clarifying power of the model-building method.

It is very likely that the war, as much as the depression, worked in the same direction. The panoply of wartime policy—Treasury finance, price and production controls, logistics of various kinds—involved economists in social engineering. Any routinization of policy, even nonintrusive policy, leads inevitably to technical questions. What will be the consequences if we do A? Vague generalities will not do for an answer; demands for quantification are just around the corner. Policy A can usually be undertaken with more or less intensity. The powers that be will not only want to quantify the consequences but they will have the power to measure where no one had measured before. Models happen.

If they happen in connection with the availability of data, as I have suggested, then success will be measured by the ability to "explain" the data. Fitness is goodness of fit. (I put "explain" in quotes to emphasize that there need be no claim to fundamental explanation. A model of inventory accumulation will likely eventuate in an equation that relates inventory spending to a small number of observable variables. If that equation actually holds to a fair degree of approximation, the model explains the data.)

There is, however, a twist, and I think it is important. If the logic of model-building, in economics anyway, is a drastic simplification, then one cannot expect any model to fit the facts in every detail. There are examples of models—and not only in economics—that have been judged to be very successful because they manage to account for fairly gross, large-scale patterns that are actually observed and measured. In practice, two consequences seem to follow.

The first is a persistent temptation to add explanatory variables in order to improve the fit. The variables do not follow from the model, or else they would already be there. But it is usually easy to think of reasonable auxiliaries, things that "should" plausibly affect inventory investment even if they were not included in the original, narrowly-focused model. Trouble arises because data are scarce in economics; more cannot be generated by experimentation. So there is a danger of "overfitting": adding variables that work in the data at hand but will turn out to be irrelevant in the next batch, making them therefore deeply irrelevant.

This plays into the second and more fundamental problem. In the nature of the case it will often happen that two quite different models can fit the facts just about equally as well. No doubt the right way to proceed is to think of circumstances in which the two models give widely different predictions and to look around for real-life situations that offer the opportunity to discriminate between them. But that may not be possible. (Chemists can do experiments. There is a movement that does experimental economics, but I cannot guess how far it can go.) So naturally the temptation becomes irresistible to compete by adding variables, making slight changes in formulation, looking around for especially favorable data, and otherwise using the tricks of the trade. It can become very difficult ever to displace an entrenched model by a better one. Clever and motivated—including ideologically motivated—people can fight a rearguard battle that would make Robert E. Lee look like an amateur. (And, of course, they may turn out to be right.) Old models never die; they just fade away. So the model-building approach to economics has its problems. But it is what we have: not formalism, and not the more discursive approach that began to break up in the 1940s and is now long gone.

As this description suggests, model-building economists tend to be natural-born, loose-fitting positivists. Progress will come from weeding out empirically unsuccessful models and improving and extending those that survive empirical tests. This is not to say that mainstream economists think explicitly about method. Philosophical tendencies may come and go. They are attended to only by a tiny fringe of economists who care about formal methodology. Their arguments make no dent in the mainstream, which goes on making and testing models.

It may be useful if I tuck in here a brief commentary on recent and current controversy within academic macroeconomics, as seen from the point of view advanced in this essay. (David Kreps's discussion is similar, but not identical.) This was not part of my original plan, but the controversy is highly visible. The conference held at the Huntington Library in March of 1995 seemed to have a lively interest in the details. Most intriguingly, however, the controversy is often presented as a dispute between formalists and informalists. It would not damage the argument I have been making if there were an element of truth in that characterization. Maybe there is, a little. But in fact I think the story is ultimately a strong confirmation of the thesis of this essay.

In the mid-1970s, the standard textbook treatment of macroeconomics was recognizably "Keynesian" or "American Keynesian." It aimed specifically to provide an aggregative model of the whole economy that could give some sort of analytical account of unemployment, excess capacity, and recession (and their opposites) as pathologies of the market economy. Opposition comes from a school of thought that did in fact invoke the equally standard formal theory of a capitalist economy (the theory of general competitive equilibrium). This school pointed out that this (microeconomic) model had no room for unemployment, excess capacity, and recession, and made the (somewhat) formalist appeal that mainstream economists were in the position of teaching on Tuesday and Thursday a macroeconomics that was fundamentally incompatible with the microeconomics taught on Monday and Wednesday. I do not think that the appeal to "microfoundations" amounted to much. Macroeconomic hypotheses had always been justified by some sort of appeal to microeconomic reasoning.

Nevertheless it was in part an appeal to formal criteria, and there was considerable force to this logic. But the "New Classical Macroeconomics" then faced the problem of explaining, or explaining away, the fluctuations in aggregate income and employment that constitute the everyday history of prosperity and recession. And this task had to be performed within the framework of a formal theory that seemed to exclude even the possibility of the events to be explained. I will not recount the ingenious proposals that were invented to perform this feat because they were ultimately felt to be implausible. No empirical successes were forthcoming. This approach languished.

The original New Classical Macroeconomics evolved into, or was superseded by, a related style of modeling called "Real Business Cycle Theory" ("real" means "nonmonetary"). And now we cut to what I take to be the chase in this narrative. The goal of Real Business Cycle Theory was the same: to show that the everyday experience of economic fluctuations could indeed be accounted for within the framework of formal general equilibrium theory, without the "impure," "ad hoc," "Keynesian" violations of standard principles. In doing so it proceeded to abandon formalism in all but name by canonizing one very simple, very special, and very maneuverable version of competitive general equilibrium—in fact, by adopting a highly specific model. It is a model of an economy populated by a single immortal family with perfect foresight. The industrial and market structure of the economy is such that it carries out, step by step, the infinite-horizon optimal plan of the single "representative consumer." The generality that is the hallmark of formalism is gone.

The gimmick is that the economy is disturbed by irregular, unforeseeable changes in the preferences of the representative consumer and/or in the technology available to the industrial sector. Economic fluctuations are thus not pathological at all; they are the best that can be done by way of adapting to these pleasant and unpleasant surprises. The model itself was already there to be used. The bulk of the intellectual effort goes into the ways of showing that the data of observed fluctuations are compatible with the demands of the model. This is not easy because the key driving forces—irregular changes in tastes and technology—are not directly observable.

So this is formalism, in more or less a window-dressing sense. In practice it is a little bit of model-building and a lot of fairly sophisticated data analysis. It is not a revolution or transformation in the way macroeconomics is done.

To be sure, there has been a dramatic change in doctrine. One genealogy of models has replaced another. One set of implications has replaced another. This was a genuine shift of ideas, perhaps related to events of the 1970s that were, at least temporarily, hard to explain with older models, perhaps related to the general mood of conservatism and suspicion of government action that affected economists as well as others. Such shifts occur from time to time, in macroeconomics and elsewhere. This one did not amount to a significant move toward formalism. The new doctrine does try to appropriate an air of "rigor"—a standard ploy—but this is mostly advertising.

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My reading of the current state of affairs is much like Kreps's. In the course of massaging the model to make it conform to the facts, the more adventuresome advocates of Real Business Cycle Theory have found it necessary to modify many of the clean but extreme assumptions that give formal general equilibrium theory its artificial vanilla flavor. As the representative-consumer-with-perfect-foresight model has been extended to allow for elements of imperfect information, imperfect competition, imperfect flexibility of prices, incomplete markets, and a teeny bit of heterogeneity among the inhabitants, it has come closer and closer to the more or less "Keynesian" model it was supposed to discredit. It is possible—though surely not inevitable—that in another decade all that will be left are some purely technical differences in modeling strategies plus an underlying difference in spirit, with one side regarding all those imperfections as (removable?) flaws in the system and the other side regarding them as the essence of the system itself.

Seen this way, the macroeconomic controversy (made more intense by ideological freight) only makes the model-building tradition seem pretty irrevocable. But then what about a historical approach to economics? Is such a thing viable? The issue is worth discussing, and it will shed some further light on the main argument. In one sense, economics *is* history. I have been insisting that the modern approach to economics is mostly about accounting for data. It is hard to imagine where else data can come from but the past. So economics is about accounting for the past. Most of the time it is the recent past, but there is no reason why the more distant past cannot be treated in the same way, if only the relevant data are available or can be reconstructed.

When I studied economic history as a graduate student at the end of the 1940s, my teacher was A. P. Usher. I read his *History of Mechanical Inventions,*[6] Clapham on British economic history, and sections of various works on monetary history. They were long on narrative and short on analysis, a lot like the elementary textbooks of a decade earlier. It did not occur to me then, as it has since, that the more distant past provides something potentially valuable to the model-building economist. A good model embodies *accurately* a representation of the institutions, norms, and attitudes that govern economic behavior in a particular time and place. There is no reason to presuppose that a successful model of the supply of labor in the second half of the twentieth century will apply unchanged to the nineteenth century when institutions, norms, and attitudes were different. Long runs of history offer the economist or historian or economic historian the chance to figure out how changes in the "noneconomic" background factors have an influence on behavior in the narrowly economic realm. It is a little like being able to extend the range of temperatures or pressures available in a laboratory.

I am not sure that is how it has worked in practice. One thing is certain: the same progression from discursiveness to model-building has happened in economic history as in economics. Economic historians have become a lot more like economists than economists have become like economic historians. Today's economic historians are very likely to be model-builders. I have the disappointing impression that they are far too willing to accept the models devised by late twentieth-century economists and apply them uncritically to the data of other times and places. There are certainly some sterling exceptions who do properly exploit the advantages offered by exotic data; my insider informant says they are few and far between. There are a few who use the study of the evolution of institutions as a laboratory for economic analysis under unconventional assumptions. There are even some who continue to do narrative economic history. On the whole one has to report that the historical approach to economics has lost most of its distinctiveness and is losing the rest. This seems to be a case of not being able to lick 'em, or not wanting to.

The case is much the same with respect to the other social sciences. I am tempted to guess that economics has drawn further away from the other social sciences in the past half-century. But the truth is that there was little or no interchange even in 1940 or 1950. Despite the existence of the occasional sport like Richard Thaler or George Akerlof, who learned from the other social sciences, most of the flow of ideas is in the other direction. There are subcultures in political science and sociology that seem to want to adopt the methods, the terminology, and sometimes the assumptions of economics. Those *Wahlverwandtschaften* are best discussed from within the other disciplines. Richard Swedberg's *Economics and Sociology* is full of interesting material,[7] but I am less optimistic than he is about any systematic development.

Some sociologists and political scientists are drawn to the way economics uses rationality—in effect, constrained maximization—as an organizing principle and as a source of ideas for model-building. You could do a lot worse. But there is an irony tucked away in that remark. Some economists, though not many, would like to look to sociology as a way of escaping from the narrow idea of rationality. Actually, that way of putting it is not quite right, so I shall try again. The program of constrained maximization has to rest on a careful statement of what is being maximized and what the constraints are. Mainstream economics takes a narrow view of both; some hardy souls would like to try out a wider range of assumptions. They look to sociology and social psychology as a source of alternative ideas. On the whole they do not find what they are looking for, though again there are notable exceptions.

This is not anybody's fault. The writings of people like Jon Elster, Mark Granovetter, Arthur Stinchcombe, and Aage Sørensen—just to take those who are closest to the economists' wavelength—are full of interest to those all too few economists who read them. But they do not provide the usable raw material (or intermediate product) that is being sought. Even a book like Elster's *The Cement of Society,*[8] intelligent as it is and on exactly the right subject, does not send an economist racing to the drawing board. I suppose, though without much confidence, that this failure to connect may arise because the other social sciences have not adopted the model-building philosophy that motivates and guides economists. Experience has taught me that I should say explicitly that I have no neocolonialist designs: sociology may be right to stay away from model-building as a mode of thought. Adjacent territories may adopt different track gauges for good and sufficient reasons, but their railroads will have problems at the border crossings.

It might be useful for me to say some fairly informal things about the analogies between economics and the natural sciences. It is an uncomfortable task. I have read the usual quota of layman books and, after forty-seven years on the faculty at MIT, I have a lot of friends who are physicists, chemists, and biologists, not to mention engineers. But it is perfectly clear to me that I have no real sense of what goes on in a physicist's or biologist's mind. Still, it is a topic that often comes up in cross-disciplinary discussion.

There is no doubt that economists are attracted to the style of explanation they see (or think they see) in physics. This is at least clear in the externals. Economists feel at home with equilibrium conditions deduced from first principles or from reliable empirical statements. Similarly, they are used to deducing dynamics from local assumptions or generalizations; economics is full of differential or finite-difference equations. All this seems fairly harmless, as long as it works. It will occasionally turn out that some piece of economics is mathematically identical to some piece of utterly unrelated physics. (This has actually happened to me, although I know absolutely nothing about physics.) I think this has no methodological significance but arises merely because everyone playing this sort of game tends to follow the line of least mathematical resistance. I know that Philip Mirowski believes that deeper aspects of mainstream economic theory are the product of a profound imitation of nineteenth-century physical theory. That thesis strikes me as false, but I would not claim expert knowledge.

To the extent that economists have the ambition to behave like physicists, they face two dangerous pitfalls. The first is the temptation to believe that the laws of economics are like the laws of physics: exactly the same everywhere on earth and at every moment since Hector was a pup. That is certainly true about the behavior of heat and light. But the part of economics that is independent of history and social context is not only small but dull.

I want to suggest that a second pitfall comes with the imitation of theoretical physics: there is a tendency to undervalue keen observation and shrewd generalization, virtues that I think are more usually practiced by biologists. There has long been a tendency in economics to promote biology as an analogy. Even a genuinely great man like Marshall took this line. Most of what is said on this subject is a hopelessly vague use of unexamined analogy, uninformed by biological theory. I am making a much weaker point, that there is a lot to be said in favor of staring at the piece of reality you are studying and asking, just what is going on here? Economists who are enamored of the physics style seem to bypass that stage, to their disadvantage.

There is another respect in which a broader biological analogy might be relevant. Many economists have noted that the evolutionary paradigm ought to be a useful way of doing business. In isolated instances it has already been valuable in economics, but perhaps a little less so than might have been expected. I attribute this to the absence of any close parallel to the quantitatively analyzable transmission mechanism provided in biology by population genetics. Now, with the rapid development of evolutionary game theory, there may be an opening for real progress. The loop closes, because what is now needed is a body of data to be exploited by the evolutionary game-modeler.

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Nothing that has been said in this essay is complicated enough to require summary. Since part of my aim has been to dispel a misperception, I should conclude by making another pass at explaining how the misperception has come to be so widely accepted. Many observers in the other social sciences and in the wide, wide world perceive that economics has become formalistic, abstract, negligent of the real world. The truth is, I think, that economics has become technical, which is quite different. (Nobody regards computer-aided tomography as formalistic.) Far from being unworldly, modern model-builders are obsessed with data.

How could this confusion arise? I have already suggested that it may be the trappings of mathematical model-building that gives the wrong impression to outsiders. Now I will try out another thought. There is a tendency for theory to outrun data. (This includes statistical theory as well as economic theory.) Theory is cheap, and data are expensive. Much the same thing seems to happen in high-energy physics. I am told that the very latest ideas in particle theory could not come close to being tested with any current accelerator or even with the superconducting supercollider if it were to be built. No one even knows how enough energy could be mobilized to do the experiments that might confirm today's most advanced speculations.

In economics, model-builders' busywork is to refine their ideas to ask questions to which the available data cannot give the answer. Econometric theorists invent methods to estimate parameters about which the data have no information. And, of course, people are recruited whose talent is for just these activities, whose interest is more in method than in substance. As the models become more refined, the signal-to-noise ratio in the data becomes very attenuated. Since no empirical verdict is forthcoming, the student goes back to the drawing board—and refines the idea even more. Oscar Wilde described a fox hunt as the unspeakable in pursuit of the inedible. Perhaps here we have the overeducated in pursuit of the unknowable. But it sure beats the alternatives.

ENDNOTES

[1]John Maynard Keynes, *The General Theory of Employment, Interest and Money* (London: Macmillan, 1936).

[2]Sumner H. Slichter, *Modern Economic Society* (New York: H. Holt and Company, 1935).

[3]Simon Smith Kuznets, *National Income and Capital Formation, 1919–1935* (New York: National Bureau of Economic Research, 1938).

[4]Sir John Richard Hicks, *Value and Capital: An Inquiry into Some Fundamental Principles of Economic Theory* (Oxford: Clarendon Press, 1939).

[5]Paul Anthony Samuelson, *Foundations of Economic Analysis* (Cambridge, Mass.: Harvard University Press, 1947).

[6]Abbott Payson Usher, *A History of Mechanical Inventions* (New York: McGraw-Hill, Inc., 1929).

[7]Richard Swedberg, *Economics and Sociology: Redefining Their Boundaries* (Princeton, N.J.: Princeton University Press, 1990).

[8]Jon Elster, *The Cement of Society: A Study of Social Order* (Cambridge: Cambridge University Press, 1989).