Hayek's Challenge

An Intellectual Biography of F. A. Hayek

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CHAPTER 15

Epilogue: A Meditation on Twentieth-Century Economics

I begin my epilogue with two quotations from Hayek, quotations whose relevance will soon become apparent:

> But if it is true that in subjects of great complexity we must rely to a large extent on such mere explanations of the principle, we must not overlook some disadvantages connected with this technique. Because such theories are difficult to disprove, the elimination of inferior rival theories will be a slow affair, bound up closely with the argumentative skill and persuasiveness of those who employ them. There can be no crucial experiments which decide between them. There will be opportunities for grave abuses: possibilities for pretentious, over-elaborate theories which no simple test but only the good sense of those equally competent in the field can refute. There will be no safeguards even against sheer quackery. Constant awareness of these dangers is probably the only effective precaution. But it does not help to hold up against this the example of other sciences where the situation is different. It is not because of a failure to follow better counsel, but because of the refractory nature of certain subjects that these difficulties arise. There is no basis for the contention that they are due to the immaturity of the sciences concerned. It would be a complete misunderstanding of the argument of this essay to think that it deals with a provisional and transitory state of the progress of those sciences which they are bound to overcome sooner or later. (Hayek [1955] 1967a, 19)

> All these things I've stressed—the complexity of the phenomena in general, the unknown character of the data, and so on—really much more point out limits to our possible knowledge than are contributions that make specific predictions possible.
>
> This is, incidentally, another reason why my views have become unpopular: A conception of scientific method became prevalent during this period which valued all scientific fields on the basis of the specific predictions to which they would lead. . . . The aim of science, in that view, was specific prediction, preferably mathematically testable, and somebody pointed out that when you applied this principle to complex phenomena, you couldn't achieve this. This seemed to people almost to deny that science was possible. Of course, my real aim was that the possible aims of science must be much more limited once we've passed from the science of simple phenomena to the science of complex phenomena. (Hayek 1983b, 191–92)

I choose to call my closing chapter an *epilogue* to signal to the reader that it will be more personal than what has come before. As I noted in the introduction, I am an economist whose area of specialization is the history of economic thought. Before I began my research on Hayek, I studied the methodology of economics. In both instances, I hoped that my studies would lead me to a better understanding of why economics developed in the way that it did in the twentieth century.[1] I believe that one of the most important contributions that the study of methodology and doctrinal history can make is to enhance disciplinary self-understanding. The less time devoted to the study of these subjects, the less that economists know about themselves and their work, its potential usefulness and its limitations.

I think that the present study has implications for the way in which we view the discipline of economics. In what follows, I use what we have learned about Hayek, about the puzzles he wrestled with and offered solutions to, about the ideas he developed and those he opposed, to argue for a number of theses.

First, Hayek's major message was one of the limits that we face as analysts of social phenomena. His view of the world contrasted starkly with that proposed by his antagonists, what one might label a *positivist* or *scientistic* view. One can best see the contrast between the two worldviews by seeing what kinds of progress each leads us to expect to occur in a science like economics.

Second, although there has been much progress of a variety of sorts, I argue that the history of economics in the twentieth century lends support to

1. Many others have tried to answer this question, and their explanations range from the ideological to the economic, from the sociological to the pedagogical (e.g., Colander 1992; Brenner 1992; Klamer 1992; Mayer 1993; Mäki 1999; Mirowski 1989, 2002; Weintraub 2002). I think that each of these explanations contains part of the truth, so the further words that I offer should be viewed as supplementing what others have said.

Hayek's views about the *empirical* limits of the discipline. The positivist hope for continuous progress has not been achieved.

Third, things are more complicated when it comes to developments in *theory*. I argue, first, that, when dealing with complex phenomena, the sorts of pattern predictions that *basic economic reasoning* permits are often very useful. Next, I claim that much of what has been taken to be progress in various branches of microeconomic theory has, in fact, been simply the endless reconfiguration of what Hayek's friend Popper called *situational analysis*. Although this reconfiguration has yielded some benefits, we have probably reached the point of diminishing returns for its ability to shed light on complex social phenomena. Finally, I suggest that alternative approaches may well be useful for shedding light on the sorts of questions that Hayek raised and note that a number of recent research programs are more consistent with the way in which Hayek thought the study of complex adaptive systems might best be pursued.

Fourth, a dominant subtext of this concluding chapter is that the effects of positivist or scientistic thought on the profession have been nearly entirely negative. One of the worst effects is to mislead economists about the nature of their own discipline, its prospects, and (perhaps especially) its limitations. A tragic related effect is to cause economists to think that the study of fields like the history of ideas and methodology is unnecessary for the training of economists. The apparently imminent extinction of these fields is a legacy of the scientistic worldview, evidence of its malignant persistence in the way in which economists understand themselves and their discipline.

Hayek and His Opponents on the Prospects for Economics

Hayek's theory of complex phenomena ultimately served as the foundation for many of his methodological conclusions. Among the conclusions that he reached, those of interest to economists might include the following:

Many of the phenomena that economists study are, in fact, examples of complex phenomena.

When we deal with complex phenomena, precise predictions will be impossible.

When we theorize about complex phenomena, usually the best that we are able to do is offer explanations of the principle by which the phenomena occur. Although this may enable us to predict broad patterns of behavior and, thereby, rule out certain outcomes, our ability to falsify theories is diminished. As Hayek put it: "The advance of science will thus have to proceed in two different directions: while it is certainly desirable to make our theories as falsifiable as possible, we must also push forward into fields where, as we advance, the degree of falsifiability necessarily decreases. This is the price we have to pay for an advance into the field of complex phenomena" (Hayek [1964] 1967j, 29).

Because we study complex phenomena, the possibility of final "crucial experiments" is more or less ruled out. As such, the elimination of contending rival theories "will be a slow affair" (Hayek [1955] 1967a, 19).

Finally, "what we can know in the field of economics is so much less than people aspire to" (Hayek 1983b, 258).

The last point sums up Hayek's ultimate conclusion about economics.

How might one go about assessing these assertions?

It should be immediately apparent that, given Hayek's emphasis on limits, there is no way to *establish* his claims. That would involve proving a negative: the limits that economists encounter today may, after all, be gone tomorrow. But one *can* contrast them with the vision offered by Hayek's opponents and then ask whose vision better describes the subsequent history of the discipline. Had the positivists been right, here is what we might expect:

Over the course of the twentieth century, there will be steady, even cumulative, progress as economic laws and law-like relations are discovered and multiplied.

Improvements in empirical methods will allow ever more precise predictions to be made.

Theory change will involve the steady accumulation of a well-corroborated theory base.

Errant theories will be gradually but steadily falsified and eliminated.

As the findings of economic science become more widely accepted, methodological debate across competing paradigms will wither away, as will, indeed, the competing paradigms themselves.

Which vision is more descriptive of economics in the twentieth century? It turns out that this is, in principle, an extremely difficult question to address, not the least because there is neither an agreed-on set of criteria by which to measure scientific progress nor an agreed-on history of economics in the twentieth century against which to test our claims.

Historians of thought have long recognized how difficult it is to characterize a particular period in the past, for assessments of what took place are always contestable. Was the Keynesian revolution a progressive problem shift, as Mark Blaug ([1987] 1990b) once averred, or simply an erroneous generalization based on local problems afflicting Britain in the interwar period?[2] Was the marginal revolution an essential transformation leading to the development of modern microeconomics, or was Philip Mirowski (1989) correct to characterize it as a mimetic mistake brought on by economists' "physics envy"? More generally, and as every reader of this book should by now understand, there are no brute historical facts against which to test our historical constructions. History, like any other empirical basis, is theory impregnated; what we include in it depends on our prior theories about what is deserving of attention. These problems vex, not just economists, but anyone trying to construct the history of a science or to assess its theories (Losee 1987).

In the current context, we can add another layer of problems in that, to answer the question before us, I must try to characterize what might be called the *very recent* history of economics. Given the huge number of changes that have taken place in economics in the past few decades, this is an undertaking more suited to an encyclopedia than to the final chapter of a book. What follows, then, is necessarily suggestive and, I hope, provocative: the latter in the sense that perhaps my speculations will provoke others to look into these matters more carefully and in more depth. With these important caveats in mind, let us now turn to the recent past and compare it against the visions of Hayek and his opponents.

Empirical Work in Economics

Even if we acknowledge the difficulty of defining *scientific progress*, it should still be clear that, by almost any definition, there has been an immense amount of progress in the area of empirical work. The profession has at hand better statistical techniques, better data, and ever more powerful computers. Huge data sets are now available, panels of data tracking all sorts of variables

2. For views that contrast with Blaug's characterization of the Keynesian revolution, see Hands (1985b) and Caldwell (1991c).

across households and through time, and the techniques to squeeze information from them seem to multiply no less promiscuously. I work in a largely empirical department of economics, and the econometric techniques that people of my generation learned as Ph.D. students are easily eclipsed by what we teach our master's students today. And, of course, modern computers are ever smaller, ever more powerful, faster, and cheaper: as far as computing goes, there have been improvements along virtually every dimension that counts. Perhaps even more impressive than the speed at which such improvements have taken place is the pace at which they are anticipated to occur in the future.

It may be, in fact, that we are only just at the beginning of a new era. Even so, had Hayek's opponents been right, one would think that, by now, we would have at least started seeing some results in economics: more precise predictions, the discovery and establishment of empirical laws, and, perhaps, even a policy payoff in terms of a better ability to fine-tune or to command and control our national economies.

I do not think that I am being overly provocative if I assert that that has not been what has happened so far. There has not been, in the first instance, a steady accumulation of well-established empirical laws. Terence Hutchison, who has, as we have seen long been an advocate of more empirical work in economics (and especially of the testing of our theories), sought in 1977 to specify more exactly what we mean when we say that we are making a *scientific prediction.* Drawing on the philosopher Karl Popper's idea that "an explanation or prediction should be accepted as 'scientific' if, *and only if,* it is deduced from a universal law that has been well tested and corroborated, and from specific initial conditions which have been independently checked," Hutchison fairly quickly concluded: "Unfortunately this pronouncement seems to rule out 'scientific predictions' in economics. *In fact economists have constantly used, and are constantly using, trends, tendencies, patterns or temporary constancies, as the basis for predictions, because, in fact, they have not available any genuine, relevant, non-trivial laws*" (Hutchison 1977, 15, 21).

More recently, Tony Lawson echoed Hutchison's assessment of the results of the profession's long quest for well-established empirical relations: "Fifty years ago Haavelmo justified his efforts in developing the 'probability approach in econometrics' with the observation that 'economics, so far, has not led to very accurate or universal laws like those obtaining in the natural sciences' (Haavelmo 1944, 15). With the passage of time this situation does not seem to have changed significantly. Econometricians continually puzzle over why it is that 'estimated relationships' repeatedly 'break down,' usually as soon as new observations become available" (Lawson 1997, 70).[3] In his book *Truth and Progress in Economic Knowledge,* Roger Backhouse concluded his own lengthy and careful examination of the nature of empirical progress in economics with these words: "Despite the immense effort, undreamed-of increases in computing power, and the development of vastly more sophisticated statistical techniques, econometrics has failed to produce the quantitative laws that many economists, at one time, believed it would" (Backhouse 1997, 136).

Those who are familiar with the literature on the methodology of economics know that Terence Hutchison, Tony Lawson, and Roger Backhouse seldom agree on *anything.* Yet their assessments of the results of the search for law-like relations in economics are for all purposes identical.

Robert Goldfarb provides additional evidence in support of these claims. He documents twenty-one cases of empirical literatures in economics in which an "emerging recalcitrant result" (ERR) pattern exists: "A number of empirical literatures in economics display the following pattern of results. First, some evidence accumulates that suggests and seems to support an empirical result. As time passes, however, contrary results emerge that challenge or even seem to overturn that initial result" (Goldfarb 1997, 221).[4] Of course, it could be that such a pattern simply demonstrates the steady advance of empirical science. That inference would require that the later results typically be viewed as more reliable than the earlier ones. Unfortunately, this is sometimes, but by no means always, the case. Goldfarb notes the implications of his finding for the use of empirical work to support policy:

3. Lawson (1997) notes that "the Lucas critique" helps explain why the relations break down but that the responses to the critique do not address the fundamental problem, the fact that the social system is an open system for which the usual tools are inappropriate.

4. To give the reader a flavor for the sorts of ERRs that Goldfarb (1997, 222–24) identifies, they include the following: a higher minimum wage lowers employment; public infrastructure investment has a high return in promoting growth; savings are quite responsive to the interest rate; cutting capital gains tax rates raises revenues; plausible estimates of nonobservable contingent values can be obtained using contingent valuation techniques; economists are less cooperative than everyone else; unemployment patterns are dominated by spells of short duration; income and substitution effects in labor are quite large; business taxes have no effect on industry location; and social security has a very large effect in depressing personal savings.

> These emerging contrary results or "potential reversals" present a dilemma for the conscientious economist who is part of an empirical literature's audience. How is he or she to make believable inferences from such a literature, when results may have already been, or in the future be, challenged and even conceivably overturned? Part of the answer would seem to depend on whether the "later" results are very likely to be more reliable than "earlier" results. More generally, an intellectually adequate answer depends on having a good understanding of what might be causing these ERRs. As this paper will show, some ERR mechanisms imply that "later" results are more dependable, while others suggest the opposite or are ambiguous. (222)

The econometrician Edward Leamer has, in such wonderfully titled articles as "Let's Take the Con Out of Econometrics" (Leamer 1983), been in the forefront in pointing out the "fragility" of many econometric results. In a symposium, he offered an assessment that both supports and may help explain some of Goldfarb's findings:

> I have a sense that most economists feel that conclusions from data sets are fragile. Somebody will add another variable, or they will control for some aspect of the time series phenomena in some other way, which will yield a substantially different conclusion. One of the reasons that we don't treat empirical work seriously is that there have been so many cases of fragile conclusions. Somebody claims to have found something, and then six months later a new equation is estimated, and the same finding seems to be reversed. It creates the feeling among economists that conclusions from data are very fragile. (Leamer quoted in Lawson 1997, 301)

What about forecasting, that is, predicting the future values of certain variables of economic interest? It has long been recognized that economic models that try to provide elaborate specifications of the relations that hold among various sectors of the economy are often inferior to less complicated "moving-average" models that simply forecast future values on the basis of past trends (Nelson 1972). As might be expected, while this has caused some econometricians to call for greater efforts to improve the links between econometric models and economic theory, it has caused others to insist that one should allow the data themselves (rather than one's theoretical "priors") to play more of a role in determining the forecast.

The latter approach has actually generated substantial progress. The use of statistical model selection criteria and of cross-validation techniques has greatly improved our ability to distinguish systematic variation from "noise" within any set of data. Given virtually any array of past values for a set of variables, it is now possible to produce a mathematical function that could generate them. Unfortunately, it is also often the case that the systematic component in many variables of interest to economists is small relative to the noise. As for forecasting "turning points" (e.g., the trough of the business cycle or the point at which a decline in interest rates reverses itself), my own departmental econometrician (Peter Bearse) put it succinctly as: "On average, we're wrong." A recent commentary on the dismal performance of a group of thirty-four forecasters in the United Kingdom, reported on by Tony Lawson, adds credence to Bearse's judgment: "Economic forecasters do not speak with discordant voices; [keeping an eye on each other] they all say more or less the same thing at the same time. And what they say is almost always wrong. The differences between forecasts are trivial relative to the differences between all the forecasts and what happens" (J. Kay quoted in Lawson 1997, 301).

Econometrics itself has undergone substantial changes in the past twenty years or so. There are now a number of competing econometric methodologies from which to choose when undertaking an empirical study. In a fascinating recent experiment (Magnus and Morgan 1999), eight teams of researchers were asked to apply differing econometric techniques to a set of problems laid out by the experimenters. Among the tasks that the teams were asked to perform were the following: estimating the income elasticity of food demand in the United States using data that had originally been used in a similar study by James Tobin in the late 1940s; repeating the procedure with a full set of U.S. data; undertaking similar exercises when additional information of various forms from other data sets is available; and forecasting future food demand for the next twelve years. (The teams were also asked to perform a hypothetical policy analysis, but none completed this part of the experiment, which itself may suggest the difficulty of moving from an empirical study to the realm of policy.) A panel of "expert" outside assessors then evaluated the exercise and the econometric methodologies employed.

As might be expected, almost all the groups came up with results very similar to Tobin's when using the original data. After that, the results diverged: "In nearly all the other tasks, we observe a considerable lack of consensus from those same methodologies that gave us consensus in the estimate of the 1941 income elasticities" (Magnus and Morgan 1999, 304). One of the assessors (Anton Barton) commented on the results of his groups' findings as follows:

> The cross-section part of Tobin's original contribution appears to be robust against recent developments of a methodological nature. That is a comforting thought. If our empirical results are very sensitive to the way the data are handled one would feel suspicious about the outcomes in general.
>
> Another feature is the failure of the forecasting part of the experiment. The results show the impact of the within model lack of precision. To this, one should add the uncertainly about the conditioning variables. Is it, in general, true that our knowledge of the future is so inadequate? It is commendable of Magnus and Morgan to have formulated the prediction question, because its answer has revealed a weak spot in our empirical research. (270)

It is difficult to draw any generalizations from the Magnus and Morgan study: it was the first of its kind, and, as its authors freely note, the design of the experiment had (as might be expected in a first-time study) some flaws. The fact that it was the first study of its kind must, however, give pause. Empirical work is ubiquitous in economics. How is it possible that economists have been so uninterested in systematically comparing the results that one might obtain from different approaches? Is it because the fact that the results are different comes as no surprise?

There are many rationales for undertaking empirical investigations in economics, but, surely, one of the most compelling is the hope that empirical studies can provide an impartial means for arbitrating disputes over policy questions. People often hold strong views on questions of public policy. Empirical work can help resolve such differences of opinion—to the extent that they are the result of disagreements over the predicted effects of alternative policies. At least such was the hope fifty years ago, when this position found a persuasive spokesman in the person of Milton Friedman, whose 1953 essay "The Methodology of Positive Economics" was, in many ways, the perfect expression of the optimism concerning the prospects for empirical work that reigned during the positivist era. Noting that "laymen and experts alike are inevitably tempted to shape positive conclusions to fit strongly held normative preconceptions," Friedman explained that positive economic science could help adjudicate such disagreements, especially to the extent that the differences of opinion actually lay in the positive rather than the normative realm, which he thought was often the case:

> I venture the judgment, however, that currently in the Western world, and especially in the United States, differences about economic policy among disinterested citizens derive predominantly from different predictions about the economic consequences of taking action—differences that in principle can be eliminated by the progress of positive economics—rather than from fundamental differences in basic values, differences about which men can only fight. . . . Agreement about the economic consequences of the legislation might not produce complete agreement about its desirability, for differences might still remain about its political or social consequences; but, given agreement on objectives, it would certainly go a long way toward producing consensus. (1953, 4, 5–6)

In their recently published memoirs, Milton and Rose Friedman note that they have long differed in their explanations of why economists sometimes disagree with one another. Milton held out the hope that the differences were over the positive matters, or the predictions that one made concerning the effects of alternative policies. Rose took a different view: "On this issue, my husband and I have always differed though I am inclined to believe that he is moving in my direction. I have always been impressed by the ability to predict an economist's positive views from my knowledge of his political orientation, and I have never been able to persuade myself that the political orientation was the consequence of the positive views" (Friedman and Friedman 1998, 217). Milton in fact confirms that he has been moving in Rose's direction: "As Rose said, by 1976 . . . I was already moving in her direction. I must confess that I have continued to move in that direction and that I am much less confident now that I am right and she wrong than I was four decades ago when I wrote the methodology article" (219). The conclusion that Milton drew from his lifetime of experience is that economists seem to be driven more by values than by the scientific findings of their discipline. But he does not reflect on *why* this is so. He does not appear to consider the possibility that empirical studies taken alone are seldom decisive in determining "the facts of the matter," which means that any study is always a potential candidate for an ERR (to use Goldfarb's language) or that the elimination of contending rival theories "will be a slow affair" (to use Hayek's).

I have compiled here a host of observations concerning empirical work in economics, so perhaps it is time to pause to assess what is being said and, as important, what is not being said. It should be clear that I am affirming the existence of various types of empirical progress in economics—as mentioned, we have better and more varied statistical methods, more powerful computers, and more detailed data. Certainly, we can describe various aspects of the economy far better than we could two generations ago. We have developed the ability to take better snapshots, so to speak, of what the economy looks like. And we have made much progress in distinguishing the signal from the noise in complex sets of data. *But, because the systematic component is typically dominated by the noise in most data of interest to economists, we have been less successful in achieving the long-hoped-for goals of steadily improved forecasts and the discovery of law-like relations.* This should disappoint anyone who clings to the positivist vision of science. On the other hand, it is just what we should anticipate in the empirical realm if we accept Hayek's contention that economics is a field that studies complex phenomena.

As for the role of empirical work in helping us adjudicate among competing theories: perhaps surprisingly, given what has been said so far, it appears to be the case that, as Milton Friedman states in his memoirs, disagreement among economists over positive matters is not as great as many people believe (Friedman and Friedman 1998, 216).[5] Where I differ from Friedman is that I doubt that whatever consensus may actually exist is due solely, or even mainly, to the results of empirical work narrowly defined. It is due, rather, to the training that one receives in becoming an economist and to observing through the eyes of an economist the way the world works.[6]

I once told a colleague that a pretty good definition of an economist is someone who *knows* that demand curves slope downward. We know this, not

5. At least this seems to be true with respect to microeconomics. Kearl et al. (1979) provide evidence of consensus among economists on economic issues. Their finding that there tends to be more consensus on microeconomic than on macroeconomic issues, and on positive than on normative issues, was confirmed in Alston, Kearl, and Vaughn (1992).

6. Interestingly, surveys have revealed less consensus among European than among American economists on positive issues. Given that Europeans appear to be more comfortable with state intervention in the economy than are Americans, this again suggests that Rose rather than Milton Friedman may have the better argument on what drives people's reading of evidence. Both groups tend to respond to the empirical evidence in ways that allow them to preserve their normative priors.

because empirical work has proved it, but because, if one is going to reason like an economist about any social phenomenon, one *must* begin from this and similar ideas, ideas that were earlier identified by the likes of Lionel Robbins. Deirdre McCloskey (1985, 57–62) makes the point persuasively when she notes that, were we to rely on empirical studies for evidence of the relation, we would be hard-pressed to have any confidence in the finding since empirical studies do not always give the "right" results. Luckily (luckily, that is, if one is an economist), there are many other reasons to believe that demand curves slope downward (McCloskey lists eight, ranging from introspection and thought experiments to analogies and recent economic history); economists therefore need not worry too much about not having proved the fact empirically.

One may wonder why I am being so sanguine about this apparently embarrassing state of affairs. There are two reasons. First, the belief that demand curves slope downward is one of the beliefs that informs what I have called *basic economic reasoning,* and *it is the power and usefulness of such reasoning that constitutes the best defense of the belief,* more of which anon.[7] Second, it strikes me that taking a sanguine attitude about this matter is the first step in throwing off the shackles of the positivist vision of science that has so dominated economics in the past century.

Basic Economic Reasoning, Pattern Prediction, and Explanations of the Principle

Let us turn next to theoretical work in economics, for which, according to Hayek, "explanations of the principle" and "pattern predictions" are the best

7. The attentive reader will notice the similarities between my statement here and my previous argument that it was the ability of Menger's *Principles* to provide causal-genetic explanations for many social institutions that provided the best argument for his methodological defense of a theoretical approach to the social sciences in the *Investigations.* In a phrase: Results matter.

Of course, not everyone *does* believe in the results of basic economic reasoning. This has been true throughout the history of the discipline. And the inability of empirical work to establish the arguments of one side or the other should guarantee that this situation is a perennial one. Each generation has to hash it out anew with the best tools at its disposal because the tools will never definitively establish one or the other side as having won.

that we can expect. When Hayek was writing about pattern predictions and explanations of the principle back in the 1950s, most of his examples were drawn from fields outside the social sciences, like biology. Indeed, his whole point in many of these writings was to show his readers that other sciences, in fact, study complex phenomena so that they would accept it as a legitimate model for economics.

What is the relation between the terms *pattern prediction* and *explanations of the principle?* Hayek's usage is pretty clear. When one studies complex phenomena, often the best that one can do is explain the principle by which the phenomenon of interest operates. For Hayek, this implied that only pattern prediction (rather than precise numerical prediction) is possible. Although Hayek's meaning is clear enough, it seems to me that, in the light of how economics has come to focus much more on economic modeling since Hayek's day, there may be a more effective way to conceptualize the relation between the two terms. When I use *explanation of the principle,* I will be emphasizing an explanation of the principle *by which* something works, an explanation that says *how* or *why* something works the way it does. I will use *pattern prediction,* on the other hand, merely to indicate that we are able to make only a qualitative (rather than a quantitative or precise numerical) conditional prediction about some phenomenon of interest.

If we use these rough-and-ready definitions and consider the microeconomic theory of Hayek's day, which in its partial equilibrium instantiation is the stuff of our own present-day undergraduate microeconomics textbooks, it appears that the notion of pattern prediction describes what microeconomic theory is able to accomplish pretty well.[8] (I will hold off on an explication of the role of "explanations of the principle" until a little later.)

So, for example, when I tell my students that price controls lead to a

8. In what follows, I restrict myself to a discussion of microeconomics. I do so for three reasons: there is (as noted in n. 5 above) more consensus among economists about the validity of microeconomic than macroeconomic results and reasoning; macroeconomics has changed much more dramatically than has microeconomics in the last thirty years, so much so that it is difficult to know how to characterize the field as a whole; and, finally, restricting the discussion to microeconomics allows the delimitation of the subject (to a certain extent). Blaug ([1980] 1992, chap. 12) argues that macroeconomics is the area in economics that comes closest to an empirically driven discipline, so it may also be that the conclusions drawn in the text about microeconomics are less applicable to macroeconomics.

misallocation of resources, that, ceteris paribus, price supports cause gluts, or that price ceilings result in the creation of excess demand, of black markets, of deterioration in the quality of the products on offer, and of nonprice rationing, I am making pattern predictions. When I tell them that, ceteris paribus, the incidence of taxation depends on the elasticities of demand and supply that a good faces, I am explaining why a tax increase on cigarettes (the demand for which is inelastic) tends to be passed on to consumers while a tax increase on luxury goods (the demand for which is elastic) tends to be borne by the producers of such goods. When I enumerate the conditions under which one might expect third-degree price discrimination to emerge (sellers must be able to segment a market and prevent resale for it to be possible, and the different segments of the market must have different elasticities of demand for price discrimination to be profitable), it helps them understand why price discrimination is more likely to be observed in some markets (e.g., markets for airline or movie tickets) than in others (markets for food or apparel). When I point out that three conditions must be met for a cartel to be successful through time (the cartel must produce a large share of total output, the good must have few close substitutes, and cartel members must be able to catch and sanction cheaters on the agreement), it makes it easier to explain why some cartels (e.g., the diamond cartel) are more successful in keeping prices high than are others (e.g., OPEC).

In all these standard classroom examples, economists provide arguments about when to expect certain patterns of market behavior, rather than others, to come about. In such exercises, we identify the sorts of variables that are important in explaining the phenomenon in question. The ceteris paribus clause is crucial, for it is there to remind us how hard it is to pass from such qualitative and conditional theoretical deductions to precise numerical predictions about such phenomena as they exist in the real world. But our models do allow us to provide plausible explanations of, and even sometimes to predict, certain ubiquitous patterns of behavior that recur in the social world.

All this is simply to say that Hayek's idea of pattern prediction might be viewed as fitting certain aspects of standard undergraduate microeconomic theory pretty well. It fits the sorts of positive microeconomic questions that, when surveyed, economists tend to express agreement on. Alternatively, if we use the language that I used in discussing Lionel Robbins's contribution to methodology, we make pattern predictions when we utilize *basic economic reasoning*. Robbins's defense of basic economic reasoning, the sort of reasoning that economists utilize all the time in the classroom, thus fits hand in

glove with Hayek's notion that, when dealing with complex phenomena, pattern predictions are often all that is possible.[9]

Milton Friedman was mentioned a bit earlier, and I will provocatively suggest here that Hayek's "pattern prediction" was also the same sort of "prediction" that Milton Friedman had in mind when he wrote his famous methodology article (Friedman 1953). The very first example that he used in that article was that of predicting the effects of a price control in a labor market: the minimum wage law. I think that, not just Hayek, Robbins, and Friedman, but a majority of at least American economists would agree about the expected effects of increases in the minimum wage—despite the recent work of David Card and Alan Krueger.

To digress a little: The economists David Card and Alan Krueger undertook empirical work (see Card and Krueger 1995) that showed that a moderate rise in the minimum wage in the 1990s had no disemployment effects, a finding that directly conflicts with the pattern predictions of basic economic reasoning. As Thomas Leonard, a historian who reviewed the debate, notes, although critics have since argued that the Card-Krueger study was flawed, only one study (Neumark and Wascher 1996) attempted to reply to Card and Krueger's own devastating claim that earlier econometric studies that established the disemployment effects were themselves flawed. ("The silence is fairly deafening" [Leonard 2000, 139].) Leonard showed that, relative to debates that took place earlier in the century regarding the minimum wage, today all economists agree that such issues should be tested empirically. But it would seem that, when empirical results are ambiguous, as they so often turn out to be when dealing with complex phenomena, economists rely on a host of other reasons in assessing the adequacy of a theory.

There are good reasons to anticipate that the empirical evidence regarding disemployment effects of a rise in the minimum wage should be ambiguous. Such effects need not show up in the data even when they exist in the world. Firms could decide to reduce the number of jobs that they *would have* offered rather than those already existing. Potential employees who *would have* looked for work might not seek employment and, thus, would not show

9. One can find further examples of what I have been calling *basic economic reasoning* in many introductory-level economics textbooks, but perhaps the best exemplar is Heyne's classic *The Economic Way of Thinking* (see Heyne 2000). Heyne died in 2000, but a new edition of his text was recently issued (Heyne, Boettke, and Prychitko 2003). Appropriately, the two new coauthors have made numerous contributions within the modern Austrian tradition.

up in either the employment or the unemployment statistics. And, given that agents in markets are forward looking, both sorts of changes could occur *before* a scheduled rise in the minimum wage even went into effect (i.e., in anticipation of the rise), further dampening the effect on collected data. In these cases, disemployment effects exist but simply do not show up. Of course, there are other instances—for example, in markets where the new minimum wage still lies below the prevailing wage—in which one would expect that a change in the minimum wage would have no effect.

However, even with all this ambiguity, there is still a core belief among most economists that a large increase (say, a doubling) in the minimum wage would have disemployment effects and would, therefore, not be an effective weapon in the fight against poverty. It is basic economic reasoning that leads to that conclusion, and it is what perhaps most clearly separates economists from other social analysts, advocates of "living wages" and the like.

Where Friedman went wrong in his methodology article was in his overemphasis on the role of predictive adequacy as a criterion of theory appraisal and in his insistence that empirical work alone should be enough to decide positive issues. The practice of science is much more complex than the positivist vision of it allowed. I say *in his methodology article* because, in his actual scientific work, Friedman fully understood the complexities, and, accordingly, he typically incorporated all manner of empirical, theoretical, and institutional insights into his arguments.[10]

In any event, the term *pattern prediction* seems to be a pretty good description of what basic economic reasoning is able to accomplish. Let us turn

10. Robert Clower begins his review of Friedman and Schwartz's 1963 *A Monetary History of the United States, 1867–1960,* with these words: "If successful prediction were the sole criterion of the merit of a science, economics should long since have ceased to exist as a serious intellectual pursuit" (1964, 364). Clower then describes Friedman and Schwartz's contribution as follows: "They blend analysis so effectively with narrative that one can hardly tell which of their historical judgments rest on fact and which on theoretical fancy" (367). Later he states: "Their historical judgments about this history are based on painstaking examination of a fantastically large body of evidence and on thorough, honest and closely reasoned analysis of its implications" (379). In his actual work, then, Friedman often did not follow the methodological dictums that he had laid out in his methodology article. By combining empirical data, theoretical insights, and extensive institutional knowledge, he and Schwartz produced an intricate historical narrative that had little to do (Friedman's own rhetoric aside) with the sort of "positive economics" that he had touted in the methodology article. One is tempted to add: And a good thing, too!

now, briefly, to Hayek's other phrase, *explanations of the principle.* Some might think that the assumptions of economic theory provide the starting point for constructing such explanations. That might be true for some of the assumptions utilized by Hayek in his descriptions of the market process, but it is not true for the economic theory of today. It is here that the distinction between *basic economic reasoning* and the *analytic models* of economics, first drawn in chapter 9, comes into its own.

Robbins had argued that the foundations of economics were based on certain facts of reality: that scarcity forces people to choose and that people try to do so purposefully. These are different from the "unrealistic" assumptions that economists use in their analytic models, assumptions like perfect rationality and full information that Robbins's critic, Terence Hutchison, claimed were the fundamental ones. In my discussion of basic economic reasoning in the previous chapter, I argued that simple models that used such unrealistic assumptions nonetheless seemed able to capture certain essential aspects of phenomenal reality, thereby enabling economists to make pattern predictions.

That leaves us with the recognition that simple, unrealistic models seem to allow us to make passably workable pattern predictions about a complex world. That also leaves us with the question: *Why does this happen?* What is it about the world that allows people who are purposeful (but not perfectly rational) and who have limited information (not perfect knowledge) to coordinate their economic activity fairly well, indeed, well enough that models that make unrealistic assumptions about their rationality and knowledge can still do a pretty serviceable job of predicting the outcomes of their actions? Hayek's investigations of the evolution of social institutions that had come into being as the result of human action, but not of human design, and that allowed the discovery, preservation, and coordination of dispersed knowledge all seem to me to be aimed at understanding the *principles* that underlie the social coordination that we observe in the world and in certain experimental situations. Hayek certainly did not finish the task, but he pointed us in the right direction. A fuller "explanation of the principles" underlying the social phenomena in question might help us understand why the simple analytic models of economists often work (and also alert us to when to expect that they would not work).

The variety of sorts of explanations in this domain may be very large indeed. It seems to me, then, that, in addition to pointing out the limits that we face, another important (and more positive) aspect of Hayek's program is to direct us to seek out explanations of the principles that underlie social phenomena. I will take up this issue again later in the chapter.

Situational Analyses in Economics

In this section, I will argue that a dominant research strategy in economics over the course of the twentieth century has been to provide models at varying levels of formality of what Karl Popper called *situational analyses.* To show this, I first describe three apparently quite different theoretical developments in economics and then show how Popper's model unifies them.

One of the developments, the economics of information, gained the 2001 Nobel Prize for three economists who contributed to its growth—George Akerlof, Michael Spence, and Joseph Stiglitz. The economics of information allows economists to analyze situations of asymmetrical information in which one party to a transaction or contract has more or better information than the other. If one assumes that the better-informed party tries to take advantage of its superior information, certain well-defined problems arise. Precontractual opportunism, for example, leads to situations of adverse selection. Thus, higher-risk patients (e.g., those who know their own health conditions when insurers do not) are more likely to seek insurance than are healthy ones, or sellers of services or products (e.g., one's own services in a labor market or goods in a product market) are in a position to deceive buyers about the quality of their services or product, again because they have more information than the buyers do. In like manner, postcontractual opportunism leads to problems of moral hazard. The standard case is when the existence of a contract leads to altered postcontractual behavior (e.g., less careful driving by those with insurance or shirking by employees) and, owing to imperfect information, monitoring or enforcement of sanctions against the behavior is difficult.

It is theoretically possible for such informational asymmetries to eliminate a market completely (e.g., only the sickest people seek insurance; as a result, insurance premiums rise precipitously; this drives the healthiest of the sick out of the market, causing a further rise in premiums, and so on, until the market collapses), but much more interesting is the light that the economics of information sheds on a wide assortment of market institutions that have arisen precisely to overcome the problems that arise owing to informational

asymmetries. Thus, insurance companies try to overcome the problem of adverse selection by pooling risks through group coverage arrangements. Sellers of services can signal their quality by offering warranties or by showing that they have obtained a qualifying degree or certificate. Buyers can likewise protect themselves by employing various screening devices. Deductibles on insurance contracts and various pay-for-performance incentive schemes can be used to overcome problems associated with moral hazard.

Another area that has grown rapidly in recent decades is transactions costs economics. Although parts of transactions costs analysis cover areas similar to those covered by the economics of information, there are distinct differences too. Whereas the economics of information is a straightforward extension of mainstream neoclassical theory, transactions costs analysis traces it roots to a diverse set of writers, some of whose work dates as far back as the 1930s, in the fields of organizational behavior, law, and institutional economics. Transactions costs analysis generally assumes that agents are opportunistic but, drawing on Herbert Simon's work, only boundedly rational (i.e., agents' actions are intended to be rational, but agents possess only limited cognitive ability). Less formally mathematical than the economics of information, transactions costs economics instead emphasizes the comparative study of organizational forms, governance structures, and the like. The common bond linking these social institutions is that all can be interpreted as economizing on transactions costs. As one of its chief interpreters characterized the approach: "The organizational imperative that emerges in such circumstances is this: *organize transactions so as to economize on bounded rationality while simultaneously safeguarding them against the hazards of opportunism*" (Williamson 1985, 32).

A third area within economics that has seen rapid growth in recent years is game theory. A body of techniques for investigating situations of strategic interdependence rather than an economic theory proper, game theory has virtually taken over certain fields within economics (e.g., industrial organization) and is extensively used in many others. Game theory was not always so well received. In its early years, and right up until the 1970s, it was mostly used to model situations of oligopoly or bilateral monopoly. One reason for its recent popularity is that it allows one to model explicitly the informational regime that agents confront. Thus, its range of applications increased naturally and dramatically as the economics of information grew. In the last decade or so, game theorists have been increasingly interested in formally modeling choice situations in which agents are only boundedly rational or, in its evolutionary variants, in which questions of agent rationality are sidestepped altogether.[11] All these moves have helped extend professional interest in game theory.

As different as these three programs might at first appear, all of them can be described using Karl Popper's model of situational analysis. This may not be so surprising if one knows that Popper claimed that all explanations in the social sciences (not just economics, but *all* the social sciences) typically take the form of situational analyses. While Popper himself was not always crystal clear about what he meant by the term *situational analysis*, his student Noretta Koertge provided a more systematic restatement, a short version of which reads:

> *Description of the Situation:* Agent A was in a situation of type C.
> *Analysis of the Situation:* In a situation of type C, the appropriate thing to do is action X.
> *Rationality Principle:* Agents always act appropriately to their situations.
> *Explanandum:* (Therefore) A did X.

A more extensive version reads:

> *Description of the Problem-Situation:* Agent A thought he was in problem-situation of type C.
> *Dispositional Law:* For all such problem-situations A would use appraisal-rule R.
> *Analysis of the Situation:* The result of appraising C using R is action X.
> *Description of the Agent's Competence:* A did not make a mistake in applying R to C.
> *Rationality Appraisal Principle:* All agents appraise their situations in a rational manner.
> *Explanandum-1:* (Therefore) A concluded that X was the rational thing to do.
> *Rationality Principle:* Agents always act on the outcome of their rational appraisals.
> *Explanandum-2:* (Therefore) A did X. (Koertge 1975, 440–45)

11. I should add that the recent evolutionary developments are less consistent with situational analysis than are the standard approaches in game theory that utilize rational agents who try to maximize their payoffs.

It should be evident that many standard textbook microeconomic theoretical arguments take the form of situational analyses. Wade Hands (1992, 28) shows us why:

> Economists specify the situation of the agent (individual or firm) usually in terms of the preferences and/or technology and the relevant constraints (prices, income, factor constraints, etc.). Included in the description of the situation is some "motivating" consideration (maximizing utility, maximizing profit, etc.). The second step is to deduce the appropriate behavior of the agent given the situation (buy more, buy less, increase production, decrease production, etc.). This second step is what constitutes most of economic *theory*, the formal deduction (usually mathematical) of the "appropriate" behavior in a given "situation." Finally, if the economist's task is to explain an observed action, the rationality principle is activated to connect the analysis of the situation with the action to be explained.[12]

Hands focuses on standard microeconomic theory, but it is also evident that many theoretical innovations in microeconomics, including the three mentioned above, may be viewed as reconfigurations of certain of the initial conditions of a situational analysis and the elucidation of their effects. In the simplest formulations of microeconomic theory, one might assume that agents have perfect information, that transactions are costless, that agents have unlimited computational ability, and so on. By altering each of these assumptions, one obtains any of a number of extensions of or alternatives to the standard account: decisionmaking under risk; exchange under conditions of positive transactions costs; the satisficing and bounded rationality models; analyses of problems arising from informational asymmetries; and so on. The general framework of situational analysis can be used to describe the mathematically formal models of the economics of information and less formal models like those dealing with transactions costs analysis. It can also be used to describe the simpler models used by those engaging in basic economic reasoning.

12. It should, however, be noted that, in another paper, Hands (1991, 117–18) questions whether situational analysis can explain either aggregated market behavior or the unintended consequences of intentional human action. This is one of the reasons why situational analyses are not enough if one is concerned with the sorts of questions that Hayek raised.

Situational analysis is very adaptable. Simply by altering their descriptions of the problem situation, economists have been able to generate large numbers of modifications and variations on their basic models. This doubtless helps explain the popularity and longevity of the approach. It has also been the source of progress in our theoretical understanding of economic phenomena. Again, think of the three areas mentioned above.

The economics of information offers a means for understanding many organizational practices and market institutions whose very existence would be unnecessary in a world of perfect information. It directs us to look at the sorts of incentive structures that might arise under different informational regimes and helps us understand the form that labor, insurance, and other contracts might be expected to take. Like basic economic reasoning (of which certain parts of the economics of information are now becoming considered), it may also help us avoid policies that are likely to lead to adverse outcomes. The partial deregulation of the savings-and-loan industry in the United States, which encouraged managers to seek out risky but high-return investments with depositors' money, a strategy that resulted ultimately in hundreds of billions of dollars in losses, has now become a standard textbook illustration of moral hazard.[13]

Like the economics of information, transactions costs analysis allows economists to make sense of a number of market institutions whose existence might otherwise be puzzling. At its most basic level, transactions costs economics explains why markets are used for certain transactions while hierarchies (the firm itself being the paradigmatic case) are used for others. Transactions costs analysis also helps explain why certain forms of nonstandard contracting (e.g., tie-in arrangements, block booking, territorial and customer restrictions on franchisees) might be used to safeguard against opportunism in certain industries. Various alternative institutional forms for organizing the work and corporate governance relations have also been analyzed using the approach.

Finally, game theory provides economists with a language and a set of techniques for analyzing situations of dynamic strategic interdependence.

13. Until the example became too dated, I would tell students in my introductory economics course about John Kareken's 1983 "Deposit Insurance Reform; or, Deregulation Is the Cart, Not the Horse," which warned about the dire consequences of deregulating banks while leaving deposit insurance intact. Kareken could not say exactly when or how large the savings-and-loan debacle was going to be, but his pattern prediction that one was coming held up. It represents basic economic reasoning at its best.

The extent of its spread has been phenomenal: terms like *zero-sum game* and *prisoner's dilemma* have become part of public discourse. Although game theory can be used to model situations of intricate complexity, it is simple enough to be fruitfully employed at the introductory level, at least in its "normal form."

These modifications provide useful tools that help us explain the world better. But, intriguingly, none of them would be considered progressive under the positivist criteria, which require that theoretical advances be ever more testable (or falsifiable) and, hence, able to support more precise predictions. Although at times the evidence is mixed, I think that, in general, the three theoretical approaches just described typically involve theories whose explanatory strength is bought at the cost of *less* predictive power.

Consider game theory first. To be falsifiable, a theory must predict certain specific outcomes and prohibit others. I remember studying game theory in graduate school in the 1970s, mostly in applications involving oligopoly, and I can distinctly remember my professor, Bill Pfouts, complaining about how the theory does not allow us to make firm predictions about market outcomes, like the theories of perfect competition and monopoly did. (Oligopolists can end up acting like joint-profit-maximizing monopolists, or like competitors when they engage in price wars, or somewhere in between. Worse, from a predictive point of view, the outcomes are rarely stable.) That was the positivist era, and predictive impotence was viewed as a severe limitation. Such positivist prejudices help explain why, in its early formulations, game theory was not widely viewed as being a particularly helpful body of techniques. True enough, it helped explain why oligopoly might yield a number of different outcomes, which some might see as an advance. But, by widening the field of possible outcomes, it necessarily forbid less and, thus, was less falsifiable, which any good positivist would have to view as a liability.

In more recent years, noncooperative game theory has been praised for sharpening our predictions in situations involving asymmetrical information, particularly in applications utilizing the theory of competitive auctions. This has potential policy payoffs since it suggests ways to organize auctions so as to increase expected revenues (Kreps 1990, 82–87; Sutton 2000, 47–57; cf. McAfee and McMillan 1987, 733–34). In this area, it would seem that advances consistent with the positivist vision have, in fact, been made.[14]

14. Sutton (2000, chap. 2) discusses two recent success stories: the theory of auctions and the Black-Scholes option-pricing model. In his closing words, however, he cautions

Other developments, however, seem to confirm Hayek's thesis. In his *Game Theory and Economic Modelling* (1990), David Kreps spends much of a chapter titled "The Successes of Game Theory" reviewing a part of the literature in industrial organization in which ever more detailed models of entry deterrence are developed. As details are added to existing models, previous predictions are sometimes overturned. The end result is a multiplicity of very specific models, some predicting one outcome, others another. Whatever its other merits, this procedure reduces the falsifiability of the *set* of models. If a one-to-one correspondence existed between a model and reality, one might get a firm prediction. But reality is always more complex than the model, so, when a prediction does not hold, there are always a number of alternative specifications that can be tried in its place until the one that fits is found. As Kreps (1990, 104) himself concludes, "Game theorists are very clever individuals, and given almost any form of behavior, they can build models that 'explain' the behavior as the result of an equilibrium in a sufficiently complex elaboration of the game originally written down."

In the chapter "The Problems of Game Theory," Kreps notes that, for certain types of games, there is a proliferation of solution concepts, or, as he puts it, "too many equilibria and no way to choose" (1990, 95). A theory that allows multiple solution concepts is one that prohibits fewer outcomes. In these cases, although the game-theoretic models are better able to capture the complexity of social phenomena, they are again less falisifiable in that they allow us to make less precise predictions. In a review of the impact of game theory on industrial economics, Franklin Fisher commented on this problem as follows: "A great many outcomes are known to be possible. The context in which the theory is set is important, with outcomes depending on what variables the oligopolists use and how they form conjectures about each other. A leading class of cases concerns the joint-maximization solution and when it

about the generality of the results obtained in studies of the former: "A comprehensive analytical survey by Laffont (1997) discusses the extent to which testable predictions can be developed, which depend on directly observable outcomes; this review serves inter alia to underline the fact that the example we looked at in the preceding section [i.e., the theory of auctions] is rather special. Over the general run of situations we encounter in practice, such straightforward predictions are not possible" (57). One wonders, too, whether the fate of Long-Term Capital Management may have taken some of the luster off the second example.

will or will not be achieved. The answer to the latter question is known to be very dependent on the context and experience of the oligopolists" (Fisher quoted in Backhouse 1997, 20). As Roger Backhouse notes: "The very same words, Fisher points out, could have been used to summarize the state of industrial economics in the early 1950's, long before the advent of game theory" (Backhouse 1997, 20). Such results doubtless produce deep depression among those who adhere to a positivist vision of science, but they are what Hayek anticipated would occur when we study complex phenomena, and, surely, the dynamic competitive interaction of oligopolists is a topic that meets any criterion of complexity that one might want to propose. Indeed, the tendency for game theorists to produce such "exemplifying" (as opposed to "generalizing") theory supports the general notion of the limits of our knowledge when dealing with complex phenomena.

I will not here discuss more recent developments in game theory and, in particular, the move toward evolutionary game theory, with its promise to expunge the necessity of the "common knowledge" and "rationality" assumptions, except to point out Robert Sugden's (2001) argument that this apparently fundamental change in the foundations of economics may actually represent a tautological response to analytic problems in the previous program. Sugden's conclusion—"a *genuinely* evolutionary approach to economic explanation has an enormous amount to offer; biology really is a much better role model for economics than is physics" (2001, 128)—is one wholly consistent with the arguments advanced in this chapter.

What about the economics of information and transactions costs analysis? Both help us understand why certain institutions and organizational forms emerge within certain market settings. Certain aspects of the world that would not make sense under the older theories are suddenly explained. But, again, the not inconsiderable *explanatory* virtues of these theories are also part of what makes them less falsifiable. And the reason is that, as I just showed, both very explicitly follow the method of situational analysis.

What does that have to do with falsifiability? In describing how the method of situational analysis works, Popper insisted that, whenever a theory employing the rationality principle is falsified, the appropriate thing to do is rethink one's model of the *situation.* Crucially, one should never reject the rationality principle. By following this methodological principle, one gets "far more interesting and informative" models (Popper 1985, 362). But it also means that, *as a matter of methodological principle, the falsification of theories based on the* *method of situational analysis is never taken as grounds for rejecting the theory.* Instead, any falsification immediately leads to an ad hoc theory adjustment, a redescription of the problem situation, thereby immunizing the theory from falsification. That Popper's description of how explanation takes place in the social sciences appears to be inconsistent with his prescriptions about the importance of falsifiability and the avoidance of immunizing stratagems has often been remarked on by methodologists (e.g., Hands 1985a; Caldwell 1991a).

Again, I should say what all this means and what not. I think that there has been theoretical progress in economics. We know more now about the problems that arise from informational asymmetries, and the importance of incentives, and how certain institutions have arisen to deal with such problems, and some of that has filtered into our everyday thinking about how the world works. Economists see the world differently from the way we did a half century ago, and that change on net represents progress. That some of this has also spilled over into what I have been calling *basic economic reasoning,* the undergraduate-level sort of analysis that allows us to make pattern predictions, is also very important. In my opinion, for all the esteem that our profession bestows on mathematical virtuosity when dealing with analytic models, it is progress in basic economic reasoning that really counts when one thinks about what progress can mean in a field that studies complex phenomena.

On the other hand, few of the changes that have occurred in microeconomics in the past century confirm the image of our discipline held by economists in the positivist era, for our explanatory progress has come at a cost: either the models that we develop are less directly falsifiable, or (at least when we deal with models that employ situational analyses) it is now a matter of methodological principle not to take falsifications seriously. This is an inevitable result when trying to model complex phenomena. Economists have often not recognized the disjuncture between their rhetoric and reality, perhaps because the ever-increasing mathematical sophistication of our models obscured it, or perhaps because the sheer variety of ways of reinterpreting economic problems as situational analyses has led us to feel that the discipline was progressing. There *has* been progress. But the progress has been different from that envisioned in the positivist era. Economists have employed positivist rhetoric for decades, but their practice has contradicted it.

Some Alternative Paths for the Twenty-First Century

The endless reconfiguring of initial conditions of economic situational analyses has yielded *some* theoretical progress. (In my opinion, the most significant theoretical developments have been those that have added to the storehouse of basic economic reasoning.) One wonders, however, whether there might be other ways to study complex social phenomena, other directions in which economics as a discipline might go? It seems to me that there are a number of ongoing areas of research that are consistent with Hayek's vision, some of which were mentioned in the previous chapter, and any number of which might bear some real fruit in the future. But it also seems to me to be foolish to go into too much detail, for trying to prophesy the future course of knowledge is, as Hayek's friend Popper famously pointed out, about as fundamental an example of the foibles of historicism as one can imagine.

Among current programs with a distinctly Hayekian tenor, the most obvious candidates are interdisciplinary efforts at the interstices of cognitive science and complexity theory, or fields that examine the role of rule-following behavior in the creation of social institutions, or those that undertake historical or experimental or multidisciplinary comparative investigations of the evolution of alternative institutional or organizational forms. As a result, within economics, parts of both the new institutional economics and transactions costs economics have distinctly Hayekian elements, as does certain work in experimental economics, as does work in the biological bases of economic behavior, as do areas like artificial society modeling, even if the proponents of such approaches do not always reckon Hayek as a precursor. Of course, one must also include here the research of those who first and foremost consider themselves as participants in the modern Austrian tradition.

The field expands even more dramatically when one considers the need to provide "explanations of the principles" underlying various social phenomena. I claimed that this was something that Hayek began but that much remains to be done. For example, we clearly need a much more carefully and fully articulated theory of complex phenomena. That theory was supposed to have provided the underpinning for Hayek's methodological claims, but it was one that (it must be admitted) he never really fully developed. As work at the Santa Fe Institute demonstrates, not just economists but also scientists, mathematicians, computer specialists, and philosophers from a variety of backgrounds might contribute to such a project. One wonders, too, whether such work might someday be able to identify, in either a general systems framework or within particular fields, the validity of Hayek's conjecture that there are, in principle, limits to what we can know.

Another area that is ripe for study, and one that historians of thought can participate in very directly, is the testing of the theses offered here by examining the historical record to see exactly what sorts of progress actually have occurred in economics and other social sciences. A hopeful sign here is that the fourth annual conference of the European Society for the History of Economic Thought held in Graz, Austria, in 2000 took as its theme the question, "Is There Progress in Economics?" (see Boehm et al. 2002). We also need a better understanding of the nature of the models used in economics and of their relation to the social phenomena that we study. The Research Group in History and Methodology of Economics at the University of Amsterdam has already done a considerable amount of initial work in this latter area. Finally, we would benefit from seeing exactly how the practice of economics compares with that of other disciplines, the natural sciences and the social, those that study complex and those that study simple phenomena. Wade Hands (2001) reports on some of the prospects for this sort of activity in his *Reflection without Rules: Economic Methodology and Contemporary Science Theory* (2001).

A final challenge is to explore the social ontology that informs Hayek's and other descriptions of social reality. Such investigations would, one hopes, help us understand why certain very simple economic models nonetheless seem able to capture essential features of social reality. We need a better understanding of what lies behind the successes of basic economic reasoning, and one could imagine such an understanding coming from studies from a number of different perspectives. We also need to understand better the relation between human agency and the social institutions that condition human action. Tony Lawson's fairly well developed critical realist program seems to me to provide an excellent starting point for investigations into such questions of social ontology.[15]

15. Although he may disagree, Lawson's "demi-reg" concept (Lawson 1997, chap. 15) seems quite consistent with Hayek's notion of pattern predictions. Paul Lewis (2002) explores the relation between social structure and agency.

Getting beyond Positivism

I have argued that Hayek's vision of the subject matter of economics and of the methods appropriate to its study allows us to make better sense of the development of economics in the twentieth century than does that proposed by his positivist antagonists. Another way of testing Hayek's claims is to note that, if he were right, others certainly would have noticed some of the same things that he did. It turns out that some economists, especially those interested in methodology or the history of thought, have long been making observations that are roughly similar to Hayek's. For example, the indictments made by Terence Hutchison (1988) and Mark Blaug ([1980] 1992, 111) that most economists fail to engage in anything much better than "innocuous falsificationism" are obviously consistent with the idea that much of economic theory follows the method of situational analysis. The philosopher Daniel Hausman's (1992, 253–54) argument that the data that economists have been using offer little hope for crucial tests of their theories is fully consistent with Hayek's claims about the empirical limitations of economics.

Where these analysts *differ* from Hayek is in their *response* to what they see happening in economics. Whereas Hayek might take such observations as being the natural and expected outcome when we study complex phenomena, these other observers urge economists to try harder: try harder to falsify, try harder to get better data. Now, as noted above, Hayek may be interpreted as urging us to try harder too, but his principal emphasis was on getting us to recognize, not just the ubiquity, but, even more important, the *permanence* of these sorts of problems when we study complex phenomena. One does not find that emphasis in the analyses of these other writers.

Other writers have drawn still other conclusions from their observations of the practice of economists. Although the philosopher Alexander Rosenberg's (1992) observation that there has been virtually no improvement in the predictive powers of economic theories mimics Hayek's views, he concludes that economics is, therefore, not a science. Contra Rosenberg, the outcome is, for Hayek, *just what one expects* from sciences that study complex phenomena. It also seems to me that Deirdre McCloskey's (1985) claim that the argumentation of economists is principally rhetoric is also wholly consistent with the views expressed here. We do not establish our theories on the basis of their having survived severe empirical tests. Our belief in them is based, rather, on a wide variety of evidence. I doubt that such a claim would ever have been made had economics shown the sort of progress that positivists confidently envisaged half a century ago. Both the birth of the rhetoric movement and the revival of professional interest in methodology are, in many ways, the direct result of the failure of economics to deliver on the promises of positivism.

I have been using the weasel word *positivism* to stand in for that array of empiricist doctrines that dominated the philosophy of science in the first half of the twentieth century and that filtered into the social sciences through a variety of channels. However one might wish to define these doctrines, their impact in economics has been largely malefic. Positivism in its various guises fostered false hopes and permitted self-delusion. It misled economists into thinking that we can, and, indeed, that to be scientific we must, always improve the predictive adequacy of our theories. When this did not occur, more and more resources were devoted to the quest, all in the name of science. Built into all this is a basic failure to recognize that, if economics is a science that studies complex phenomena, by its very nature its prospects for such progress are limited. Such self-understanding is liberatory and counts as knowledge of a sort. It is a hard lesson to learn—but an important one.

Our failure to recognize the limitations of economics has cast a long shadow over the discipline. Not only does it affect how some mainstream economists regard their work. But it is also to be found in the writings of people like Mark Blaug, Terence Hutchison, and Daniel Hausman—all of whom hold out the hope that, if we just try harder, "real" empirical progress will be possible—and in Alexander Rosenberg's charge that reliance on folk psychology prevents economics from becoming a "real" science. If we believe Hayek, economics is a science, but it is a science that studies complex phenomena. For such sciences, a philosophy of science that makes steady improvement in predictive adequacy and the discovery of law-like empirical relations the principal criteria of scientific status or scientific progress is inappropriate.

It has now been almost fifty years since Milton Friedman, writing in the heyday of positivism, enshrined prediction as the goal of positive economic science. His goal was clear and noble: to use empirical methods to decide on positive issues and, thereby, to reduce disagreement. We knew a lot less then than we do now about the prospects for such a program's success. One can always hold out some hope for such an outcome, but to fail to acknowledge the problems with the positivist worldview at this late date is nothing short of a scandal.

It should, perhaps, come as no surprise that Hayek took a very different view from Friedman on the prospects for empirical work in economics. Indeed, in an interview in the last decade of his life, Hayek put it this way: "You know, one of the things I have often publicly said is that one of the things I most regret is not having returned to a criticism of Keynes's treatise, but it is as much true of not having criticized Milton's [*Essays in*] *Positive Economics*, which in a way is quite as dangerous a book" (Hayek 1994, 145).

It would represent great progress indeed if some leading members of the profession would emphasize the limitations of economic science in their statements to the public and, perhaps more important, in their recommendations concerning pedagogy.[16] Unfortunately, as we will see in the next section, there is little evidence to suggest that most of the economics profession has either the ability or the desire to heal itself.

A Final Casualty: The End of History and Methodology?

It is as a historian of economic thought that I must speak in conclusion and in a very personal way to a final legacy of positivism in economics. If the positivists had been right, there would have been cumulative progress in the science of economics. Had that been the case, one would have much less use for intellectual history, which apparently deals with the superseded theories of the past. If the positivists had been right, all the relevant true results would, after all, be there to be found in the latest working papers.

Paul Samuelson gave a speech to the History of Economic Society in the 1980s in which he stated that the reading lists of certain graduate economics courses at leading universities consisted almost wholly of working papers. One suspects that this is even truer today. It would, thus, appear that many

16. A shining example is the lecture "Identification Problems in the Social Sciences and Everyday Life" given by the econometrician Charles Manski at the November 2002 meeting of the Southern Economic Association. Manski noted that one can usually get a point identification of a variable by making very strong assumptions, whereas one can get "partial" or range identification of the same variable with much weaker assumptions. He argued that the quest for precision often drives economists to ignore the important information that one can get from employing partial identification procedures. Manski's conslusion was very Hayekian: to understand the limits of what can be known itself constitutes an advance in knowledge.

economists have accepted the positivist assumptions. The history of one's discipline has, on this view, little relevance for the scientist. One can study it for fun on one's own, but it has no importance for the training of scientists.

Similar things have been said, of course, about the study of economic methodology. From a positivist's point of view, there is only one "methodology of economics"—it is what students learn when they train to become economists. We do not need a separate field devoted to the study of methodology.

If this portrait of the dominance of the positivist view regarding the status of history and methodology sounds like a caricature, do not be fooled. If anything, it is an understatement. Economic methodology was never a formal field for graduate training, and it is taught today at only a handful of institutions, most of them in Europe. The history of economic thought was formerly considered a legitimate area, but it has since been eliminated as a field of study from nearly every leading graduate institution in the United States.[17] As historians of economic thought retire, they are not replaced.

The short-term consequences of such disciplinary fratricide are as disquieting as they are evident. Unless they had an undergraduate course in the history of economic thought or enough of an interest in the subject to pursue their studies independently, newly minted Ph.D.'s in economics today increasingly have no knowledge of the history of their discipline. They know the major names—Smith and Marx and Keynes—but their knowledge of these figures' ideas does not go much beyond the sound bite. Their exposure to less prominent figures, like Ricardo, is far more restricted ("Did he invent the Ricardian equivalency theorem?"). They certainly do not recognize names like Menger or Wieser, or Lerner or Lange, and have, of course, read none of them, not even the most famous. The only history that they know might be dubbed *theorist's history*, in which the great name is invoked to set up a problem ("Hayek was concerned about information . . ."), the rest of the time being spent building a model that examines the problem.[18]

The longer-term consequences of this downward spiral are equally daunting. Economists with no knowledge or appreciation of history are making decisions about its importance in the curriculum. If current trends continue, there will be no more history of thought taught by economists trained in the field, not even at the undergraduate level. (If none are trained in graduate school, there will eventually be no one to teach it at *any* level.) We will gradually but inevitably lose our touch with history. A science ignorant of its history is a science more likely to be arrogant as well as ignorant—ignorant of both its arrogance and its ignorance. It is also a science more likely to be led astray, more prone to divigations that a knowledge of history might have prevented. It is a sad fate.

17. There is, perhaps, a sign of hope. In France, a backlash against standard economics education has developed among the students, in the form of the post-autistic economics (PAE) movement. For more on the PAE movement, visit its website at www.paecon.net.

18. A classic example of theorist's history is Joseph Stiglitz's (1994) treatment of market socialism. In Caldwell (1997a, 1875–86), I argue that, among other costs, such approaches inevitably tend to misunderstand alternative paradigms, such as the Austrian position.

The argument one hears in defense of this shortsighted practice is always the same. "If we hire a historian of thought, we will not be able to hire an econometrician, or a labor specialist, or a theorist who will be able to work with others in the department and allow us to advance our science." It is an opportunity cost argument, one that presumes that the contribution made by a historian of thought must, at the margin, always be less than that provided by another sort of economist, indeed, *any* sort of economist. The argument might make some sense *if* the positivist vision of science on which it is premised were right.

I have argued and provided evidence that the positivist vision is a false one. And it was for the most part those who studied the history and methodology of economics who recognized the nature of the problems that this approach engendered. Most of the observations that I have made in this final chapter are not unique; they have been echoed, as noted above, by many historians of thought and specialists in economic methodology. I do not think that it is an accident that people in such fields, even those who begin from very different starting points, should come to similar conclusions, conclusions that are often quite different from those reached by practitioners of economics about the nature of their work. In their efforts at understanding, historians and methodologists make it a practice to step back from a field, to try to see it in a different light. If the observations that have been advanced here are right, this is an extremely valuable exercise. It implies that many economists have not understood the nature of their own area of study and that, because of the unspoken dominance of positivist ideas, they have been misled about what their discipline can and cannot do. And this has misled them in what they allow to be taught.

I doubt that the current direction of economics, the dismissal of history and of alternative approaches, would be possible without the positivist hope that the steady agglomeration of new techniques would one day solve the riddles of empirical and theoretical adequacy that have for so long eluded economists. Like the program of the German historical school economists, the positivist vision requires the efforts of many people, all working toward a common goal. One can only hope that this narrow-minded approach will suffer the same fate as the German historical school.

But this does not look likely, at least for now. So I end this meditation on a sad note. This has been a history of ideas that began with an account of the origins of the Austrian school of economics. This was a school whose tenets were forged in battle. The first war waged, that between Austrian theorists and German historians who thought that history *was* theory, was in many ways truly a wasted effort, for it obscured the legitimate roles of both history and theory in understanding social phenomena. The next war was against socialists of many stripes, but particularly those Viennese Marxists who combined a particular socialist vision with positivist doctrines of the proper way to do science. Although his awareness of the German historical school's errors colored Hayek's earliest work, for most of his life it was against the combined forces of socialism and positivism that he fought. It was a lonely struggle. But, at least by the end of his life, he could claim that considerable progress had been made against the socialists. The same could not be said about positivism, later variants of which eventually shaped the way in which social scientists, economists prominently among them, came to understand (or, better, misunderstand) themselves. Given his background, Hayek felt from the start that a radically empiricist approach to the subject matter of economics could not succeed, and he came to believe that the theory of his day left out important aspects of social reality, that, indeed, to be a social scientist, one had to understand economics, but that that was not enough. Whatever else one may think about his views, there is certainly evidence that supports his claims about the limitations of economics. And, if his claims are right, they suggest that there may be alternative ways to do economics. That is Hayek's legacy and his final challenge.

This book presents an alternative perspective on my profession's history. And—I will come clean here about something that is probably already obvious—having taken my cues from Mitchell and Hayek, it is my hope that the story that I have told may help in some small measure to alter my profession's practice. If the current trend continues, there will be, literally, no historians

of economics who have been trained as economists to provide future analyses of the discipline. That would be the final casualty of the pernicious doctrines against which Hayek fought for so long. I close with the plea that the trend be reversed, that the history of economic thought be restored to the graduate curriculum. We owe it to our students—and to ourselves.