Does Economics Make Progress?

Robert M. Solow

When President Katz asked me to give this talk, I realized that I had a choice. I could talk about the economy or I could talk about economics. Since I am not a visiting outlander, and we are en famille, I thought it would be more interesting if one of your friendly local economists were to talk about economics. You hear too much about the economy anyway, too many contradictory things; and that is, in fact, precisely what motivates me to talk about the discipline rather than about its object.

Many years ago—you will see just how many—I read an entertaining story in The New Yorker. It was about a small midwestern university, and it made a contrast between the dominant figure on the faculty, the Professor of Animal Husbandry, and a wispy little Professor of Physics. The Professor of Animal Husbandry was a big, expansive man with a large, blonde wife—braids for sure—and six handsome children. The U.S. Department of Agriculture financed his work generously, so he had a comfortable office and many assistants, and traveled widely. The Professor of Physics was a little, balding, mousy man with a little, mousy wife. They were childless and lived in an attic. The Physics Department was penniless, the laboratory was rundown and bare, and the Professor walked when he went anywhere; besides, he never left town. Then, one week, the United States exploded the first nuclear bomb. Suddenly the Professor of Physics had the same effect on faculty meetings as E.F. Hutton, but no one listened to the Professor of Animal Husbandry anymore. He shrank in size and showed bald spots, and his wife’s complexion worsened. The Professor of Physics added both height and weight, and grew a handlebar mustache, a great head of hair, and aviator goggles. His wife had triplets almost instantly. He was seen
driving his new Jaguar to the airport on his way to a conference in the Bahamas, and so on. It was a pretty funny story.

The status of economics in the academic pecking order has had its ups and downs, too, and for similar reasons. It was ace-high during the long economic upswing that began in the Kennedy Administration and lasted almost for a decade. But the inflation of the 1970s has done for economists what the bomb did for the Professor of Animal Husbandry. I was tempted to admit that these days I feel defensive about economics, but that's not really true. But I feel as if I feel defensive.

Maybe the awful truth is that economists have never been popular. In Thomas Love Peacock's *Crochet Castle*, there appears an economist, a Scot naturally, named MacQuedy, whom I take to be a satire on J.R. McCulloch, a boring and dogmatic follower of Ricardo. (Peacock novels consist mostly of country-house conversations, rather like American Academy meetings, only with smaller numbers of people and better written.) MacQuedy claims that the Scots are the modern Athenians. They have mastered metaphysics, logic, and moral philosophy. "The Athenians have only sought the way and we have found it; and to all this we have added political economy, the science of sciences." To which the Reverend Dr. Folliott replies: "A hyperbarbarous technology, that no Athenian ear could have borne. Premises assumed without evidence, or in spite of it; and conclusions drawn from them so logically, that they must necessarily be erroneous."

But there is more to your ambivalence about the science of economics than mere resistance to intellectual imperialism. I think that what worries you—what *should* worry you—is an all too common spectacle: You pick up your *New York Times* on the day after some significant economic event or policy proposal has occurred. Some enterprising reporter has talked to two respectable academic economists and quotes them as saying quite opposite
things about the matter at hand. I don't mean that they have revealed different values—people will always differ, for example, about how much interference with individual liberty is permissible in the common interest, and they will differ about what the common interest is. I don't even mean that they have revealed different forecasts about the future—the economic future depends intimately on uncertain events, including extra-economic events, beyond anyone's capacity to predict accurately, and although I think economists forecast too much, that can be accounted for by mere foolhardiness. I mean that those two respectable academic economists seem to differ radically in their understanding of cause and effect; one of them says that the new policy proposal would have consequences in thus and such a direction, and the other says something quite contradictory.

You would have to be very naive not to wonder what kind of a "science" it is that encompasses such vast differences of opinion about apparently basic matters. Obviously every non-trivial discipline has disagreements at the frontiers of research. There is always an issue just beyond the margin of understanding; and if it is at all an interesting issue, there will be more than one theory about what the landscape will look like when the fog lifts. But that is not what you see when you pick up your *Times*. In the first place, the questions being discussed are pretty straightforward, the sort of thing the Queen of the Social Sciences should have well in hand. And in the second place, the disputants do not express themselves tentatively, as one would naturally do about questions on which the research remains to be done. They simply tell you the answers, only the answers are incompatible.

How does this happen? Is there very much less to economics than its practitioners would like you to believe, or is there a respectable science hidden in there struggling to get out? If the second alternative is the correct one, as I believe and hope to make you believe, why
does the discipline look so bedraggled on the surface? The state of the economy should not—or at least need not—bear much relation to the state of economics. You do not blame meteorology for a cold winter.

It is of course important that many of the questions economics deals with carry a lot of ideological freight. The outcome of economic analysis bears on some enduring and divisive issues of political and social choice. How big should the government’s economic role be? Should the government confine itself to providing for the national defense, policing the streets, and enforcing contracts? Or should the tax system be used to redistribute income, to generate more—or less—equality than the unrestrained market would do? Should business decisions be regulated by legislation—from emission controls on cars, to limitation of hours and conditions of work, to the pricing decisions of airlines, electrical utilities, and banks? Even more broadly, should the government undertake deliberately to influence the general level of economic activity, to smooth out booms and recessions, or is government economic policy part of the problem and not part of the solution?

The answer each of us gives to these policy questions will depend to some extent on our deepest political beliefs and our attitudes toward the nature of social life. But positive economics is intensely relevant too. You will probably want to know how the market system works before you decide if you would like the government to try to make it work differently. You will want to know how the market distributes incomes, or sets interest rates or handles air pollution or generates business cycles.

More than ideology is at stake in these issues. There is hard cash too. Those who have something to be taxed have a lively interest in taxation; those who set prices and get to keep the revenues have a natural interest in regulation; those who fill bureaucratic offices are not natural believers in minimal govern-
ment. One of the important differences between economics and ornithology is that political and business empires are not much affected by the findings of ornithologists. Economics, however, is not for the birds.

Don't misunderstand me. I am not hinting that economists are corrupt, that they fudge their research findings either for lucre or for a cause. That may happen once in a while, at some level of consciousness. But I don't suppose economists are any more or less corrupt than biochemists or oceanographers or literary scholars. The fact that the stakes are large has a more subtle effect. The telephone keeps ringing, and the caller is a newspaper writer or a television commentator or a staff person for a Congressional committee or the Senator himself or the lawyer on one side or the other of an antitrust case. They need answers now, and they generate a lot of pressure for hasty answers.

Worse yet, they generate a lot of pressure for simple answers. If they don't get a simple answer they will simplify whatever they get. The reporter for Time or CBS News will listen to a complicated paragraph which specifies assumptions with care and admits that the statistical evidence has some weaknesses, and will reduce it to one unqualified, wrong sentence. That being so, there is a powerful temptation for the economist to skip the complicated paragraph and replace it by one declarative sentence. It may not do justice to the issue, but at least it will be one's own. In that way, any practiced reporter can make a few phone calls and report that Professor A says that monetary policy is too tight and Professor B says it's too loose. They could trace their difference of opinion to a difference in their reading of some complex and noisy evidence, or perhaps to a difference in goals. But all you know is that A and B say quite opposite things, and you naturally wonder if either of them actually knows anything worth knowing.

Should Professors A and B realize that this will happen, and politely decline to say...
anything to the reporter or committee staffer or antitrust lawyer? Perhaps, in an ideal world; but you should realize that, in our world, the result would be that only the least scrupulous scholars would have any influence on public affairs. People like this audience, of course, have a terrific advantage. You can ask one of your economist colleagues what it is all about and what is the true source of controversy, and he or she will tell you the straight stuff—I hope.

All this cannot be the whole story, however, and I don't think it is even the most interesting part of the story. Professors A and B do come to different conclusions about monetary policy, or about the consequences of deregulating the price of natural gas, or about the mechanism of inflation. I have tried to say why the difference is not as bald or as simple-minded as the media make it seem to be. But there remains genuine scientific controversy that seems to last longer than one would like. Otherwise Professors A and B would be ashamed into agreement or into silence. What is there about the subject matter and research methods of economics that makes the resolution of clearly-posed controversial questions so slow and uncertain?

I want to describe a peculiarity in the way economics goes at things. Let me take a specific field of research for concreteness: the behavior of consumer spending. From the theoretical side, economics is dominated by the principle of methodological individualism. The pure theory of consumer spending is a theory about the spending of an individual consumer, a wonderfully elaborate theory. It is understood that the individual consumer's spending decisions may be influenced by social forces and the opinions of others, although economics does not in fact go into that sort of thing very much; the key is that the theory always takes the individual consumer's decisions as the factor to be explained. But then something funny happens. It is apparent to common observation that the spending decisions of a single consumer have a large un-
predictable element from day to day, or week to week, or even year to year. If I ask my wife in the morning what we are going to have for dinner that night, she may very well say she doesn't know. She hasn't decided, and it's ridiculous to suppose that economic science could hope to predict whether it will be sausage and peppers or chicken with cashews. Of course, a single consumer's total expenditures for all food in a year are much more regular and predictable than the day-by-day diet. But even so, the margin for error is pretty big. You might easily have spent ten percent more or ten percent less in a given year. Your annual expenditures on new autos might be zero or $10,000 depending on all sorts of causal forces. But there is another funny thing: nobody cares about what an individual consumer does anyway. We really want to know about the market for automobiles or fish, or even about the aggregate of all consumer spending on all nondurable goods in all of 1982.

When we pass from theory to practice in economics, we pass from individual to collective behavior. Now there is no "pure theory" of aggregate consumer expenditure or even of aggregate spending on food or on eggplant. In pure theory, aggregate consumer spending is nothing but the sum of all consumers' spending as individuals. But there is no more hope of building up a practical understanding of sausage consumption from below than there is of building up a practical understanding of a Boeing 707 molecule by molecule. So when an economist sets out to make an "econometric model" of aggregate consumption (whether of sausage or of goods in general) the guidance he or she gets from theory is only general, not specific. Theory provides guidance as to what the main determinants are likely to be, which of them favor spending on consumption and which work against it, and perhaps a little about orders of magnitude. But there is no complete tightly specified model handed down by theory. Then it always turns out whenever you test a reasonable and flexible model against the aggregate facts there is a large com-
ponent of noise left over. No explanation of aggregate consumer spending is remotely perfect—and consumption is one of the easier things to study. Some of the noise comes from errors of measurement and inability to measure precisely the right quantity, some comes from the intrinsic randomness of individual behavior combined with the fact that individuals in a society are not statistically independent random variables, so an event that turns one of them inexplicably off sausage is likely to affect many others. And some of the residual error comes from the primitiveness of our account of individual behavior.

I am not asking for your sympathy. The point is that one of the consequences of this way of doing business is that there will very likely be at least a couple of econometric models of aggregate consumer spending that are intellectually defensible, and fit the data reasonably well and approximately equally well. Since they do fit the data approximately equally well, you might think it doesn't matter which of the competing representations we accept. Indeed, as long as the questions to be asked do not involve any extrapolation from the everyday observational magnitudes, as long as we are not asking ourselves what would happen if something—a tax, say—were substantially changed, it probably doesn’t matter much. But of course more often than not, exactly what we want to know is how a temporary or a permanent income tax change will affect consumer spending. As soon as we get away from the immediate neighborhood of everyday observations, the alternative models are perfectly capable of giving different results. You can see how the door is opening for Professor A and Professor B.

Laboratory science has a classical way of deciding between alternative models of an empirical situation. If the two models are truly different they must give different predictions under some circumstances. An experimenter can try to recreate just those circumstances, controlling everything else, or at least nearly everything else that matters. Since the two
theories have different implications, at least one of them must turn out to be wrong. That is an excessively idealized description of experimentation; sometimes the experimenter doesn’t know exactly what needs to be controlled, sometimes a controlled experiment is very difficult to conduct, sometimes the experimental error is large enough so that no solid inference can be drawn. But the method of controlled experiment has a lot of successes to its credit.

I hardly need remind you that economics has no possibility of controlled experiment as a way to discriminate between alternative models of aggregative phenomena. All we have is history’s single experimental run. Suppose that the tax change we are interested in is actually made, and a couple of years go by. Will it be possible after the fact to dismiss at least one of the two competing explanations? Not necessarily. History being history, many other things will have changed as well as the tax laws. Some of those extraneous changes will have calculable effects, but others will simply add to the noise. In the end it may still be difficult to discriminate between the two theories: especially because an ingenious and persevering protagonist of the theory that looks worse can always go back and doctor it a bit—amend that constant, add that variable—and save the theory just as Ptolemaic astronomers kept saving their picture of the heavens by juggling epicycles. It is too pessimistic to say that old theories never die, but sometimes it seems as if they fade away a lot slower than General MacArthur.

Before I go on, let me give you an important example of the sort of thing I mean, as current as this morning’s newspaper. Everyone agrees that there is a lot of inertia in the process of inflation, in the sense that inflation, once it has started, tends to keep going unless something in the environment changes. Why is that? There are two competing explanations. One emphasizes the passing on of costs: every business, when setting its price, sees that the prices of the things it buys have been going
up, and it naturally wants to pass the higher costs on to its own customers and thinks it will be able to do so. If the inflation were to end suddenly today, many businesses, those that have not increased their prices lately, would be left holding the bag—their costs having risen and their profit margins narrowed.

The other explanation emphasizes expectations about the future: every business, when setting its price, anticipates continuing inflation and naturally wants to keep up with the pack. Not to raise price now is tantamount to cutting one's relative price in the near future, and most businesses see no reason to do that.

The first thing I want you to notice about these two stories is that each has some intrinsic plausibility. They do not directly contradict one another in any obvious way. They could both be capturing an important aspect of reality, and probably they are. It is bound to be very hard to discriminate between them, or to decide how much of the truth belongs to one and how much to the other. The reason why it is so hard is because (a) they are both clearly consistent with the brute fact that inflation has a lot of inertia; (b) one of the sources of inflationary expectations is likely to be the experience of past inflation, so even the forward-looking story is in practice backward-looking; and (c) direct observation of some of the basic constituents, like expectations about future inflation, is hard to get, maybe impossible.

In fact, both models have respectable protagonists. There has been a lot of econometric work, attempts to embody each hypothesis in a formal model whose behavior can be compared with the actual course of inflation. I tend to favor the passing-on-of-costs view, partly because I have a healthy suspicion of self-sealing explanations in terms of "expectation" which seem to have a ready-made way of dealing with whatever happens. But even I have to admit that there is little to choose between the two hypotheses in terms of their ability to explain the actual course of inflation. Each gives a respectable fit. Neither is perfect, but neither is a lot less perfect than the other.
Does it matter which story is right? Unfortunately, it can matter a lot. Suppose that inflation has been going on for a while, and you would like to bring it to a stop. If you are of the backward-looking, cost-oriented persuasion, you will be led to the opinion that it cannot be done in a hurry, because any sudden end to inflation will leave part of the economy holding the bag, having experienced recent cost increases without a price increase. The losers will find that outcome unfair. They will try to get their own back; and if policy can succeed in stopping them, it is likely to have bad side-effects because it will have inflicted real losses on part of the economy, so their production and employment is likely to suffer. You will be tempted to try for some coordinated, phased, gradual, deceleration of the ongoing inflation.

If you are of the expectations-oriented persuasion, you will see things differently. In principle, there is no reason why expectations about future inflation cannot be changed dramatically overnight. All that is needed is some gesture, some conviction, some promise, perhaps something slipped into the water supply. I drift into sarcasm, but only because sometimes the Reagan Administration's representatives seem to adopt that line: it hardly matters what we do—if only you will believe us, your belief will make itself come true. Professors A and B are halfway through the door.

There is one more general point I want to make before I turn explicitly to the question of progress in economic analysis. Granted that there are no controlled experiments to permit discrimination between competing hypotheses, could we not hope that patience and the accumulation of data will eventually weed out the unsound theories and leave us with mounting confidence in the survivors? Even history's one experimental run should do the trick if only it goes on long enough. Yes, that does sometimes happen, and it is indeed the way progress gets made. But not always, for a reason that illustrates another important difference between economics and natural
science. Long runs of data may not carry conviction in economics because over a long enough period of time the question may remain the same but the answer may change. Much of what happens in economic life depends on social institutions, attitudes, standards of acceptable behavior, and the like. These things change, partly for reasons quite outside of economics, partly in response to what has happened in the recent past. Economic time series are not stationary, a statistician might say. If a theory that worked well in the 1950s and the 1960s goes sour in the 1980s, that does not necessarily mean it was wrong in the 1950s. It may just have stopped being right. But if that is the case, then the observations of the 1950s and 1960s may no longer be relevant for someone trying to understand the 1980s. Not everything comes to the economist who waits.

Here is a current example. For almost two decades after the Korean War, there appeared to be, in the U.S., a reasonably stable relationship between wage inflation (and therefore price inflation) and the general state of the economy as measured by the unemployment rate and a few other readily observable variables. That relationship became almost a household word under the name “the Phillips curve.” In the 1970s, that previously stable relationship broke down. Rates of inflation were several percentage points higher than previous experience with comparable unemployment rates would have led any observer to expect. Some patching-up could properly be done—no relationship derived from history can be expected to allow properly for causal factors that have previously been dormant—but even so, the past decade has not been kind to the Phillips curve.

This sort of experience can evoke two different responses from the profession. One response is to suppose that the Phillips curve was always doomed to break down, and to find reasons why the relation was flawed in the first place. That can be done, and has been done. I think most macroeconomists take that line.
The intrinsic flaw they have found is related to my earlier story about expectations. Its relevance has not been verified, probably cannot be verified convincingly—"expectations" being such a slippery concept—but neither can it be dismissed.

I have the nagging feeling that the breakdown of the Phillips curve in the 1970s might have a sloppier explanation. Institutions evolve. Economic behavior itself changes under the pressure of experience. The events of the 1970s were in some ways different from those to which the system had been accustomed: the series of "supply shocks" from oil, food, raw materials, and devaluation that struck the U.S. price level were unprecedented. The Phillips curve may have broken down because it was no longer true, not because it had never been true.

Some economists would regard what I have just said as a confession of superficiality. If the answer changed after the 1960s, it must have changed for a reason. And that reason, the law of motion of changing answers, is the bedrock truth that economics ought to be seeking and discovering. If it is a matter of institutions, attitudes, and standards of acceptable behavior, then there must be laws governing them. I don't even need to decide if I agree with that diagnosis: it asks for more than economics can ever hope to deliver.

The title that I announced for this talk was "Does Economics Make Progress?" You might think that the answer I had in mind was "No." But of course if I had thought that, I would have talked about something else. Where Professors A and B have trod, Professor S can go. The true moral of the story is different, and it is time I came to it.

Yes, economics does make progress. Even if I stick to the hardest and most important part of the subject, aggregative economics, it has made considerable progress. In the easier parts of economics, the bottom of the iceberg that you do not see because it is not newsworthy, things move even faster. I want to say
a few words about that before I finish up my main theme.

In pure theory, the axiomatic part of economics, which tries to exhaust analytically all of the implications of some explicit—and, one hopes, defensible—assumptions, there has been vast progress in power and refinement. I even think all that elaboration is useful to the working applied economist. But it is not easy to talk about it in mixed company, and I shall not try.

There is, however, another level of theory that is of great significance in economics. I don’t know if something similar goes on in other scientific subjects. It amounts to teaching ourselves how to think about some concrete problem rather than what to think about it. The idea is to specify the main causal factors and hunt for their connections and interactions, especially the unsuspected connections and interactions. The end product of this kind of theorizing is something like a wiring diagram for an electrical circuit. It tells you what goes where—or, better still, for those who remember the pop songs of forty years ago, you push the little valve down, and the music goes round and round, and it comes out here. This sort of enterprise, if it is to lead to understanding, needs to be guided by facts, but “stylized” facts will do, a few key magnitudes and those only approximate.

Economics has had lots of successes in that line of work. We understand much better now, for example, the principles that ought to govern the management of the North Atlantic fishing grounds. We have a much clearer idea—though this has been helped along by a large infusion of data from large-scale social experiments—how to analyze the effect of income-support programs and of taxation on the supply of labor at the lower end of the income scale. We have a much more complete picture than we used to have about the role of international exchange rates and how they are related both as cause and as effect to international flows of trade and capital.

The truth is that even in the difficult field
of aggregate economics, even at the excessively visible tip of the iceberg, economics makes progress. It makes progress just where it can avoid the pitfalls I have been trying dispassionately and self-servingly to describe. If we study aspects of collective behavior that are strong enough to be clear through the inevitable noise, persistent enough so that we can eventually collect and analyze a wide range of data, and academic enough so that they do not get too deeply involved in politics and its yen for simple answers, we do actually learn how the modern capitalist economy works. I want to conclude by telling you a little about two such successes, chosen adventitiously from the handful I could think of.

The first case history is precisely the example I chose at the very beginning—the behavior of consumer spending in the aggregate. It has a special charm, because many of the major contributions were made by members of the local academic community. The story begins with a common observation. Other things equal, rich families save a larger fraction of their annual income than poor families, and therefore spend a smaller fraction. Common sense is amply confirmed by surveys of the spending habits of families at different income levels. You would therefore expect that a whole country would save a larger fraction of its aggregate income as average family income rises through time, perhaps doubling every couple of generations. But when Simon Kuznets first began to create and analyze time series of the national income and its major components, almost fifty years ago, he discovered that it wasn't so. There was no tendency for the saving rate to rise, even though income per family did go up. This was a major intellectual puzzle, when I began to study economics seriously just after the Second World War.

In the 1950s, James Duesenberry and Franco Modigliani—then young men but now, I regret to say, a few years older than I am—Independently came up with similar explanations. Both had to do with the fact that families
probably see their standards of living in relative terms. Thus at any instant a family would spend a smaller fraction of a larger income, if everyone else’s income and spending were unchanged. But if all incomes increase together, the rich will have to spend about the same fraction of their higher income to maintain their same relative consumption advantage. The mere thought is pretty obvious, but the trick is to embody it in a form that can be made to work on the data.

A few years later, Milton Friedman produced a related but different theory, which introduced the notion of “permanent income” and built on the idea that families might spend the same fraction of their long-run income whatever their current income level. Current high-income families spend a smaller fraction than current low-income families because the first group is statistically likely to have incomes above their long-run level, and the second group more likely to have suffered temporary setbacks. Finally Modigliani and others did a more careful analysis of saving as a life-cycle phenomenon, as a way of smoothing spending over a lifetime during which income varies both randomly and systematically. This life-cycle story is probably the most widely accepted theory of aggregate consumer spending today. It has no trouble accounting for the Kuznets constancy of the saving ratio. It can explain quite a lot of other things about consumer spending. What it cannot do with any but the most modest success is to track or predict short-run fluctuations in the household saving rate, which can easily change by 1½ percent—or one-quarter of itself—in the course of half a year. It is, on the whole, a story of scientific success and, more than that, of progress. Our understanding of aggregate consumer spending has improved substantially, and all during my time as an economist.

Here is another example of progress—and remember, I am sticking to big questions with economy-wide significance, leaving aside the little everyday victories. Fifty years ago, the economics profession was trying, ineffectual-
ly at first, to understand mass unemployment; it had by then persisted long enough so that even Professor Pangloss, if he had taught at the University of Cambridge, could hardly have described it as a frictional maladjustment. It was natural then, as it would be now, to investigate the relation of unemployment to real and nominal wages. A.C. Pigou, then the leading economist of the English-speaking world, asked himself: what is the real-wage elasticity of the aggregate demand for labor? That is: by how much would the demand for labor rise if the going real wage were one percent lower? In the then current state of knowledge, Pigou might not have known what to do with that information if he had possessed it. But anyway he didn't possess it, nor was the statistical base adequate to provide good evidence. So Pigou made an estimate out of pure thought: putting one thing next to another, relying on common knowledge and educated guesses, he concluded that a one percent reduction in the real wage rate would add something like four or five percent to the demand for labor. One implication was that a small round of wage reduction would make a big dent in the problem of mass unemployment. Pigou was too canny to believe that universal wage-cutting could be a cure for the Depression; he felt there was something wrong with it, though his own analysis didn't tell him what was wrong.

If the real-wage elasticity of the demand for labor were four or five percent, it would be a very important fact for the understanding of economic fluctuations. Since Pigou's time there has been a vast amount of work done on the demand for labor. There is incomparably more and better observational material; history's single experimental run is fifty years longer and has been measured far more intensively. There has been a lot of analytical and econometric effort poured into the labor market. I can't tell you the details; the result is that we now think—not confidently, but reasonably—that the elasticity of demand for labor is about a tenth as big as Pigou
reckoned. A one percent reduction in real wages would likely, after a year, say, increase the demand for labor by less than half of one percent. There might be a larger long-run effect—that is hard to know, but it is irrelevant for business-cycle fluctuations anyway.

There have been other interesting by-products of this body of research. For instance, it now appears that in the U.S. trade union collective bargaining is able to achieve for members a wage advantage of some ten or fifteen percent over what unorganized workers would achieve in similar circumstances. Trade unions do other things as well. But what I want to emphasize is this: our understanding of the way the economy works is altogether different, and better, now that we know the elasticity of demand for labor to be nearer one-half than to five. Just for example, wage cutting may be a useful or even necessary policy for a troubled industry like automobiles, but it makes no sense at all as a generalized policy for fighting recessionary unemployment.

I could go on. I could produce other instances where our understanding of economic life has progressed substantially over the years. And I could introduce you to some current puzzles and controversies, where reasonable men and women can differ and do—and unreasonable men make the situation worse. But my list would be idiosyncratic and you would soon forget which is which. My message is that economics makes progress the way any discipline makes progress: by thinking carefully and respecting evidence.

I can finish with a story of which I am the butt and a former president of the Academy is the hero. Some years ago, an MIT colleague and friend, a distinguished theoretical physicist, called me up and suggested we have lunch. He wanted to talk about his son, who had just graduated from Harvard College in mathematics and proposed to switch to economics and do a Ph.D. at MIT. My colleague wanted to know what sort of subject his son was getting into, what sort of things a professional economist thought about, and all that.
I did my best to explain; and at one point I lapsed into self-pity and said that I thought economics was an extraordinarily hard subject, trying as it did to extract basic truth from very complex and unpromising raw material. At that point my colleague said: "Actually, I suspect that at the research frontier economics must be just about as hard as any other intellectual discipline; because if successes were really much harder to come by, young people would choose the easier fields to go into, and eat up all the easily available successes, until the only research problems left were just about as hard as those in economics." I nearly sank through the floor of the faculty club. My colleague was obviously right, and he had achieved the truth by using what is the quintessential economist's argument! A subject that Viki Weisskopf can invent over lunch can't be all bad.

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