PRINCIPLES OF ECONOMICS

by Benjamin M. Friedman*

Most kinds of intellectual endeavor hold out the prospect of a particular satisfaction, that associated with expanding the possibilities for thinking about ourselves and the world in which we live. Economics is no exception. To be sure, economics does have its particularities—an idiosyncratic mixture of a priori theorizing and data-based empiricism, a commitment to apply the scientific method despite the inability to carry out replicable or even controlled experiments, a closeness to certain contentious political issues, and so on—and as economists we are rightly aware of them. But in the end it is the similarity to other avenues of the intellectual enterprise that is more compelling, including not just the physical sciences but history, philosophy, and even literature and the arts. As a consequence, the core principles of what makes for good economics are probably pretty similar to the route to finding satisfaction in most other intellectual pursuits: Have an agenda, and know why it's important. Be awake; look around. Be ambitious but not over-ambitious. Have staying power. Decide who is the audience, and learn how to reach it. Keep things in perspective.

These principles may sound obvious, or empty, or both, but I doubt that when I first became an economist I understood them in the way I do now, and I certainly don't pretend that I have unfailingly adhered to them at every point since. Economics, again in common with so many other endeavors, is very much a matter of learning by doing. I think I have learned, along the way, about what the satisfactions of doing economics are and what general working principles are helpful for achieving them. My object here is therefore not so much to report what I have done, or even what I always now do, but to extract from both what I believe works best.

Have An Agenda, and Know Why It's Important

The agenda of economics is to understand an important aspect of the human experience: why we behave as we do in certain contexts, both individually and collectively; what consequences follow from the fact that we behave in this way; and in light of this behavior and its predictable consequences, what we might do, either individually or collectively, to improve our lot in this world. Saying this, especially to trained professionals, may seem either trivial or trite. But it is surely not trivial, and if it is trite it is also very often forgotten.

A distinction between empirical and axiomatic approaches to the questions at hand is familiar in many sciences, and economics is again no exception. In my own work I have always felt more comfortable following an empirical approach, by which I mean starting with some aspect of economic behavior that we actually observe and seeking an explanation. Why do aggregate production and income grow faster at some times than others, and sometimes not at all? Why do interest rates vary, and why do they covary among one another, as they do? How do businesses decide how much to borrow, and in what form? The axiomatic approach, starting with a few first principles and logically determining what consequences follow from specific additional assumptions, has been just as central to economic inquiry if not more so. But the greater risk, I usually think, is that of applying impeccable logic to proceed from assumption to conclusion when neither bears much actual connection to the behavior of the real people and institutions, and hence the real economies, that I regard as our subject's proper object of study.

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This paper will appear in Michael Szenberg, ed. *Passion and Craft, Economists at Work.* Ann Arbor: Michigan University Press.

Under either approach, however, it is essential to be able to say why the effort is worth while in the first place. The initial question I try to answer for myself whenever I embark on a fresh project—when I begin a new (at least for me) line of research, or pursue an intriguing loose end left by work I have been doing, or offer a new course for students—is "Why am I doing this?" What can I learn, and why might that be valuable? Is the behavior I want to examine important in its own right? Or is the knowledge to be gained important because it might shed light on some related question? In that case, why is this other question important? The main reason I find it more comfortable to begin work from an empirical direction is that that way I find it easier to answer these questions—and they can often be hard questions—about what I am doing and why.

By contrast, setting out to do research without thinking through why it is potentially worth doing is like trying archery in the dark. There is some small probability that any randomly directed arrow will reach the target, and with enough bowmen taking enough blind shots, inevitably some will. Similarly, some few economists who are entirely unaware of the broader context that might make others value their findings will probably hit the bull's eye anyway. But the likelihood of doing so is far greater if a keen sensitivity to just that broader context shapes not only the selection of the question to be attacked but also the means of investigating it. Some empirical findings, and some theorems, become important because they give answers to questions people genuinely want answered; others don't because they don't.

The immediate implication of this seemingly obvious point is that, with limited time to spend, not everything that is doable is worth doing. Specifically, not every extension to a theorem is worth proving, nor is every empirical observation worth explaining. Even more to the point, especially for purposes of younger researchers, the mere fact that so-and-so has published a paper on some subject or other does not by itself make that subject worth further investigation. (It may not have merited so-and-so's original paper either, but that's a different matter.) Reading the journals is an excellent way to learn research methods; it's a poor way to choose research topics.

What does lead to good research questions? Here too, I have usually found the attractions of the empirical approach compelling. If the object of economics in the first place is to understand certain aspects of behavior by individuals and institutions, or its consequences for whole economies, then the most straightforward way to find simulating topics is to observe that behavior. For behavior in the aggregate, that mostly means listening to the questions concerned people are asking. For individual behavior, just watch. For behavior by institutions, find a way to watch.

When I was a graduate student, I took on a series of either part-time or limited-term assignments for various components of the Federal Reserve System. One was to study the conceptual structure underlying the Board staff's presentation of information to the Federal Open Market Committee. (The key question was how to structure the conditionality of future economic outcomes on the Committee's own monetary policy decisions.) Another was to serve on a committee, made up of representatives from the Board and some of the regional Banks, to recommend how best to introduce money growth targets into the Open Market Committee's policy decisions. (In those days—as is the case again today—the Federal Reserve didn't use money growth targets.) The first job I took after finishing my formal education was working at a New York investment banking firm. I wasn't in the economics department (the firm didn't have one at the time) but rather divided my time between the part of the business that worked with corporate clients on their bond issues and the part that sold the securities to institutional investors. Much of my subsequent research—on the theory of economic policy, on targets and instruments of monetary policy, on corporate borrowing decisions, on portfolio behavior and the determination of interest rates, on the role of credit markets in influencing macroeconomic activity—grew out of these early first-hand exposures to actual economic behavior. For just the same reason, in more recent years I have valued highly the opportunity to work with some financial institutions on the kind of sustained basis over time that has let me watch, and ask questions about, how they conduct their business. (By contrast, I rarely accept one-shot assignments.)

Regardless of whether research is empirical or axiomatic, however, the question of importance remains essential. The value to me of those early opportunities to see some interesting institutions at close hand was not just in suggesting research questions but in showing me who wanted to know the answers to what questions, and why. The object—the source of satisfaction from the enterprise as a whole—is not to maximize the number of published papers to one's credit but to shed as much useful light on the subject as possible. If the first question I ask myself is why I think a potential topic is important, the second is who will be interested, or better yet surprised—or even better still, discomfited—by the potential findings. In much of my work on monetary policy, for example, the objective has been to show that key aspects of behavior in the economy in which we happen to live make mechanistic rules for central bank conduct unhelpful. I thought that work was worth doing (and I still think so) not just because the subject is inherently important but also because so many people prefer to think the opposite. The ultimate question for any researcher is always how people will see that particular slice of the subject differently because he or she has worked on it.

Be Awake; Look Around

If the objective is to shape one's own research agenda in light of the actual behavior we observe and seek to explain, it helps to pay attention. New phenomena—the corporate debt explosion of the 1980s, for example, or the OPEC oil price increases in the 1970s—are especially interesting, either because they represent a new form of behavior or because they provide a new window for analyzing aspects of economic behavior that are already familiar but only from other lights. But it is striking how much there is to learn from simply watching people and institutions do what they have always done, or from listening to people describe what they do.

One reason this kind of observation of the ordinary is so important is that economic thinking (that is, the thinking of professional economists) is so often blinkered by the assumptions we impose and, moreover, that those assumptions are themselves so arbitrary. Aspects of everyday behavior that do not fit conveniently within the framework determined by whatever assumptions are fashionable at the moment remain, for all practical purposes, invisible. An example on which I worked for a while, alas in the days before doing so was fully respectable, is credit rationing. It is embarrassing today to recall the air of derisive ridicule with which distinguished economists not long ago dismissed even the possibility that lenders might adopt any strategy other than raising the interest rates they charged so as to bring loan demand into equality with loan supply. The fact that almost everybody who knew at close hand about loan markets thought bankers did sometimes ration credit, and said so, was simply no match for the fact that there was no formal maximizing model capable of rationalizing such behavior. But as soon as someone thought to bring to bear in a formal way such notions as asymmetric information, adverse selection and moral hazard, then of course credit rationing might occur, and a subject once better ignored in polite professional company became open game for accepted scientific investigation.

The point is not that simplifying assumptions (in this case, perfect information) are not useful—indeed, they are necessary to carry out any serious analysis—but that the conventionally accepted simplifying assumptions of the day are often highly arbitrary and hence subject to change, and therefore that there is no shame in choosing new ones when observed behavior doesn't fit snugly within the usuals. Just as for a long time the prevailing theoretically correct thinking rejected even the possibility of credit rationing, for a time (mercifully brief) the prevailing theoretically correct opinion took on faith that because people's expectations were rational, pre-announced monetary policy actions simply couldn't affect output or employment. In this case it wasn't long before numerous economists pointed out that the models that gave rise to this conclusion rested not only on a quite specific (and, on reflection, perhaps unsuitable) notion of "rationality" but also on a host of other questionable assumptions like frictionless adjustment of prices and wages. Even so, for some years every conference on macroeconomics was forced to listen to the repeated assertion that economists would have to proceed as if this model were a good characterization of the world because "it's the only well worked out model we've got." Here again, the presumption was that behavior simply could not exist because there was (as yet) no maximizing model to account for it.

For purposes of doing theoretical economics, the antidote to such wrong-headedness is to look for new assumptions. As in the credit rationing example, maybe information isn't perfect. As in the monetary policy example, maybe markets don't adjust frictionlessly. The range of conventional assumptions subject to challenge is enormous. Maybe personal utilities aren't independent. Maybe aggregation does matter. Maybe the dependence of this on that isn't linear. (Much interesting literature in recent years has usefully explored conditions that give rise to multiple equilibria, but of course that possibility follows immediately when the relevant behavioral relationships are nonlinear.)

For purposes of empirical work, the message is that an observed phenomenon is no less interesting to study just because nobody has written down a maximizing model to explain it. Indeed, in that case empirical findings may be the best clue to what assumptions need changing in order to deliver just such a model. As I have listened over time to the questions that my friends in public policy institutions and in private business firms ask, I am often struck by how little we—economists—have to say about what they want to know. (Sometimes I am struck by how much we know, but here my point is different.) In part, these lacunae persist because it is genuinely hard to learn about some kinds of behavior and their consequences. But in some cases we have just not asked the right questions.

A whole other reason for paying attention to what is happening, and to what people are saying, is that the behavior we study changes. Not behavior in the sense of the ultimate underlying "meta-model," of course; but what economists actually study is not the meta-model but behavior in one usually tiny piece of it that takes the rest as given. For just this reason, institutions—legal arrangements, business practices, social mores, and so on—matter importantly for many aspects of economic behavior. And when those institutions change, economic relationships that depend on them, in ways either obvious or subtle, change as well. There is a tautological sense in which it must be true that inflation is "always and everywhere a monetary phenomenon," but that is not the sense in which many people in the United States understood this notion a couple of decades ago, before observed inflation and the conventional M's began to go their separate ways. Simply to assume that answers to important questions derived from past experience remain right answers is to miss much of what is interesting and important about our subject.

Finally, yet another reason why it helps to look around is that the questions people ask change too. To be sure, issues like the real costs of disinflation, or the value of creating a market for price-indexed securities, or the gain in efficiency from indexing the tax code, are always valid subjects for economic research. But it is hardly surprising that more people want to pay attention to the findings of research on those questions when prices are rising rapidly than when prices are more nearly stable. For the same reason, whether government budget deficits in a fully employed economy crowd out private capital formation, or under what circumstances a deficit would have to be monetized, was not much of an issue in the United States before the 1980s. This did not mean that there was no point in addressing such questions before then. But the context that determines whether any specific piece of research speaks to a matter of broad concern, and hence has the potential ability to have significant impact on widespread thinking, clearly changed. People who don't look around don't notice.

Be Ambitious, But Not Too Ambitions

Rabbi Tarphon, a noted sage of the first century, declared that "You are not required to finish the task, but neither are you free to neglect it altogether." Tarphon's injunction has always seemed to me a useful beacon for researchers, especially in economics. The part about not neglecting the task is obvious enough, but I think the idea that finishing it is not required is useful, indeed important, for maintaining a sense of purpose.

A curious outsider, taking a fresh look at economics, is less likely to be struck by how much we know than how much we don't. Few established empirical findings are genuinely stable across time and space. Most theoretical results depend on a vast array of simplifying assumptions. Many of these assumptions—atomistic competitors, independent utilities, linear functional relationships, identical "representative" agents, and so on—have over time become sufficiently conventional in the eyes of practicing researchers that they seem to require no justification (indeed, they are often taken for granted without even an explicit mention); but to the thoughtful outsider they may seem not just strange but factually wrong (as, of course, they are). Especially for someone newly beginning a research career, the resulting temptation can be to reject the entire working apparatus of modern economics as epistemologically flawed, and set out to erect a whole new edifice in its place.

That strategy is a recipe for failure. Discontent with the artificiality of whatever set of arbitrary assumptions is in fashion at the moment is a healthy motivation for making progress. Seeking to abandon useful workaday assumptions whosesale is a bar to making any progress whatever. There is tension but not conflict in wanting to change many aspects of how economists think yet actually investigating only one such change at a time. There is conflict but not fundamental inconsistency in attacking one unappealing assumption in one line of research while going ahead to use that same assumption, unappealing though it may be, in another line of research where the focus is different. The history of our subject shows that progress comes incrementally, in the middle ground between finishing the task and neglecting it altogether. Economics is a task that no one is required to finish, not even in one lifetime much less in one paper. The practical consequences of trying to finish this particular task are often indistinguishable from those of simply neglecting it.

A different form of over-ambition in economic research is the Icarus problem: trying to fly too close to the universal sun, in the sense of supposing that a particular piece of research comes closer to the ultimate meta-model than it (or anything else that is really feasible) can. The meta-model by definition takes all factors into account. It doesn't change with circumstances not controlled for, because it controls for all relevant circumstances. By contrast, fruitful economic research focuses on only a few key variables at a time, leaving the rest aside. This is not a flaw to be endlessly lamented but a fact to be usefully remembered.

In particular, this means that the universality to which we might like to pretend for our findings, because we appropriately aspire to it, just isn't there. Our results are local results. As environments and institutions change, so will even our favorite empirical relationships, and even our favorite theorems depend on more assumptions than we usually enumerate. This does not make our work valueless, just limited. By now many of the empirical relationships describing credit market behavior (and especially the borrowing behavior of firms) that I labored to investigate some years ago no longer correspond to current data. I may be sorry about that, but I do not have to regard the basic lessons of that work as worthless. The models I used were at best only small pieces of the meta-model, and as factors that I omitted from my analysis changed, so did the observed behavior.

A closely related temptation, also to be avoided, is the monocular syndrome—that is, the tendency of economists to assert monocausal explanations for complex phenomena. For many if not most problems, the most effective research strategy is not only to work on explaining one aspect of economic behavior at a time but also to focus on only one part of the explanation at a time. Not infrequently, a useful exercise is even to see how far it is possible to go in explaining the behavior in question on the basis of the one causal factor under investigation at the moment. All this makes for good economics. But it is important not to take such exercises too seriously, and so conclude that some important aspect of economic behavior really does have only one causal force behind it.

For reasons that are closely related to both the Icarus problem and the monocular syndrome, I have always been reluctant to extrapolate what we know from one context to others where essential aspects of the environment are different. A useful example is the study of hyperinflation (about which I too once wrote a paper). Hyperinflations are certainly interesting phenomena in their own right, not least because of their sometimes powerful political consequences. But can we apply the lessons drawn from examining the demand for money during hyperinflations, when one influence on portfolio choice is enlarged to a magnitude such that it actually does dwarf all others, to draw inferences about money demand under more ordinary circumstances? Can the experience of ending hyperinflations usefully inform our estimate of the likely costs of a transition from moderate but persistent inflation to price stability? I am usually inclined to be skeptical of such extrapolations. Instead, if I want to learn about a question, I try to study it in its own context. (For just the same reason, I almost always disappoint foreign journalists who ask me what advice I would give their own governments. I'm not being either politically careful or overly polite; I just don't think I know.)

Yet a different form of over-ambition in economic research is to require too much of a model, and in particular to strive for false depth. Here the example that comes most readily to mind is the treatment of the demand for money. Some years ago it became fashionable to argue that it is illegitimate to draw inferences about monetary policy from any model that lacks an internal explanation for why people hold money. (For reasons that I never understood, in much of this literature it was further regarded as bad form to acknowledge that the reason for holding money might have something to do with its usefulness in effecting transactions.) Why people hold money is surely a useful and important question for economic research to address. But it is also surely useful to do different research on the basis of assuming that people in fact do hold money and proceeding on from there. Insisting that both efforts must cohabit within the same model is a bit like wanting the driver's manual to contain a chapter on the origins of the convention that cars go on green and stop on red, or on why different countries opt for the right or the left side of the road. Division of labor does have its uses.

Have Staying Power

One of the hardest things to decide in pursing any agenda, including an intellectual one, is how long to stay the course. Nobody wants to give up too easily, just because people are initially resistant to a seemingly worthwhile idea, or because a few pieces of partial evidence point the other way. At the same time, nobody wants to hold onto an idea long after overwhelming evidence has contradicted it. Resolving this tension is rarely easy.

On balance, though, I'm usually inclined to stay the course more persistently than not. One reason is that much of economics suffers deeply from the short sample problem. It is not just that we can't conduct replicated experiments to address most economic questions, or that the one history we have does not represent a controlled experiment. The added difficulty is that for purposes of many of the questions we want to ask, that history is short. It is short in part because environments and institutions matter, and they change. We may have data on the volume of bank loans extending back into the nineteenth century, but the loan market today differs from the markets of earlier eras in so many ways—loan securitization, hedging capabilities, and competition from the commercial paper market as well as from abroad come immediately to mind—that the relevance of data from decades ago is of limited value for many research purposes. Our one history is short also because observations are not independent across either time or space. Regardless of whether we divide the data yearly, quarterly or monthly, how many genuinely independent observations does the post-war rise and then decline of inflation contain? How many independent observations does the growth experience of twenty-four OECD countries contain? While this line of thinking is certainly not ground for despairing of ever learning from empirical analysis, it does make me pause before too quickly changing my mind because I have seen one new set of regressions.

The continually shifting tide of fashion in acceptable assumptions provides yet another reason for resisting pressure to abandon an idea that usefully seems to explain the behavior we observe. As the example of credit rationing shows, what respectable opinion deems impossible can become part of what "everybody knows" with astonishing suddenness. I sometimes wonder whether I should have continued doing research on credit rationing, since I have always believed it is an important aspect of bank behavior. I know I would not have worked out the crucial maximizing model based on asymmetric information and adverse selection—my personal toolkit is not well designed for that particular task—but I am at least curious about what evidence and insights a sustained program of empirical research on just this aspect of financial behavior might have produced.

But saying that one should stay the course despite opposition and even some contrary evidence is not to say never to change one's mind. The object, after all, is to learn. Sometimes observed behavior actually does present pretty dramatic statements one way or the other. For example, I used to be receptive to the idea that saving is positively interest elastic, and I therefore was sympathetic to the general class of policy proposals for stimulating private saving to which a positive elasticity gives rise. After the decline in U.S. saving rates in the 1980s, in the face of truly extraordinary increases in real after-tax returns, I have changed my view. (I think the same decline in saving, in the face of record government deficits at full employment, was likewise pretty devastating to the notion of Ricardian equivalence; but on that one I was a disbeliever much earlier on.)

I have also learned over time that the United States is much more of an open economy that I used to think. The biggest mistake I made in thinking about the policy issues of the last decade and a half was to under-estimate how much the U.S. Government's budget deficit would affect the country's net export balance (and thereby change the direction of capital flows), and correspondingly over-estimate how much it would effect our domestic investment. The standard closed-economy model that shapes my most basic economic intuitions just wasn't adequate. I've also learned over time that price inflation is a much more serious problem than I used to believe—even though I still don't think our profession (me included) has much understanding of why.

So, changing one's mind is important too. But on balance, when the issue is in doubt, I'm inclined to stay the course and wait for others to change theirs. Most of the pictures on the walls in my study are portraits. The largest by far is of Winston Churchill, a man of determinedly held views if there ever was one. From the late 1920s on, Churchill was not just out of office but without real influence, his views rejected and ultimately ridiculed by the conventional wisdom of the time. He did not hold public office again until the fact of the opening of the war made it obvious that he had been right all along, and he became prime minister just nine months later. He was then sixty-five years old.

Decide Who Is the Audience, and Learn How to Reach It

I occasionally hear it said of some economist or other that he would be happiest just writing papers and putting them in his desk drawer, deriving ample satisfaction from the repeated act of analytical creation without ever showing its fruits to other people. I have never met such an economist. In a very few instances I have heard an economist I knew described in this way, but in each case I knew the person well enough to realize that what was said about him wasn't true.

Most economists, perhaps all of us, want not only to do interesting thinking but to communicate it to others. More than that, most of us want to persuade other people to accept our thinking. The principal means of communication are talking and writing. Of the two, writing is what lasts.

In our era writing by academics in general, and by economists in particular, has become the standard butt of stock jokes. I think that's unfair. To be sure, much writing by economists is simply bad. But much is quite good, and many economists write extremely well. Making younger economists think that they have somehow inherited a generic professional disability, a kind of congenital handicap against which they will have to contend for the entirety of their careers, does no one a service. The point is simply that writing well is an important part of communicating effectively, and an especially important part of persuading effectively, and that this is true for economists in the same way it is true for people who seek to communicate and persuade in countless other professions. As with anything else, the main secret to success is working hard at it. In the case of writing, this mostly means going back to it again and again and again—to find just the right word, to restructure a sentence or a paragraph, to insert a new thought, and sometimes even to change around the whole logical flow. My colleague John Kenneth Galbraith once referred to "the appearance of effortless ease that creeps into my (Ken's) prose on about the eighth draft." He was indirectly offering me advice, and I've tried to take it seriously.

Some dimensions of the matter, however, probably are harder for economists. The one I think is especially important is that many economists want—appropriately—to communicate with several different audiences who happen to use different languages. We want, in the first instance, to speak among ourselves. But academic economists also need to speak to their students, and business economists need to speak to others in their firm or to their customers. Many economists also want to speak to policy makers from time to time. Some occasionally want to address a more general public.

The problem of different languages is real. My first exposure to the Federal Reserve System was a summer job in the research department of the Federal Reserve Bank of New York. By then I had studied economics for four years in college and two more in graduate school. Although most of the people I talked with at the bank that summer were professional economists, I quickly realized that I just didn't understand what they were saying. (I don't mean that I didn't understand why the theory underlying what they said was valid; I literally did not understand many of the conversations taking place.) As I eventually discovered, they were in fact talking about things I had learned about. But they used a different vocabulary than I knew, and they left much of the context implicit.

Vocabulary and context are crucial to communicating effectively, and it makes little sense to address an audience in anything other than its own vocabulary or without providing the right context. I think much of the usual popular derision of academic writing stems from the reaction of one audience, either practitioners or perhaps even interested laymen, to material written for research professionals who constitute a wholly different audience. The vocabulary is strange, and even the words that should be familiar lack the context to give them genuine meaning.

American populism has always exhibited an anti-intellectual strain, and so the Congressman who wants to score points by making fun of the silly professors can easily draw laughs by reading selected passages from the professional journals in just about any academic discipline. While few layman are inclined to think they should be able to understand astrophysics or Byzantine theology, however, many non-economists do think they should be able to understand matters of economics. More importantly, citizens in a democratic republic have not only a right but, indeed, an obligation to understand major issues of economic policy. While I am often struck by how little economists know about the questions that interested laymen or public policy officials or business executives ask, in many cases I think we do know much that is useful. But it remains to communicate what we know to them. I think it is to our credit that so many economists want to address these nonprofessional audiences. But we can do so effectively only if we use a vocabulary that they can understand and if we provide the context that makes what we say meaningful.

Here too, what makes this kind of communication succeed is largely putting effort into it. If I think Congressmen, or bankers, or businessmen may be interested in the findings of the research I have been doing, I have to accept the fact that simply sending around reprints of my latest journal articles won't do. I have to decide whether I want to convey my ideas to those audiences or not. And if I do, then I know I have to write an account of those ideas directed at the audience I want to reach.

Some of my academic colleagues who read my *Day of Reckoning* book, as well as some friends in the financial community, told me they would have found the book easier to follow—not mention a lot shorter—if I had included some tables and time series plots to exhibit the most important trends and relationships in the data. They were right. (One person, whom I didn't know, sent me a letter saying he assumed I must have been writing from a set of tables, and asking if I could provide him with a copy.) But I didn't write that particular book for them. I deliberately chose a purely literary presentation—no tables, no data plots, no diagrams, and certainly no equations—because I wanted people to read it who would simply have put it down if they had paged through it and spotted any of these devices. I knew that once people actually decided to read the book, some well chosen tables and plots would have made it easier for many if not most. But I decided that for this particular effort at communication, the audience I wanted to reach included large numbers of people who, if they saw tables and data plots, would probably never read it at all.

Writing a book this way—producing a purely literary presentation of a subject we economists usually discuss among ourselves using both short-hand and short cuts—was, of course, time consuming. It took away from research I otherwise could have done. (That book was not research; I like to think of it as high-class journalism.) But I took the time because I thought that that particular effort at communicating, and persuading, was important. I felt about it, in some ways, a sense of moral obligation.

Keep Things in Perspective

One of our Presidents once remarked that a major personal challenge for people charged with public responsibility, especially at high levels, is to take their decisions appropriately seriously yet not take themselves too seriously. I think scholars face the same tension. We devote our lives to research and teaching on issues that we deem important. We take these issues and our work on them very seriously, and we are right to do so. But we do ourselves—and others too—a disservice if we fall into the trap of also taking ourselves too seriously.

Steering clear of this particular temptation is no doubt a matter of many dimensions, but in my own experience two especially stand out. First, some of the friendships I have valued most over the years have been (and still are) with economists whose views often directly contradict my own. We disagree with each other in our papers, we debate each other at conferences, and we argue with each other when we get together just to enjoy each other's company. I admire these friends, and I have learned from them. But more important, in the end, they are my friends and I value them simply for that. Another eminent sage, Isi ben Judah, asked "Why do scholars die prematurely?" His answer? "Because they abuse one another." Taking ourselves less seriously than we take the ideas on which we work may or may not enable us to live longer, but I think it does help to keep our work from obstructing personal relationships that can be deeply satisfying.

The other sense in which trying not to take ourselves too seriously has been important to me reflects a lesson I learned in a vivid way years ago when I worked in investment banking. I not infrequently worked on assignments with Robert Baldwin, a quite senior partner who soon afterward became head of the firm. I remember especially clearly the experience, on several occasions, of sitting in his office with a team of other partners and staff members, trying to schedule an important meeting with one major client or other. Somebody would suggest a date, everybody in the room would agree, and then Bob would check his calendar and declare that that was impossible because it was the day of his son's school play (or hockey game, or whatever was the particular event that time). Everybody else would exchange knowing glances, as if to say "This guy is nuts but we have to humor him," and eventually somebody would go on to suggest a new date. In the meanwhile, my own (silent) reaction was more along the lines of "This guy is the only one here who understands what's important."

Balancing our personal and our professional involvements is a tension that we all face. As is usually the case with such tensions, having a clear sense of priorities helps. I've always had mine pretty clear. My wife and sons come first.

But all this brings me back to where I began: Having an agenda is crucial. So is knowing why it's important.