

Three Nobel Laureates on the State of Economics

Interviews with Robert Solow, Kenneth Arrow, and Amartya Sen

At the start of a new century—indeed, a new millennium—it seemed more than appropriate to ask three of our most distinguished Nobel Prize winners to discuss where the profession now stands. We asked Robert Solow, Kenneth Arrow, and Amartya Sen to reflect on what they and other economists have learned in the last generation and what they failed to learn, as well as in what direction the profession should be heading.

Robert Solow

Q As we enter a new century, it is appropriate to look back and review what we have learned and, shall we say, “mis-learned,” in economics in the last generation or two. In what ways did knowledge advance most?

A. The first thing you have to say about the last generation or so is that the availability of decent data, including large micro data sets, has improved dramatically. That has changed the way most economists do research. Even macroeconomists need to

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verify that the presumptions they make about aggregative behavior are not out of line with ordinary economic behavior—individual by individual or firm by firm.

Q. What new conclusions has this yielded? What has it turned up that wasn't obvious before?

A. Let me take a body of research that interests me the most, concerning changes in employment in individual manufacturing firms. It turns out that gross flows through the labor market are very much bigger than the net flows we record. Changes in employment or unemployment are the net of large quantities of job destruction and job creation.

Q. So what they have found is that we have enormous job losses and job gains, though the net change in unemployment is comparatively small.

A. Right. Mind you, aggregate demand and aggregate supply remain useful concepts, but how you want to play with those things intellectually, how you want to think about them, is certainly affected. The labor market is different in an economy in which there is a lot of churning of jobs, even with only a small quarter-to-quarter change in unemployment.

Q. What other areas do these new data sets affect?

A. Well, it is important for anyone who is interested in poverty, to take just another of many examples. These studies from longitudinal surveys help us determine whether the population of poor people in any given year consists of the same people or a large flow of people into and out of poverty. We can only make such analyses because a lot of money has been spent and a lot of effort has been put into maintaining longitudinal databases and making them available to scholars who can compute with them.

Q What do you conclude from recent work in growth theory?

A. The fact that we now have national income accounts data for a hundred-odd countries has proved irresistible to economists interested in growth. They use the data to try to determine what factors affect growth over a twenty-five- or thirty-five-year period. I have never done that because, on the whole, I disapprove of it.

Q. Because of the quality of the data?

A. That is one thing. But what makes me question the value of the work is that the direction of causality is up for grabs.

Q. You mean, does capital investment cause productivity growth or does productivity growth cause capital investment?

A. Yes, but more. It works out even in such things as whether openness to trade improves economic growth, or countries that are growing satisfactorily are willing to open those trade barriers. And the irresistible tendency of part of the growth theory profession has been to always take the growth rate as the dependent variable and political and social and legal things as the independent variables—in other words, the causal factors. That seems to me to be utterly unjustified. It is probably often the other way around. So I do not care much for it. And it has also proved to be the case that empirical results, however you interpret them, are not terribly robust. Nevertheless, researchers are coming up with ways to deal with some of the problems. So some good comes of it, but I'm generally suspicious.

Q. A great deal of modeling has been done in growth theory in recent years. What have we learned about why countries grow?

A. To tell you the truth, we have not learned a lot in the last twenty or twenty-five years other than the empirical facts. The attempts to learn more about the sources of innovation—to endogenize technological progress—have not led very far. If a growth economist is asked, "What must I do to grow faster permanently?" not very much can be said other than to encourage innovation, to encourage (for small countries) the adoption of

innovations wherever they come from, and to get out of the way of the products and the processes. Also, to try to develop human capital—that is, to try to develop an educated, skilled, and trained labor force. And most of that could have been said twenty-five or thirty years ago.

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Q Let me move in another direction. At this juncture, are you disappointed in Keynesianism?

A. No. I started as a Keynesian, and I am still one—which is not to say that I have not learned. The crude, as we sometimes say, hydraulic Keynesianism of the early postwar period certainly needed improvement. And I'll even say how and where. The foundation of what we think of as Keynesian economics—and you understand that I'm not at all concerned with what Keynes really meant, I am talking about American Keynesianism—was built on the observation that, over the business cycle, prices are slow variables and quantities are fast variables. If you pick up any elementary text, if the market is in disequilibrium, the reflex reaction is that when demand is in excess, prices will rise and that will eliminate the excess demand. And if there is excess supply, prices will fall and that will eliminate the excess supply as consumers buy more. Then you go on to ask what kinds of institutions—monopoly, regulation, cartelism, whatever—will prevent that adjustment from taking place. But the first thing you think of is prices. American Keynesianism was built on the observation that on the business-cycle time scale, if there is inadequate demand to fully employ the population and use up industrial capacity, then it is not the case that prices adjust quickly to restore full employment and full utilization.

What does happen is that employment and output fall until demand is just adequate to buy it back. That is the basic insight. So, a lot of the time aggregate output is limited on the demand side, not on the supply side.

Now, one can go too far. To conduct macroeconomics without paying any attention to prices, other than interest rates—that is, to imagine that all wages are rigid and all prices are rigid—is going too far. And we had to learn how to do macro-

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economics giving a little more room for equilibration through prices without giving up the basic insight that remains true: In business cycles, it doesn't do to say there is a set of prices that will restore full employment because you cannot get those prices to change right away. So I have learned from both theory and observation that you need to pay more explicit attention to the way the price mechanism works on the business-cycle time scale. The economics profession, on the average, has gone much too far in rejecting that American Keynesian insight, and instead is moving too far toward the “real business cycle” theories that presume that prices do it all. We are always in equilibrium, they argue, because the prices have fully adjusted.

Q. What is the fallout from real business-cycle theory in the real world?

A. The real-world fallout is kind of interesting, because if you go all the way with the real business cycle, there isn't anything you ask the political process to do other than to get out of the way.

Q. The real business cycle presumes that we are always in optimal equilibrium.

A. Right. If vulgar people think there is something like a recession going, that is their mistake. The real business-cycle people say it is not a bad thing. It just means there has been a shock to the economy to which the most favorable adaptation is a reduction in output and employment.

Q. So we are always in the best of worlds?

A. Exactly. It is interesting that the kind of intellectual predominance of that view has not led to anything much in the real world because the theory does not suggest doing anything in the real world. The real world, it insists, has already taken care of things. This has hampered acceptance of real business-cycle theory outside the refined economics profession because “practical people,” whether they are government officials or business people, know damned well that in a recession things are not so great. And to be told that there is nothing to be done is really not very satisfactory. They do not argue that you cannot do anything or you may make matters worse. They argue that nothing needs to be done.

Q Has the advent of real business cycles and the diminution of the Keynesian view taken fiscal policy off the table?

A. What this change in the economics profession has surely done is to make it harder to advocate the sort of standard macro policies that came from American Keynesianism. I do not really understand how it has been discredited, but I know I am told that it has been discredited. The practical importance of this intellectual shift has not been as great as it might have been. So far as I know, anyone who does macro-modeling for policy purposes or for predictive purposes, whether it is DRI or another firm or the Federal Reserve Board, is still using for that purpose a very large-scale version of a kind of American Keynesian model with some modifications.

Q. Are there other general areas in which you believe knowledge has seriously advanced?

A. I think so. One is the new industrial organization. When I was a student, industrial organization consisted of thinking about antitrust policy and what was called workable competition, and doing industry studies. Today industrial organization has a body of theory that is concerned with business strategy; that is to say, decisions that take into account what their domestic rivals or foreign rivals are doing or are about to do. Industrial organization theory and practice now talk about legal institutions and other institutions that are likely to promote more efficient, more strategic decision-making on the part of individual firms. That has changed the texture of discussion in antitrust policy and regulatory policy quite a lot, and for the better. Another area I was going to mention, but I know less about, is the evolution and refinement of the theory of finance. Twenty-five years ago the best you could say about the frontier in financial economics was that there were new measures of risk involving the mean and variance of the distribution of returns and the Modigliani–Miller theory about capital structure—that, given restrictions, the level of debt did not matter. Today the financial markets are much more sophisticated—which does not mean they are more stable, of course, but much more sophisticated—and discussion by economists of the financial decision-making of firms is much more sophisticated and much more realistic.

Q In your view of the profession, was there any major disappointment, theoretically speaking, in the last twenty-five years?

A. In my view, yes. Formal game theory, although it has done quite a lot, has not quite lived up to what one might have hoped. I don't want to come on as someone who thinks that game theory

was a detour or mistake, because a lot of that industrial organization that I suggested was a favorable surprise was based on ideas from game theory. But if you look at the frontiers of game theory, it does not seem to have produced as much as one might have hoped.

Q. Are there any other disappointments?

A. Yes, large econometric models have not lived up to expectations. They turned out to be much too complex.

Q. What about going forward? What are the major issues that need pursuing?

A. Let me return to the analysis of growth and technological progress. It is possible to advance the analysis of the way technology progresses in two ways. The first is to somehow combine the abstract theoretical notions we have about the way market structure affects the rate of innovation with the work that is done in actually analyzing, almost at a sociological level, what goes on in industrial research laboratories.

The second line that growth theory needs to pursue is how technological advances are diffused. This is examined now, but it could be done in greater detail. Of great interest is the diffusion process across national boundaries. This needs to be studied in great detail—much greater detail than we have had. As I said earlier, we have not learned enough yet about how countries grow.

Kenneth Arrow

Q When you were starting out in economics, what were your biggest hopes?

A. I think my biggest hopes were methodological—to apply

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new developments in mathematics to economics. That doesn't surprise you, I'm sure. Those hopes were to apply econometric methodology and build models to be tested against data. When I started in the 1930s, econometrics was a fairly recently coined word.

Q. As we look back, what do you think has developed from econometrics?

A. Of course, there has been enormous change in the use of systematic and empirical methods and model-based methods. These have taken many forms, rather different from what we envisioned. There is a great deal more information now than in 1940. Something as simple as unemployment was not well measured compared to the kinds of surveys that exist today. National income accounting was just a graduate-student experiment in those days. So all the tools that are taken for granted today were just beginning to exist at that point. Maybe the work is disregarded, or you might disagree with it, but there is a kind of economic analysis now that is somewhat helpful in meeting any policy problem we are interested in.

Q. Was there a time when you thought econometrics would result in greater gains than have occurred?

A. I think so. We envisioned very large, complete models, in far greater detail, that attempted to cover the economy as a whole and take into account its many interactions. What we have mostly, to this day, are single-market analyses, or aggregate models of the entire economy, such as the monetary models used today.

Q. You mean we could not manage the more detailed models?

A. Yes.

Q. What was the flaw—what was the central problem?

A. We did not realize how difficult dealing with these interactions was going to be. I got a glimpse of this difficulty because in my military service I was a weather forecaster, a meteorologist. What I found then was another example of a very complex,

interacting system. It had a big advantage over economics because the fundamental theory was very well understood.

Q. The fundamental theory of weather?

A. The elements that went into weather. Not the theory of weather, but what happens if air carrying water vapor rises or falls with changes of pressure or temperature, and phased transitions—all of that, which was very well understood because we had a lot of laboratory experiments from which you could deduce these things. And, yet, with all this knowledge, which was stronger than anything we ever had concerning the elements of the economy, we could not forecast very well.

Q. Is one of the outcomes of experimenting with large-scale econometric models that we have learned more about the relationships in the economy, as well as the causal directions?

A. I am not sure that has come out of the large-scale models. We learned something from the experiments that actually were run for a while in the 1960s and 1970s, such as the income maintenance experiments or the health insurance experiments, where you have much better data. We could not replicate the scale of the whole economy. Here is an example. We developed the idea that consumption does not depend just on current income. Franco Modigliani developed his lifetime-cycle hypothesis by looking closely at a very narrow set of data, where you can get much more detail than you could fit into a complete model. The trouble with complete models of the economy is that the quality of the data is not uniform. A scientist cannot study everything, so he must study those places with the best data.

Q Are there some conceptions that you held, say, in the 1950s, that you had to give up over time?

A. Yes, indeed. One I was convinced of was the value of economic planning. I believed we could correct flaws in the eco-

conomic system. In particular, I was concerned with the inefficiencies due to unemployment and the incomplete utilization of labor and capital. I thought we could overcome these problems by government intervention based upon overall planning. Also, of course, I was concerned about planning for the future direction of economies. I thought this direction could be enforced. Experience has shown that we were too optimistic. I am not talking about the experience of the socialist countries; I am talking

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about the experience of Western Europe. There was a lot of planning work done after the war, some of it by extremely competent people. And also, developing countries did a lot of planning.

Q. Were any of your attitudes then reinforced by economic research?

A. Yes, one was the importance of planning, not the overall economy but specific projects. In the old days, it was water resources, for example. There is a great value in cost-benefit analysis for specific projects. A prime example is climate change and environmental interventions. The value of these analyses has turned out to be very high. We have improved greatly in our conceptual understanding in these fields, and they have turned out to be very useful in calibrating a problem. We certainly cannot be precise about it. My experience in interfacing with those working in biology and physics is that, as in my own experi-

ence with meteorology, their errors have high orders of magnitude as well. The economic analysis problems in, say, trying to estimate the value of cleaning the water do not look much larger than the physics or biology of the problem would be.

Q Do you think there is a tendency to minimize the magnitude of error when economists make public policy recommendations?

A. Yes. Statements made for persuasive purposes do not give full account of the errors.

Q. Academic research acknowledges those errors, but when research is applied, the magnitude of potential errors somehow becomes minimized.

A. There is a real problem—and I have talked to psychologists about this—in that it is very hard for people to grasp the concept of error. In public debate, if you say, “Well, I could be wrong,” nobody will listen to you. Nevertheless, it is extremely valuable to say that this is the most likely outcome, subject to this error or that error. It is scientifically the right thing to say, but it winds up giving a very blurry message.

Q. Do you think economists have been more subject to oversimplifying than those in other professions?

A. No, I do not. When I see natural scientists debate issues of public policy, they have the same degree of certainty that economists have. Remember estimates of costs of the superconducting collider, which kept on escalating.

Q. What about the very general question of market imperfections? Has economic theory itself underestimated the degree of market imperfections, information problems, and so forth?

A. Yes and no. When I was young, the big stress was on distortion due to monopoly. There was much discussion about im-

perfect competition theory, which was the big theoretical development of the 1930s. One of the leaders was Joan Robinson, and it was thought that there was a lot of inefficiency throughout the economy. Refined empirical evidence suggested that these inefficiencies were perfectly real, but they were not that large—not that we should not do something about them in egregious cases. But now our analysis has shifted to the externalities typified by environmental concerns, and it is accepted that they will involve costs to the area or region. The concerns did not start with economists, but they began to analyze these costs and, in a way, the issue is now generally recognized. People will argue over the magnitude of the problems, or some die-hards will take the position that my property is my property. But most people, most decision-makers, will take the view that there is a real case to be made, obviously to be fought out, that such costs are to be borne beyond the business itself.

Q. You have written recently that prejudice against African Americans still exists in the labor markets and has not been relieved by markets, as some economists claim it should be.

A. That has not been the case. To the extent that there has been improvement in the situation, it has largely been due to government intervention. So there are a lot of important problems that essentially require coordinated action with the government. Government is not the only coordinating agency, but it is the big one. It can take into account a larger variety of viewpoints. It is not an impartial judge, but it is an instrument.

Q. Are you concerned that some areas in economics have taken market solutions too far for ideological reasons?

A. There is no question that people, including economists, are motivated by their ideologies. Our ideologies are shaped in part by our education, and our education is shaped in part by the ideologies. There is a mixture, which shows up even in applied fields. My colleague Victor Fuchs has been surveying economists.

He gets several opinions on particular policies, and he also gets their opinions on the underlying theoretical or empirical issues. It turns out that they are not very well correlated. In other words, people can agree on facts and disagree on conclusions. The point is, people should not confuse them—they should not say, “This is the result of economic analysis,” instead of saying, “This is a result of my values.”

Q If you were to pick out one or two of the most impressive achievements of economics in the last generation or two, what would they be?

A. To me, the biggest new concept in economics in the last thirty years is the development of the importance of information, along with the dispersion of information. Specifically, the work is called asymmetric information theory. It is a move away from markets because it says there are all sorts of economic relationships—contracts, in a generalized sense. It puts much more emphasis on small, personal interactions, and much emphasis on the distortions in prices. There are private regulatory relations in a firm. We try to make these issues more understandable. I don’t say we have good quantitative theory because some of the empirical facts do not fit the theory very well. But it is much harder to fit the empirical facts to the competitive market theory. This gives a very different perspective to many things, including government policy.

Q. Can you give an example of that?

A. On the one hand, many things we think would be inefficiencies in a market system can be contracted out by private parties, so some of the problems do not seem unmanageable. On the other hand, frequently the individuals in a transaction, because of their limited knowledge, have no perception of the wider implications of what they are doing. In such cases, the

government should have a bigger role and can take a broader view about the knowledge available for interactions.

Q. Is there another achievement of the last generation or so that strikes you?

A. Another is the emergence of the ability to think dynamically and with regard to uncertainty. It shows up best in the analysis of the organized markets—securities markets, foreign exchange markets. There is still a good deal that we do not know, but we have some idea of how to describe the way people look ahead, how they form anticipations, and that these anticipations have a major impact on current economic activity. In the securities market, this is clear. Studying this market enables you to isolate something that is actually a more general phenomenon. That is why so much work has been done on it. But it demonstrates that the prices rule is wrong, even apart from the problems associated with asymmetric information, because the future prices are what rule. But future prices are not fact. Only current prices are fact.

Q. So future prices are estimates.

A. Yes. So the role of psychology, for example, becomes much more important. One idea that has come through this is the rational-expectations hypothesis, which, for all its inherent limits, is a pretty impressive accomplishment. It is deviations from that hypothesis that we are studying now, and they provide the basis for future research.

Q. Have there been one or two major disappointments?

A. What we thought was an understanding of macroeconomic fluctuations has turned out a bit poorer than was anticipated, which, of course, has policy implications. Specifically, I am talking about the idea that fiscal policy could regulate the business cycles. I think it took a while to understand that anything you do to counteract the current recession has future implications: It changes the money supply, raises the government deficit, and

so on. Apart from practical implications, this dynamic situation has intellectual implications. I am certainly not a believer in the “real business-cycle” story that we are always at full employment. It is obviously false.

Q. So you are inclined to believe that government has less power over macroeconomic fluctuations as a result.

A. Yes, I am. But government is not without power. There are

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tools other than fiscal policy. In the past few years in the United States, we have relied very successfully on monetary policy, but that is just good luck. This general area is something I do not have as much confidence about. Why are European experiences so different from American ones? In fact, there is a problem in Japan, which has used monetary policy and it does not work. It is not a cure-all. So what is happening? I would say that today monetary policy works under the particular conditions of the moment. But there are circumstances in which it will work and some circumstances in which it will not, just as with fiscal policy.

Q. Maybe there are fewer universal answers to such questions than you once thought, and maybe the circumstances require different kinds of policies.

A. I would never deny that. There are principles, but they cannot be applied in some slapdash way. We know the theory will work out differently with different assumptions. Europeans have a system very different from ours, yet it has worked. And our system might not have worked there.

Q. Economics, especially in recent years, has been highly criti-

cized for becoming too statistical and quantitatively oriented and, some say, less relevant to the real world. How do you respond to that?

A. I am a pluralist. I never thought that pure theory was going to work well. I do not think that pure quantitative analysis will work well. I think you need some philosophical basis. But you have to be influenced both by quantitative data and by quantitative analysis. I am sorry to be so bland. I think you always have to be working on a number of different levels.

Q. No, actually that is not bland. I find that an interesting answer, but, given the state of the economics profession today, have they not sufficiently nourished pluralistic voices in this regard?

A. No, I do not think so. And I do not want to discourage quantitative work. It is hard work.

Amartya Sen

Q You have worked closely on economic deprivation in your career. What do you think we have learned in this field?

A. Well, that has varied over the years. There was a period when I was studying hunger in general and famines in particular. I had seen a famine myself in 1943, when I was a nine-and-a-half-year-old boy in Bengal, in which 2 to 3 million people died. It seemed plausible, on the basis of what everyone knew at that time, that the food supply had not really been reduced very much. I found this very striking. When I proceeded to study the famine, it turned out that the food supply was hardly reduced at all. In fact, it was considerably higher than two years earlier, when there was no famine. How could there be such massive hunger and star-

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vation and death while the food supply situation was not at all disastrous, and the market economy was functioning? Moreover, in terms of the distribution of suffering, it seemed that most of the population in the famine-affected state of Bengal (what is now Bangladesh and the state of West Bengal in India combined) did not have any problem whatsoever in continuing to live a normal life and securing enough food to eat.

In fact, the famine had a peculiarly class-based structure. That turns out to be characteristic of many famines. They rarely affect more than 5 percent of the population and almost never more than 10 percent. This finding had an important implication—that you cannot begin to understand famines in terms of aggregate phenomena such as the total food supply. You have to go into the question of how people acquire food and what I call entitlement, how they establish their ownership or entitlement over food. In a market economy, this is done through purchase in the market, of course. In that economic mechanism, one can see the kind of problems that ultimately can lead to starvation. An important conclusion was that it is quite easy to prevent famines by creating the lost income of the famine victims. While a flood or drought will have some effect on total food output, it can have a more immediate and more severe effect on how people earn an income. In Bangladesh in 1974, for example, many laborers lost their employment in transplanting rice as a result of the flood, and these people began to starve (the local flood did, later on, reduce total food output somewhat, but the famine occurred and was over well before all that happened). People without employment are prone to hunger and starvation no matter what the total food supply is. Rising unemployment in general also affects the incomes of many. One can find a resolution of the problem by offering employment. Since the problem affects only a small proportion of the population—typically no more than 4 percent—even a very poor

economy can afford to provide people with that level of employment and income. Of course, if you can get more food from outside, that will also help to some extent.

But that was simply the economic side. On the political side, I also encountered the rather remarkable fact that there had never been a famine in a democratic country, whether rich or poor. And that political fact is readily related to the economic analy-

There had never been a famine in a democratic country, whether rich or poor. And that political fact is readily related to the economic analysis, namely that famines are extremely easy to prevent.

sis, namely that famines are extremely easy to prevent. So you have to turn your attention not only to economic incentives but also to political incentives. Why is it that some governments act quickly to prevent famines and others do not? And why in particular did all the governments that failed to provide such protection happen to be nondemocratic governments? The reason is mainly that while famines affect millions of people, they never affect the rulers, the military dictators, the leaders of one-party states, or the colonial rulers in foreign lands. In contrast, while the government officials of a democratic country may not themselves starve, nevertheless if a famine occurs, then they would tend to lose elections, be criticized by opposition parties, be attacked by the media, and be censured in parliament, if there is a parliament. So they have to act quickly. Thus, one lesson is that political and economic factors can be inextricable.

Q. How did democratic countries react to potential famines? Did you look at any specific examples?

A. Take India. India continued to have famines until independence in 1947. The last famine is the one I described in 1943. Even though the Indian economy was not very well managed and quite often badly managed, no famine took place after that because, faced with a threat of famine, the government had to act quickly by creating temporary public employment. And that happened again and again. In contrast, China in many ways was actually doing better than India in aggregate terms even before the economic reforms. Nevertheless it managed to have the largest famine in recorded history when the Great Leap Forward failed in 1958. Between 1958 and 1962 China lost nearly 30 million people in this gigantic famine. The policies that had failed and had led to the famine remained uncriticized by the media since the media was not free and unattacked by opposition parties since there were no opposition parties; did not lead to an electoral defeat since there were no elections; and were not the subject of agitation in the streets or in the parliament because street agitation was not allowed and there was no multiparty parliament. As a result, the disastrous policies continued for three years while 10 million people on average died each year. Even though China was doing many good things at the time and the leadership was quite sympathetic, the discipline of democracy was completely missing.

Q Can you elaborate on whether you think democracy itself can make contributions to the economy's growth?

A. One has to distinguish between different questions here. It has been claimed that democracy is bad for economic growth. A number of people have presented that thesis. There is very little evidence for that. The most extensive intercountry comparisons that have been made by people such as Robert Barro show little evidence that economic growth is affected one way or the other by democracy. But, of course, development is not just economic

growth, in my view. It is also a question of providing economic security and giving protection to the vulnerable. And I was mentioning just now that famine prevention is encouraged by a democratic system. That is the first of the three major things that democracy does—to provide political incentives, which also provide economic security. The extreme case, of course, is famine, as I mentioned. In Africa the unleashing of famine in the 1970s and 1980s occurred when the democracies were subverted one after another, from the mid-1960s onward, by military coups of one kind or another, caught up in the cold war so that any military dictator would immediately have the support of the Soviet Union if it were anti-American, and the support of the United States and the West if it were anti-Soviet. So the dictators always had friends and legitimacy in some kind of international gathering or assembly. This produced a system in which Africa became blanketed with dictatorships, and famine followed shortly after that. Countries that did not go in that direction, such as Zimbabwe or Botswana, did not end up with famine, even though they often had a much greater decline of food output.

Even today, the severe cases of famine and acute suffering at this moment—North Korea and Sudan—are two great examples of nondemocratic governments.

Q You have also commented on how political factors affect how we define economic needs. Can you explain?

A. What we define as needs really depends on analysis of what can be done and what cannot be done. There are many things we would like to have that we do not see as needs. For example, it would be really quite nice to live in good health for two hundred years. But we do not see that as a need because it is not feasible. So the whole analysis as to what is a need turns on what we regard as feasible. In the social context, that really re-

quires a public discussion about what can or cannot be done. Must 43 million Americans live without medical insurance, or can medical insurance be extended to them? That is, of course, a public discussion. So what we define as a need requires open public discussion and discourse. Of course, a democratic forum may not be adequate. In fact, the American public discussion has been very limited on health insurance for all. But if you did not have a forum at all, what is a need and what is not could not really be identified. So our values, our identification of needs, really turn on our understanding as to what is feasible and what is not and on our exchange of views on this and also on what is important and what is not. This is something else, then, that we are learning. Many people have argued that economic needs come prior to any public discussion or conversation, that they are therefore prior to political freedom. But economic needs and political freedom are very closely linked.

Q What was your third point?

A. My third point is that, as has been analyzed by many people going all the way back to Aristotle, human beings are intensely political and social animals, and, as such, their ability to participate in political discussion, to be able to have a free exchange of views and social and political thinking, is part of the freedom that we value in and of itself. It is not just that we want to be well-fed robots. We also want to be participating individuals in the running of the society. So democracy also has a central importance as a value. In some ways one might think that perhaps the most important thing that happened in the twentieth century is the recognition, which did not exist at the beginning of the century and does now, that in some ways democracy is a central requirement of social life. Rather than asking whether such-and-such population is ready for democracy, the ques-

tion has essentially shifted to: How could the country be a more successful, real democracy? In general the balance of opinion at the end of the twentieth century has shifted radically in favor of democracy in a way that was not true even in the middle of this century.

Q. Do you think it is plausible that democracy might actually contribute to economic development in the narrow sense—that is, in raising incomes because it makes people more economically secure, it enables them to work harder and gives them the opportunity to educate themselves?

A. You have many different questions there. Does democracy, in itself, help economic growth—the growth of GNP? There is not enough evidence statistically to indicate that that is the case. The statistical picture does not allow any strong conclusion either way. So the main reasoning in favor of democracy does not rest there. It rests on the importance of political freedom and liberty. It rests on the relevance of political incentives and their effectiveness in providing economic and other kinds of security. And it also rests on the constructive role of democracy and related political freedom. In analyzing what is feasible, what is a need, and in the formation of values, I would say that is the direction to go. Also there is a real danger that if one tries to justify democracy in terms of economic growth, one may be overlooking the crucial recognition that democracy has an intrinsic value, which has a constitutive importance in human life and a constructive role in our value formation, whereas economic growth is only instrumentally important.

Q When we look to the future, where would you like to see more research done, what would you like to see us work on finding out?

A. One thing that is clear is that there are no model countries

in the world. In each region you look at, you find that there are different types of failures. The American economy is successful in some respects, indeed in many respects. And yet there are, as I was discussing earlier, many millions of people without medical insurance. It is not just that these people do not have medical insurance, this situation affects human life, too. As I showed in a *Scientific American* article in 1993 (titled "The Economics of Life and Death"), if you compare large groups of relatively deprived Americans whose income per head is much higher than that of the third world, quite often their chances of living to mature ages is lower than for people in some of the poorest countries in the world. For example, African Americans have a lower chance of reaching an advanced age than, say, the Indians in the state of Kerala, which happens to have a good health service and a good education service. Kerala is a state of 30 million people in the south of India. African American men's chance of survival to a mature age is also lower than for Chinese men. It is lower than in Sri Lanka, and so on. So they have that kind of problem in the United States.

Europe does not have this problem because it provides health care more equitably. Of course, American health care is excellent. The only thing is, a lot of people are excluded from it. There is a bigger inclusion in Europe. And yet it has double-digit unemployment figures, which would be quite intolerable in the United States. Given its focus on self-help, employment ends up being very central in the political ethos of the United States. On the other hand, social help is a central aspect of European understanding, so the millions of medically uninsured people that America tolerates would not be easily tolerated in Europe. If you look elsewhere, in the third world you see again a mixture of failures and successes. East Asia has been quite successful in terms of economic growth, but it is not immune to crisis. And when a crisis occurs, there is very little social security in

many of these countries (such as Indonesia, Thailand, or even South Korea before recent reforms). In India, some of the basic social opportunities such as providing literacy to the population have been very badly neglected. For every university-educated graduate in China, there might be six graduates in India, and yet China is getting quite close to complete literacy, whereas in India that is not the case. In fact, only about 60 percent of the adult population is literate. In Africa, of course, the issue of democracy is a very big one. So everywhere in the world you find these major problems. One thing that is needed is to learn from the best experiences of different countries and to combine their lessons. There may be no model country, but there is enough variety in the experiences of different countries in the world and different political and economic systems to learn something from all of them, which could be integrated. That is very important to work on.

Q You said there was one other recommendation for future study.

A. It is quite important to integrate the lessons that emerge from economics, from politics, from sociology, anthropology, and even literature and cultural studies, not to mention philosophy. And this is not the same thing as arguing for interdisciplinary studies because that could be very mechanical. I am ferociously keen on strong disciplinary training within each discipline. But with that disciplinary training, the opportunity of learning from other disciplines is very great. We have already discussed this to some extent. I found out a lot about famines personally by moving from economics to politics. Of course, quite a lot of my own work had been in social choice theory, and that, in a sense, travels over different disciplines. It is politically important to determine how preferences are formed and how economic and political views are arrived at. This also affects welfare economics.

The subject of social choice theory was opened up by French mathematicians in the eighteenth century and taken up by other mathematical economists such as Kenneth Arrow. So a lot of analytical reasoning of an abstract kind goes into it. I found it extremely educational to try to understand the nature of the world on the basis of trying to understand what is happening in different disciplines, without losing the need—which I always like to emphasize, and I do with my students—to do your discipline with great vigor and great involvement.

Q Do you think that economics, as it is generally practiced, has been too insular?

A. Well, I would not like to make such a generalization. It is very difficult to generalize about any subject, and this applies to economics as well. When I was president of the American Economic Association and we invited papers for submission to be held in the annual conference, I was struck by how varied the interests of the American economists were. For nearly every problem I could think of, there were several proposed papers. It would be a great mistake to blame economists as a tribe. Maybe a large fraction sometimes goes in one direction or another, and has often been in one particular direction in recent decades. Nevertheless, within the body of economic writings and economic research, a great variety of problems is being investigated. So if we read more of one another's work—not just the very well-known economists' work but also that of many people doing really exciting work in economics in the United States and in other countries—there is a great deal we can learn. The heterogeneity of the subject and the heterogeneity of interests that drive the works of different economists are major forces that give strength to our discipline.

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