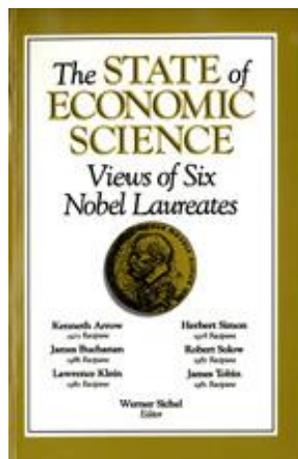

Upjohn Institute Press

[The State of Economic Science]

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Chapter 2 (pp. 25-39) in:

The State of Economic Science: Views of Six Nobel Laureates

Werner Sichel, ed.

Kalamazoo, MI: W.E. Upjohn Institute for Employment Research, 1989

10.17848/9780880995962.ch2

In a series of papers, beginning in the mid-1950s, **ROBERT M. SOLOW** focused on factors affecting the long-term growth of national income. He developed the theoretical foundation as well as the key to the empirical measurement and estimation of the effect of technological change on output.

Professor Solow holds B.A., M.A., and Ph.D. degrees from Harvard University. He has been awarded honorary degrees from U.S. and foreign universities including the University of Chicago, Yale, Brown, Tulane, the Sorbonne in France, the University of Warwick in England, and the University of Geneva in Switzerland. Professor Solow has been associated with MIT for his entire professional career. On leave from MIT, Dr. Solow was a fellow at the Center for Advanced Study in the Behavioral Sciences at Stanford University, Marshall Lecturer at Cambridge University, Eastman Visiting Professor at Oxford University, Overseas Fellow at Cambridge University, Devries Lecturer at the Netherlands School of Economics, Wicksell Lecturer at the University of Stockholm, Mackintosh Lecturer at Queen's University, Mitsui Lecturer at the University of Birmingham, and Visiting Lecturer at Warwick and Manchester.

Dr. Solow is a past president of the American Economic Association and the Econometric Society. In 1961 he was awarded the John Bates Clark Medal by the American Economic Association. Dr. Solow is a member of the National Academy of Sciences, the American Philosophical Society, the American Academy of Arts and Sciences, and a fellow of the British Academy and the Academia dei Lincei in Rome. Dr. Solow has been appointed by two U.S. presidents to serve on various commissions, has served as a senior economist with the Council of Economic Advisers, and has been Chairman of the Board of Directors of the Federal Reserve Bank of Boston.

Professor Solow has written several books and more than 100 articles in scholarly economics journals. Titles of Dr. Solow's books that are indicative of his work include: *Linear Programming and Economic Analysis*, *The Nature and Causes of Unemployment in the United States*, *Capital Theory and the Rate of Return*, and *Growth Theory*.



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A Nobel prize—rather like James Bond’s 007—appears to be a license to have an opinion about anything. But economics was founded by Adam Smith on the rock of the Division of Labor, and specialization is the name of the game. So I am going to specialize on the state of macroeconomics, mostly theoretical but with a few glances at applications. Distinguished and clever economists have been heard to remark knowingly that they understand microeconomics perfectly well and know what they think about this or that, but do not understand macroeconomics at all and find it a mystery. I have no patience with that ploy. Macroeconomics is what it is all about. If you do not understand the business cycle, unemployment, inflation, the real exchange rate, well, you do not understand economics at all. Microeconomics is easier, of course; it does not set itself such hard problems or aim at passing such hard tests.

My goal is to describe the current state of macro theory and to reflect on how it got there, its relevance for practical policy, and its possible evolution. I would like eventually to reach some understanding of why there is so much disagreement in public on what appear to be fundamental issues, with equally able and eminent economists taking contradictory positions. This situation gives rise to rude jokes and it explains, no doubt, the occasional coy attempt to dissociate oneself from the whole

embarrassing exhibition. In considering these questions I am not going to try always to be judicious. That would be dull for all of us. I have strong prejudices on these matters and I will express them freely, probably sounding less tentative than I feel.

Debates about the fundamentals of macroeconomics usually structure themselves as arguments for and against “Keynesian economics.” One reason for this formulation is that macroeconomics as we know it really begins with Keynes and the *General Theory*. Before that there was “Business Cycle Theory.” Economists, like other people, observed that there was some alternation of good times and bad times. The question arose, then: Why does it happen just that way? Business cycle theory looked for behavior patterns and market mechanisms that could be shown to be capable of generating and propagating cyclical fluctuations in the whole economy. They found plenty. Some of them were interesting, and remain interesting, just as economics is. The business cycle theory that I learned and taught did not quite amount to macroeconomics, however. It lacked a comprehensive theory of the determination of the level of economic activity, a theory of “output as a whole” as Keynes later called it. This is not a merely aesthetic complaint either. As the Great Depression of the 1930s showed, a model of regular repetitive cycles is not an adequate representation of the aggregate economy. Things happen that do not lie comfortably in that sort of model.

So macroeconomics in the modern sense really dates from the *General Theory* and that is one reason why it remains the focus of so much contemporary argument. The people who wrote the first reviews were my teachers, and some of them are still functioning today. There is an additional reason: it was a provocative book, an intentionally provocative book, and it still provokes. It was also an undigested book, in the sense that it contained several distinct story lines. These are not well integrated with one another and indeed they are not always compatible with one another. This protean character makes it a good subject for debate, not only with its enemies but also among its avowed friends.

Eventually, maybe by some process of natural selection, an Authorized Version evolved. It is sometimes described as “American Keynesianism,” although two of its main sources were famous articles by John Hicks and Franco Modigliani. The main components of this stan-

dard model were and are an aggregate demand side derived from some version of IS-LM, and a theory of the price level, sometimes anchored in a given, inflexible, nominal wage, but not always. In practice, this model has been used with the presumption that most of the time the economy is operating below its potential for employment and output, with the realized levels of employment and output determined mainly by the demand side. For a long time the implicit belief was that the demand side is more volatile than the supply side. That may once have been a valid induction from history. More recent events have taught a contrary lesson. Nowadays supply shocks get at least equal billing, and movements of the price level and/or changes in the rate of inflation, and expectations about those things, play a more prominent role than they used to. There is no need for me to provide any detail, because you will recognize that I have described the model that is embalmed in most elementary and intermediate textbooks of macroeconomics, and embodied in the big complete econometric models.

The original controversy between monetarism and Keynesianism was carried on within this framework. There were really two separate issues. One was quite specific: it had to do with the nature of the demand for money, especially its interest-elasticity. Within the model, this boils down to questions about the shape and stability of the LM-curve. The second issue was considerably broader; it had to do with the strength of the forces pulling the aggregate economy back toward its potential for output and employment after a disturbance. Within the model, this boils down to questions about the flexibility of wages and prices and their relation to employment and output. These issues are separate in the sense that the answer to one does not determine the answer to the other. But they are related: the first has to do with the way a monetary shock sorts itself out between velocity and nominal demand and the second with the way a shock to nominal demand sorts itself out between output and the price level.

There was, of course, an argument about policy lurking behind the analytical issues. One side believed that steady growth of the money supply (or the monetary base) was the best and only necessary macroeconomic policy; the other believed that activist fiscal and monetary policy could improve macroeconomic performance. It is hard

to shake the notion that there was an ideological fire behind all that intellectual energy. I suggested that the two analytical issues were in principle separate. So there are four possible positions one could take, but only two of the four boxes were ever seriously occupied.

In the past 10 or 15 years, macroeconomic theory has revolved around a slightly different axis, though the genealogy is pretty clear. Monetarism evolved into “new-classical” macroeconomics and American Keynesianism into something for which I have no catchy nickname. As is so often the case with macroeconomics, theoretical developments have both external and internal roots. They are in part a response to events out there in the real economy, and in part a response to gaps and anomalies that show up in the working-out of the theory itself.

The main external event was the inflation of the 1970s. I think of it as having been set off by OPEC and raw-material inflation generally. Others, especially from the new-classical school, would regard that attribution as a typically shoddy piece of Keynesian *ad hoc*ery. It is not my job right now to analyze that inflationary episode. I am discussing the recent evolution of macroeconomic thought. From that point of view, what was important about the post-OPEC inflation was the appearance of a major sudden economic impulse that did not originate on the side of aggregate demand. For that reason alone there was no ready analysis from the Keynesian consensus. The embarrassment was compounded by the sharp role played by inflationary expectations, beginning with the late phases of the Vietnam War, especially expectations about future public policy and its consequences.

Neither development, neither supply shocks nor inflationary expectations, is incompatible with Keynesian macroeconomics. Contemporary textbooks like those of Dornbusch and Fischer and Gordon handle them as a matter of course, within a framework that is recognizably American-Keynesian. But the consensus was caught napping, to put it mildly. It took a while to recover. In the meanwhile, and even afterwards, the Keynesian consensus discovered that it had no adequate policy tools for meeting a supply-side-induced inflation compounded by entrenched inflationary expectations. That is perhaps not the fault of the theory; no one else has a good policy answer either. But there is at best cold comfort in that excuse. We had allowed ourselves to become too

optimistic about the tightness of our analysis and our capacity to guide the economy. The grip of the consensus was weakened.

The internal impulse that triggered and reinforced the new- classical movement was quite different. It was the conviction that macroeconomic theory ought to have “microeconomic foundations.” I think that this conception has been subtly misread, however. So far as I can see, macroeconomic arguments have always been justified by an appeal to microeconomic convention or knowledge. Just think of the way the consumption function and its variations are expounded in textbooks, or the way everyone introduces the chapter on the aggregate investment function by explaining the maximization of present value. The new school insisted on something much more formal, the grounding of macroeconomic models in complete individual-agent-based microeconomic theory. That demand certainly resonated in the profession at large. There is nothing wrong with it in principle, but I think that it has been a blind alley in practice.

The reason is that, in practice, the demand for micro foundations almost had to become a demand to build macroeconomic models on Walrasian foundations. If the felt need was for a formal connection, if a sound macro model had to be the aggregation of a complete, developed micro model, then Walras was all we had available off-the-shelf. The trouble is that Walrasian general equilibrium theory begins by assuming away all of the problems that make macroeconomics interesting. (The *Elements* is not a book about business cycles, after all, but precisely the opposite.) The consequence of this historical accident has been that much high-caliber mental effort has gone into elaborate attempts to prove that unemployment is either nonexistent or healthy.

It will be noticed that I have not used the phrase “rational expectations” to characterize new-classical macroeconomics. That is because I think that the assumption of rational expectations is neither necessary nor sufficient for new-classical results. What is characteristic of the school arises even without rational expectations. It depends rather on two other, apparently less plausible, assumptions: that all markets are smoothly cleared by flexible prices, and that all business decisions are merely the carrying-out of the atemporal and intertemporal wishes of the households that own the firms. (That would account for the popularity

of representative-agent models in this tradition.) Conversely, if you start with, say, a Benassy-Malinvaud fixed-price model, in which markets are foredoomed not to clear, then adding in rational expectations can easily reinforce its Keynesian air. The main thing adding rational expectations does for a modeler is to allow multiple equilibria, and that is not especially good news for new-classicism.

A sharp version of the basic new-classical claim is that there is no specifically macroeconomic problem distinct from the general economic problem of scarcity. Presumably there could be, but in our world it does not happen. What you are seeing when you look at business cycle fluctuations is an economy adjusting optimally to exogenous real shocks to taste and technology, to changes in the weather, for instance. (I am describing the later version of this stance, usually called “real business cycle theory” rather than the earlier version that was more akin to monetarism in locating the main source of disturbance in monetary shocks.) Thus if there are cycles, they are adaptive rather than dysfunctional (apart perhaps from some unavoidable noise). Of course we all wish we were richer (i.e., more productive) and we all wish we could predict the future better, but neither is a meaningful object of macroeconomic policy on the business cycle time scale.

Let me be even more explicit. You are asked to believe that the real economy behaves as if a single immortal consumer were making optimal resource allocation decisions, intratemporally and intertemporally, constrained only by technology and available information. This already assumes that the production economy simply responds to the consumer’s wishes. You must decide if that is a credible assertion.

I must tell you that there are no great empirical or predictive successes associated with this theory. If it said anything, it said that the disinflation of 1979-83 would be accomplished without a recession. That turned out to be false, of course. Is there a defense? Yes: it can be said that the conditions for a controlled test of the theory were not met, that the disinflationary monetary policy was not credible, for example. The practical man’s comment that we are not likely ever to get a more credible disinflationary monetary policy can be shrugged off. The trouble is that it is always possible to claim that the conditions for a controlled ex-

periment have not been adequately met, so to say it in this instance is not to say much.

New-classical macroeconomics likes to take a rather different view of empirical verification. It prefers to set itself the task of reproducing, at least qualitatively, the pattern of variances and covariances in observed time series, and it pats itself on the back whenever it succeeds in doing so to an acceptable approximation. I think this is a misleading procedure; but the point I want to make is a general one and it applies on my own side of the fence as well. The trouble with judging the empirical validity of theories in this way is that the test almost certainly has low power against many interesting alternatives. That is to say: many other models of the economy can do just about as well in fitting that limited class of facts. The conclusion is that even “success” of this kind provides reason to accept new-classical macroeconomics only if it is preferred for other reasons. If you find it implausible, as I do, then you are not in the slightest obliged to accept it on its own empirical grounds. And of course the same goes for other theories, unless they can produce empirical tests with considerably more discriminatory power than these.

Here, I think, is an important contribution to understanding the widespread, perpetual, and apparently endemic disagreement that characterizes macroeconomics to the distress of all of us. Deep down, we all know that, as soon as we come to truly subtle questions, econometrics does not substitute well for the controlled experiment as a device for discriminating between competing theories. One has the uncomfortable feeling that if you try hard enough—always on subtle and complicated matters—you can find data, functional forms, statistical techniques, lag structures, that will tell you what you want to hear. Economics is not alone in this, by the way. A tuned-in person can see the same thing happening with global climate models as they look for traces of the theoretically reasonable greenhouse effect. Their problem is much the same as ours. The questions are subtle, the data are noisy, and there are many forces at work simultaneously.

There is a lesson here for macroeconomics, I think. To begin with, we should stick to first-order questions and accept only robust answers, at least when we are being serious. I have no objection to playing around, trying things out; that is one of the ways we learn. But for public con-

sumption, the standards should be different. Second, we are not only entitled to use common sense and to make judgments of plausibility based on general observation, but there is no sensible alternative to doing so. Such conclusions are vulnerable; that goes without saying. They have to be defended without rancor and without cant. And finally, in so doing, the broadest possible variety of evidence should be mobilized. There are other roads to knowledge besides formal statistical inference. They have to be used critically, as does formal econometrics for that matter, but we cannot afford to dismiss any bit of information the world offers.

You will have gathered that the new-classical way of going about macroeconomics is not my preferred way. But I would not advocate going back to the “hydraulic” Keynesianism of the 1960s even if that were possible. The key to doing better, I think, is to pay attention to the “macro foundations of microeconomics.” I hope I mean something more than cuteness by that phrase: I mean, roughly, that we are entitled to ask what sorts of microeconomic mechanisms both look right and do justice to the nature of the general economic environment in which they are expected to function.

There is now a self-conscious “New Keynesian Macroeconomics” that tries to do just that. One wing of it tends to emphasize transactions costs, information asymmetries and similar imperfections, and shows that, in that kind of environment, the economy by itself can easily achieve unsatisfactory states (equilibria) which might be improved by corrective fiscal and monetary policy. There is another strand that places greater emphasis on imperfect competition, increasing returns to scale, and trading externalities—the tendency for optimistic (pessimistic) choices by some to validate optimistic (pessimistic) choices by others. These mechanisms lead to the conclusion that the economy may be capable of many self-sustaining equilibria, some much better than others. I have a fairly vague feeling that this second approach is on to something deeper than the first. That feeling—it is not much more than that—governs my choice of illustrative examples for non-Panglossian macroeconomics.

To start off, let me refer back to IS-LM-based American Keynesianism. Most of the time, as I mentioned, it rested on the hypothesis that the nominal wage was the sticky price that kept the labor market from clearing at full employment. (In the standard version, the price

level for goods was taken as perfectly flexible. One of the advances made by the Benassy-Malinvaud fixed-price literature was to enlarge the picture by treating the goods market and the labor market more symmetrically.) Since the nominal wage is not permanently fixed, textbooks pointed out, and many still point out, that the nominal wage adjusts only slowly to the state of the labor market. The model is then one of disequilibrium, possibly prolonged. Underemployment lasts as long as the disequilibrium lasts.

Now Keynes himself certainly believed that the nominal wage was sticky in this sense in Britain during the 1920s and 1930s. He even suggested why that might be: because resistance to nominal wage cuts in a decentralized labor market is the only way that workers can defend their *relative* position in the wage structure. It is less well remembered that Keynes argued that wage stickiness was probably a good thing, that perfect wage and price flexibility could easily be destructive of real economic stability. His reasoning went like this. In a monetary economy, the nominal interest rate cannot be negative. Hence the *real* interest rate must be at least equal to the rate of deflation. (That is what holding cash would earn, after all.) If wages and prices were to fall freely after a contractionary shock, the real interest rate could become very large at just the wrong time, with adverse effects on investment. The induced secondary contraction would only worsen the situation.

I can report that Frank Hahn and I have verified Keynes's intuition within a model that is in every respect respectable. That is to say, we can exhibit situations in which complete wage flexibility, while maintaining full employment after a shock, drives the model economy off on completely unstable trajectories of pointlessly fluctuating output that never return to the original steady-state equilibrium. Somewhat slower wage adjustment would make things better, not worse. And there is a (complicated) monetary-fiscal policy that is in principle capable of nipping the whole process in the bud and getting over the initial shock with minimal disturbance.

The point of this exercise is not to demonstrate the wisdom of the Great Lama. It is much more devious. If perfect wage (and price) flexibility is not always the best way to run an economy, then it is perhaps less peculiar that economies should develop institutions that limit or

discourage aggressive wage-cutting in times of moderate unemployment. Hahn and I have actually produced a formal model of just that kind. In it the labor market is modeled as a kind of repeated game involving workers and employers. We show that there is an equilibrium strategy for workers in which the unemployed refrain from competing for jobs, as long as the unemployment rate is not too high. What makes this an equilibrium strategy (i.e., one from which it pays no individual to depart unilaterally) is the threat that any violation of the norm will lead to a long period of unrestrained competition in the labor market. In that event no worker does better than the reservation wage, whereas adhering to the norm gives even currently unemployed workers the expectation of sooner or later acquiring a job at something higher than the reservation wage.

There is a general methodological lesson here, and it is what I am after. The model just described has many equilibria; in fact there will generally be a whole interval of wage rates and corresponding unemployment rates, any one of which could persist if once established. (Which one actually occurs may then be a matter of historical accident.) The point is that this multiplicity of equilibria arises easily as soon as one gets away from the notion that price-mediated market clearing is the only equilibrium concept worth discussing. Noncooperative game theory has taught us that the fundamental idea of an equilibrium is the “strategic” definition I have used here, a choice of behavior patterns that leaves no participant impelled to make a unilateral change. If it seems to you, as it does to me, that the current state of the economy could have been different—I am suggesting a thought-experiment about positions of rest, not about short-run dynamics—then the idea that there can be many self-sustaining equilibria should be your cup of tea.

The non-Panglossian branch of modern macroeconomic theory has produced some other models that fall into this same category. Several of them rest on an idea that goes back into business cycle theory well before the *General Theory*, what I earlier called a trading externality. It is, in far too simple terms, the notion that widespread optimism is self-justifying, but so is widespread pessimism. Businesses and households who are optimistic about their own market prospects will make decisions that, taken together, create strong markets and thus

validate their initial optimism. If they had all been pessimistic to begin with, they would have done things that validated their initial pessimism. If you can believe that then you believe that there are (at least) two equilibria, a high-level one and a low-level one. It would not be too far-fetched to think of one as prosperity and the other as recession.

Needless to say, that simple thought is not even a sketch of a sketch of a theory. All the economics remains to be done. But it has been done, several times in several contexts. Just by way of example, Walter P. Heller (the son of my old leader at the Council of Economic Advisers) has studied an economy consisting of two monopolistically competitive industries, each of which sells only to the employees of the other. (This artificial-sounding condition would seem quite natural if there were many industries. It is meant to serve a reasonable purpose.) As imperfect competitors, each firm has to form expectations about the location of its demand curve. In effect, then, it must form expectations about the production and employment decisions of the other industry. The other industry is meanwhile doing exactly the same thing. With a few unrestrictive conditions on demand-elasticities, Heller is able to show that the optimism-pessimism story actually holds in this set-up. There can indeed be two or more self-sustaining equilibria and it is no trick at all to describe reasonable conditions under which the high-level equilibrium is clearly better for everyone than the low-level one. This model has the amusing property that the government can bring about the high-level equilibrium simply by announcing in a convincing way that it will do so. If the announcement is believed, the government will never actually have to do anything. This economy has nothing to fear, one might say, but fear itself.

A different and in some ways more powerful conceptualization of the same general idea can be found in the model of “search equilibrium” proposed by my colleague Peter Diamond. In Diamond’s story, people “accept productive opportunities,” some of which are more advantageous than others, produce goods, and then look for buyers, who are people just like themselves, having produced something to sell. Buyers and sellers are completely symmetrical; we can call them traders. It is better to be a trader when there are lots of traders, because then it is easier to find a partner with whom a mutually profitable exchange

can be carried out. If there are a lot of traders, then I will be inclined to accept somewhat less attractive productive opportunities. Better market prospects justify greater production. By acting like that, of course, I add to the number of traders out there and thus justify greater production by others. Diamond is able to show that this set-up, too, is very likely to provide two or more self-sustaining equilibria; when it does, the ones with more output and employment are better than the ones with less. In an added *tour de force*, Diamond and Fudenberg prove that this model can even produce regular business cycles in its primitive form of economic activity, each phase leading regularly to the next.

It is a fair criticism of the approach to macroeconomics that I have been describing (and favoring) that it seems to produce only a collection of fragments. The new-classical scheme at least produces a complete model that can be equipped with empirically based parameters and simulated. Its behavior can then be checked against selected characteristics of the world of observation. The older American Keynesianism went even further and culminated in the large econometric models that still grind out detailed forecasts month after month. (There are, of course, smaller models too.) The more recent shoots from the Keynesian tree have the character of examples, illustrations of possibilities. They are sometimes phrased in fanciful ways, as if to emphasize that they are not intended for econometric treatment.

There are two responses to this criticism. One is simply that it will take time to develop these newer possibilities into a form fit for empirical application. That may indeed be true; but it is not the response I want to make. To my mind, the role of macro theory (even, in a sense, applied macro theory) is not necessarily to make a single all-purpose model to represent the world. It is certainly not unconditional forecasting. It is rather the uncovering of mechanisms that cause the economic system to malfunction in significant ways, and then the analysis of kinds of policy measures, directions of policy even if not exact doses, that are potentially therapeutic. I would resist the notion that the market failures in question are “mere aberrations” of the system; they *are* the system. Nevertheless, my sort of macroeconomics is inevitably less monolithic than the other. This may explain the attractiveness of the new-classical model; it looks much more like a candidate for System of the World.

If neatness is your dominant concern, and I offer you a menu that includes a whole bunch of little models, not easily put together, a certain kind of mind will choose “None of the above” even if that answer violates common sense.

How might this version of macroeconomics evolve? One could imagine continued analytical study of these and other mechanisms that give rise to occasional recessions and bursts of inflation, along with an ongoing attempt to evaluate their importance in the modern economy. This effort would be partly econometric, partly institutional discussion, storytelling, educated judgment, all of those things, but I would certainly look for rough quantification. Notice how there will be room for differences of opinion even within this paradigm. Especially if the key concept is the multiplicity of equilibria, there will always be the empirical problem of characterizing the sort of equilibrium the economy is in at the moment, and choosing the relevant model.

Perhaps I am suggesting moving away from the image of economics as the physics of society toward the image of economics as something more like ecology or medicine or cell biology. I am not referring to any analogy of content, but just a view of scientific effort that is less formal and general and reductive, and more tolerant of a variety of models suited to a variety of problems and contexts. From that point of view the sort of model developed so elegantly and attractively by Lucas and Prescott is just one of many possible mechanisms; its applicability has to be argued anew in each concrete situation. In optimistic moments, I think that evolution has already started.

This apparently academic subject is actually real and relevant and contemporary. There seems to be general agreement that the probability of a recession in the United States before the end of 1990 is something like one-third. Suppose it happens. What will we do? What will the Democratic Congress think is the right stance for the federal government? What will the Republican President propose? What will the conservative but more professional Federal Reserve decide to do? Will there be any coordination among them? And how will the argument be conducted?

There are two main currents of thought that have their existence both inside and outside professional economics. One is generally *laissez faire*.

It says that the unfettered private enterprise economy is well-behaved and self-correcting. Whatever it does, however it behaves, is probably all for the best. The optimal government policy is to get out of the way. The recession will run its course, and anyway it is not really a recession. The other main attitude says that although there is nothing basically wrong with the free enterprise economy, there are certain areas where it is vulnerable to market failure. Some of these are “microeconomic”—like excessive pollution or misleading labels on food—and require regulation of some kind. But there is also macroeconomic market failure, a tendency occasionally to lapse into recession—a systemwide underproduction of goods and therefore underprovision of jobs—or inflation or stagflation. That calls for compensatory stabilization policy, and sooner is better than later.

This dichotomy does not need academic economics to keep it going. It has deep roots in ideology and self-interest. But it is reflected in an ongoing debate within academic economics, and especially within macroeconomics. The debate is not in the first instance about policy but about the correct “model of the economy.” The balance of influence shifts from time to time, partly in response to what happens in the real world (not only in the economy but also in public opinion), and partly in response to which side seems to have the analytical upper hand. Within academic economics the two sides are often labeled Keynesian and anti-Keynesian (currently “new-classical”). As I have explained, that is because a great watershed in this debate—which has been going on for centuries—occurred in 1936 with the publication of the first model of the economy supporting the macro failure view ever to achieve academic respectability. The label sticks, even if today the detailed context has little to do with the original. Needless to say, in this typology I would be classified as a Keynesian, for good reason.

Should any of this matter to citizens? I offer a partisan but—I hope—not narrow-minded answer. When the next recession rolls around, do not be seduced by ideology into believing that government is necessarily part of the problem and cannot be part of the solution. Government often is part of the problem because it is often incompetent, often dominated by false beliefs about the world, often moved by ulterior motives. But there is no general theoretical truth that guides you one

way or the other. You should try to listen as impartially as you can manage to analyses offered by economists of every persuasion, trying to get at the picture of the world, today's world, that underlies each diagnosis. Then you should fearlessly form judgments, fearlessly but tentatively. Above all, you should try not to be bored.