DUMB AND DUMBER IN MACROECONOMICS

Robert M. Solow

So how did macroeconomics arrive at its current state? The answer might provide a lead as to where it ought to go.

The original impulse to look for better or more explicit micro foundations was probably reasonable. It overlooked the fact that macroeconomics as practiced by Keynes and Pigou was full of informal microfoundations. (I mention Pigou to disabuse everyone of the notion that this is some specifically Keynesian thing.) Generalizations about aggregative consumption-saving patterns, investment patterns, money-holding patterns were always rationalized by plausible statements about individual--and, to some extent, market--behavior. But some formalization of the connection was a good idea. What emerged was not a good idea. The preferred model has a single representative consumer optimizing over infinite time with perfect foresight or rational expectations, in an environment that realizes the resulting plans more or less flawlessly through perfectly competitive forward-looking markets for goods and labor, and perfectly flexible prices and wages.

How could anyone expect a sensible short-to-medium-run macroeconomics to come out of that set-up? My impression is that this approach (which seems now to be the mainstream, and certainly dominates the journals, if not the workaday world of macroeconomics) has had no empirical success; but that is not the point here. I start from the presumption that we want macroeconomics to account for the occasional aggregative pathologies that beset modern capitalist economies, like recessions, intervals of stagnation, inflation, "stagflation," not to mention negative pathologies like unusually good times. A model that rules out pathologies by definition is unlikely to help. It is always possible to claim that those "pathologies" are delusions, and the economy is merely adjusting optimally to some exogenous shock. But why should reasonable people accept this? During the past three years, unemployment has increased by three million with real wages stagnant and productivity growing, possibly abnormally fast. Capacity utilization has fallen by 10 percent, with trivial inflation and some prices falling. Real business investment in equipment peaked in the third quarter of 2000, fell by 20 percent to the first quarter of 2002, and has risen by a scant five percent since then. Is this a stagnation pattern? Does it reflect large-scale, perhaps irrational, overinvestment in the 1990s? Should it not be studied as such? Why should anyone take it as the solution of an Euler equation? It would not be hard to imagine a better path for the economy. Why should the burden of proof fall on those who see an ordinary standard pathology here? The odd thing is to regard this history as the working out of an other-worldly model.

What is needed for a better macroeconomics? My crude caricature of the Ramsey-based model suggests some of the gross implausibilities that need to be eliminated. The clearest candidate is the representative agent. Heterogeneity is the essence of a modern economy. In real life we worry about the relations between managers and shareowners, between banks and their borrowers, between workers and employers, between venture capitalists and entrepreneurs, you name it. We worry about those interfaces because they can and do go wrong, with likely macroeconomic consequences. We know for a fact that heterogeneous agents have different and sometimes conflicting goals, different information, different capacities to process it, different expectations, different beliefs about how the economy works. Representative-agent models exclude all this landscape, though it needs to be abstracted and included in macro-models.

I also doubt that universal rational expectations provide a useful framework for macroeconomics. One understands the appeal. Think of it this way: Herb Simon was surely right about bounded rationality; no one would deny that most economic agents are actually like that, and natural selection does not work fast enough to eliminate them. Why did the notion of "satisficing" never catch on? I think it is because the assumption of complete rationality tells the modeller what to do, whereas bounded rationality only tells the modeller what not to do. That is not helpful. Something similar is true about rational expectations. If there were a nice parametric family of alternative ways to model expectations, it might catch on. Most of us would happily go along with the notion of expectational equilibrium: if specific underlying expectations generate an outcome in which those expectations are systematically and non-trivially violated, that situation can not be an equilibrium. It is what happens then that needs thought. The situations that agents need to anticipate need not even be probabilistic, surely not stationary. The popular device used to be adaptive expectations; that may have been inadequate. Maybe this is a case for the application of psychological research (and sociological research as well, because the formation of expectations is a social process). Maybe experiments can be designed. Heterogeneity across agents and classes of agents is certainly important precisely here. One would like a simple, definite way to proceed, if that is possible. A good example of the sort of thing I mean is the way the Dixit-Stiglitz model made monopolistic competition easy. (The trouble is that we are dealing with an unobservable.)

Although I am going to take this back in a moment, it is certainly worthwhile mentioning the problems connected with real and/or nominal wage and price inflexibility and its sources in market structure, limitations of information, human nature, the specialness of zero, etc. This is an old issue in economics, macro and micro, and a lot of progress has been made in measuring and understanding it. Mere sluggishness is part of the picture, and that is easily modelled, but there is surely more that is less easily modelled. The devil finds work for idle hands to do, as you may have noticed.

Now here is a peculiar thing. When I was in advanced middle age, I suddenly woke up to the fact that my colleagues in macroeconomics, the ones I most admired, thought that the fundamental problem of macro theory was to understand how nominal events could have real consequences. This is just a way of stating some puzzle or puzzles about the sources for sticky wages and prices. This struck me as peculiar in two ways.

First of all, when I was even younger, nobody thought this was a puzzle. You only had to look around you to stumble on a hundred different reasons why various prices and factor prices should be much less than perfectly flexible. I once wrote, archly I admit, that the world has its reasons for not being Walrasian. Of course I soon realized that what macroeconomists wanted was a formal account of price stickiness that would fit comfortably into rational, optimizing models. OK, that is a harmless enough activity, especially if it is not taken too seriously. But price and wage stickiness themselves are not a major intellectual puzzle unless you insist on making them one.

The second peculiarity was that the path from nominal events to real consequences was not my idea of the fundamental problem of macro theory anyway. All along, I had been thinking--and this may be a Keynesian inheritance, though I doubt it because I may have picked it up from Gottfried Haberler's *Prosperity and Depression*, where my generation learned about business-cycle theory before "macroeconomics" had been invented--that the main problem was to understand why real shocks that took the economy out of some satisfactory equilibrium led to such a prolonged and sometimes unsatisfactory adjustment. These are medium-run problems--the capital stock moves--and there clearly are medium-run fluctuations in modern industrial economies. (This is documented for the U.S. in a recent paper by Comin and Gertler.)

Keynes claimed to have found the way to account for this: he thought he had a theory of unemployment equilibrium. The reason adjustment took so long, or never really happened, is that the depressed state was actually an equilibrium. Most of us today think that Keynes failed in that effort; he lacked the tools. The exception was the case of wage rigidity, but we knew that all along. In my youth, we thought that macro-pathologies were disequilibrium phenomena, and then the puzzle was: why is the process so slow?

This choice between equilibrium and disequilibrium thinking may be a false choice. If I drop a ripe watermelon from this 15th-floor window, I suppose the whole process from t_0 to the mess on the sidewalk could be described as some sort of dynamic equilibrium. But that may not be the most fruitful--sorry--way to describe the falling-watermelon phenomenon.

So I would hope that macro theory could get back to focussing on the adaptation-to-real-disturbance problem, without falling into the implausibilities of real-business-cycle theory. (Even RBC theorists may fight their way out of that paper bag.) The Ur-Problem may be: start in a situation of growth equilibrium (*not* necessarily a steady state, but don't get me started on that one), and imagine a real shock, perhaps a failure of *real* effective demand (!). What happens next? That may be the story of the period from 2000 to now, the real shock having been massive overinvestment in response to unrealistic profit expectations (accompanied by accounting swindles, just to make Joe happy).