Toward a Macroeconomics of the Medium Run

Robert M. Solow

These days macroeconomics has become more respectable than it used to be. I can remember when many economists liked to say: Microeconomics is not problematic, but I just don’t understand macroeconomics. There was a definite implication that something must be wrong with macroeconomics, not with the observer. Of course macroeconomics cannot be “exact;” it has to work by rough analogy and empirical compromise. Maybe a certain raffishness is inevitable. Most economists work on microeconomic problems, with increasing use of new microeconomic data. But now it is widely understood that macroeconomics is at the heart of economics; it will not do to be snooty about it. This centrality will continue, for the best possible reason: the need to understand current events, especially unfavorable ones, and to formulate policies—even benign neglect is a policy—to deal with them. For the same reason, the characteristically close connection between macroeconomic theory and empirical work is very unlikely to change.

My main goal in this essay is to say something about leftover open questions in macroeconomics, as well as new phenomena that need to be accommodated in early 21st century macroeconomics, and the sorts of ideas that might make progress possible. The occasion and the space available require that I keep to a fairly high level of generality. This is not the time to single out specific research questions: I think there is a need for more investigation of the short-run relation between output and employment, for example, but this is not the occasion to go on about it.

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Foundations

The main vehicle for recent macro theory has been the small, complete, general equilibrium model, and it is hard to think that anything might replace it any time soon. But “general equilibrium” does not have to imply “competitive” or “Walrasian” or “Arrow-Debreu” in this context, and certainly not the optimizing-representative-agent version that has become strangely popular. In fact, there are choices to be made. Are prices flexible or sticky, or are some prices to be modeled one way and other prices differently? What is almost the same thing, do markets clear all or most of the time, or is some other equilibrium concept more useful?

Even within a small model, there is room for variety. The bond market is not much like the labor market, and that is coming to be recognized even within the real business cycle tradition. The best way to treat the goods market is still unsettled. I hold the minority view that the fixed-price models of Malinvaud (1977), Bénassy (1986) and others were never given a fair trial by American macroeconomists. No doubt there were some good reasons for this negative verdict; for example, the fixed-price models lacked any serious story about how prices eventually move—a pretty awful deficiency in an inflationary time—and suffered from closely related ambiguities about the concept of “effective demand” in quantity-constrained markets. One would think that progress requires embedding macro theory in a world whose predominant market form is some version of imperfect competition; and then it will be necessary to tackle head-on the issue of price-setting by firms with market power, but also beset by friction and uncertainty. Sticky prices are already turning up in or near the real business cycle approach, surely a good sign (Christiano, Eichenbaum and Evans, 1997).

Another foundational question is whether agents in macro models should be described as making optimizing decisions or proceeding by rule of thumb. In my more optimistic moments I suspect that this dichotomy may be more apparent than real. By now, to assume that a representative consumer maximizes a discounted sum of constant-elasticity utilities subject to a lifetime budget constraint is practically to adopt a rule of thumb. Conversely, any defensible rule of thumb is likely to resemble an approximate solution to some optimization problem. But in my more pessimistic moments, I think that the only reason to insist on optimizing behavior is to get welfare conclusions that no one believes anyway, the most spectacularly implausible one being that the observed business cycle is really an optimal adjustment to unexpected shocks to technology. Maybe it is possible to imagine a mixed solution to this problem, involving limited optimization combined with some dependence on rules of thumb.

One reason for reaching in this direction is the importance of expectations in macro theory, and the unresolved question of the best way to deal with the formation of expectations. Here the range of defensible opinion is too wide for comfort. In some quarters, the hypothesis of model-based rational expectations is simply taken for granted as the only legitimate game in town. In some quarters, it is permissible to wonder if the macroeconomic world is representable as a stationary stochastic process at all and, if it is not, what one would mean by a reasonable
way to form expectations about future outcomes. (This seems to be another way to ask the old Knightian question about the difference between risk, which can be represented by a probability distribution over possible outcomes, and uncertainty, which in his terms referred to a radical lack of knowledge in which neither the possible outcomes nor a probability distribution could be articulated in any useful way.)

It is hard to guess how this question will be resolved or compromised in the next decade. It is acutely uncomfortable to have so much in macroeconomics depend on how one deals with a concept like expectations for which there is (inevitably?) so little empirical underpinning, and so much room for invention. The rational expectations side of the argument has the considerable advantage of definiteness and convenience. It also has the disadvantage that it is thought to be implausible by the very people whose mental processes it is meant to describe. The other side of the argument has the disadvantage of indefiniteness; it leaves a lot of options, with no good fact-based way of choosing how to represent expectations.

One element of a compromise is to accept that any candidate for medium-run macroeconomic equilibrium must also be an expectational equilibrium, in the sense that the outcome does not brazenly contradict the initial expectations that produced the outcome. Then the focus shifts slightly from the formation of expectations to the revision of expectations. I offer the poorly-informed judgment that the learning literature has not yet got to the point where it can be directly useful to macroeconomics. But this is clearly an important area for research, both model-building and data-analysis. The state of the art will be described in the forthcoming book by Evans and Honkapohja (forthcoming). Part of the problem may be that the relevant expectations about an observable are never point expectations, nor do they take the form of an easily parametrizable probability distribution. What seems to be wanted is some notion of a (fuzzy?) set of outcomes that are near enough so that one would not go to the trouble of revising one’s initial view.

**Growth**

The second wave of runaway interest in growth theory—the endogenous-growth literature sparked by Romer and Lucas in the 1980s, following the neoclassical wave of the 1950s and 1960s—appears to be dwindling to a modest flow of normal science. This is not a bad thing. The alluring prospect of a viable (predictive?) endogenous growth theory does not seem to be a whole lot closer now than it was at the beginning of the wave. Nevertheless, a wider variety of growth models is now available for trying out; and some of the main empirical uncertainties have been specified, and perhaps narrowed down even if not settled. Still, it is hard to see where growth theory is going.

A few undercultivated areas of research can be picked out. For example, not very much has been done with open economy growth models, despite the promising start made by Grossman and Helpman (1991). This is not to suggest complete neglect: there are quite interesting “trade and convergence” papers among the
ubiquitous international cross-section studies, and there are purely formal models of open economy growth. Just to mention one underresearched angle, however, we still make lists of the ratio of R&D spending to GDP, country by country. I have never quite understood the point of the normalization. After all, a real dollar of research expenditure ought to buy a given chance of a discovery; a tiny economy that spends a higher-than-average proportion of its economy on R&D is unlikely to become a scientific leader. More relevant, in this context, is that the habit seems to ignore the diffusion of technology, which has to be part of the many-country picture. There may be a specialized literature; if so, it needs to be linked to endogenous growth.

Another major open question is the role of human capital in growth. Here there is plenty of literature; the problem is that results tend to be inconclusive, or even contradictory. A reasonable guess is that vital measurement issues have to be settled first (Judson, 1996; Klenow and Rodriguez-Clare, 1997). In empirical work, including those ubiquitous cross-country regressions, “human capital” almost always means the stock or flow of “schooling,” and it is easy to understand why. Nevertheless, schooling is an input to the production of human capital, not a measure of the thing itself. Moreover, we all know that human capital is accumulated outside of school as well: at home, on the job, in the army, even on the street. The practical question is so urgent—how much should a poor country spend on building human capital, and how should spending be allocated?—that the research question should have high priority.

A look at recent issues of the *Journal of Economic Growth* confirms that endogenous growth theory is no longer expected to be the key that unlocks the secret of the universe. Skillful and sophisticated model-building goes on, mostly about small refinements and novelties. One would like to see more work driven by important policy issues, and driven toward the exploration of new sources of data. To take a concrete example, it is anyone’s guess whether the U.S. productivity trend is now tilting back toward the higher growth rates of the first few decades after the war, or merely experiencing a blip that will eventually go back to slow growth. Does endogenous growth theory have anything to tell us about the likelihood of either outcome?

**Fluctuations**

Most of today’s more heated macroeconomic controversies relate to the shorter time scale relevant for business cycle fluctuations. It is at first discouraging that most of the excitement is about perennial issues, déjá vu all over again. Constant Reader’s natural reaction to yet another Keynesian-Classical debate is to fwoow up (which was Dorothy Parker’s reaction to Winnie-the-Pooh). I will offer two thoughts that suggest maybe things are not quite that bad.

The first is that moderates on both sides of the fence have done a little converging; stridency comes from the extremes. As evidence, I point to the 1997 AEA symposium “Is there a core of usable macroeconomics we should all believe?” with contributions from Blanchard, Blinder, Eichenbaum, Solow and Taylor (pub-
lished in the May 1997 “Papers and Proceedings” issue of the *American Economic Review*). There it seemed possible to specify a common framework within which a fairly broad range of macroeconomists, still with different preferred modeling strategies and different priors about some key parameters, could conduct normal science. No doubt there will still be disagreement about key issues in practice. The advantage of a common framework is that it may be possible to trace the source of those practical disagreements to differences about the likely values of key parameters, which are surely discussable, or to differences in objectives, some of which may be discussable.

The second thought bears exactly on the persistence of disagreement about parameters. My hypothesis is that the precise location of the right answer to some of the perennial vexed questions may shift from time to time, as attitudes, experiences, folk theories, institutions and policies out in the real world themselves change. So the debating advantage tips from time to time and the animals get stirred up.

I have to admit to being one of the animals, so a warning is in order that my judgments may have a sectarian tinge. It seems to me that the original real business cycle theory project, to understand observed fluctuations as the efficient response of a well-functioning, intertemporally competitive economy to unforeseeable shocks to technology and tastes, has petered out after only minor gains. The main obstacles to success are probably the persistence of market imperfections in some sectors, especially the intrinsic implausibility of any attempt to represent labor-market outcomes as market-clearing, and the attempt to push forward-looking behavior to a very distant horizon. To these I would add the suggestion that a commitment to representative agent models is a serious mistake. Many of the frictions and occasional flip-flops that characterize macro-behavior seem to stem from the heterogeneity of agents, with respect to underlying beliefs, expectations, market power, access to capital, and so on.

My reading is that the work of Christiano, Eichenbaum and others has moved in the direction of incorporating frictions and imperfections into the real business cycle framework. The result sounds a little more like the observed economy. At the same time, those starting from more “Keynesian” presumptions—the quotation marks are to signal that I am using the word as a kind of shorthand, not to be overinterpreted—have been paying more attention to requirements of intertemporal consistency. Something dynamically more sophisticated than old-time IS-LM seems to be emerging, although it has to be said that the IS-LM standby has served remarkably well as a first cut at thinking about aggregate demand. The gap between the two broad short-run approaches has narrowed perceptibly (although it has surely not disappeared) and that creates some space for a common framework to emerge.

When we turn to the individual building blocks of any macro-model, there is no shortage of research problems for the next decade. There have been enough surprises in the behavior of both inventory investment and fixed investment in recent years to suggest that the standard models need at least recalibration and possibly more extensive rebuilding, just to take one example. The diminished
volatility of inventory investment and the apparently easier sustainability of high fixed investment in the 1990s are worth attention.

My own belief is that this is not so much a matter of correcting past errors as of keeping up with a changing reality. One of the attractions of the pristine real business cycle model is that this kind of question is not allowed to arise: the “right” formulation of each little part emerges always from the basic infinite-horizon optimization problem that the economy is supposed to be solving. What looks to the naive observer like an erratic shift in the elasticity of This with respect to That is really explicable in terms of “deeper” parameters. That may be so—how would one know?—but to take this injunction seriously imposes an impossible burden on the poor working macroeconomist. Attitudes toward risk are hard enough to isolate; the laws of motion of attitudes toward risk are the other side of beyond. If this sort of parametric instability is at all frequent, it has consequences for theory, and also for the best strategy for extracting information from the past about the present.

Here is just one example of the way that changes in the economy itself may call for the rethinking of standard analysis. As a (retired) teacher of intermediate macroeconomics, I would like to suggest that the good old Mundell-Fleming model needs updating to reflect changes in the mobility of portfolio capital, the ease of foreign direct investment, the ability of firms to outsource intermediate goods, the (sometimes strategic) response of goods prices to variations in exchange rates, and other such institutional-technological developments. Open economy macroeconomics can only get more important in the future.

Wages and Prices

Nothing illustrates better the close connection between theory and fact in macroeconomics than the issue of wage and price behavior in the short-to-medium run. In the 1970s and 1980s, the entrenched standard model was the expectations-augmented Phillips curve in the version that incorporated a natural rate of unemployment or NAIRU. It gave good empirical fits, so not many questions were asked. (I always had doubts about the robustness of the theory and the data analysis that were supposed to support the standard NAIRU model; but as long as it worked pretty well, no one was inclined to rock the boat. By the way, it never worked well in Europe, and still does not.)

In the background was a model of the determination of the NAIRU itself, usually at the intersection of a conventional negatively sloped aggregate demand curve for labor and some sort of positively sloped “wage-setting” curve that made a higher real wage correspond to greater employment. There were alternative versions of the rationale behind the wage-setting curve, the serious contenders being the efficiency wage model, the insider-outsider model, and a model working from the reconciliation of aspirations on the two sides of the labor market. These all led to roughly the same sort of wage-setting relation, so it was neither easy nor urgent to discriminate empirically among them.

There are conceptual problems with the labor-demand curve too; it wants to be
a function of the real wage (the wage deflated by an index of product prices), but only the nominal wage can be thought of as given to the representative, imperfectly-competitive, price-setting firm. The problem can be dodged in special constant-elasticity cases, so it is not worth talking about now. The older approach stressed nominal bargaining, with prices depending on wages and wages depending on prices and the real wage emerging. That approach had something to recommend it, although simple mark-up formulas could have used a dose of modern-style forward-looking reasoning.

That was then. In the 1990s, the standard model appeared, superficially at least, to be going sour. As this is written, the unemployment rate has been below the once canonical 6 percent for five years, but the GDP deflator has decelerated during that interval, and hourly compensation, while accelerating slowly, is much more viscous than the 6-percent-NAIRU story would have led a reasonable person to expect. (Reconciling the course of European unemployment with the standard model requires even more in the way of late-Ptolemaic epicycles.) Of course, the NAIRU story can always be saved by agreeing that whenever the rate of inflation is falling, the current unemployment rate must be above the NAIRU. Yes, but unless the NAIRU changes only very slowly or very rarely or is well predicted by the background model, the story has been saved by emptying it of content.

Surface observations like this are never decisive, however. The final verdict may be that special factors are enough to account for the aberrations of the 1990s. The list of possible special factors is long: the changing age distribution of the labor force, the special deflationary role of computer prices, a respite from rapidly rising health care costs, the appreciation of the dollar against the yen, and others. That may turn out to be the best answer after the research is done. One has to hope that the research is done critically. The trouble is that there are always special factors, but they are only noticed when you need them. It would be a pity to miss an opportunity to rethink the macroeconomics of wages and prices.

In the Medium Run

Suppose that short-run quarter-to-quarter macroeconomics is best done with predetermined (not necessarily constant) prices and wages, and a little ad hoc dynamics tossed in. Suppose that long-run, decade-to-decade, macroeconomics is best done in the growth-theory manner, with prices and quantities mutually adjusting. Then there must be a medium-run, five-to-ten-year time scale at which some sort of hybrid transitional model is appropriate. Prices play a role, but not simple market-clearing, so income-driven processes may dominate events.

Is that sort of approach respectable, or even possible? Robert Lucas thinks not. In a sharp review of James Tobin’s *Asset Accumulation and Economic Activity* (1980) he insisted, as I remember, that any attempt to be “Keynesian” in the short run and “neoclassical” in the long run is simply illogical. I can see what he meant. How does someone who is being Keynesian from quarter to quarter ever stop? How does
someone who is being neoclassical ever find room for a fixed-price interlude when markets do not clear?

Yet that sort of fudging goes on cheerfully in analogous situations. When I walk around Boston or sail around Vineyard Sound I navigate without hesitation as if the earth were flat, and I get there just fine. If I were flying to Timbuktu or sailing to Sydney, I would surely navigate as if the earth were spherical. The analogy is not perfect. Here is a rather better one that I understand less well: in everyday life, I am assured, Newtonian physics is perfectly adequate, but over much longer distances and higher velocities it would be a bad mistake to omit relativistic effects. Presumably there are intermediate velocities at which the choice might be worth thinking about.

Come back to macro theory. I can easily imagine that there is a “true” macrodynamics, valid at every time scale. But it is fearfully complicated, and nobody has a very good grip on it. At short time scales, I think, something sort of “Keynesian” is a good approximation, and surely better than anything straight “neoclassical.” At very long time scales, the interesting questions are best studied in a neoclassical framework, and attention to the Keynesian side of things would be a minor distraction. At the five-to-ten-year time scale, we have to piece things together as best we can, and look for a hybrid model that will do the job.

Among the services that such a hybrid model should be able to provide are interpretations of divergent trends in unemployment in Europe in the 1980s and 1990s, of low-level stagnation in Japan in the 1990s, and of the record of employment, productivity and low inflation in the U.S. since 1992. Nice examples of medium-run macroeconomics are Olivier Blanchard (1997), Paul Krugman (1998), and Thomas J. Sargent (1999). Years divisible by $10^3$ are not especially good for more of that sort of thing, but they are not especially bad either.

References


