Is There a Core of Usable Macroeconomics?

By OLIVIER BLANCHARD*

My answer to the question in the title is an unambiguous yes. That core is very close in spirit to what Paul Samuelson identified more than 40 years ago as the "neo-classical synthesis." It is based on two propositions:

(i) In the short run, movements in economic activity are dominated by movements in aggregate demand.
(ii) Over time, the economy tends to return to a steady-state growth path.

Together, these two propositions deliver a complex characterization of the effects of shocks and of macroeconomic policy. In the short run, larger budget deficits can increase output and investment; but in the long run, they decrease capital and output. In the short run, higher money growth can increase output; but in the long run, it is more likely to decrease it. These intertemporal changes in sign are what makes macroeconomic policy difficult and the politics of macroeconomic policy often so heated.

Within this core, there are parts that are well understood, parts that are understood a little, and parts no one has much of a clue about. Here is my list, starting with the bad news, macroeconomists' limited understanding of the medium and long run.

I. The Medium and the Long Run

The steady-state growth path is not the nice competitive path of simple textbook models. Market imperfections are central to both the level and the slope of the steady-state growth path. One cannot (usefully) think about R&D, one of the main determinants of total factor productivity, in competitive markets. One cannot (usefully) think about unemployment in competitive labor markets.

The last ten years have seen major developments in both the analysis of technological progress and the analysis of unemployment. New growth theory has led to models that look at the role of R&D, of human as well as physical capital, of social capital and infrastructure. Linking the process of job creation and job destruction to the flows in labor markets has provided a much richer understanding of the nature and the determinants of unemployment. Yet, macroeconomists are a long way from having even a decent quantitative understanding of either the rate of growth of total factor productivity or the natural rate of unemployment.

We are still largely baffled by differences in rates of total factor productivity growth, either across time or across countries. Why the average rate of total factor productivity growth appears to have decreased by roughly 1 percent in the United States since 1973 is still a mystery. Based on introspection and my daily fights with computer software, I like the recent line that links the slowdown to difficulties in using new information technologies efficiently. But I may like it only because it has not been around long enough to take the heat that most previous hypotheses have taken.

After 15 years of high unemployment in Europe, analysts have developed a long list of potential causes but are still far from explaining either the timing of the increase in European unemployment or the differences in unemployment experiences across European countries. Comparing Spain (with its 24-percent unemployment rate) with Portugal (with its 6.7-percent rate) or Austria (4.3 percent) is an exercise in humility, not a triumph of economic theory. And while my faith in a pure hysteresis theory of unemployment has declined, the evidence points to substantial history-dependence: high sustained unemployment changes people, social attitudes, and government programs, and it leads to a higher natural rate. If the long run depends on short-run movements in activity, this is a serious

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moderator to the dichotomy between an invariant steady-state path and fluctuations around it, which underlies the core.

Unemployment and total factor productivity are not the only features of the steady-state growth path that need to be explained. Much attention has been focused on changes in the wage distribution over the last 20 years. The rather wide agreement that biased technological progress rather than trade is to blame for increased wage inequality is not much more than an admission of ignorance. Much less attention has been focused on movements in capital and labor shares. But major changes have taken place in these as well. In the last 15 years, the share of capital in GDP in the OECD-Europe has increased from 30 percent to 38 percent; in France, it has increased from 28 percent to 40 percent. These are major changes in income distribution. Again, macroeconomists do not have much idea of why these changes have occurred. (My own research has convinced me that one proximate cause is the behavior of inflation. The share of capital income is high when inflation is low. But this raises more questions than it answers.)

II. The Short Run

Let me turn to the better news, macroeconomists' understanding of the short run. Ever since John Maynard Keynes, the standard explanation for why movements in aggregate demand affect output in the short run has relied on the presence of nominal rigidities. Lags in the adjustment of individual nominal prices and wages prevent both the price level and the interest rate from adjusting as they would in a world in which prices and wages were continuously adjusted. As a result, the argument goes, movements in aggregate demand lead to movements in output, rather than to movements in the price level.

Nominal rigidities, and their implication for short-run movements in output, are why macroeconomics looks so different from microeconomics. It also makes most macroeconomists uneasy: can such mundane aspects of the economy, such as whether and how often firms adjust wages or prices, really make so much difference to the aggregate outcome?

"New Keynesian" economists have shown that, as a logical matter, small rigidities can indeed add up and lead to large effects. But the uneasiness remains. Aren't these explanations essentially ad hoc, designed to save an intellectually bankrupt line of thinking? Isn't there another way of looking at fluctuations that would be more intellectually attractive?

This explains why efforts are regularly made to rebuild macroeconomics without recourse to nominal rigidities. The most recent attempt has been the real-business-cycle approach to fluctuations, an approach so logical and elegant that it can be taught to students just after they have seen the law of supply and demand for the first time. But the force of facts is hard to avoid. Fifteen years after the launch of this approach, nominal rigidities have reentered real-business-cycle models. Except for aesthetics, recent models by Robert King, or Larry Christiano and Martin Eichenbaum, do not look very different from the models developed by John Taylor in the mid 1980's.

The reason is not hard to find. Right or wrong, the IS-LM model, and its intellectual cousins, the Mundell-Fleming model and the various incarnations of aggregate supply-aggregate demand models, have proved incredibly useful at analyzing fluctuations and the effects of policy. In the United States, the Volcker disinflation and the Reagan deficits went by the book: in true Mundell-Fleming fashion, the slowdown in money growth produced high nominal and real interest rates, a large recession, and a dollar appreciation. The Reagan deficits led to an expansion of output, high real interest rates, a dollar appreciation, and large trade deficits. In true IS-LM fashion, the attempts of consumers to save more led to the 1990-1991 recession, and the expansionary policy of the Fed quickly ended it.

Indeed, from the disinflations of the early 1980's in the United States and Europe to the output performance of countries within the European Monetary System in the 1990's, to the effects of nominal undervaluation in the central European countries in transition, and to the effects of the currency board on Argentina today, the empirical evidence has confirmed over and over again the amazing power of monetary and exchange-rate policies to affect
real activity—often for the best and sometimes for the worst.

Furthermore, a number of formal quantitative studies, using either event-study or just-identified-VAR methodologies, have shown that the monetary mechanism described by Franco Modigliani and others is indeed present in the data. For example, a recent study by Ben Bernanke and Ilia Mihov finds that a decrease in the U.S. federal funds rate of 100 basis points leads to an increase in output that peaks after about one year at 0.8 percent and disappears after two or three years; in contrast, the response of the price level is gradual, indeed insignificant for most of the year after the change in the interest rate. Other studies have painted a similar picture. They have also shown that, as one would expect, long-term rates respond less than one-for-one to short rates, and a decrease in the short rate leads to a dollar depreciation. There are a few twists and puzzles however. A fairly consistent finding across studies is that, while the nominal rate is back to normal within a year, the effects on output are drawn out over a much longer time. Whether that fact can be explained within the existing framework is an open question, but with potentially important implications.

With the self-imposed need for European countries to satisfy the Maastricht criteria and the drive toward a balanced budget in the United States, fiscal issues are coming back to the fore. Some have argued that this is one place where the standard wisdom may no longer be true, that expectational effects may reverse standard Keynesian effects, even in the short run. Decreases in government spending, the argument goes, may convince financial markets that the future is bright and lead to an asset boom that more than offsets the direct contractionary effects of reduced public spending.

The argument is loosely based on the experiences of Ireland and Denmark in the 1980’s, two countries where dramatic budget reductions were associated with growth, not recession. But too many things happened during those episodes for the evidence to speak conclusively. Currently, Roberto Perotti and I are using the same just-identified VAR approach used to analyze the effects of monetary policy, but this time to look at fiscal policy. Our results, based on the evidence from each of the G-7 countries, are preliminary; but they are interesting. What we identify as exogenous changes in taxes typically appear to have traditional effects: increases in taxation lead to a contraction in output for some time; the magnitude often points to a substantial multiplier, above 2 in the United States after a year or so. The evidence is much more mixed for what we identify as exogenous changes in government spending: we often find no significant effect on output. This does not square well with the standard model.

In short, the good news for policymakers is that there is indeed a core of usable macroeconomics; the good news for macroeconomic researchers is that there is a lot of work still to be done.