

Commentary: Separating the Business Cycle from Other Economic Fluctuations

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In most academic studies of optimal monetary policy, a central bank is assumed to have two main objectives. One objective is keeping inflation low and stable. The other is keeping the economy's output of goods and services close to its potential—or, equivalently, keeping unemployment close to its natural rate. This approach has two virtues. The first is that it is consistent with basic theory: Under some reasonable conditions, this objective function can be derived as a reduced-form expression for the utility of the representative household. Just as important, this objective function appears to approximate those things that real central bankers (as opposed to the hypothetical central bankers in a theorist's model) really care about.

Optimal policy in these models often takes the form of a Taylor rule. The central bank is supposed to set the short-term interest rate as a function of inflation and the deviation of output from potential.

Bob Hall's paper takes aim at the practical application of this framework. He argues for three related but distinct propositions. First, he claims that it is difficult to estimate potential output, especially contemporaneously. Second, he argues that potential output exhibits substantial high-frequency variation, so the normal presumption that it follows a smooth trend is suspect. Third, he suggests that modern

theories of the labor market call into question the very concept of the natural rate of unemployment and thus potential output.

The bottom line from these arguments is that central bankers should be wary when their staff of economists produces estimates of potential output and the output gap and that they should avoid relying on these estimates when setting monetary policy. The alternative is to focus almost exclusively on the other variable in their objective function—the rate of inflation.

I agree with Bob's first argument completely. The natural rate of unemployment and potential output are extraordinarily difficult to estimate. Work by Staiger, Stock, and Watson (published in a National Bureau of Economic Research, NBER, conference volume about a decade ago) convinced me that estimates of the natural rate come with a huge standard error. The fundamental problem is that the residuals in the Phillips curve are so large that the parameters of this relation cannot be estimated with much precision. Even if the natural rate were completely constant, it would take many years of data to pin it down precisely. And the fact that the natural rate changes over time as a function of demographics, institutions, and labor-market policies only makes the problem worse.

This means that central bankers should be suspicious of any estimate of the natural rate. Excessive reliance on imprecise measures can be a significant problem. It was probably not a coincidence that the 1970s were a decade characterized by both rising inflation and a rising natural rate of unemployment. Not realizing how much the natural rate had risen, the Fed thought that there was more slack in the economy than there really was. Despite several decades of research since then, I am not sure that today's economists, faced with a similar set of events, would be any better at estimating a change in the natural rate in real time.

Bob's second claim is that potential output exhibits significant high-frequency variation. I find this conclusion less compelling. He reaches this result by feeding measures of total factor productivity

into a neoclassical growth model, using the model's predicted output as a measure of potential. This measure of potential output moves around a lot, suggesting that much of the variation in actual output does not translate into movements in the output gap.

It is well-known that total factor productivity fluctuates substantially over the business cycle. The question is how to interpret that fact. The real business cycle theories on which Bob is building treat movements in total factor productivity as if they were exogenous changes in the economy's productive technology. But these movements could just as easily be endogenous responses to the business cycle. If businesses hoard labor in downturns to avoid hiring and training costs when the economy turns around, then labor effort could well be cyclical, causing total factor productivity to be procyclical. But this need not mean that technology and potential output are changing.

Let me be clear that I am not suggesting that I know that potential output follows a smooth trend, as is so often assumed. Bob, along with the real business cycle theorists, has raised the real possibility that potential output does exhibit high-frequency variation. The unexpected acceleration in productivity growth since 1995—most likely associated with information technology—does highlight that potential output can move around. But I don't think the kind of calculations presented in this paper shed much light on how substantial the year-to-year fluctuations in potential output really are.

The third line of argument that Bob pursues in this paper is the suggestion that the whole distinction between the natural rate of unemployment and cyclical unemployment is misguided. From my perspective, this part of the paper is half-baked. This is not to say that the argument is wrong, only that it is insufficiently developed to evaluate it with much confidence.

The distinction between natural and cyclical unemployment has its foundation in Milton Friedman's classic address to the American Economic Association, where Friedman forcefully propounded the

hypothesis that monetary policy has important effects on output and unemployment in the short run, but no effects on such real variables in the long run. This natural rate hypothesis is part of most modern theories of the business cycle, and it is what generates the distinction between natural unemployment as the steady state toward which the economy gravitates in the long run and cyclical unemployment as the short-run deviation of unemployment from that steady state.

Rejecting the distinction between natural and cyclical unemployment is, therefore, tantamount to rejecting Friedman's natural rate theory. It is not clear, however, what Bob would put in its place. The real business cycle theorists want us to assume that monetary policy is incapable of affecting output and unemployment even in the short run. Bob is too practical of a macroeconomist to take that proposition seriously, but he has not fully spelled out what his alternative vision is.

In the end, I agree with Bob that monetary policy makers should take estimates of potential output and the natural rate of unemployment with more than a grain of salt. But I am disinclined to sign on to his suggestion that we reject the textbook approach to economic fluctuations. Instead, I agree with him for the more prosaic reason that these variables are hard to estimate with much precision.

This brings me to the policy question: If we cannot estimate potential output or the natural rate of unemployment, what are monetary policy makers to do? The obvious alternative is to focus exclusively on inflation, as some inflation-targeting central banks are doing now.

Such a regime of pure inflation targeting would seem to be inconsistent with the Fed's dual mandate of being concerned about both price stability and full employment. It turns out, however, that this is not necessarily the case. In some modern theories of the business cycle, a monetary policy that aimed exclusively at stabilizing the price level would achieve, as a byproduct, stabilization of output at its potential level. Olivier Blanchard has called this fact the "divine coincidence." If

the divine coincidence is actually true, it would conveniently solve the conundrum raised in this paper, for the central bank would not need to measure potential output in order to keep actual output at potential. It only would need to stabilize prices.

Let me spend a few minutes explaining why the divine coincidence might be true. Consider first shocks to the aggregate demand for goods and services. Expansionary demand shocks tend to push prices up and output above potential; contractionary demand shocks put downward pressure on prices and depress output below potential. Because the price level and the output gap are moving in the same direction, a monetary policy that stabilizes one will automatically stabilize the other. In other words, a central bank that follows a policy of pure inflation targeting will, as a desirable side effect, insulate output from shocks to aggregate demand.

Now consider shocks to productivity. A positive shock to productivity, such as those we have experienced since 1995, puts downward pressure on prices and tends to increase output. A central bank committed to pure inflation targeting would respond with more expansionary policy, increasing output further. This might seem to be destabilizing output. But remember that the positive productivity shock also raises potential output. In many standard models, these two effects exactly balance. That is, if the central bank keeps the price level on target, output and potential output will increase by exactly the same amount in response to a positive productivity shock. Once again, stabilizing prices automatically stabilizes output at potential. You can now see why Blanchard calls this the divine coincidence.

Is this coincidence true in the world, or is it just an artifact of some oversimplified macroeconomic theories? The literature on this topic is not sufficiently developed to give a definitive answer, but my guess is that it is more likely an artifact. One can certainly write down a model well-grounded in theory in which the divine coincidence does not arise. The key feature of such models is that supply shocks are not simply shifts in productivity but also represent shifts in how distorted

the economy's production process is. For example, imagine that because of market power, prices are a markup over marginal cost. If supply shocks in part represent shifts in the size of that markup, then it turns out that the divine coincidence does not arise. In this case, monetary policymakers face a tradeoff between stabilizing inflation and stabilizing the output gap. Whether this kind of supply shock is an important feature of the world is, I believe, a crucial unanswered question.

So, what does all this mean for the practice of central banking? Unlike Bob, I am not ready to give up on concepts such as potential output and the natural rate of unemployment. But I agree with him that we measure these concepts poorly and that this fact suggests increased emphasis on measures of inflation. Some might argue for an exclusive focus on inflation, but I don't see the current state of monetary theory as necessarily supporting such an extreme view. In the end, central bankers have little choice but to look at all the data, apply a healthy dose of skepticism, and muddle through.