

General Discussion: Separating the Business Cycle from Other Economic Fluctuations

Chair: Roger W. Ferguson, Jr.

Mr. Svensson: I was a bit puzzled by this paper. It seems orthogonal to monetary economics and monetary policy. And, as Bob Hall said, he doesn't draw any conclusions for monetary policy, except for some comments on the Taylor rule. It seems to me that Bob isn't really discussing modern monetary economics in his paper because in modern monetary economics the u^* , y^* , and r^* that you see in the models are not constants or a smooth trend or the result of a simple neoclassical growth model. They are the outcome of a hypothetical flex-price equilibrium. That flex-price equilibrium includes a lot of real frictions, imperfect competition, information restrictions, imperfect labor markets, etc. In a way, the u 's, y 's, and r 's in Bob's paper are actually the u^* , y^* , and r^* in modern monetary economics. Modern monetary economics is, to a large extent, about the gaps between y and y^* , r and r^* , and u and u^* . Those gaps are the difference between the sticky-price/sticky-wage equilibrium and the hypothetical flex-price equilibrium. As both Charlie Bean and Greg Mankiw emphasized, one way of looking at monetary policy is that it tries to move the economy toward the flex-price equilibrium. That is why the loss function for monetary policy includes those squared gaps, in addition to the squared gap between inflation and the inflation target. Modern central banks, to a large extent, think in these terms. Some of them have sophisticated Kalman-filter time-varying estimates of

u^* , y^* , and r^* in order to calculate the gaps. These estimates are, of course, model-dependent to a considerable extent.

The sticky-price/sticky-wage part of modern monetary economics is very important. If nominal price and wages are flexible, the gaps are zero. Then monetary policy is very trivial. It is just a matter of stabilizing inflation or the price level without any real effects of such stabilization. So, Bob's paper does not get into the most crucial aspects of monetary economics and monetary policy. Therefore, I find it orthogonal to the debate about monetary economics and monetary policy.

Mr. Summers: I suppose it says something about Bob Hall's paper that Lars Svensson thinks it's orthogonal to the question of monetary policy. Properly interrupted, it largely justifies the monetary policy approach of the last 18 years as described by both Alan Greenspan and Alan Blinder this morning.

It seems to me there is a certain amount of sensationalism that goes on in the paper. The part I would quote from the paper is from the final page, which was stated exactly correct after 48 pages: "Nothing in new thinking has shaken Friedman's basic message, however, that unemployment is invariant to the monetary policy regime. In that sense, natural rate theory is thoroughly embedded in all modern thought."

I always have regarded the natural rate hypothesis, purchasing power parity, and some parallel statement with respect to real interest rates as being a statement to superneutrality. It doesn't matter how monetary policy influences the long-term inflation rate. The real performance of the economy will be the same, with the important qualification that all central bankers add: If inflation of the sort of reverse Phillips curve is higher, things will be worse because the real economy will function less well. That, it seems to me, is the essence of Friedman's argument and the essence of the animating insight that has led monetary policy around the world post-1980 to be much better than monetary policy pre-1980. There is nothing to be gained from systematically having a higher rate of inflation. It does not follow from that in any way, shape, or form that any of the variables

that cannot be influenced by monetary policy are constants or are unaffected by their own past evolution. Much of what the paper explains at great length is that there are lots of reasons to think that the underlying rate of productivity growth varies significantly, that the rate of unemployment ground out by the matching process varies significantly, that the points at which consumers and firms trade off present and future in investing vary very substantially through time and rather more than economists traditionally had supposed, and that there is a large component of that variation that is not merely transitory and cyclical that has to be recognized. It seems to me if you believe that, the broad monetary policy approach that is counseled is one of not focusing on estimates of specific parameters that cannot be estimated precisely and, in any event, are likely to change. Rather, the approach should be one that would involve trying to gauge the precise state of the economy at any instant, trying to gauge the precise set of pressures operating on prices at any instant from as many different indicators as possible, and using that as a basis for judging the appropriate stance of monetary policy, which in many ways parallels the approach that has been taken.

It seems to me that the central message of this paper is a further elucidation of what is surely a hallmark of the difference between the Greenspan period and much of the academic literature: a reluctance to rely on estimates of specific parameters from particular models in guiding monetary policy. This reluctance largely would be the right thing if you believed in superneutrality but believed that the numbers that correspond to u , r^* , the equilibrium productivity growth rate, and the real exchange rate were subject to all kinds of highly persistent shocks and, in an aspect that is important and the paper doesn't emphasize, were also likely to be history-dependent. This does bring in a different set of implications that the paper might have emphasized.

Mr. Kohn: I was glad to see 50 pages of equations, charts, and reasoning to justify Alan Greenspan's answer to the question "Where is the natural rate?" when he says, "We'll know it when we see it."

I agree with Larry Summers' description of making monetary policy. We look at all kinds of forces and see how they are interacting at the current time, constantly updating our analysis. I do think that, even if we were to target just inflation, we would need to include among those forces some estimates of output gaps, interest rate gaps, that sort of thing, taken with the big grain of salt that Greg and Charlie talked about. We do find that some smoothing of productivity does help explain price increases in a markup model over unit labor costs. We also find that smoothed productivity fed into an output gap model does help in a small way to explain future inflation. I don't think those are entirely consistent with what Bob Hall was saying, but I do think that we can't help but at least take conditional guesses at those gaps and feed them into the many forces Larry Summers was talking about.

Mr. Mussa: I was glad that Charlie Bean was on the program commenting because it gets me to note that the Great Seal of the United Kingdom has on it two animals: a lion, not natively resident to the United Kingdom for many centuries, and a unicorn, a fictitious beast.

Now, the natural rate of employment, potential output, and the neutral rate of the real interest rate are all unicorns. They don't exist really. We have a concept of what they are. I would say other unicorns are a supply curve and a demand curve, the intersection of which determines the equilibrium price and quantity. Haven't met them in the forest either. The question is, Are they useful concepts to enable us to conceptualize and discuss important issues of economics and economic policy, taking into account that they are fictitious beasts and not to be taken all that seriously all the time? Like the supply curve and the demand curve and concept of equilibrium price, the natural rate of unemployment and potential output are useful...provided one doesn't take them as being measured too precisely.

I have a couple of other remarks. I do like Bob Hall's paper. It is highly provocative, and that is the useful thing about many papers, and particularly about Bob's papers. There is an important link, as Larry Summers says, to the discussion of monetary policy. It goes

back to this issue of rules versus discretion. If I follow a Taylor-type rule but I exercise a refined discretion in estimating y^* and r^* , then am I following a rule or am I exercising discretion? It seems to me that the answer to that question was provided 75 years ago by Winston Churchill in his autobiography when he comments that he was asked the following question: What is your position on the controversy between predestination and freewill? Winston said, "Mark my words carefully. They are the same." That is fundamentally true. There is no policy that doesn't follow some systematic behavior and that doesn't embody some principles of consistency of action. There is no absolute rule that is going to be followed independently of how the facts of an ever-changing world evolve over time. We operate in a system in which there are both rule and discretion, and there is no way to avoid that.

Finally, on sticky prices: I am a great fan of sticky prices and have written many papers on the issue, partly to irritate my colleagues at the University of Chicago when I was on the faculty there. Sticky prices are a very important phenomenon. I agree with Hall, however, that sticky prices are not enough to explain the phenomenon of unemployment and its fluctuations.

I had a sticky price in my apartment in Chicago, but the price and the quantity were both fixed. There are a lot of sticky prices of that kind. They are important for understanding price dynamics and why there is some inertia in the inflationary process and, as Charlie Bean suggested, providing some part of the explanation for fluctuations in employment. But they can't be the entire explanation. Neither is the real business cycle theory.

I used to irritate my colleagues at Chicago even more by saying the real business cycle theory was that theory according to which the 1930s should not be known as the Great Depression, but instead should be known as the Great Vacation.

Mr. Alexander: I would like to make a comment and ask a bit of a question. It seems to me that central banks are not the only ones that are facing the problem of trying to extract trends out of macro data. Financial market participants have to do it as well. This general theme that has been running through the day of how difficult that is in this world where there are big structural changes translates naturally into a world where people who are making investments in long-life assets have to make those same judgments and face those same uncertainties. I would make the comment that the information content of long-life assets is in some sense impaired by all these uncertainties as well. I would ask those who have thought more about monetary policy how that filters into how you use the information that is or is not in long-life assets in making monetary policy decisions when they face those same kinds of uncertainties.

Mr. Sinai: Hall's paper is interesting, provocative, and persuasive, particularly the empirical work of the paper. The comments that the concepts of potential output, the natural rate, and the real rate of interest are not very useful in the making of monetary policy is a clear implication that you can't measure them precisely enough or, therefore, usefully in policy. My observation of the Greenspan era is that the Greenspan Fed and particularly the chairman followed that prescription during these years and exhibited a tremendous amount of agnosticism on each of those concepts. I am not sure about the last one—the neutral rate of interest.

My question really is for Bob, which is unfair in a way. We have been told—those of us who are in the markets—that the Federal Reserve is removing accommodation and moving toward something called neutrality. I think you are saying that is the wrong thing to do and to say. You may or may not be saying that, but that is an implication or some guides to the Fed that you are offering. I can tell you for sure there is some pricing of that notion in the markets—in the fixed-income market and in the equity market—that the Federal Reserve will raise rates to some neutral level, stop, and then all will be fine. If that isn't the case, then neutrality doesn't make a lot of sense.

That is a big change. Do you have a comment on that or want to offer an alternative to the neutral rate as the third leg? What your evidence says—and I agree with it—is there are so many definitions of it and so many ways to figure a neutral rate that it makes very little sense to me to talk about a neutral rate, and maybe it does make a lot of sense to say, “We’ll know it when we see it.”

Mr. Blinder: I have two questions about the implicit advice in this very fascinating and, as has been said, “Hall-like” paper. The first one is about the advice to monetary policy makers. I am pretty sure you said that the proper way to amend the Taylor rule is to throw out the r^* and either the y^* or the u^* and then use the rule. (This last part is the part I am not so sure about.) If you do that, what you have done, of course, is impounded those things into the constant, which is completely the opposite of what you have urged we should do. Can you please clarify that?

Mr. Hall: It’s the change in the interest rate.

Mr. Blinder: Okay, it’s just Δr . The second is about the explicit advice to economic theorists, which is to worry much less about sticky prices and worry much more about matching in labor markets. The question here has been raised in other ways by other people, but I’ll put it slightly different. One of the central questions of macroeconomic theory—and, one might argue, its most central question—is, Why isn’t an increase in money like a currency reform? When you move the zeroes on the currency, nothing is supposed to happen. But if you increase the money supply by 10 percent, all prices don’t go up by 10 percent. It sure seems to me that we ought to be looking at sticky-price theories to understand that much more rather than at labor market matching. I wonder if you would disagree with that?

Mr. Hall: I don’t.

Mr. Cotis: I found the paper fascinating, but I am not convinced that productivity shocks have such an important influence on output

fluctuations at high frequency. Here, I have three remarks from an obscurantist practitioner.

My first remark is about your ex post accounting. You show a high contribution from productivity variance at high frequency and assume it is mostly supply-driven. But the counterfactual of a world dominated by demand shocks also would exhibit a high productivity variance. It is especially true because the productivity you use in the table is total factor productivity rather than just labor productivity, and, for obvious reasons, the productivity of fixed capital is highly cyclical in the short run in the face of demand shocks.

My second remark is about output gap measurement. I do not share the view that output gaps are absolutely unobservable. First of all, you have a number of natural output gap indicators, such as rates of capacity utilization or business sentiment indicators. Business sentiment indicators are *not* about the level of activity, but about the mismatch between production potential and demand. These natural indicators are fairly well correlated with artificial output gap indicators. Conversely, in a purely supply-driven world, business sentiment should remain immobile because there would be no such mismatch between production capacity and demand.

Finally, some output gap indicators are obviously less reliable than others. This is the case for HP filter estimates, which indeed have weaknesses at decision time, not only because of data revisions but also because these indicators imply forecasting future growth. Here, forecasters have a natural but unfortunate tendency to revert to normal growth while they should revert to normal output. A vivid illustration of this bias could be observed at the peak of the previous cycle, when growth forecasts were tabling on a gentle but unlikely slowdown rather than a recession. As a result, the use of HP filter estimates led to overestimating potential growth and to flattening the variance of HP-based output gaps at decision time. To remedy these difficulties, what you need is a variety of output gap indicators and good judgment in selecting the least imperfect for policy purposes. All in all and

despite all the pitfalls, the notion of an output gap has an important role to play in the conduct of macro policy. I think it is unavoidable.

Mr. Hall: Some of these comments really have been excellent, and I am going to make some significant revisions in the paper.

There was one thing that many of the interventions commented on—and it was my fault. Let me take a quick stab at saying what I really believe. That has to do with price stickiness.

The paper discusses why the received model—the model that Lars Svensson referred to—is not necessarily wrong but it is just one of many models of monetary non-neutrality. The first thing I would say is that I believe what everyone in this room believes, which is that monetary interventions do not have a neutral effect. The central bank can expand employment and output by lowering interest rates. I am not a strict monetary neutrality guy...never have been. I'm always looking for explanations within the macro theory I respect that would help me understand non-neutrality.

Several people spoke as if there were sticky prices on the one hand and a matching model on the other hand—but one of the basic points, and this is the very first point I made in my recent research in this area (which appears in a paper of mine that was in the March *AER*), is that there is a distinct, different role for sticky prices, and contrary to what Charles Bean said, they could be nominally sticky. The equilibrium approach, not the traditional Keynesian sticky price (not the “call option model,” as I call it in the paper), is compatible with there being a nominal wage norm that does not change when the environment changes. As long as it remains within the bargaining set, then it's an equilibrium wage. Bean again summarized the guts of that very nicely.

There is plenty of room for monetary non-neutrality and for a new version of sticky nominal wages and also sticky prices in this new view. It is not that I am throwing out any nominal stickiness and, therefore, am unable to explain monetary non-neutrality; rather,

there is a whole new class of models to think about. These are competing models to the received model. I certainly don't believe there are central bankers who are making a mistake of saying that the Calvo sticky-price model, as it has been implemented everywhere and is very thoroughly explained in Mike Woodford's book two years ago, is the one true model. I don't happen to believe it, for the reason this paper explains. It is competing in the marketplace for ideas. The main point I am making here is there are other new concepts that don't have the disequilibrium character. They do not have the character that there are a y^* and y because they are not disequilibrium models. They are equilibrium models, but they still have monetary non-neutrality. They still make central banking a huge challenge and presumably a challenge similar to the ones we have described. The equilibrium view provides a new theory of what we believe about monetary non-neutrality. That takes care of a major set of the comments, and I'll try to spell it out more in the revision.

The other comments referred to the empirical work that I agree overstates the role of changes in productivity as contributor to movements of output. That happens to be an area that I have been doing research in for a long time, and I'll probably have to get back into now. I certainly agree, for example, that if we had good measures of capital utilization, we would put capital utilization in parallel to labor utilization (which is already there). Labor utilization is the unemployment rate, and we would like to do the same thing with capital, but we lack good data. If we could do that, we would call fluctuations in capital utilization cyclical, it would reduce the productivity calculation role, it would raise the cyclical role, and it would change those numbers. I doubt it would change the number that says that the seven-year contribution of so-called cyclical variables is about the same as the one-year contribution. That factor is not going to change because if you look at measures we have of capital utilization, you find the same perplexing properties of those you do for other utilization measures. They don't track the cycle that closely. They have very long-lasting fluctuations as well as the cycle. Again, this is the first order of importance. I agree and I say in the paper already, but I'll underscore it even more, that

one-year-against-seven-year finding is going to be very hard to shake loose. The predominance of productivity shocks would change if you could measure things better, and it would decline. The cyclical variables would rise, but they would rise at the seven-year difference level as well as the first-year level. That is really a factor that needs to be kept firmly in mind.