

Speech by

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Comment on Bob Hall's "Separating the Business Cycle from Other Economic
Fluctuations"

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It is always a pleasure to read a Bob Hall paper because one knows that one will be forced to think anew about an issue. So it is even more of a pleasure to be invited here to discuss his paper on business cycles.

The kernel of Bob's argument is that the movements in the main US macroeconomic aggregates, both at cyclical frequencies and longer, are the natural consequence of a well-functioning economy responding to shocks to productivity and real spending. Far from following a relatively smooth trend, potential output is stochastic. Indeed in the canonical model, there is no Keynesian-style output gap at all.

The claim that business cycle fluctuations are primarily an equilibrium response to real shocks, particularly to productivity, was originally advanced more than twenty years ago by Finn Kydland and Ed Prescott (1982). Bob provides similar evidence in his paper today. But how convincing is that evidence? Not very, I would argue. For the fact that the data *can be* reasonably well explained by a suitably calibrated neoclassical growth model subject to the right sort of shocks does not mean that they necessarily *are* generated in this fashion.

Bob's evidence for the importance of productivity disturbances relies on a decomposition of output growth in which relatively little of the movements in detrended output are attributable to movements in recorded inputs and a large fraction to total factor productivity. But total factor productivity is measured as a residual, so measurement errors in output and inputs end up here, inflating the estimated contribution of productivity disturbances. More significantly, no allowance is made for variable factor utilisation over the cycle. Indeed it is precisely the existence of match-specific capital of the type that subsequently appears in Bob's own characterisation of the labour market that makes labour hoarding during downturns potentially so important. Neither of these objections is new and Bob acknowledges them in passing. But to me they constitute a reason for scepticism, especially when other approaches, such as the structural vector-autoregressions of Blanchard and Quah (1989) and their many imitators, suggest that the contribution of technology to output fluctuations at business cycle frequencies is rather more modest.

Partly because of that evidence, much recent business-cycle research has been directed to incorporating Keynesian-style sticky prices and wages into the workhorse neoclassical growth model. Bob is somewhat dismissive of the importance of sticky prices, noting that while a lot of effort has gone into rationalising why prices might be sticky, rather little effort has been devoted to explaining why sellers post prices rather than undertaking bilateral bargaining in the first place. And, to be sure, such bilateral bargaining does take place for many intermediate products. But thank goodness it doesn't occur in the retail sector otherwise the lines at the supermarket checkout would be enormous!

One of the least satisfactory features of the early real business cycle models lay in the absence of a compelling explanation for fluctuations in employment and unemployment. Sufficient variability in employment is only possible in a market-clearing world if labour supply is very elastic with respect to the wage. But that runs against microeconomic evidence suggesting that labour supply is relatively wage-inelastic. Various ingenious solutions have been suggested, including intertemporal non-separability in leisure and the introduction of fixed costs of working. But none of these solutions are very convincing.

Bob argues that the way forward here is the recognition that workers and jobs are heterogeneous and cannot be costlessly matched to each other. Instead, forming a job match requires a capital investment in search, hiring and training by both sides. The presence of such sunk costs then means there is a post-match surplus to be shared between employee and employer. In the standard matching model of Mortensen and Pissarides (1994), the wage is assumed to be a weighted average of the outside options of the worker and the firm, both of which are sensitive to the state of the labour market. Hence, these models do not generate much variability in employment. But Bob observes that it is not credible for either party to threaten to take their outside option. As a result, a range of wage bargains can be supported, which are likely to be much less responsive to the state of the labour market than in the standard model. However, although the bargained wage has no effect on the continuation of existing matches, it *does* affect the rate at which new job vacancies are opened. This is an important insight, which produces a credible explanation for wage rigidity and for substantial fluctuations in unemployment.

However, this is a model of real, not nominal, wage rigidity. It provides an explanation why fluctuations in output may be associated with substantial fluctuations in unemployment, but not why shocks to nominal spending might have real effects. Bob is implicitly of the view that such shocks are transmitted very largely into prices even in the short run. The problem with this conclusion is that it runs against the very large empirical literature which concludes that nominal disturbances in the shape of monetary policy shocks have substantial real effects at horizons out to a year or more (see, for instance, the survey by Christiano, Eichebaum and Evans, 2000). Now it is possible that literature is fundamentally flawed and that the monetary policy disturbances in those exercises are merely proxying omitted real variables that shift equilibrium output. But I doubt it. So, to me, the evidence still suggests that nominal rigidities, whether in prices or wages or both, are a part of the story of the business cycle.

But here, I think Bob's characterisation of the labour market may have something to offer those who think sticky prices are important, for the presence of real rigidities is necessary to ensure that nominal rigidities have purchase (see Larry Ball and David Romer, 1990). So, if nominal prices are sticky because of a fixed cost of changing prices, but real wages are very responsive to unemployment, a fall in nominal demand and employment will lead to substantial fall in wages and in costs, making a cut in nominal prices worth undertaking. Only if real wages are relatively unresponsive to activity can nominal prices remain fixed.

What does this all imply for the conduct of monetary policy? Relatively little is said in the paper about monetary policy, but what there is, is framed in the context of the eponymous Taylor rule, which makes the deviation of the real policy rate from its neutral level an increasing function of the deviation of inflation from target and of the output gap. Bob's analysis implies that both the neutral real rate – if we can even measure it properly – and potential output move over time in ways that may be hard to predict. Indeed since his model of fluctuations is an equilibrium one, the output gap presumably should always be identically zero.

As already indicated, I do not buy the argument that nominal rigidities are irrelevant, in which case, unless there are multiple equilibria, one could in principle define an output gap. But even then, is it a useful construct? Bob's straw man policymaker takes output, fits a Hodrick-

Prescott filter through it, and then plugs the resulting detrended output series into the Taylor rule in order to set the policy rate. This, he argues, is a misguided approach if potential GDP is stochastic.

But I don't know of any central banker who behaves like this (and if they did, they probably wouldn't be in a job very long). Taylor's rule was originally meant as a rough reduced form empirical description, not a prescription for how policy should be set. One of the oddities of macroeconomics over the last thirty years is the way we have moved from assuming that policymakers optimise and private agents act like automatons to its polar opposite, whereas in fact both are presumably optimising, in a fashion, given the information constraints they face.

When central bankers deliberate over interest rates, we form a view of the outlook for inflation and output over the medium term together with the attendant risks, and then make a judgement on the appropriate policy stance in the light of that outlook. An assessment of the slack in the economy is an essential ingredient in forming that view, but it is not made merely by fitting a smooth trend through GDP. Instead our staffs employ models of varying complexity in which potential output and the natural rate of unemployment evolve stochastically in the way modern macroeconomic thinking suggests and also rely heavily on other sources of information, including a whole range of official data and business surveys. And we are acutely aware of the uncertainties, treating statistical estimates of the output gap with a very considerable degree of scepticism.

To conclude, Bob is certainly right to highlight the fact that potential output is not a smooth trend and that the natural rate of unemployment and the neutral rate of interest are not constants. But I do not believe we can dispense with the concepts altogether. If he is correct in his belief that the bulk of output fluctuations represent movements in the equilibrium of the economy, then surely at a minimum we need to have some idea how that equilibrium is likely to evolve if we are to stabilise prices effectively? And if, as I believe the evidence suggests, nominal rigidities are an important ingredient of the business cycle, then some assessment of the slack in the economy is also essential. Bob is surely correct that making that assessment is very difficult. But then I think we knew that already!

References

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