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Comment

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Introduction

This paper embeds the theory of unemployment in Galí (2011a, 2011b) into an otherwise standard medium-scale New Keynesian model and estimates it using the methods of Smets and Wouters (2007). The paper offers one substantive motivation for this merger. Specifically, one of the classic issues in business cycle theory is to identify the quantitatively important shocks that generate these fluctuations. Although Galí's modification to include unemployment does not affect how a given shock affects employment, it turns out that by utilizing data on both employment and unemployment, it can nonetheless be helpful in determining which type of shock might have caused a given change in employment. For example, in standard New Keynesian models, preference shocks and wage markup shocks have similar implications for employment, and existing estimation exercises cannot distinguish between them. But given that these two shocks have very different implications for policy in New Keynesian models, it is important to distinguish between them.

The intuition is straightforward: if employment goes down because of a preference shock that makes households value leisure more highly, one might reasonably expect that the decrease in employment will primarily show up as an increase in nonparticipation, with relatively little effect on unemployment. In contrast, if employment goes down because wage markups have increased and firms reduce their demand for workers, one might reasonably expect that the decrease in employment will primarily show up as an increase in unemployment with relatively little effect on participation. It follows that having an explicit model

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of unemployment may prove useful in identifying the quantitatively important shocks that lead to business cycles.

The idea that distinguishing between unemployment and nonparticipation might help to identify shocks is a promising one. To the extent that the current paper is intended primarily as a concrete example to illustrate this general point, I think it is largely successful. The analysis is clear and the arguments are clearly exposited. Especially because the introduction of unemployment into the framework does not disturb the workings of the model at all, one can see clearly how adding unemployment as an observable affects the identification of shocks.

The key results of the exercise in the paper are that preference shocks are not very important at business cycle frequencies but are relatively more important at lower frequencies. I do not find these qualitative conclusions objectionable, and I suspect this would be true for most economists. Specifically, I do not view literal preference shocks as a major potential source of business cycle fluctuations. And while the large changes over time in demographics, family structure, and female labor force participation might not correspond literally to preference shocks, in the context of a stand-in household model it may be that preference shocks is a useful way to capture them.

However, even though I find these conclusions reasonable, I do not view the current paper as providing much new evidence to influence my opinion on the matter. To view the exercise in the paper as more than just an illustrative example requires that the authors address two questions. First, is the model of unemployment implicit in this paper a good one for the purposes at hand? Second, is the methodology of Smets and Wouters good at inferring what shocks drive business cycles? Oddly enough, the paper is completely silent on these questions.

One theme that I will stress in these comments concerns the issue of micro-foundations. The authors write in the first paragraph of their paper that one of the appealing features of the class of models that they build on is its "sound, micro-founded structure." I find this statement somewhat curious. To be sure, the model is certainly micro-founded in the sense that the model specifies the economic primitives and is explicit about the behavior of individual agents in the model, implying that the analysis is not subject to the Lucas critique.

However, in my view the desire for micro-foundations goes beyond responding to the Lucas critique. Specifically, the other advantage of providing explicit micro-foundations is to make it possible to connect with micro-data to confirm that the behavior of agents in the model on key dimensions is in line with how these agents respond in the data. I will note specific instances of this in what follows, but in my view a key limitation of this paper, and more generally of the literature to which this paper belongs, is its failure to pursue micro-foundations in this sense.

Using Galí's Theory of Unemployment to Identify Shocks

Two of the virtues of Galí's theory of unemployment are that it is both intuitive and tractable. But given that the goal of this paper—to uncover the relative quantitative importance of various shocks—is very much a quantitative one, I was quite struck by the lack of attempt to argue that this theory of unemployment was in some sense a good quantitative theory of unemployment. "Good" could mean various things in different contexts, depending on the issue being addressed. Given the objective of this paper, I think that "good" should at least mean that it is able to distinguish how unemployment responds to various "labor supply" and "labor demand" forces. Neither this paper nor the previous papers by Galí devote any effort to arguing that this model of unemployment is good in this sense.

Galí's theory takes as given that the only source of unemployment is the market wage being inefficiently high. (The model offers two distinct channels for why the wage may be too high at any particular moment; it could be due to the markup, or it could be due to nominal rigidities that slow down the adjustment of wages to various shocks.) Attributing a significant role to wages in the determination of unemployment is certainly consistent with much other current work on modeling unemployment, including those that follow in the tradition of Mortensen and Pissarides (1994). But two questions arise. First, what is the appropriate theory of wage determination? Second, are there nonwage factors that also have a significant quantitative influence on aggregate movements in unemployment?

Regarding the first question, Galí's theory holds that every type of specialized labor is represented by its own union and that each union sets wages in an uncoordinated fashion. While one can use this as an example to illustrate the workings of the model for one particular wage-setting mechanism, it seems an obvious nonstarter as a theory of wage-setting in the US economy. One would like to see the authors provide some argument based on micro-data that this offers a reasonable foundation for thinking about wage determination.

Next I consider the issue of nonwage factors in influencing unemployment. If nonwage factors sometimes play a significant role in influencing unemployment, then a model that rules them out by assumption will likely not be a good tool for inferring the role of "demand" versus "supply" shocks. The large empirical literature that was started by Davis and Haltiwanger (1992) suggested that the massive reallocation of jobs across establishments is a likely source of some unemployment. Models such as Lucas and Prescott (1974) show that unemployment can result in such settings even with wages set competitively. This literature alone should make one skeptical of the assumption that all unemployment is due to wages being inefficiently high.

But let me also note two prominent contexts of low frequency unemployment changes where nonwage factors seem to potentially be important. The first context is the rise of European unemployment in the 1970s and 1980s. Assessing the relative importance of labor supply responses in accounting for this rise remains an open question. Ljungqvist and Sargent (1998) argue that a significant part of the increase is accounted for by the choices that workers make in the face of an increase in turbulence combined with very generous long-term Unemployment Insurance (UI) benefits. Assuming some element of this story is correct, the framework of Galí and coauthors cannot help us evaluate its importance, since it will simply infer from the rise in unemployment that the wage markup must have increased.[1]

The second context is the rise of US unemployment in the 1970s. Shimer (1998) argued that a large part of the increase in unemployment in the United States during the 1970s could be attributed to the entry of the baby boomers into the labor market, combined with the fact that younger workers have different unemployment dynamics. If forced to account for the data, the framework of Galí and coauthors would again infer that wage markups increased. This would be consistent with Shimer's argument if young workers have different relative unemployment dynamics because of differences in relative wage markups. However, if the matching process works as in Jovanovic (1979), at least some of the difference reflects labor supply channels in the presence of informational frictions.

To summarize the previous discussion, there is currently no consensus on the role that various factors play in accounting for the variation in aggregate unemployment rates over time and across countries, especially at low frequency. It is most certainly of interest to explore the extent to which one given factor (say, for example, wage markups) might help us understand this variation. If one pursues this avenue, a key element of the exercise must surely be to look for independent measures of the movement in wage markups. But what this paper does is to effectively assume that unemployment is entirely due to the wage being too high and then use that assumption to back out what wage markups (gross of nominal rigidities) must have been. In my view this does not offer much of an advance in developing theories of unemployment.

More generally, and in the spirit of my earlier comment about micro-foundations, I find the framework adopted by this paper to be quite limited in its ability to connect with the data. For example, the leading framework used to think about unemployment emphasizes what Blanchard and Diamond (1992) call the flow approach to labor markets. This reflects the fact that most people think that labor market flows reveal useful information. At the most basic level, it seems potentially relevant to decompose unemployment rate differences across time, demographic or skill groups, or countries into parts that are due to differences in duration and frequency of spells. (See, for example, the analysis of Blanchard and Portugal [2001] contrasting unemployment in the United States and Portugal.)

In my view, a model of unemployment that abstracts from flows is not very amenable to confronting various empirical facts that a good theory of unemployment should help us understand. Put somewhat differently, I think a good theory of aggregate unemployment should also offer a good theory of individual unemployment. Of course, it could be that the authors wish to argue that focusing on labor market flows as part of developing a theory of unemployment is misguided. If so, then the authors should articulate this view.

Using the Methods of Smets-Wouters to Identify Shocks

With my remaining space I want to note some issues with using the Smets-Wouters machinery as a way to identify business cycle shocks. A recurrent finding in the literature that estimates medium-scale New Keynesian models is that wage mark-up shocks are a quantitatively important source of business cycle shocks. Central to this finding is the fact that in a stand-in household model with standard functional forms, one finds significant movements in the so-called "labor wedge" at business cycle frequencies. It follows that a method that uses shocks to make this model match the data will necessarily find large shocks to the labor wedge. However, recent work by Chang and Kim (2007) and Chang, Kim, and Schorfheide (2011) show that models with heterogeneity and incomplete markets produce outcomes that look like shocks to the labor wedge in the aggregate data even when the sole driving force is a neutral technology shock.

I believe the assumption of complete insurance markets implicit in the authors' model is motivated by a desire for tractability and the hope that it is not substantively important. That is, I do not think that they are trying to argue that there are (approximately) complete markets for idiosyncratic income shocks. I think many researchers mistakenly interpret the work of Krusell and Smith (1998) to imply that incomplete markets and heterogeneity are not of first-order importance in business cycle settings. What they actually showed is that a few second moments are relatively unaffected by incomplete markets and heterogeneity. But, more importantly, the papers by Chang and coauthors previously noted show that this interpretation of Krusell and Smith's work is definitely not well founded in slightly richer settings.

The current paper contains another result that serves to illustrate the substantive implications of assuming complete insurance markets. When the authors used standard preferences the model implies a negative correlation between employment and participation in the face of some shocks. The mechanism behind this correlation is as follows. Because of complete insurance, when aggregate employment increases, consumption of all individuals increases, including those who remain nonemployed. Because consumption of the nonemployed increases, their desire to work at a given wage level decreases and the participation rate decreases. Because this mechanism generates a counterfactual correlation between employment and participation, the authors adopt an ad hoc and nonstandard specification of preferences in order to minimize the magnitude of this effect.

But intuitively, this counterfactual correlation seems to be an artifact of the assumption of complete insurance. To see why, consider the following scenario under complete and incomplete insurance. There are two individuals who are currently nonemployed, and in the next period one of them becomes employed but the other remains nonemployed. With complete markets, both of the individuals will experience an increase in consumption, and for the individual who remains nonemployed, this increase in consumption (can) lead them to no longer desire employment at the given market wage. But with incomplete markets, only the individual who becomes employed will experience an increase in consumption, and so there will be no effect on the desire of the second individual to be employed.

So while one might argue that one of the benefits of incorporating unemployment into the analysis is that it provides additional discipline on preferences, I think the more appropriate interpretation is to warn researchers about the perverse substantive effects that accompany the assumption of complete insurance in settings with indivisible labor and idiosyncratic income shocks.

As a final comment on the use of these methods to identify business cycle shocks, I would also note the recent work by Eusepi and Preston (2009). They show that moving from the assumption of a stand-in household with separable preferences (over consumption and leisure) and divisible labor to one with nonseparable preferences and indivisible labor has a dramatic effect on what the estimation exercise delivers for the relative importance of various shocks.

By way of summary, let me say the following. The earlier comments should not be taken to imply that we should not perform the types of exercises that Smets and Wouters and many others have carried out, in which we use explicit models to infer the relative importance of various shocks. Instead, they are to remind us that the results of such an exercise can be very sensitive to the details of the model specification and that as a result interpretation of the shocks requires a great deal of care.

Summary

The authors argue that extending standard macroeconomic models to explicitly include three labor market states can prove beneficial in diagnosing the role of various shocks in accounting for aggregate fluctuations. They demonstrate this by embedding Galí's theory of unemployment into an otherwise standard medium-scale New Keynesian model and estimating it using the methods of Smets and Wouters (2007). The analysis is clear and the paper is nicely exposited, and as a concrete example of the general point being made I think the paper is successful. But as either a compelling quantitative theory of the US aggregate labor market or a reliable model to be used to uncover shocks, I think the paper is much less successful. To my mind a key weakness and limitation of the exercise is the lack of attempt to connect with micro-economic data on individual unemployment spells, or wage determination and price setting.

Endnotes

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1. As noted earlier, the wage may also be high due to nominal rigidities. In the discussion that follows I will assume that these are not very relevant for low frequency movements, though this is not critical to the arguments being made.

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