

Commentary

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As Robert Shimer (2005) emphasizes, modern theories of the labor market recognize the conceptual value of decomposing unemployment fluctuations into hires and separations. Thus, we have

$$u_{t+1} = u_t(1 - h_t) + (1 - u_t)s_t,$$

where u_t denotes unemployment at date t , h_t denotes the hiring rate at t , and s_t denotes the separation rate at t . Or, if we want to look at changes,

$$\Delta u_t = u_{t+1} - u_t = (1 - u_t)s_t - u_t h_t.$$

If, for example, h_t and s_t are constant with respect to t , then starting at any initial u_0 ,

$$u_t \rightarrow u^* = \frac{s}{s + h}.$$

This kind of discussion is now standard fare in graduate and good undergraduate courses.¹

Here is a question: Given $\Delta u_t > 0$, as might happen during the downturn of a typical recession, is it because separations are high or because hires are low, and does it matter? The version of the paper that I read contained some examples attempting to illustrate just why this matters, and I want to discuss them briefly. As the first example, consider the sectoral shift hypothesis, which is

¹ Shimer tries to make a distinction between this approach and an earlier literature that decomposes changes in u_t into changes in *incidence* and *duration* of unemployment. This is tenuous because the incidence probability is simply s and expected duration is simply $1/h$. Perhaps there are situations (e.g., with time aggregation) where there is a relevant distinction.

the idea that recessions are best thought of as declines in some sectors. This decline, it is suggested, will show up as an increase in the separation rates, $\Delta s > 0$, with little change in the hiring rates, $\Delta h = 0$. But why? We know h can be big even in recessions. Declining firms could keep s the same, reduce h , and downsize through attrition. So without further elaboration, and I don't see what this would be, the decomposition of changes in u into (i) the component due to changes in h and (ii) the component due to changes in s does not strike me as somehow being the key to evaluating the sectoral shift hypothesis.

A second example in the paper concerned the literature on firing costs. According to the discussion, this literature thinks of recessions as times when $\Delta s = 0$ and $\Delta h < 0$, which is in some sense the opposite of the sectoral shift hypothesis previously mentioned. Hence, knowing from the data whether recessions are times when $\Delta s = 0$ and $\Delta h < 0$ or times when $\Delta s > 0$ and $\Delta h = 0$ is the key to distinguishing between these stories. But, wait a minute: Does anyone actually think firing-cost models and sectoral-shift models are competing world views? Why aren't they two aspects of one story? At some level this reflects a confusion between impulse and propagation mechanisms: Sectoral shifts could be the underlying cause of economic downturns, while firing costs could have effects on the dynamics. Note that I am not trying to champion this position here; I simply want to ask what the fuss is about.

A third example concerns the Keynesian litera-

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Federal Reserve Bank of St. Louis *Review*, July/August 2005, 87(4), pp. 509-12.

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ture. The idea seems to be that if there are nominal rigidities in wages, but new hires can get around these rigidities, then in fact recessions are actually times when firms really want to hire new workers. This strikes me as a bit silly, as it seems to imply that the Keynesian model predicts that we can avoid recessions (caused by sticky nominal wages) by having firms swap workers. Whatever. I don't think Shimer was ever very happy with these examples, and that is why they do not appear in the final version. So why am I discussing them? Well, first, maybe the points contained in this discussion are at least part of the reason why they were left on the cutting room floor; and second, it seems to me that we do need to think more about why the decomposition of changes in u into the part due to changes in s and the part due to changes in h is interesting.

Perhaps it is interesting for its own sake. Fine. I am quite prepared to take this as given for now. The real motivation for the paper is "to document the cyclical behavior of the hiring and separation rates in the United States for 1948-2004." I think this is a good idea; at least, it's "something to do." Let it be said that this is not as easy as one might think. For example, a big effort is made to take into account composition effects due to heterogeneous agents and movements in and out of the labor force, as I will discuss here. Also, the results do at least provide a clear cut answer to the previously mentioned question: The finding is that there are substantial fluctuations in h , and much less in s , and indeed fluctuations in s are even described at one point as "acyclical."²

Another of Shimer's findings—"perhaps the most surprising"—is the following: "Whatever forces make it harder for an unemployed worker to find a job during a recession also seem to make it harder for an employed worker to find a better job." Is this really *surprising*? Perhaps not, but it does suggest that it is worth thinking about the cyclical behavior of hiring rates seriously. For this, one needs a good measure of h , and at the end of the day this is what the exercise is all about. Given this, it seems to me that Shimer does an admirable

job coming up with a new and improved measure. Again, heterogeneity in h^i across agents is relevant. In particular, with homogenous agents, the different possible measures compared in the paper are equivalent, and hence it is only with heterogeneity that Shimer's measure is therefore either new or improved. But there should be little doubt that heterogeneity may be important in this context, and so considering this new measure of h seems useful.

Let me move on to some more detailed discussion of the actual exercise. Suppose we define *short-term unemployment* by $u_{t+1}^s = (1 - u_t)s_t$; then we have

$$u_{t+1} = u_t(1 - h_t) + u_{t+1}^s,$$

at least under the hypothesis that the labor force is constant (see forthcoming description). The advantage of this formulation is that we have a direct measure of u_{t+1}^s in the data. Hence, we can construct

$$h_t = 1 - \frac{u_{t+1} - u_{t+1}^s}{u_t}$$

as an empirical measure of hiring. Notice that, even if we have heterogeneity in hiring rates, $h_t = E_i h_t^i$. A few facts: $E_t h_t = 0.44$; and (after filtering), $cor(h_t, u_t) = -0.94$, $sd(h_t) = 0.12$, and $sd(u_t) = 0.20$.³

Now we get down to some serious issues. First, compositional effects. This issue is simple: Do hiring rates really *change* during recessions, or are there just more low- h people in the unemployment pool during recessions? Shimer dismisses the importance of compositional effects, but not at all casually. He does a good job of trying to address the problem, but it is a problem that as a matter of principle can never be resolved to full satisfaction. For example, suppose we have two types of workers, type L and type H , where $h^L < h^H$. Now suppose that, for any number of reasons that are easy enough to imagine, more type L workers lose their jobs in a recession. Shimer's approach is to divide the workers in his sample

² Saying they are "acyclical" seems to be a slight exaggeration, but this perhaps is a quibble about semantics.

³ As is typical in the literature, Shimer goes on to emphasize that " $sd(u_t)$ is 70 percent larger than and $sh(u_t)$ "—which seems to add little to our knowledge once we have been told that $sd(h_t) = 0.12$ and $sd(u_t) = 0.20$.

into groups by race, sex, and so on. But although this is useful, it does *not* completely resolve the potential problem.

For example, suppose that my two types are *lazy* workers with low h^L and *not-so-lazy* workers with high h^H . What observable characteristic is Shimer going to say proxies for laziness—race or sex? Obviously this is not going to be a viable approach. Of course, what I am saying here is obvious: There is no way to *really* control for unobserved heterogeneity. So while I am sympathetic to what Shimer has done, and perhaps it is the best that could be done, it is simply not a definitive result that heterogeneity is not what is driving the behavior of his empirical measure of h . I shall not beat this to death; but at the same it does deserve mention.

Shimer goes on to worry about changes in the labor force. Suppose we drop the maintained hypothesis that the labor force is fixed. Letting x_t denote exit from the labor force, we have the following new version of our law of motion for u_t ,

$$u_{t+1} = u_t(1 - h_t - x_t) + u_{t+1}^s,$$

and hence we have

$$h_t + x_t = 1 - \frac{u_{t+1} - u_{t+1}^s}{u_t}.$$

Therefore, his empirical variable h_t is really measuring the exit rate from u_t , either into employment or out of the labor force, and not the hiring rate, per se.

Shimer argues that empirically this is not a big deal. Fine, but there is a sense in which a whole other issue is raised. At least for some purposes, we might prefer to think about the world in terms of a two-state model, where e workers are employed and $1 - e$ are not employed; and let's not worry about decomposing the latter group into those in and those out of the labor force. Because, in reality, virtually everyone is "in the labor force" in the sense that they would be willing to take some job (the right job) if it came along, even if the official data do not recognize this. Of course, some people who are not working are more actively searching for work, or more willing to accept work, than others; this is a matter of degree, and it is obviously quite arbitrary to define some

criterion by which we label some in and others out of the labor force. One may say this is a side issue in terms of the focus of the current project, but given we are engaging in a careful measurement exercise, it is not illegitimate to ask what it is we ought to be trying to measure.

To move on, in addition to constructing a measure of the hiring rate, h , the paper also considers the separation rate, s , for the purpose of comparison. This is somewhat tricky because of time-aggregation issues. Shimer takes this seriously, but it is a slippery slope. He worries in particular about false job-to-job transitions recorded in the data, because a worker may have lost a job and found a new one between surveys. Sure. But what about the other side of the coin? Obviously many people have jobs, line up new ones before either leaving or losing the old jobs, but spend a little time between jobs doing things like moving, collecting unemployment insurance, chilling out, or whatever. These may look like transitions from e to u to e , but for many purposes it seems better to think of them as "really" more like job-to-job transitions. Can we tell from the data? Does it matter? Is there any way to resolve the issue satisfactorily? These seem like good questions.

By the way, there is also a model in the paper—a model with on-the-job search, of course, since how else could one expect to discuss the large number of job-to-job transitions in the data. Shimer assumes for simplicity that the distribution of job offers is time invariant. Given this, after some routine on-the-job-search algebra we get some nice results. He uses the model to come up with measures of how many workers switch jobs each month *involuntarily* (i.e., with an intervening spell of unemployment) and *voluntarily* (i.e., without same). I am not sure these words constitute the best choice of language, especially given the time-aggregation issues raised in the previous paragraph, but this is his choice. Another issue is that the numbers he comes up with are sensitive to assumptions about arrival rates of offers for employed and unemployed workers. What should we do about pinning down these arrival rates? Perhaps one can calibrate to match the observations on labor market flows in, say, Fallick and

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Fleischman (2001). Or one can look to empirical estimates of the Burdett and Mortensen (1998) model. Or one can make them up.

Let me try to wrap up. First, I want to say that this is agreeable work. As Finn Kydland (or is it Ed Prescott?) often says, “we should all be in favor of good measurement.” The finding is this: It is changes in h and not changes in s that drive fluctuations in u . Shimer’s conclusion is that the “received wisdom” about “job destruction rising dramatically during recessions” may need to be reassessed. I found the argument compelling. There are outstanding issues. Does unobserved heterogeneity mean we can never really know if h varies over the cycle—or if it only seems to—due to compositional effects? Do we want to think about the labor market in terms of two states, say, employed and not employed, or three? What should we make of the time-aggregation problems, and how can we best correct for potential false job-to-job transitions as well as false job-to-unemployment-to-job transitions? As usual, reading a paper by Shimer made me think about many interesting things, including several that are not completely resolved in the paper. And although I see how this could be taken either way, in this case it is meant as a compliment.

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