

# Commentary: Labor Market Fluidity and Economic Performance

---

*Richard Rogerson*

Through a long series of contributions, Steven J. Davis and John Haltiwanger have taught all of us a great deal about the connection between job and worker flows and aggregate economic activity. Their work is careful and thoughtful, and I always learn a lot from their papers. This paper is no exception. I would like to begin my discussion by providing a policy context for the current paper. Although the labor market has been recovering for several years, labor market outcomes still remain somewhat depressed relative to previous levels. A key question for policymakers is to assess what levels the labor market will ultimately reach during the recovery, or put somewhat differently, how far the labor market is from its ultimate position. One possible answer is provided by looking to the past and assuming that the labor market will eventually recover to pre-Great Recession levels. But one concern with this approach is the possibility that there have been various structural changes in the labor market that have changed the trend level of labor market activity, implying that previous levels of activity are no longer informative in this regard. Researchers have suggested several candidates for the list of factors that might have had substantial negative effects on trend labor market outcomes. For example, David Autor, in work with various co-authors, has argued that two factors on this list are technical change

and globalization. The paper by Davis and Haltiwanger documents that the U.S. labor market has experienced a trend decline in “fluidity” and its objective is to assess whether this decrease in fluidity, or loss in dynamism, might also deserve to be on the list of candidate factors, and if so, how high up on the list should it be.

Before commenting on the specific results in the paper, I think it is important to take a step back and discuss what the basic economic story is for why a trend decline in labor market fluidity would lead us to expect a trend decline in labor market outcomes such as employment and participation. There is in fact a fairly long literature that has been concerned with the relation between labor market dynamics and aggregate outcomes, mostly focused on patterns across countries. In fact, the term “Eurosclerosis” was coined in the context of the possibility that lower fluidity in European labor markets relative to the United States, caused by labor market regulations that made it more costly for firms to decrease the size of their workforce, were an important driving force behind the relatively poor aggregate labor market outcomes in Europe compared to the United States. My own view of the findings in this literature is that the most definitive impact of lower fluidity is to lower productivity, with ambiguous but limited impacts on employment and participation. The intuition for the productivity impact is clear: in a changing world, productivity grows unevenly across sectors and even firms within a sector. A well-functioning economy needs to continually reallocate resources toward those firms that are becoming relatively more productive, and this process of reallocation is an important contributor to overall productivity growth. Research in the last two decades has made it clear that this is a quantitatively important process. Less dynamism in the labor market impedes this reallocation and hence lowers productivity.

This same literature has long noted that less turnover in the labor market has opposing effects on employment (or unemployment). On one hand, a less dynamic labor market means that it is harder for workers without jobs to find a job, thereby tending to decrease employment and increase unemployment. But on the other hand, lower turnover means that fewer workers are thrown into unemployment, thereby tending to increase employment and decrease

unemployment. In fact, a well-known paper by Blanchard and Portugal (2003) studied Portugal and the United States, two of the most extreme cases in the OECD in terms of low and high labor market fluidity, and found that despite the dramatic differences in labor market flows, both economies had very similar unemployment rates.

Against this backdrop, Davis and Haltiwanger suggest that there are plausible stories for why lower labor market fluidity might lead to some combination of lower employment, higher unemployment and lower participation. One such story is contained in the paper by Shimer (2001). He argues that a higher share of young people in the labor market decreases the effective recruiting costs of firms since there are more workers looking for jobs that are a better match to their skills, and offers evidence in support of this mechanism. Davis and Haltiwanger are reluctant to commit to this particular mechanism, and sketch out an additional possibility involving human capital accumulation and marginal workers. But my reading of their paper suggests that their view at this point is that an intelligent person can come up with plausible stories for why labor market fluidity might impact aggregate employment, and that the main issue at this point is to see what the data have to say about this effect, leaving to a later date the issue of figuring out through what economic channels this effect might be working.

Uncovering the effect of labor market fluidity on aggregate employment poses many challenges. First, there is a problem of reverse causation: very basic intuition tells us that if employment growth is high we would expect to see a lot of turnover in the labor market since workers and firms need to find good matches for the newly created jobs.

Second, and as emphasized by the authors, many different underlying factors may be behind the decline in fluidity, and there is no reason to think that the effect of fluidity on aggregate employment is independent of what caused the change in fluidity. This means that we have to think more deeply about the underlying causes of the change in fluidity. The approach taken by the authors is to use instruments to isolate one particular component of the change in fluidity. An open issue is whether the demographic instruments that they use

are potentially picking up other effects that might directly influence overall economic activity.

A third challenge concerns where to look for the desired evidence. Because the authors are interested in the aggregate, or macroeconomic, impact of trend changes in labor market fluidity, it seems reasonable to look at macroeconomic data. But using macroeconomic data from a single country to infer the effect of a variable with a smooth trend is exceedingly difficult, since there are invariably many other variables with similar trends and one cannot easily discern which of the trending variables is really responsible for any changes that one sees in the data. For this reason, Davis and Haltiwanger do not look to aggregate data for evidence, arguing that using cross-state variation is a promising path forward since there is substantial heterogeneity in the extent of trend changes in fluidity across states.

I am sympathetic to the view that there are severe limitations on the ability of aggregate data to isolate the desired effects. But before I discuss the specific results obtained from an analysis of the state-level data, I think it is of interest to take a quick look at the aggregate data. In particular, I want to suggest that a quick look at the aggregate data would reasonably lead one to be a bit skeptical about the impact of reduced fluidity. To see why, I want to imagine that we were having this discussion back in 1999. According to the results in the paper, the decline in labor market fluidity in the United States seems to start in the early 1980s, so that by the end of the 1990s we have been living with decreased fluidity for almost two decades. If the effects of decreased fluidity were particularly important, then we might reasonably expect that they would have surfaced by this point. But as of the end of the 1990s, the labor market has just witnessed the strongest 20 years of employment growth in the entire post-World War II period, and moreover is coming off of a very robust period of productivity growth. At a minimum, this would seem to make one somewhat skeptical of the hypothesis that the decrease in fluidity from the early 1980s to the late 1990s was very damaging to aggregate labor market performance. As noted above, none of this is definitive, since other factors may have been changing, which obscured

the effects of decreased fluidity. Or maybe the nature of the change in fluidity was different in the 1980s and 1990s than in the 2000s.

These issues aside, the empirical analysis using state-level data concludes that exogenous decreases in fluidity associated with changes in demographic structure lead to significant negative effects on employment. Taking the estimated effect from state-level variation and their instrumental variable procedure and applying it to the aggregate change in fluidity observed from 1998 to 2011, Davis and Haltiwanger find that they can account for basically the entire drop in employment over that period. That is, their estimates imply that given the observed decline in fluidity, the labor market in 2011 is exactly where it should be, and that we should not expect any additional recovery absent an improvement in fluidity.

This is a very strong result (perhaps too strong, as I discuss below). On the basis of this result, Davis and Haltiwanger would argue that decreased labor market fluidity definitely deserves to be high up on the list of factors that are affecting the trend level of aggregate employment. While I am persuaded that decreased fluidity should be added to the list of potential factors being considered, at this point I think it needs to be added with an asterisk; actually, with three asterisks.

The first asterisk has to do with the fact that the authors are estimating effects from cross-section variation across states, and then assuming that this can be used to infer aggregate effects. While the idea of using geographic variation to infer macroeconomic effects has recently become quite popular in many contexts, I think it is very important to realize that this practice is not a panacea from the perspective of inferring aggregate effects. Let me give a simple example to illustrate. Suppose that a state like Texas initiated some change that we agree is best interpreted as exogenous, and that this change represents an improvement in the business climate in Texas. Suppose we use the state-level data to infer the effect that this has on employment in Texas relative to other states, and detect a positive effect.

We simply cannot assume that this estimate would correctly predict what would happen to aggregate employment in the United States if all states were to adopt the same change. The reason for this

is that when the business climate improves in Texas relative to other states, part of the increased employment in Texas relative to other states may simply reflect the fact that some economic activity has shifted from other states to Texas. That is, the estimates may be picking up a reallocation of economic activity from one area to another and not an increase in overall activity. In order to predict what would happen to aggregate employment if all states were to adopt the same change, we need to know how much new economic activity was created by the change in Texas, as opposed to reallocation of existing activity. Note that the reallocation of activity from other states into Texas need not take the form of businesses that literally move from other states into Texas. It may take the subtler form of new businesses or business expansions being more concentrated in Texas than would have previously been the case. For example, corporations like Wal-Mart Stores Inc. or Starbucks Corp. may choose to open fewer new stores outside of Texas and more stores inside of Texas relative to previous plans. The bottom line from this is that estimates from cross-state variation do not necessarily provide the appropriate information needed to assess aggregate effects.

The second and third asterisks have to do with how the authors derive the overall aggregate effect of the decline in fluidity, even if we assume that the estimate from state-level data is appropriate for this purpose. There are two different issues here. First, as noted above, there are potentially many different factors contributing to the decrease in fluidity, some of which may even have opposing effects on aggregate employment. The strategy used by the authors is to use an instrument to isolate one component of the change. When they go on to gauge the potential size of the aggregate effects implied by their estimate, they apply the estimate derived from the one component isolated by their instruments to the entire change in aggregate fluidity observed in the data. But there is no rationale for why the estimate obtained using the instrument would apply to the entire observed change in fluidity. Since their instrument picks up about 10 percent of the cross-state variation in changes in fluidity, we might conservatively think that their estimated effect might apply to as little as one-tenth of the overall change in aggregate fluidity. This would decrease the importance of this factor by an order of

magnitude and hence is potentially very important to assessing the overall importance of this effect.

Second, when the authors ask how large of a change in aggregate employment they can explain taking their estimated effect at face value, they apply it to the change in fluidity from 1998 to 2012. But we know that at least part of the drop in their measure of fluidity between 2007 and 2011 is due to the decrease in employment that was part of the Great Recession, since as I noted earlier we know that lower economic activity will necessarily lead to lower fluidity. I think the spirit of their exercise is to tell us what we should have expected for the aggregate level of employment even in the absence of the Great Recession, and for this reason they should be basing this calculation on the extent of the trend change in fluidity that was occurring prior to the Great Recession. This would also diminish the size of their implied effects.

Before concluding, I would like to offer an alternative perspective on how fluidity might matter for the aggregate economy, one that will be very reminiscent of arguments made during the 1994 Jackson Hole conference. At that time, the discussion was focused on understanding why labor market performance was so much worse in many European countries than it was in the United States. A simple idea was that European labor markets were less fluid than the U.S. labor market due to a variety of labor market policies, institutional features and regulations, and this lower fluidity was leading to poorer aggregate performance, especially in terms of labor market outcomes. But it was noted that this argument was too simplistic—after all, many European countries had these same types of policies in place back in the 1960s, and at that time European labor market outcomes seemed to perhaps even dominate those found in the United States. So it could not be that whenever you have policies that lead to “rigid” labor markets you should expect to see bad labor market outcomes.

As a result of this argument, Krugman (1994) offered a more subtle explanation that linked labor market rigidities to aggregate outcomes. Specifically, he argued that rigid labor markets might be important in influencing how an economy responds to certain types of shocks. At the time, he argued that all economies were being exposed to

changes in technology that were lowering the marginal product of less-skilled labor relative to more-skilled labor. In the United States, with its flexible labor market, this shock led to higher inequality but no long-term increase in unemployment. In Europe, in contrast, labor market rigidities did not permit relative wages to adjust and the result was a long-run increase in unemployment. This “shocks and institutions” view was subsequently pursued by many researchers, with two notable examples being Ljungqvist and Sargent (1998) and Blanchard and Wolfers (2000).

By analogy, one might conjecture that the reduced fluidity of the U.S. labor market is not that big of a deal during “normal” times, but that it matters a lot for how the economy responds to the types of large shocks that it experienced during the Great Recession. That is, dynamism might matter most for determining how quickly the economy responds to adverse shocks. If this is the case, then the decrease in fluidity might not influence the level to which the economy will recover, but it might produce a very slow and drawn out recovery. An open question in this setting is what kinds of policies can effectively compensate for the lack of dynamism and potentially accelerate the pace of recovery.

In summary, Davis and Haltiwanger have clearly documented a decline in the fluidity of the U.S. labor market, a decline that has already lasted for roughly three decades. They provide evidence based on cross-state regression analysis to suggest that this could have important negative consequences for aggregate employment levels in the U.S. economy. While I think there are several caveats that need to be noted, I think this is an important finding and one that merits additional research to better understand the underlying economics at work. I think it is of particular interest to explore the implications of decreased fluidity on wages and productivity.

## References

- Blanchard, Olivier, and Pedro Portugal. 2001. "What Hides Behind an Unemployment Rate: Comparing Portuguese and U.S. Labor Markets," *American Economic Review*, vol. 91, pp. 187-207.
- \_\_\_\_\_, and Justin Wolfers. 2000. "The Role of Shocks and Institutions in the Rise of European Unemployment: The Aggregate Evidence," *Economic Journal*, vol. 110, pp. 1-33.
- Krugman, Paul. 1994. "Past and Prospective Causes of High Unemployment," Federal Reserve Bank of Kansas City, *Economic Review*, vol. 79, no. 4, pp. 23-43.
- Ljungqvist Lars, and Thomas Sargent. 1998. "The European Unemployment Dilemma," *Journal of Political Economy*, vol. 106, pp. 514-550.
- Shimer, Robert. 2001. "The Impact of Young Workers on the Aggregate Labor Market," *Quarterly Journal of Economics*, vol. 116, pp. 969-1007.

