

**Report on:**  
**Global Factors, Unemployment, Adjustment and the Natural Rate**  
**MS # 114**

My general impression from this paper is very positive: The paper looks at a highly relevant question, and adds very interesting new evidence. But I think that the authors run the danger of trying to pack too much material into this paper. The paper is incredibly dense to read, and sometimes, the transitions from one section to the next, and within the sections, are rough. For example, I think that it is too much to put section 3 on the adjustment of unemployment, and section 4 on the Phillips curve estimates into the paper. It is not clear to me how the two sections relate to each other conceptually. Also, the derivation of the Phillips curve comes a little bit out of the blue for a non-specialist. Thus, my suggestion is to either drop section 3 or section 4. I vote for dropping section 4, since section 3 is more accessible to a wide audience. I think this paper makes an excellent contribution to the literature by using only the results from sections 1 to 3.

Instead, I encourage the authors to address some issues in the remaining sections. I have two main comments on sections 1 to 3: First, it may be worthwhile exploring alternative interpretations of the common component to unemployment. While the authors emphasize the labor demand side as the common component of unemployment, a second candidate is the supply side, an issue that is also neglected in Nickell-and-friends literature. For example, common components in demographics, particularly a common component in the working-age population, could also lead to a common component in unemployment. Similarly to the common component found in unemployment, you'd expect this component to move slowly over time. It would be good if this interpretation could be ruled out. Probably the share of the working-age population moves too slowly over time to correlate much with the common component in unemployment, and would not track the common component of unemployment as nicely as investment. In particular, it may be hard to track the uptick in the late 80s, and the subsequent fall in the early 90s as well as investment does. Yet, even so, I think it would strengthen the paper if the authors made these points explicitly by showing this evidence.

My second comment relates to the analysis of the adjustment process in unemployment. I think it would be more consistent with the interpretation of the authors to perform the analysis of the unemployment adjustment by using  $u_{it}^* = a + bf_{it}^I$ , where  $f_{it}^I$  is the common factor from the investment shares. In fact, I think it's incorrect to use the common component to unemployment  $f_{it}^U$  as it is done in the paper, because that component is affected by *all* contemporaneous unemployment rates, on which it is subsequently regressed. Obviously, this has to correlate. While this problem is alleviated by running country-by-country regressions, the conceptual problem remains that there has to be a correlation by construction. Using  $f_{it}^I$  instead of  $f_{it}^U$  solves this problem.

Some minor issues that I think would warrant more discussion, in no particular order:

- The authors briefly mention that the first principal component of unemployment loads more weakly onto US unemployment than any other unemployment rate. I think it would be nice to have a brief discussion about this, as the US is an important economy to consider. At first, I thought this was an indication that the authors are mainly capturing

a European unemployment effect. However, this does not appear to be true, as other non-european countries like Japan, Canada, and Australia also have high coefficients on the common component. This would suggest that labor demand in the US is less sensitive to the expected returns of investment. Does this correspond to what we know otherwise about the sensitivity of employment in the US? This may also relate to your observation on the US on p. 13.

- Since the analysis of dynamics always is to some extent driven by the data, there is an issue arising from multiple statistical tests without having a strong hypothesis. It might be useful to err on the side of caution and adjust the critical values of the hypothesis tests for this, e.g., using the Holm (1979) procedure. However, this last point only applies to the dynamics, and to the extent that the authors use the argument that a significant coefficient on  $\Delta f_{it}$  is also evidence that the global factor affects unemployment (p. 10).
- There should also be a discussion on the issue of how to calculate the standard errors. Positive serial correlation is notorious in these kinds of applications, and this leads to a downward bias in the standard errors (Bertrand et al., 2004). It is probably not feasible to correct for this in this context, but I think it warrants a discussion.

## References

- Marianne Bertrand, Esther Duflo, and Sendhil Mullainathan. How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics*, 119(1):249–275, February 2004.
- Sven Holm. A simple sequentially rejective multiple test procedure. *Scandinavian Journal of Statistics*, 6:65 – 70, 1979.