

Answers to Referee 1

The reviewer made several points. We quote them in bold typeface before we answer.

One of the authors (Johannes Ludsteck) has a paper that seems very related to the current paper, but is not cited. This is unacceptable. The current paper needs to highlight what it adds relative to the older paper.

The referred discussion paper (by Johannes Ludsteck and Harry Haupt) focuses on an aspect of Reder competition which is complementary to the one in this paper: Ludsteck & Haupt investigate the effects of up- and downgrading on individual wages. This paper investigates recruitment in occupations above or below the formal qualification of new hires. Therefore research question, samples and methods (quantile regressions) are different. We will cite the Ludsteck & Haupt paper and comment on that.

The structure of the paper is a bit messy, and is sometimes difficult to follow. In order to focus the paper, why not first describe the results (regarding the hiring standard and the occupational composition wage) for Germany, and then compare them with the US findings. Here, it would be useful to include the findings for both countries into the same table.

We admit that we were not really happy with the structure of the paper and think that following the suggested structure of the reviewer would help us to improve the readability of the paper considerably. Until now the description of the econometric models is too long. We plan to shorten the section on the grouped probit model or move it to the appendix as it is highly confirmatory for the linear model results.

One proposed explanation for the lower responsiveness of hiring standards in Germany is the German apprenticeship system, which may lead to entry barriers into occupations for workers who did not complete an apprenticeship in that occupation. I like this explanation. However, I find the proposed test for this explanation (restricting the sample to apprenticeship occupations) very weak. I am not even sure the authors should present this test.

It is clear that the restriction of the sample to apprenticeship occupations cannot be expected to yield razor-sharp evidence. Nevertheless we think that the idea to use this restriction is straightforward and the results should be reported as – to the best of our knowledge – the available data do not contain information for more clear-cut tests. We suspect that concealing the result would introduce publication bias.¹

My biggest concern regarding the empirical analysis is the noise in the education variable. As the authors point out, it may well be the case that firms classify a worker as low skilled if he is currently performing tasks that a low-skilled typically performs, although he is in fact a skilled worker. This in itself could explain the lower magnitudes in the German data. I am also concerned about simply dropping workers with missing education. This could produce a severe selection bias if the education variable is predominantly missing for less-skilled workers. The education variable could be much improved by using the longitudinal aspect of the data, and I strongly suggest the authors to repeat their analysis for a ‘cleaned-up’ education variable.

For a 2 percent sample of our data, there are imputed education variables (see Fitzenberger et al., 2006). Our experience with these variables suggests a rather low impact of the cleaning procedure on outcomes of econometric models. However, the reviewer demonstrates convincingly that the impact of missing values on our results may be exceptionally high in our application. We suggest to clean up the variable exploiting the longitudinal aspect of the data. Although the Fitzenberger et al. algorithms are not completely applicable to our data set (it contains only one observation per year), we will try to adapt it as far as possible and re-estimate all our results with the corrected and imputed version of the variable.

The data section could be much shortened. It contains quite a few not too important details that could be moved to an appendix or dropped entirely.

We tried to describe the data in detail since they are not well known to the public and some details are important for comparison with the U.S.

¹With publication bias we mean that only confirmative, significant and ‘welcome’ results are published. This will create overconfidence in empirical results among the scientific audience.

results. To shift the description to the appendix and to shorten it is a good idea.

Regarding the result section: Since the goal of the paper is to replicate Devereuxs analysis for Germany, the authors should use the exact same methods as Devereux whenever possible. I would therefore focus on the linear model in Section 4, and considerably shorten the discussion about the grouped probit model. Since the grouped probit model has some advantages over the linear model, the authors may also report these findings. But the main point should be that the linear model is a good approximation. The same goes for Section 4.

We fully agree. All our experience from comparisons of discrete response models with OLS suggest that OLS provides excellent approximations and may even be superior in many cases as the discrete response models are based on additional non-testable distributional assumptions. We will shift the grouped probit model results to the appendix.

I found the introduction quite difficult to read. It could be considerably shortened, focusing on what this paper actually does: a comparison of the responsiveness of hiring standards and occupational composition wages to the business cycle between the US and Germany, and what we learn from this analysis. The general motivation in the first two paragraphs of the introduction seems out of place.

We fully agree and again want to thank for the quite instructive and constructive suggestions.

Reference

- Fitzenberger, B., Osikominu, A., and Völter, R. (2006) Imputation Rules to Improve the Education Variable in the IAB Employment Subsample. Schmollers Jahrbuch, 126; 405-436.