

SUNY-Stony Brook
Department of Economics

Professor ??
Journal of ??

Dear Professor ??:

Enclosed please find the referee report of the paper “?????” This letter serves the purpose of an executive summary of the report, another file includes the report for the authors, and a third file includes the reviewer’s evaluation form.

The paper is well written and has clear objectives, it uses excellent data, and provides fairly clear results. However, there are a number of issues that are not considered in the detail necessary to make this a paper of broad interest to labor economists, and what is more problematic, it is unclear the question they answer was expected to have a controversial (non-trivial) answer to start with, meaning that the results are far from surprising. As a reader, in many instances I had the feeling the paper would be a better fit for a health journal. I have tried to provide some comments that might help this paper reach a broader audience.

The authors have access to an amazingly complete data set which is a random sample of administrative records of the Danish population, with employer-employee matches, and detailed health information and histories. This allows them to tackle the question of the health consequences of a job displacement event supposedly better than any previous researcher. They do so for males and females, and find that especially for males there is essentially no effect of a displacement event on health outcomes related to stress.

The very nature of the question is, however, problematic. The authors do not present enough evidence that shows that changes in health outcomes are to be expected as a result of job displacement, in fact in the conclusions they mention some studies with results that are not very different from theirs. As they also acknowledge, this is especially problematic in Denmark, a country that during the sample period had one of the most generous, if not the most generous, unemployment insurance system in the world, and a fairly vibrant economy where people found jobs very quickly after losing them. It is not enough to argue that the replacement rates in Denmark are not that high for the average or median worker, the issue is that

those displaced workers are likely to have much higher replacement rates than the average worker.

It is true that the authors try to control, in part, for the unemployment history after the displacement, but they do in a very weak way. I am not convinced that is enough to consider those that had some unemployment spell after the displacement. In fact, I do not see almost any point in answering the question for those that find a job right away, say within 3 to 6 months, or less. I think the main results should be shown only for those with truly significant unemployment spells of more than 6 months. This is especially important if the authors analyze a health outcome that can happen many years after the event. I understand this might be a much smaller sample, but otherwise I do not see the interest of the overall results. Also, just taking into account the unemployment spell might not be enough. What about the conditions of employment after the spell. It could be that some workers, in a healthy economy, might be finding better and better jobs, in a process of job matching, in this environment there is no reason to believe we are going to find deteriorating health among once displaced workers.

The authors should also provide good summary statistics, and maybe even some figures, of the employment histories after the displacement event, this is very important, not only because of the point I made above, but also to get a sense of how many of them face repeated displacement events. The authors choose to ignore this important source of information, and I find that very problematic. The propensity scores to match treatment and control groups are a simple probit, when they have access to a data set in which they can at least control for a person specific unobserved components, which is likely to be very important for the question at hand. The authors themselves argue in the conclusions that Denmark is an economy with considerable turnover, it is easy to imagine that this turnover is likely to affect some groups of the population much more than others, and these groups might have some unobserved characteristics worth modelling in order to disentangle the sources of their health outcomes. This is not difficult to do and can have a potentially significant effect in the results presented. Taking into account the panel nature of the displacement event can also allow them to exploit the possible variation in the level of generosity of the system or the economic conditions, while controlling for individual specific components.

Another problematic issue is the choice of health outcomes to analyze. I understand the authors can not make everyone happy and have to choose a set of variables to use, and I agree that given the nature of the data they have the chosen variables are reasonable, as a starting point. However, by the very objective of making these results as general as possible to other countries, and maybe to a broader literature, it might be of interest to actually pick other health measures, more widely used in the literature, which although more likely to be subject to measurement error, might provide a potentially different story to their findings.

For example, say they do the same calculations with a self-reported measure of health, or an objective measure not matched with the administrative data. Given arguments in line with the justification hypothesis, it might be that they do observe that those with unemployment spells of a significant length show a decline in their self-reported health. Then they can make an argument, which is extremely important for a much broader audience, that the use of those measures alone can mislead,

in some circumstances, empirical researchers. I am a firm believer that those self-reported measures are very useful and very good if used appropriately, but I would not be surprised that if compared with proven diagnostic measures they show some (maybe small but significant) biases.

Overall, I think the paper needs some more work to convince a critical reader of the interest and nature of the question (what is the mechanism at play that can lead to health deteriorations after a displacement, and how does it relate to the length of the unemployment spell and the kind of work they find after the displacement) at hand and value and robustness of the results. The paper provides too many figures, and some of them seem to be quite repetitive. I understand they try to be thorough, but sometimes it comes across as tedious. I am on the fence regarding whether providing results for males and females is even useful, given the noisiness of the results for women. Maybe this should be investigated further. I conjecture this has to do with the potentially more sketchy employment histories of women, and the higher known incidence of some conditions related to depression among women. A more careful treatment of unobserved heterogeneity and employment histories might take care of some of these open issues.

Thank you for giving me the opportunity to be a referee for Journal,

Sincerely,

H.