Reviewer's report

Title: Physicians' choices of dual practices and the effects on labor supply in public hospitals: Results from a register based study.

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Reviewer: Claire Cameron

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Statistical review of paper “Physicians’ choices of dual practices and the effects on labour supply in public hospitals: Results from a register based study” by Karl-Arne Johannessen and Terje P. Hagan

General Statement:
This looks like an interesting paper and a very interesting analysis and I am sure, to people in area of public health it will be of great relevance. My comments come from an applied statistical viewpoint and many questions I have about this paper may just be a matter if clarification.

The background information for this article seems to suggest it addresses the potential negative impact of dual work by physicians on the public health services of Norway. It also suggests (and I think this is more likely the case) that it looks into the characteristics of the physicians (and their working lives) who undertake dual work. I think it would be helpful to describe explicitly what the aims of the study are.

I am concerned that the aims of the study have been lost in the complicated analysis and description of the statistics. I would have thought that a question that has public health relevance should be answered in a way that people in the public health sector can read and understand. I do not think that is the case with the way this paper is presented, currently. I have a strong background in statistics and I find it hard to get a clear picture of what these authors have actually done and what their rationale is for their statistical decisions.

Major Compulsory Revisions:
1. I cannot understand what you mean when you describe the probability of having dual work 'ProbDual'. The reason I think it is important to get this definition clear is because it underpins the modelling and the inference that you later do. It is defined in paragraph three of Materials and Methods as ‘the odds ratio for scoring 1 on a dichotomous variable in logistic regression (0 = not engaging in dual work, 1 = having dual work)’. Is it the odds ratio or is it the logit? If it is the odds ratio, it cannot be put directly into the GEE as the outcome. If it is either the odds ratio or the logit, it is not measuring the probability of having dual work.
2. It is not clear to me what the benefit is of using the two different subsets of the
3. The description of the data (paragraph two of Materials and Methods) says ‘we used two samples extracted from a population of 18,888 physicians’. Do you mean that the combined registers recorded 18,888 physicians and that two different subsets of the data were used for the analyses? Is every physician in Norway present on these registers? I think it would be beneficial to clarify this statement and to give some idea of any potential bias (if any) that may exist from these data sources.

4. There are three types of modelling employed. It is essential that the authors describe clearly why it was necessary to use these three types of models to answer the questions under consideration. It seems, on the face of it, to be overly complicated and I have found it hard to follow what exactly the authors are doing and why.

5. Life-cycle models come from the economics literature (so not my area of expertise). In everyday terms, what is meant by ‘where the actors maximize the lifetime sum of discounted utility derived from consumption, leisure and some individual attributes.’ (paragraph eight of Materials and Methods)? I do not see the need for the specification of these models in terms of mathematical formula when this has not been done for the other types of models, and the information contained in those formulae are not used elsewhere in the paper. If you must include those formulae, you need to say what the subscripts denote (as well as the individual terms). It is not clear to me how these models address the research question (which has not been clearly stated).

6. The mixed models are described in the paper as ‘a mixed model approach with fixed effects’ (paragraph nine of Materials and Methods). The outcome is 'PublHours'. Are there any random effects in this model? If so, it should be stated what they are. I imagine they could be either physician or hospital. If there is no random effect, there is no need to fit a mixed model. What data are these models applied to and why? I know the tables imply that it is to both the balanced and unbalanced panel data, but it would be good to have more information about that written in the text. There is also little opportunity to see what these models are. That is, what terms exactly are included and why? I notice, in Table 3 where, I presume, you are reporting on the mixed models there are interaction terms listed. There should be information provided on how the decision was made to include or not include the interaction terms in these models? References should be provided for this methodology.

7. The description of the GEE part of the analysis in paragraph ten of Materials and Methods is ‘The probability of engaging in dual work (ProbDual) was analyzed using Generalized Estimating Equations (GEE) with logistic regression’. I am not sure why these are used in preference to mixed models when mixed models can also be set up to have a dichotomous dependent variable and they can account for correlations between observations. I wonder if it is possible that GEEs handle the missing observations differently to mixed models. I imagine with the balanced data this would not be an issue. Is the random effect, in this case, individual physician? The comment that you can allow for correlations between individuals suggests that this is the case. As with the mixed models, you...
need to explain how you decided to include or not include your interaction terms (Table 2). Again, you should include references for this method?

8. The only results you report (which, presumably are the most important ones) in the Abstract are those on dual work and what factors impact on the probability of engaging on dual work. There is no mention on work hours or the different subsets of the data – it makes me wonder if you need those separate analyses.

9. I think this article would benefit from the collaboration of a biostatistician.

Minor Essential Revisions:

1. I don’t understand how that 14,033 works in terms of the longitudinal data. In the manuscript you say ‘ranging from 6,820 in 2001 to 10,041 in 2009’ (paragraph two of Materials and Methods). It might be helpful to describe somewhere what the numbers are for each year.

2. Results Section: It is hard to follow all the results within the 6 figures and 3 tables. I can’t help but think that the amount of information could be reduced.

3. I don’t think it is helpful to list the variable names in the tables. The information there should stand alone without it being necessary to refer back to the text. You should say what the dependent variable is in each case. Is it necessary to include maximums and minimums in Table 1? Some of the variables in this table would be better off with a median reported (like debt, for example, because it likely to be skewed). What is the mean of the ‘mean position fraction’? ‘Percent married’ is, presumably, not a mean. I think this table could be more clearly presented.

4. Table 2: I don’t understand the caption ‘Probability of having dual work by odds ratios…’. Are these probabilities or odds ratios? If they are probabilities, why are some greater than 1? If you have used a form of logistic regression, it is customary to report the odds ratios and their 95% confidence intervals. You should use your understanding of the context to inform what associations are of interest. Because your sample is so large, you are likely to have many significant results using p-values.

5. Table 3: What are these effects? You have described your data as balanced and unbalanced panel data. So when you say in the title ‘Estimates from panel data analyses’, couldn’t that be from any of the models? It is more useful to present effect sizes and their 95% confidence intervals rather than p-values (as mentioned earlier). I don’t understand at the bottom of the table you have listed some fixed effects. Aren’t all the variables in the table in the models? Wouldn’t all of them be fixed effects? I am very confused on that point.

6. In paragraph 5 of the results you talk about probabilities, but then you switch to odds and odds ratios in the next paragraph. This is confusing.

7. Paragraph eight of the results. Are you talking about the life-cycle models or the mixed models? If you are not using a random coefficient model, presumably you are using a random intercept model (if it is a mixed model). By ‘lagged dependent variables’, do you mean that you are including baseline measures of 'PublHours', or all previous years values? If these are the life cycle models, perhaps you should define t (from the model, earlier described). In that case, the
lagged variable is just the previous time period (presumably year).

8. There is a lot of jargon used where I would prefer plain English descriptions. For example, ‘negative predictors’ and ‘lagged variables’. When it comes to discussing your results and your finding, you need to use language that non-statisticians (and non-health economists) can understand.

Discretionary Revisions:

1. I notice at the end of the section called Background there appears to be an incomplete sentence (‘effects of dual practices on the labor supply in public hospitals?’)

2. In paragraph four of Materials and Methods, you describe the hourly wage – is that 'NetCapInc'? It does not appear to be named there.

3. Figure 3 seems to me to be about debt (given its place in the text) but the legend says it is about mean working hours. I think this is a mistake.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests