Reviewer’s report

Title: Factors associated with tocolytic hospitalizations in Taiwan: evidence from a population-based and longitudinal study from 1997 to 2004

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Reviewer: Ciaran S Phibbs

Reviewer’s report:

This is an interesting study that exploits a highly integrated set of data from Taiwan and some natural changes to make inferences about physician demand inducement. I have some comments and suggestions that I think will make this a clearer and more important manuscript. Basically, the authors have a really great dataset, and there is some useful information that they have not presented. The current manuscript isn’t that long, so the editor may allow the extra space to present these data. I also recognize that the authors may have a separate manuscript in preparation that addresses the issues of additional analysis that I suggest. If so, that is fine, but they should provide a copy of this manuscript in their resubmission. I have no comments on the methods.

1. The authors have excluded many of the conditions that are associated with high-risk deliveries, including multiple births. While I can understand why they did this, it also makes it hard to put many of the findings that they report in context. Since it is clear that the authors have the data, at a minimum, they need to include some descriptive statistics about this excluded population, and the resulting overall statistics. Specifically, on Table 1 they report the number of antenatal and tocolytic hospitalizations for the study sample, and note that these are lower than what has been reported previously. This would be expected, given that they have excluded many of the cases most likely to need these hospitalizations. Thus, I think that they should report these rates for the excluded cases, and also for the entire population. This would make it much easier to understand the authors data and compare them with other countries. Related to this, I think that the authors should report the rates of prematurity and extreme prematurity (or low and very low birth weight). Given the potential for data problems with gestational age, LBW and VLBW may be the more useful numbers. This would be most useful if they reported this for the aggregate populations, and then separately for those who did and did not receive tocolysis. Another bit of information that would be useful would be to look at how many of the cases with tocolytic therapy have a diagnosis of preterm labor. This should be 100%. Since there is some gray area in the diagnosis of preterm labor, this doesn’t mean the demand inducement isn’t occurring.

2. Since they have the data, an interesting addition would be to look at the percentage of the tocolytic hospitalizations where glucocorticoids were also administered. In cases of preterm labor, for at least the first hospitalization, glucocorticoids should also be administered.
3. Something that isn’t clear and needs to be clarified is the difference between antenatal hospitalizations and tocolytic hospitalizations. I read the manuscript as if these were mutually exclusive, but I am not sure. If so, this seems a bit odd. If a woman is admitted in preterm labor, standard therapy is to use tocolysis. Thus, I would think that there is a high degree of overlap in these hospitalizations. If I am wrong, and these hospitalizations do overlap, then the authors need to add information about the extent of overlap.

4. I think that there are issues with selection bias in several of the authors control variables. For example, there is good evidence that high-risk deliveries have much better outcomes when they are transferred to a hospital with high-volume, tertiary-level, obstetric and neonatal services. Thus, one would expect that larger, higher level hospitals with have higher use of tocolytic therapy. This should be acknowledged in the discussion. I do not think that the authors need to address this in their analysis. If they want to look at it, distance to the provider, or differential distance between types of providers, is probably a good instrument.

5. One thing that would be an interesting addition, would be to compare the model the authors estimated with the same model, estimated on the excluded cases. Given the much higher risk profile of the excluded case, one would expect a higher rate of medical indication. Thus, my prior is that the parameter estimate for logger regional fertility rate would be smaller. This is just an interesting potential extension. I my opinion, this would make the manuscript more interesting. But, I don’t think that the authors should be required to undertake this extension.

Minor comments.

6. In the Introduction, I think that the authors should make clearer the issue about the effectiveness of tocolytic therapy. They do indicate that the evidence is mixed. This is probably a bit generous and some additional caution is probably warranted. But, they also need to add that there is no therapy that is proven to be effective at preventing preterm labor. Tocolysis is used because it is the therapy that has the best evidence to support any effectiveness at all. Given the very serious consequences of extremely preterm labor, it is almost universally agreed that it should be tried for cases <32 weeks gestation. As a related point, the evidence is much stronger that tocolysis can delay labor a little bit, and this is very valuable if this time is used to administer glucocorticoids and give them a chance to work, as there is very strong evidence that glucocorticoid therapy speeds lung maturity and has very positive effects on many different neonatal outcomes.

7. Methods. The authors do not mention that they did a full set of regression diagnostics. Given that Edward Norton was involved in the analysis, I am sure that these were conducted. They should just explicitly state that they have do so.

8. Page 10, bottom. When the authors discuss the potential effects of IVF/ART, they should also note that these technologies are associated with a much higher risk of multiples and other high-risk conditions.
Level of interest: An article of importance in its field

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.

For full disclosure, since it is public knowledge, I am chairman of the Leapfrog Group's Evidence Based Hospital Referral program neonatal committee. This committee would have an interest in the findings. But, our deliberations our based on published scientific evidence. Further, the topic of this manuscript is only marginally related to the focus of this committee.