Reviewer's report

Title: The influences of Taiwan's National Health Insurance on women's utilization of prenatal care: Investigation of differences between rural and non-rural areas

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Reviewer: Embry Howell

Reviewer's report:

General

This is an interesting article about an important topic. It is important to have an international perspective on how financing affects prenatal care. However, there are some issues with the design of the study and measurement that should be addressed before it is published (see below).

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. Measurement:

I have two problems with the measurement approach, the most important being recall. If I understand the design correctly, you asked women in 2000 about their prenatal care experience for earlier pregnancies going all the way back to 1990. At the most, this would be a 10 year recall period. This is a very long time for a person to remember something even moderately detailed, and especially very detailed things such as whether they followed the prenatal care recommendations of their doctor. I do not think these data would be valid.

I also have concerns about the concepts for two of your measures: “Regular use of prenatal care”—which is defined in Table 3 and the text as being whether the woman followed her provider’s recommended number of prenatal visits—and “Continuity in seeking prenatal care from one health care facility”—which is defined as receiving all prenatal care from a single provider. Regarding the first, in addition to major issues with recall discussed above, I imagine that there may be different standards of care around the country, and particularly between rural and urban providers and different types of prenatal care providers (Ob/gyn vs. family doctors vs. midwives). A woman may remember that she followed her doctor’s advice exactly (indicating full prenatal care), but that advice may have differed substantially from another woman’s advice. You would want the care to be compared to a uniform standard across the country. Did you ask how many prenatal visits they had, or whether they initiated care early in their pregnancy? Those would be better measures.

In terms of the continuity measure, it may be appropriate for a woman to receive care from multiple providers, especially in the case of a high-risk pregnancy. I would think this might happen more often in rural areas, where a person is primarily followed in a clinic but went to a specialty provider for some of the care. So I am worried about the urban/rural comparison for this measure.

I would be reluctant to present the results for these two measures (especially the first). I think you would still have an interesting paper with the last two measures (which would show a shift in types of prenatal providers and some access issues—the main points of your paper). If the data are presented, the exact wording of the question should be in the article.

2. Design:

I was a bit confused about the design. You say that you excluded all women who had more than two records in the data base (presumably women who had two or more children born between 1990 and 2000). How then did you obtain data for the children born before 1995? Please clarify exactly how you obtained records for the early period of your study.

Why did you exclude births in 2000? Your explanation did not make sense to me (that you wanted the study to focus on the 1990s); it would seem that the more data the better.

Table 1 shows that your comparison group (women who delivered before the intervention) is very different
from your study group, according to their age. This probably means they have other differences such as parity, income, education, and many other factors that are harder to measure that affect prenatal care use. You cannot control for those differences in the models, because you do not have the data for the earlier time period.

I believe you ruled out what would be a superior design for the study (which would overcome problems with the differences in your comparison groups). If you have sufficient sample size to do it, a better design would be to select only women who gave birth before the intervention (health insurance expansion) and after it. They serve as their own comparison group. You then compare their prenatal care experience before and after the new insurance program. It would be best to include only women affected by the intervention. This design was used in a study of the impact of Medicaid pregnant women in Ohio (Embry Howell, Health Services Research, vol. 39, no. 4, August 2004), and it worked well.

3. Findings:

a. It was surprising to me that variables typically associated with prenatal care use did not appear to be related in your study, including marital status, education, and income. The same was true of continuity of care (and women with no abnormal conditions had significantly “better” continuity—see comment above). This is probably because of the measurement and design issues mentioned above.

b. Why are the findings for the years of the study so different? It appears that the first year after the intervention there was no effect on any of the outcomes, and then in 1998-99 there was an effect for both the type of health care facility and the convenience of transportation. Can you give some information on how the insurance program worked that would have such an effect (shifting rural women from the local prenatal care clinics to larger providers)? Perhaps the insurance does not cover such providers, or they were shut down for some reason? Or do women prefer to go to larger providers, and if so why? This seems to be the most robust finding from your paper, so it requires some more explanation. Since it did not occur right away, perhaps there was a change a year after the insurance went into effect?

4. Abstract and Discussion:

I certainly would not agree with some of the points in the abstract and discussion, as follows: “…the two groups differed in how regularly they used the prenatal care services, and the introduction of NHI did not reduce these differences;” “We conclude that the NHI did not substantially reduce disparity in regular use of prenatal care services between rural and non-rural areas, and it did not improve continuity…”; “and “…we found that the NHI did not bring with it an increased likelihood that Taiwanese women would seek prenatal care more regularly.” While these may be true, I do not think your paper demonstrates them well.

You introduce a large number of tables in the Discussion section. They belong in the Results section and should only be included if they are clearly related to the points of the paper. I did not think that Tables 9-11 were particularly pertinent, and could just be mentioned in the text with a reference to the data source for those interested.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. There are some small grammatical errors throughout.

2. Awkward wording in the abstract: “…how women utilized prenatal care for their children…”

3. I found the presentation of statistics in Table 4 (the “DD” and “Diff” statistics) confusing. It would be less confusing just to present the actual percentages (columns 2-5 and 9-12).

4. What are the “Thresholds” in Table 8?

5. The “Huber/White/sandwich estimator” was unfamiliar to me. Please provide a brief explanation and a citation.

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Discretionary Revisions (which the author can choose to ignore)
1. In addition to the age differences between your comparison and study group, the cohort of children born in 1993-1995 appears to be different. There is a higher proportion of males, and more children were born in rural areas. Is there some explanation for this? There are more boys than girls in each year, after 1993. Is there an explanation for that?

2. There could be a problem with the estimates for the interaction terms. (See the article by Chunrong Ai and Edward Norton in Economic Letters, 2003.)

**What next?:** Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests.