Reviewer's report

**Title:** Health behaviour modelling for prenatal diagnosis in Australia: A geodemographic framework for health service utilisation and policy development

**Version:** 3  **Date:** 13 May 2006

**Reviewer:** Jane McElroy

**Reviewer's report:**

General
I think this is an interesting way of teasing out a health issue based on data collected for entirely different reasons. The paper could be quite an interesting read if the methods were more transparent and the reasoning behind the analytic plan more transparent, and the magnitude of difference between different segment understandable. Naturally (as will be apparent from the comments below), I also feel calculating the CI for these results would dramatically improve the paper such that meaningful comments could be made about the difference among the segments.

Responses by the authors from the first review
I read the paper without looking at the response to reviewers comments since I felt the paper had to stand on its own. After I finished the above comments, I looked over the authors comments and noticed that some of my comments were repeats from the last review. When I read the authors responses to comments, some of these were addressed in these comments but not in the paper. So if I were a new reader and read this paper without the attached comments from the author, I would be frustrated with the description of the study.

I am puzzled as to why some of the concerns or questions I had were not addressed in the text but rather provided in a note from the authors. I understand that some need not be part of the paper since they reflect my lack of familiarity with Australian geographic units and terminology and that might be somewhat unique. But others were appropriate in the revision.

Opaque statistics
We have decided not to offer any form of statistical analysis for the comparison of data relating to the geodemographic segments makes the paper considerably weaker and the conclusion even less strong than I originally thought. There are statistical methods to calculate CI on these data so I am puzzled as to why the authors choose to not do so.

If the authors choose to not test these data in any rigorous way, then I think providing graphs that show apparent but maybe not real differences between the segments may be misrepresentative and should be eliminated to lessen misunderstanding about the results. As stated in the section responding to the abstract, lack of statewide geodemographic consistency in uptake of prenatal diagnosis can stand if we very very loosely define consistency to mean more or less exactly equal values. Of course, this phrase implies there is a meaningful difference in the different segments which absolutely is not supported in these analyses. Whether there are substantive differences or not remains to be explored.

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

Abstract: The conclusion speaks beyond the analysis. Making a concluding statement about the need for appropriate health service provision for families of children with Down Syndrome is fine but it was not what the paper was about uptake of prenatal diagnosis and prevalence of Downs Syndrome. The paper did not analyze data regarding the distribution of appropriate health service provisions for families with Down Syndrome.

The overall conclusion: lack of statewide geodemographic consistency in uptake of prenatal diagnosis can stand if we very very loosely define consistency to mean more or less exactly equal values. Of course, this phrase implies there is a meaningful difference in the different segments which absolutely is not supported in these analyses. Whether there are substantive differences or not remains to be explored.
Method

All women residing in Victoria who had an amnio or CVS in 1998 or 2002 and who expected date of delivery was also in that year were included in the study.

Please respond to the concern about inclusion criteria in the paper—"it is not sufficient to read the response to the concern in a note back to the reviewers. I suspect that if I have this confusion, there will be others.

I struggle with the terminology.

Denominator data were obtained from all recorded births (Down syndrome prevalence) or confinements (prenatal diagnosis) at the PDCU in 1998 and 2002.

Do the authors mean to say that the denominator in their calculations where they use this denominator data is DS prevalence—(the number of DS births in 1998/number of person (child bearing women?) in population in 1998) x 100. I am still not clear what the denominator is and it seems very odd that the denominator would contain a prevalence calculation so I think it may be just syntax issue that needs to be fixed.

I find it unacceptable that the geocoding method, particularly the match rates and sensitivity scores that provided a match was not described. The entire project rests on the premise that the geocodes were accurate and that the women/births were correctly assigned to the proper LGA. Without this information, the quality of the analysis cannot be fairly determined. For the US, postcodes change over time and I wonder if this is an issue in Australia and if so how this was dealt with in the geocodes. Further, it seems implausible that all the postcodes contained zero errors? How were obvious typos dealt with.

Some specifics on the geodemographic segment paragraph

This sentence lack precise description of what was done and needs to be clear to the reader. In the authors response, they note that they respond to this concern by saying a limitation is lack of street addresses. Of course that is a clear limitation; however, the authors analyzed the data and assigned geocodes somehow. The method that was used to do this needs to be explained.

If I took a stab at figuring out what the authors did, based on my experience with this problem, then I would say, the sum of number of births by postcode was determined from birth registry data for years 1998 and 2002. For postcodes in which their boundaries bisected one or more LGA, the number of births was allocated to each intersecting LGA based on proportion of block groups within each bisecting boundary, or it could be based on the proportion of each postcode population.

It could be based on the percent of area that bisected the LGA or it could be based on something like what US census uses proportion of block groups within each bisecting boundary, or it could be based on the proportion of each postcode population.

After reading the authors response to my concerns, it seems the authors divide the birth data, which is summed by postcode, as a proportion of the total population of the LGA? Correct? If so, this needs to be stated and it would be better to divide the data based on population of child bearing aged women. If postcode 3351 had 100 births, then Ararat LGA = 8 births, Ballarat LGA= 14 births, Golden Plains LGA = 61 births, and Pyrenees = 16 births from this postcode.

Please describe what was done in the manuscript.

Data analysis

Please describe the calculation to obtain the numbers in Table 1 for "graded household income." I would guess that the denominator is the income for the lowest category. If so what is that? What if the difference between 1.49 and 1.51? How much difference in $; is it $1000 or 10,000? With no data, the magnitude of the difference between the categories and between high medium and low categories cannot be determined. Please provide an example and if my guess was right the denominator $ amount used in the calculation.

Please also describe how the "Births (% of metro or rural) was calculated and what it means in the text. Again what is the denominator for these percentages, total number of births in the metro area? Again,
another definition is needed. How was rural and metro defined? By LGA or postcode at xx population or xx child bearing female population or what?

For modeled rates of prenatal diagnosis, was this calculated by age or by the two age groups: under 37 and 37+ years. Please specify in the text. Iâ€™m guessing that it was just calculated by the 2 age groups and if that is so a different answer might occur depending on the age structure of the segment. I would encourage the modeled rates to be based on age by 2-year intervals (as was done in Table 2) since the risk dramatically increases with each year. I donâ€™t know the distribution of the mothers ages by segment, so I donâ€™t have any idea how stable the numbers would be by segment if the data were calculated by 2 year groups. If the clumping is due to small numbers this needs to be noted in the manuscript.

Another important component of graphing these rates in providing confidence intervals. It is unclear whether there is any real difference between any of these valuesâ€”all depends on CI. Even within the urban or rural segments, it is unknown whether there is any real difference in any of the values. This picture without CI tells one storyâ€”that urban and rural are really different and there is limited variability within the two stratum--but I would not be surprised if very few if any of the segments were different from each other and that the strataums are similar.

Since the whole analysis revolves around the cutoff at maternal age of 37, it is not clear to me why the authors report an alternative cutoff point to report results of change in percent of maternal age except possibly due to a different cutoff point being statistically significant whereas the one used in the analysis, age 37 was not. Delete this sentence. When the advanced maternal age cut off was set at 35 years, increasing significantly to 46.7% in 2002 (p=0.04, not shown).

Table 2 doesnâ€™t match the methods as far as I can tell.

All women residing in Victoria who had an amnio or CVS in 1998 or 2002 and who expected date of delivery was also in that year were included in the study. This was in the methods section describing the study population. Where does 1997, 1999, 2000, 2001 come from? OK to include it but it needs to be described properly in the methods section.

Secondly, I donâ€™t understand why the authors are reporting different time periods for births vs DS births in Table 2. Why do they not report the same periods, particularly for all births.

The same comment about the need for CI for figure 1 and 2 also holds for figure 3 and 4. Once the CI are taken into consideration, then reporting the results might substantively change.

Since the CI of these analyses need to be taken into consideration before conclusions can be drawn, I cannot comment in any meaningful way to most of the discussion.

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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)

Background

2nd paragraph, last sentence: while second trimester maternal serum screening is available throughout Victoria, all pregnant women residing in rural regions have few options but to travel to a metropolitan centre to have a prenatal diagnostic testâ€

What does few optionsâ€ mean more precisely? Can rural women have a prenatal diagnostic test somewhere beside metropolitan centre? How far do rural women have to travel (average)? A½ hour to a location, 2 hours to a location? Although in the next paragraph, the authors somewhat dismiss this characteristicâ€”travel time, please be more precise in characterizing the situation since an attempt was made to describe travel time.

In the text, I would not bother to report p-values for the table 2 and 3 results. I donâ€™t feel it contributes in a substantive way and having it in the table is sufficient.

The y-axis on all figures need to be labeled (if these figures are kept)

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Discretionary Revisions (which the author can choose to ignore)
Second point in the conclusion from the abstract and manuscript that the authors may or may not agree with is in my opinion the tiresome call for more education as an action plan. In fact, implicit in the push for education is the assumption that lack of knowledge (or ignorance) is what is driving people’s decisions. As highly educated people, it is hard not to think this. "If they only knew..." However, I would argue that many variables comprise decision making and it isn’t as simple as educating people to affect behavior change.

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

Level of interest: An article of limited interest

Quality of written English: Acceptable

Statistical review: No