Reviewer’s report

Title: Parent-reported measures of child health and wellbeing in same-sex parent families: a cross-sectional survey

Version: 1 Date: 9 April 2014

Reviewer: Kirsten Hancock

Reviewer’s report:

This is a topic that receives a great deal of public attention and speculation with very little empirical research available to inform either public or academic discussion. This paper therefore has the potential to make a significant contribution to the literature and to public discourse in Australia and the authors are to be congratulated on embarking on this study. However, there are aspects of the analytic methodology and the manuscript overall requiring further attention before it can be published. As a result, my recommendation is for major compulsory revision.

Major Compulsory Revisions

Broad comments

1. The manuscript currently addresses two aims and presents a large number of mainly null results in 10 tables. As a result, the detail provided in the methods and results is overly brief, making it difficult to understand many critical parts of the methodology. For example, the authors compare outcomes from their sample of children to different samples of children without describing these other samples in any detail. Without this context, it is difficult to understand what meaning can be drawn from the findings. The paper would be vastly improved by focussing on one of the aims (e.g. health and wellbeing outcomes for your sample of children) and providing more detail on the methods and analytic process. The parts of the manuscript relating to stigma would then be best presented in a separate paper where sufficient detail can be provided to address those questions. This would also allow the authors to address further questions that would be interesting and relevant, for example, is the effect of stigma on outcomes different for younger or older children, or for children from different family structures?

2. Providing more detail on the analytic methodology and being more thorough in the analysis of your sample will address the significant concerns I have regarding issues of confounding. Many of the analyses compare results from a ‘normative’ population of children (Mellor, 2005). In Mellor’s study, children were sampled from schools in one Australian state, and no details were provided in that study regarding the demographic backgrounds of those children nor how representative of the Australian population those children were. In this respect, there is no way to tell if you are comparing like with like. Similarly, in comparisons
with the Hayes (2007) sample, this is a study where SDQ ratings are provided by teachers and not parents, and again there’s no way to tell how similar the samples are. In the only table that adjusts for background characteristics, potentially the most significant analysis (Table 9), there is no information explaining which data or study is being compared against.

3. I am sympathetic that in studies like these it is difficult to achieve a representative sample, and this is one of the downsides to convenience sampling. However, the issue the authors face regarding representativeness is two-fold. One is that the sample may not be representative of same-sex attracted parent families, and issues of respondent bias which is addressed to a degree. The other is that these families may differ in systematic ways to the rest of the population, and as such, drawing comparisons with the rest of the population is very difficult. For example, in this sample 74% of the children have a parent with tertiary education. In comparison, 2004 data from the Longitudinal Study of Australian Children (LSAC) has shown that fewer than 30% of 4-5 year old children had a mother with a bachelors degree. Given that parent education (particularly, mother’s education level) is one of the strongest predictors of child outcomes, I would hypothesise that the children in this study should have higher outcomes on average than other children in the population – issues of stigma etc notwithstanding. This is touched upon somewhat in the manuscript (e.g. some same-sex attracted parents need significant resources to conceive or for surrogacy), but is an issue that requires further attention and discussion.

The rest of my comments relate to specific sections of the manuscript. Should the changes above be adopted, many of the following comments will be repetitive and/or redundant.

4. The introduction was too brief and did not provide a satisfactory review of the relevant literature. In particular, more detail was needed around the role of stigma, why it’s relevant for children in same-sex attracted parent families and the potential impacts for children and parents.

5. Page 5, para 2. It is unclear why this paragraph on healthcare providers is relevant. Are perceived barriers to healthcare an example of stigma? Is this the only example available? Is there a wider literature on stigma (e.g. for different populations) that may be of relevance here? Why is it the role of healthcare providers to advocate for conducive environments (as opposed to other service providers)? Explain.

6. “The impact that stigma has…” At this stage you haven’t talked about stigma, the types of stigma that children in same-sex attracted families encounter. It is not clear what the impact is, let alone why it can’t be underestimated. More information is needed here.

7. There is insufficient detail in the methods to have a clear understanding of the survey methodology. Referencing prior methodology papers is helpful but not adequate.
8. Page 9 – “Where complete datasets were available…” These datasets need to be detailed. Which datasets, currently they’re not specifically referenced? How many participants? General data collection methods? What variables were included in these datasets?

9. Page 9 - I can understand how mixed effects linear regression is used for the stigma analyses, but not for the ones that are presented in Table 9. The analytic strategy isn’t clear here, and it hasn’t been elaborated on in the results. This section needs significant work.

10. Results. There are serious issues of confounding that make it very difficult to extract meaningful comparisons or conclusions from the results. Either a new analytic strategy should be employed, or if the current approach is maintained, then significant extra information is needed to put the results of this study into context. This can be achieved either by comparing the demographic characteristics of this sample to the demographic characteristics of the normative populations, or by comparing the demographic characteristics of the families in this study with other representative samples, or Census data.

Minor Essential Revisions

11. Page 4, para 1. The introductory paragraph needs to be clearer about the direction of the paper.

12. Page 4, para 2. Before a discussion of the limitations of previous samples, it would be good to review a range of studies (assuming there is more than one) that examine outcomes for children in same-sex attracted parent families. Then discuss the limitations of those studies.

13. Page 4 para 2 “Convenience samples are also fraught with problems”. Isn’t sample size and generalizability such a problem? The first few sentences need revision for better ordering and flow.

14. Page 4, para 2. “There is no evidence to suggest…” Is there bias in the samples or not? If not, why is a new sample needed? This sentence sounds contradictory, needs revision.

15. Page 4, para 2. “This limits data to same-sex couple families…” Can you give examples of the types of families that are not captured by this approach?

16. Page 4, para 2. Have the studies that extrapolated from population studies managed to estimate the number or proportion of children in Australia that are in same-sex couple families?

17. Page 5, para 1 “As such, the only methodology available…” You haven’t mentioned conducting a household survey which could give you a more representative sample, though these are expensive to run given the probable prevalence of children in same-sex couple families (see previous comment). Convenience sampling is therefore not the only methodology, but it’s the only one that is accessible given limited resources.
18. Page 5, para 1. “By working through…” The intention of this sentence is unclear. Are you suggesting that convenience samples are generally poor, but if you can recruit in a systematic way you can achieve better samples? If so, this needs to be stated more explicitly.

19. Page 5, para 2. Perceived barriers when accessing health services… can you provide some examples from the study? “This can lead to a lack of understanding…” From whom? Please be specific. “Family is an essential contributor…” There is a large literature you could draw upon to support this statement, e.g. Bronfenbrenner.

20. Page 6. Your first use of the acronym ACHESS has not yet been defined (page 6).

21. ‘Data’ are plural. The preferred term throughout is ‘data were’ rather than ‘data was’.

22. Please provide references when referring to studies (e.g. HOYVS) and instruments (e.g. SDQ).

23. Page 6. How many participants completed the online and paper surveys?

24. Page 6 “full comparative Australian population data…” Do you mean data sets? Are they really Australian population data given one is a survey of Victorians? Try not to overstate the representativeness of these studies – particularly when these studies do not provide details regarding demographic characteristics.

25. Table 1. The purpose of Table 1 is to provide an overview of the characteristics of your sample. Because some index parents report for multiple children, it is unclear how accurate the parent-related characteristics are. If, for example, female index parents have lower incomes because they have more children then they will be over-represented in this table. If data are presented based on individual parents rather than children, does it change the distribution of any of the variables? If not, then it is fine to leave as is and make a statement to that effect. If there are differences, these should be stated. How many parents are reporting for 1, 2, 3 etc children?

26. Page 8. A more appropriate term for what the SDQ is measuring is social and emotional wellbeing (or social-emotional problems or similar), rather than health.

27. Page 8. How does the internal consistency of the adapted scale compare to the one developed for lesbian-parent families?

28. Re Table 1. Who are the non-biological children (24%) if not a partners child, fostered or adopted? Are they step-children? Perhaps some comment can be made about this.

29. Page 10. the last sentence of para 2 needs to be at the start of para 1.
30. Page 11. “This compares to 90% of all Australian children who were initiated with breastfeeding”. This statistic helps the reader to understand how similar or different this sample is to population norms. More of these comparisons would be helpful, particularly in relation to the characteristics covered in Table 1.

31. Page 12. “…after adjusting for socio-demographic characteristics…” Please state which characteristics were adjusted for.

32. Page 12. “The findings clearly demonstrate that parent-reported child health in these families is at least equivalent to children from representative population samples.” This sentence overstates the importance of your findings. A lack of a statistically significant result does not necessarily mean there is no difference. It is more appropriate to say there was no evidence of any difference between the different groups. Furthermore, one of the samples you compared to used teacher reports, so the results were not comparable.

33. Page 12. “…which was also found with the previous smaller studies…” Please reference these studies.

34. Page 13. “This is important, socially and policy-wise”. What are the social and policy implications? How many children in the 2011 Australian census were in same-sex couple households? Without this information, it is hard to understand whether 11% of these households having male parents is important.

35. Page 13. “Socio-demographically…” The discussion here of demographic differences needs to be given much more attention throughout the manuscript.

36. Page 14. “Despite the lack of an easy supply…” Could this result be explained by the 11% of children in male-parent families that were born to previously heterosexual relationships? Presumably these children could have been breastfed by their mothers, but you should be able to test this with your data. Some comment is required.


38. Page 15. Your discussion about same-sex couples being more likely to share household duties etc should also be in the introduction, and be used to set up hypotheses or expectations around what you expect find in the results.

39. Page 15. Is there any evidence available regarding the stigma that other families encounter to put these findings into context? For example, single parent families, migrant families, disadvantaged families?

40. Page 15. “Our findings support and strengthen the idea that stigma related to parental sexual orientation is associated with a negative impact on child mental and emotional wellbeing”. This statement isn’t supported by the results. Stigma was associated specifically with family cohesion and general health. While it is possible that stigma has an impact on these aspects, could it also be possible that children in families with poor cohesion or those with poor health are more
likely to perceive stigma?

41. Page 16. “however this has in part been allowed for in the statistical analysis by incorporating numerous control variables…” Only two of the tables controlled for any of these differences, and only one was in relation to a wider population of children (Table 9). Furthermore, the detail surrounding the analysis for Table 9 is unclear. Therefore this limitation has not been adequately addressed. In addition, see earlier comments about the two issues related to representativeness – one being sampling and respondent bias issues, the other being how similar this population of families is to other Australian families. Both of these issues, and their different impacts on potential results, need to be discussed.

42. Page 16. “An unavoidable limitation for consideration is the temporal difference…”. It is not clear what this limitation is. Is this meant to reflect a concern that data from other samples were collected in the mid 2000’s? This is only a problem if the prevalence of mental health or general health problems have increased or decreased over time. This paragraph could probably be deleted.

43. Page 17, para 2. The Hayes study referenced under the tables used teacher-reports of the SDQ. If this is so, your statement that “particular comparisons with population normative data are valid given that parent-report was used in both contexts” is partially incorrect. If it is parent-report, then this needs to be explicitly stated in the methods and correctly referenced.

44. Table checks. Tables 4-6 could be combined together, with separate wafers for boys and girls to make it easier to observe differences by gender (discretionary).

Table 7 – The standard deviation for total difficulties score for the normative sample (1.71) appears unusually low. Please check this is correct.

Table 9 – What is the source of the population normative data?

Discretionary Revisions

45. Page 14. “The small differences…” The SDQ can be used as more than a screening tool in a research context. An alternative to your current approach is to examine the prevalence of likely social and emotional problems (e.g. conduct disorder) using the clinical cut-points of the SDQ.

Level of interest: An article of importance in its field

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

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

Declaration of competing interests:
I declare I have no competing interests