Author's response to reviews

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

Authors:

Simon R Crouch (simonrcrouch@hotmail.com)
Elizabeth Waters (ewaters@unimelb.edu.au)
Ruth McNair (r.mcnair@unimelb.edu.au)
Jennifer Power (jennifer.power@latrobe.edu.au)
Elise Davis (eda@unimelb.edu.au)

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Author's response to reviews: see over
Dear Editors

Thank you for the opportunity to revise our manuscript for publication in BMC Public Health.

We would like to thank both reviewers for the time they have taken to consider our manuscript and to provide detailed and incisive reviews. We have made a number of revisions to reflect their comments and to improve the presentation of our findings. We will address each reviewer’s comments (in bold) on a point-by-point basis below. Given the extent of the changes we have not highlighted them in the text, rather we have identified the changes by page, paragraph and line below.

Reviewer 1 – Kirsten Hancock

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?

Overall we have endeavored to improve the level of detail to better explain the methodology and results, in doing so we hope that it is now clearer which data are compared with which samples throughout. To aid in this, and to reduce the number of tables, which as you rightly point out display a large number of null results, we have removed the comparisons
with the data from Mellor and Hayes. We feel that this does not alter the findings or conclusions in any meaningful way and makes for a more straightforward and precise consideration of the data. While we recognize that splitting the paper may make the data slightly easier to follow we feel that it is important that overall health and stigma are presented together to give the complete picture. By adding to the background section this link is hopefully clearer and better supports the combination of data in this paper.

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.

   We have significantly increased the information around the methodology and analysis strategy, which hopefully brings better understanding to how we have handled the data. As mentioned, the Mellor and Hayes studies have been removed as comparators and we have included a new table, table 2 (p31), to show some key comparisons around the datasets. Throughout we have amended wording to ensure the reader understands that the population samples are drawn from Victoria and not the whole of Australia, which may not have previously been understood. The title of table 5, p34 (previously table 9) has been amended to better indicate which datasets are being compared.

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.

Further information on demographic characteristics and how they compare to population data (ie from the census) and normative samples has been included throughout the manuscript and greater emphasis placed on the impact that this has on interpreting the results included (table 2, p31; line 9, p13; line 13, p13; lines 1-2, p14; paragraph 2, p16; paragraph 1, page 20).

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.

The introduction/background has been substantially expanded to give readers a clearer context for the work (start of page 4 to paragraph 1 page 6). The role of stigma in same-sex families has been particularly highlighted (paragraph 1, page 5).

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.

Paragraph 2, page 5 - This paragraph has been substantially changed to be more about physical health in general with the emphasis taken off health care workers.

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.

Stigma is now included as an integral part of the background work in this area (last 2 lines of page 4 to end of paragraph one on page 5).

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.

The methodology has been substantially expanded with more detail and clearer explanations (pages 7-12).
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?

Each of these issues has been addressed in sections for both the HOYVS and VCHWS (last paragraph page 10 to end of second paragraph page 11).

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.

The analysis process has been re-written to more clearly explain how analyses were conducted and how they were interpreted. In doing so the presentation of results should now be clearer (last paragraph page 11 to end of first paragraph page 12).

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.

Additional demographic information from population samples has been included, table 2 (p31), as well as elements of comparison data at a national level (eg median household income from the Census – line 13, p12; and parent education levels from LSAC – line 9, p13). The consideration of possible confounding has also been given greater weight in the discussion (paragraph 2, p16 to end of paragraph 1, p17; paragraph 2, p19 to end of paragraph 1 p20).

Minor Essential Revisions

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

The whole introduction has been re-worded to provide more detail and a clearer direction – a new introductory paragraph (p4, paragraph 1) sets the scene and paragraph 2, p7 highlights the aim.

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.
A thorough review of the literature is now included. Paragraph 2, page 4 describes previous research in general. Paragraph 1, p5 describes work around stigma. Paragraph 2, p5 describes limited perspectives on holistic health. Paragraph 3, p5/paragraph 1, p6 describes the role of parent gender.

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.

The wording in these sentences has been changed to make them clearer – p6, paragraph 2, line 3.

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.

This sentence has been re-worded – p6, paragraph 2, lines 6-7.

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?

Two examples have been provided and a reference for more background – last 2 lines of p6.

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?

The number of children with same-sex couple parents is now part of the first paragraph of the introduction (p4).

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.

The recognition of theoretically possible household surveys has been included and a brief explanation of their lack of suitability given (lines2-5 p7).

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.

This sentence has been re-worded for greater clarity – end of paragraph 1, page 7.
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.

This paragraph has been substantially changed and incorporated into an early part of the background section – p5 paragraph 2.

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

The ACHESS is written in full – p7, methodology.

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

The word ‘data’ has been amended to plural throughout

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

HOYVS and VCHWS have been referenced throughout (paragraph 1, p8; paragraph 2, p10; paragraphs 1 and 2, p11).

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

This information is now presented in the results – penultimate line, page 12.

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.

Clarification about the location of normative samples has been included (last paragraph page 10 to 2nd paragraph page 11; first paragraph, p14).

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?
The pattern of family demographic data is no different when presented at child and parent level. It was decided to present all data at the child level for consistency and to ensure ease of understanding for the reader.

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.

Recognition that the SDQ measures social and emotional wellbeing has been added – last 3 lines, p9

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

Comparative Cronbach alpha has been added – end of paragraph 1, p10

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.

This data comes from a series of yes/no questions with multiple answers possible (although practically speaking a child cannot be biologically related to the index parent AND their partner). As such the data does not necessarily tell us who these children are, although it is likely that they are the biological children of a previous partner.

29. Page 10. The last sentence of para 2 needs to be at the start of para 1.

This has been changed – reference to table 1 now starts paragraph 1, p13.

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.

Additional comparisons have been added to some results that are described from table 1 – line 9, paragraph 1, p13; line 13, paragraph 1, p13)

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

These characteristics are described in the footnotes of table 5 and not included in the text in order to maintain flow and in accordance with guidelines for presenting data such that data is not unnecessarily repeated. If required we can add it in the text.

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.

This sentence has been changed in line with the comment so as not to overstate results – beginning of p16.

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

These studies have been referenced – line 4, p16

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.

End of paragraph 1, p16 - This section has been reworded for greater clarity of meaning and can be related to new census data in paragraph 1, p4.

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

Greater attention has been given to demographic characteristics throughout the text (paragraph 2, p16 to end of paragraph 1, p17; paragraph 2, p19 to end of paragraph 1 p20) and is supplemented here.

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.

This wording has been changed to explain some of the reasons behind milk supply – paragraph 2, p17.


This wording has been changed to better represent our meaning – line 8, p18.

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.

This has been added to the introduction/background – beginning of p6.
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?

A brief section reflecting on other family contexts has been added – paragraph 1, p19.

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?

Possibly due to poor descriptions of data sets earlier in the manuscript we believe you have misinterpreted the findings that we present in relation to stigma. The family cohesion and general health findings are comparisons with population data and are independent of stigma. The scales associated with stigma are presented in paragraph 3 on p15 of the new manuscript. While family cohesion is included there are also associations with mental health, emotional symptoms etc. – more broadly mental and emotional wellbeing. As such this section has not changed.

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.

Issues of comparison, bias and associated limitations have been addressed throughout (and are explained in the responses above).

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.

This paragraph on temporal disparity has been deleted as suggested.

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.

The Hayes study has been removed

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?

Old tables 4-7 have been deleted, and the old table 9 (new table 5) given a new title for greater clarity

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.

This is an interesting alternative and worth further consideration but we have opted not to reflect this in the current manuscript.

Reviewer 2 – Marie Leiner

Major Compulsory Revisions

This study is of high importance and needed. However, there are methodological problems that need to be solved and it should not be published until they are corrected.

The study needs to clarify the following items.

Information about the validity of the questionnaire

The authors need to clarify this part "Survey preparation comprised a scoping review of the literature,[1] consultations with same-sex attracted parents and adult children with same-sex attracted parents, and development of a survey instrument that demonstrated overall coherence with embedded, established, psychometrically validated and reliable measures of child health. This is not a correct way to demonstrate the reliability and validity of the questionnaire. If they used it in another study they need to comprise information in this paper to provide the reader the validity of the questionnaire. It is not the obligation of the reader to go and read their other article."
The details on the reliability and validity of the questionnaire have been re-worded to provide more clarity – specifically that the questionnaire is comprised of three validated instruments (the CHQ, the ITQOL and the SDQ) with some additional questions (paragraph , p8). It is not suggesting that we have tested the reliability and validity ourselves (although the reliability of the BSS for our sample is presented in terms of the Cronbach alpha – paragraph 1, p10).

The test scores found in the study indicate non normal data that needs to be normalized using the usual conversions. Data transformations are commonly used tools that can serve many functions in quantitative analysis of data. The use of three data transformations most commonly discussed in statistics texts (square root, log, and inverse) for improving the normality of variables need to be attempted to apply parametric test. As a consequence, I cannot discuss the results because all the analysis needs to be redone or use non parametric data. e.g. Total difficulties mean 8.78 (4.24) 8.14 (6.15)

It is commonly accepted that QoL data is often not normal in its distribution. While most authors ignore this fact we acknowledge that transformation is one way of dealing with this issue. Transformations did not provide any significant benefit when considering the raw data however, and through detailed reflection on the chosen statistical tests, in conjunction with consultation of statistical experts from the University of Melbourne Statistical Consulting Centre, plots of residuals and considerations of equality of variance suggested that the chosen models held up to greater scrutiny and as such the analysis is valid. This approach is summarized in the analysis section of the methodology (paragraph 2, p12) and is supported by published reviews (eg Lumley et al, 2002, Ann Rev Pub Health).

Any results and possible conclusions in this study are not valid until this conversions are done.

We do not agree that the results and possible conclusions are invalid, and instead agree with the statistical review of the first reviewer, whose concerns have been further addressed above.

We hope that we have adequately addressed the reviewers' responses and look forward to your further decision.

Yours truly,

Dr Simon Crouch