Author's response to reviews

Title: Mental Health, Sexual Identity, and Interpersonal Violence: Findings from the Australian Longitudinal Women's Health Study

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Author’s response to reviews:

Our sincere thanks to the editor and reviewers’ thorough critiques which enabled us to craft a stronger manuscript. We have included each reviewers’ comments and our description (in a different font) of how it was addressed.

Reviewer 1

1. In the background you have used the term "intimate partner violence" at line 24. I think you meant to use "interpersonal violence" (IPV) to be consistent with the rest of your manuscript.

Actually, we were citing the work of Vichealth, who reported on intimate partner violence. However, we agree that including the parenthetical acronym (IPV) here is confusing as we had used this acronym to mean interpersonal violence throughout the manuscript. We deleted “IPV” from this sentence. In addition, given the potential for confusion we have deleted all IPV acronyms throughout the manuscript.

2. Following your hypotheses and in the last paragraph of your background, which begins "Examining sexual minority subgroup differences, as well as...." I noted that sentence was long and difficult to read. May I humbly suggest a full stop in the middle to break it up, i.e.,: ".......which population groups are at greatest risk. This may help explain mental health disparities observed among sexual minority groups"

Thank you for this suggestion. We have edited the sentence accordingly.
3. Do you want to define "AOR" somewhere in the manuscript? If a clinician had poor statistical skills this may confuse them as they may be accustomed to seeing just "OR". I think all of the other abbreviations are fine.

This is defined in lines 24 on p. 13.

Reviewer 2

1) A clear operational definition of IPV is needed in the opening paragraphs. It is not until the description of measures in methods that the authors are explicit in exactly what they mean by IPV.

We now include the operational definition of IPV in hypothesis 1, which is the first appropriate possibility. This information is also included in the abstract.

2) IPV is measured based on 5 items reflecting distinct domains (physical, severe physical, emotional, sexual, harassment). Is there a literature supporting IPV as constituting these specific domains, and could this info be incorporated into the introduction and/or description of measures in the methods.

Given that an increasing number of studies are showing that sexual minority women as a whole report higher rates of nearly all forms or types of violence, we included all of the types of violence assessed in the ALSWH (physical abuse, emotional abuse, sexual abuse, harassment, and being in a violent relationship) in order to capture the broadest range of experiences. Our primary purpose was to determine whether rates of different types of violence differ by sexual identity and to examine the relationships among violence, including multiple types of violence, and mental health. We also indicate that the violence measures draw from K. Hegarty’s composite abuse scale.

3) Was the Anxiety Symptoms measure a single item? Need to clarify.

We have added text to clarify this (Line 6, p 10).

4) No info on the internal consistency of the Mental Health Index in methods - although IC is included for other measures.

We have added this information (Line 22, p. 10).

5) Table 3: Emotional violence - replace "---" with "***"

We have corrected this in Table 3.
6) Fifth paragraph of discussion: The authors may wish speculate as to why IPV fully explains increased mental health issues in lesbians, but not bisexual or mainly heterosexual women. What additional factors may explain elevated MH in bisexual/mainly heterosexual women (after controlling for IPV). Social isolation? Lack of community connectedness? Individual difference variables (personality factors) which may be linked to both bisexual/mainly heterosexual identification and MH outcomes? Obviously this will need to framed in a way which does not pathologise non-monosexual women - but need to provide some potential explanations for your findings.

We completely agree. This is addressed somewhat in paragraph 3 of the discussion, and reiterated at the end of paragraph 5.

7) Check paragraph 3 - You state that greater reported violent relationships in mainly heterosexual/bisexual women may be explained by the fact that their partners are more likely to be male - but then in the next sentence, provide evidence with of similar rates of IPV in same-sex and opposite partners. I think your original assertion seems defensible based on the data you provide, and cross-cultural data of sex differences in violent behaviour generally (and in sexual relationships generally). Moreover - your sentence on rates of IPV in same-sex and opposite-sex relationship is not convincing - you don't clarify whether these studies are comparing male AND female same-sex vs. opposite-sex relationships, or just comparing within gender. If the former, it seems possible that the male-male relationships are driving this effect. Again, obviously this is a politically sensitive topic - so I understand the motivation to tread carefully. Nevertheless, I don't think this should come at the expense of providing a clear potential account of your data.

Thank you. We have added text to the discussion to clarify the sex of the perpetrator in one of the studies.

8) Examination of coefficients of the link between type of IPV and MH (Table 3), show that "emotional violence" is by far the strongest predictor of MH outcomes. This also looks to be the most subjective component of IPV measured (i.e., the others tend to reference specific, time-limited objective events, whereas being "bullied" or "critizied" involve a strong evaluative component on behalf of the respondent). It might be worth mentioning this issue of causation in the discussion - i.e., it might be that emotional violence leads to MH outcomes, but also, that those with elevated MH difficulties, may also perceive more emotional violence (thus explaining why this facet is so strongly linked to MH outcomes relevant to other facets measured).

We respectfully point out that some of the other violence measures also include a reasonable degree of subjectivity in their exemplars. For example, use of force as in the example “forced to engage in unwanted sex” may be interpreted in many different ways by participants. Similarly, exemplars of harassment (stalking, loitering, interfering with property, offensive mail or telephone calls) is also more subjective than the physical and sexual abuse items. However, harassment yielded similar coefficients as those of physical abuse. Nevertheless, we agree that the issue of causation is important to address in regard to the results for emotional abuse, and the results more generally. We have added the following to the Limitations section of the
Discussion: The current study was conducted using cross sectional data, so no inferences as to causation can be made. For example, although it is possible that emotional abuse leads to poor mental health outcomes, it is also possible that poor mental health might lead to heightened perception or reporting of emotional abuse.

9) A potential discussion point also may be whether it is reasonable to assume that mainly heterosexual women experience the same type/severity of minority stress as faced by bisexual or lesbian women. At this point exactly who adopts this label and why - is still poorly understood. As you indicate - without data collected on sexual behaviour (past or present), it's unclear whether "mainly heterosexual" women in your sample simply represent otherwise heterosexual women who acknowledge the potential to be attracted to women (or are open-minded to the potential of such attraction in the future) in the absence of past/present behaviour or even intention to act on this attraction. Whether increased MH difficulties in this group (relative to exclusive heterosexuals) can be explained by > sexual minority stress is still unclear.

Existing research findings indicate that mostly heterosexual women have health inequalities that are more similar to lesbian or bisexual women than to heterosexual women. So, although we agree that we do not have a clear understanding of the range of women who select this category, it is likely that this group includes more than those women who have the potential to be attracted to women or who may have had a past sexual experience with women. For example, in a qualitative interview study of adolescents of diverse sexual orientations Austin and colleagues (2007) found that adolescents preferred a sexual identity item that included the intermediate options mostly heterosexual and mostly homosexual, which many said reflected their experience of feeling between categories. In a separate study Austin and colleagues (2008) found that young sexual minority women who identified as mostly heterosexual were significantly more likely than their exclusively heterosexual counterparts to report having experienced childhood sexual abuse.


Reviewer 3:

GENERAL COMMENTS
The in-text referencing throughout the paper often unnecessarily employs both numbering and author name systems. Based on the journal referencing guidelines, the author names should not be needed in text.

We have edited based on the journal’s referencing guidelines.

There is nothing to be done about this, but it may be worth noting that I associate 'IPV' with intimate partner violence, and it took me a while to realise that that is not what the paper was measuring. To my knowledge there is no way of differentiating these acronyms though. It was helpful that the acronyms were written out in full in the subheadings of the results, which quickly alerted me to my mistake.

Thank you. We have removed all IPV acronyms in the manuscript. We believe that this will avoid having readers confuse interpersonal violence and intimate partner violence.

ABSTRACT

It would be worth including the age range in brackets after 'young women' in the abstract.

This has been added to the abstract (Line 7, p 2).

BACKGROUND

The background is substantive, and while all relevant, could be written more tightly to avoid repetition.

Currently the paragraph structure essentially reads:

1 Prevalence of violence against women; 2 Relationship between violence and mental health; 3 Violence among sexual minority women; 4 Health disparities among sexual minority women; 5 Different patterns within subgroups of sexual minority women; 6 Different patterns within subgroups of sexual minority women; 7 Mental health disparities among sexual minority women; 8 Link between violence and mental health in sexual minority; 9 Study aims.

For example, paragraphs 2 & 8 linking violence and mental health could be consolidated; paragraph 7 on mental health disparities could be combined with Para 4; Para 6 could more directly lead into the study aims, emphasising the literature gaps this study fills.

These are not prescriptive but may help with the flow of the background.

We appreciate this comment and have reordered and edited the introduction. We believe that this substantially reduces redundancy and improves the flow of information in this section.
METHODS

The age range of the analysis sample could be more clearly stated. The analysis is based on the youngest cohort group (18-23yo at first survey) at the third survey, which are completed every three years. The resulting age of sample is 25-30 years, but this is only mentioned near the end of the methods. It would be easier for the reader if this was stated upfront (e.g. pg. 7, line 54).

This has been added to the abstract (Line 7, p 2) and again in the text before the measures (line 19, p. 8).

Is the measure of sexual identity based off a validated tool? It appears to be a modification of the Kinsey scale, and should probably be referenced as such if so.

The sexual identity measure does resemble a modified Kinsey scale in that it includes response options representing a range from exclusively heterosexual to exclusively lesbian. However, the ALSWH archival records indicate that this measure was developed specifically for the ALSWH. In addition, although we are not aware of any studies that have validated the measure, it has been used in a number of other studies, including the second author’s 17-year longitudinal study of sexual minority women’s health and a 20-year longitudinal study of alcohol use among women in the general population (Sharon Wilsnack, PI).

Pg. 10 line 46 - clarify annual income

Income has been clarified (Lines 13-14, p. 11).

Pg. 10 lines 48-53 - I think this method of relationship classification, why standard, is particularly problematic when looking at a younger population. Many young women will be in a relationship that does not classify as de facto, but equally may object to the official classification of being 'single'. While obviously this cannot be changed now, I think it should be acknowledged as a limitation of the paper.

As I refer to later, I am a bit concerned that the analysis is too limited, without enough variables other than basic sociodemographic background, to truly answer the second aim of to what extent the association between sexual identity and poorer mental health is accounted for by experiences of IPV.

The language used in the paper is standard in Australia. Based on our other research, including qualitative studies conducted by the second and third authors (Hughes & McNair), we believe that young women understand and apply ‘defacto’ to their non-married relationships to the same degree as older women.

RESULTS

Pg. 14 line 43 - the sentence on the weakest predictor reads strangely to me. It may not be worth highlighting this finding in the text, given it can be inferred from table 3.
Thank you. We have deleted the sentence.

Is it possible to assess and control for other potential mediators of mental health, such as alcohol and drug use? Research demonstrates a higher prevalence of problematic alcohol and drug use in sexual minorities, and these behavioural factors may also explain/mediate the relationships between sexual identity, IPV and mental health.

We agree that these are interesting and important factors for future research. The purpose of the current investigation was to establish the relationships between sexual identity, IPV and mental health in a community dwelling nationally representative sample. As such, examination of health behavior was beyond the scope of the current study.

DISCUSSION

The discussion describes the major findings well and puts them into context with existing literature. Caveats around the findings and interpretation are included. The limitations - and particularly that the study only looked at sexual identity and not other dimensions of sexual orientation, are addressed. However, I have drawn attention to a few other issues which limit the analysis, such as the limited range of variables controlled for. This is referred to in the final sentence of the conclusion, but should be highlighted more clearly.

We highlight this at the end of the limitations section (Lines 14-16, p. 21).

Pg. 17 lines 51-56 - I don't think it's necessary to repeat these stats in discussion

Thank you, we have eliminated the repetition of the statistics in the discussion.

A sentence or two clearly articulating the implications of the findings on ongoing research and services could be added to the discussion.

Thank you, we have added a few sentences articulating the research and clinical implications of the findings.

While the main discussion nicely explains the results, I find the emphasis in the conclusion that mental health outcomes are more closely associated with IPV than sexual identity somewhat problematic from a research translation point of view, as it detracts from these groups of women having increased need of support and services. I am not sure why one would assume that sexual identity is in itself causative of poor mental health.

Indeed, our point is that sexual identity itself is not causative of poor mental health (an argument that is often put forth by conservative groups) but rather, poor mental health is associated with society’s responses to sexual minority people.

The IPV was more closely associated with mental health problems than sexual identity is true in this analysis, but I'd argue that not enough variables have been examined to extrapolate these
findings and say definitively that violence explains the mental health disparities experienced by sexual minority women. Sexual minority women remain at higher risk of IPV and resulting mental health outcomes, and both violence prevention and mental health services targeting women with diverse sexual identities are strongly called for.

Thank you, we have added this at the beginning of the Conclusion.