Reviewer’s report

Title: Lifetime prevalence of suicidal ideation among men who have sex with men: a meta-analysis

Version: 0 Date: 07 Jun 2017

Reviewer: Travis Salway Hottes

Reviewer’s report:

This systematic review and meta-analysis summarizes the prevalence of suicide ideation among men who have sex with men (MSM), pooled across a total of 25 observational studies. This topic is important and methods used are for the most part appropriate; however, I have several major concerns for the authors to address (each of which is detailed in my comments below): (1) the authors should provide more discussion of previous systematic reviews and meta-analyses on the topic of suicide-related outcomes (whether ideation or attempts) in sexual minorities within the Background section, and should justify specifically what their review adds to this literature; (2) several relevant empirical papers from my own literature file seem to fit the authors' inclusion criteria but do not show up in this review, suggesting the authors need to broaden their search terms and databases, and do a careful review of the reference lists of other systematic reviews on this topic; (3) data on the two time-frames of analysis (12 month ideation and lifetime ideation) should not ever be pooled together, given that these are conceptually distinct outcomes; (4) exploration of heterogeneity should be sharpened and expanded (see detailed comments below); (5) the authors could go further in interpreting their results in the Discussion, specifically pointing to results that are novel (versus those that have been well-established in this field of research).

Detailed comments:

Background

1. The Background section should be lengthened, and I would suggest the authors add (at least) the following details that are missing from the rationale for their study:
   a. The Background should include a discussion of previous systematic reviews published on the topic of suicide-related outcomes among sexual minorities (including MSM); notably: King 2008; Marshal 2011; Hottes 2016; and Ploderl 2015.
   b. Then the authors should justify or explain why an additional systematic review on this topic is needed. It is not true that there has been no meta-analysis pooled prevalence of suicidal ideation in MSM (see King 2008). Nonetheless, the authors could make a case for why that review should be updated or refined. Additionally,
they should justify a review of a less-specific outcome (suicide ideation) than that used in the Hottes AJPH 2016 review (suicide attempts).

c. Why is the review restricted to men, given that there is also evidence that women who have sex with women experience elevated rates of suicide-related outcomes?

2. In the first sentence, the authors should modify their statement to indicate that suicide is a leading cause of death among MSM (not the leading cause of death); the cited paper simply compares estimated rates of suicide to estimated rates of HIV mortality, which indeed suggests it is a leading cause of death, though not the single largest cause.

3. The authors should provide some discussion (in Background or Methods) about the limitations of measuring/studying suicide ideation (a relatively common and non-specific marker of suicide risk) versus suicide attempts (or deaths). David Klonsky has published extensively on this topic.

4. Lines 43-45: The authors imply that the range of estimates for suicide ideation in MSM, 10-55%, is a problem, or a matter of reliability. In fact, there may be good reason why these studies had very different estimates (e.g., the samples were different). This doesn't mean they can't explore the heterogeneity, but they shouldn't ignore it.

Methods/Results

5. There are several studies in my literature file that report prevalence of suicide ideation among MSM (or gay/bisexual men). To name a few: Brennan 2010; Carragher 2002; Cochran 2000; Gilman 2001... The authors should probably broaden their search terms and databases (perhaps try CINAHL, Scopus, Google Scholar, PsycInfo) to retrieve some of these missed publications. They should also carefully check the reference lists of the 4 previously published systematic reviews mentioned above.

6. What is the authors’ definition of community-based and population-based samples? Does community refer to any community or communities of gay, bisexual, and other MSM?

7. Relatedly, did the authors exclude any studies based on special populations (e.g., those studies based on mental health or psychiatric clinics)?

8. How were the quality scores calculated by study? It would help if the authors presented the items from the AHRQ scale (in order to determine if/how those items apply to this topic), and perhaps included a table showing how each included study scored on each item of the scale (this is often included as an appendix).

9. It is not surprising that the estimated pooled prevalence of lifetime suicide ideation was higher than 12-month suicide ideation. I would suggest that the authors should keep these two outcomes separate throughout the presentation of results. For instance, in Table 2, it
becomes difficult to compare the prevalence estimates by sampling method, age, and HIV status because they are mixing together lifetime and 12-month measures (what if by chance both of the studies with MSM <18 measured lifetime ideation, while some or most of those with MSM 18+ years measured 12-month ideation?).

10. Given that there were only 2 studies with participants <18, but 12 with participants 18+ years, why didn't the authors explore older age groups (i.e., higher age cut-off)?

11. One of the strengths of this study is that it pooled data from a wide range of geographic settings (countries). Why do the authors not explore heterogeneity by grouping these countries geographically and analyzing sub-groups, as they've done for age, sampling method, and HIV status?

Discussion

12. I do not understand why the authors compared their results to medical students, college students, and the elderly (in second paragraph of Discussion). Why are these appropriate comparators?

13. I don't think the authors need to discuss/explain why lifetime prevalence was higher than 12-month prevalence (paragraph 3 of Discussion)--the conceptual/measurement difference in time-frame should be acknowledge a priori (in methods).

14. Also in paragraph 3 of Discussion, the authors suggest that RDS or snowball sampling may overrepresent the MSM population. Given that these sampling methods yield exclusive samples of MSM (through networks), it's not clear what the authors mean by "overrepresent". Overrepresent some group within MSM? If so, which group(s)?

15. In paragraph 4 of Discussion: the authors should be more cautious (or at least nuanced) in their interpretation of age-related findings. It is already known that suicide ideation is more prevalent among adolescents than among adults (whether MSM or not); however, suicide mortality rates increase with age. In this context, I would be hesitant to make firm conclusions about the age group to which "more attention… should be paid".

16. There are minor grammatical errors throughout the paper that should be corrected.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes
Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

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