Reviewer’s report

Title: Trends in heterosexual inexperience among young adults in Japan: analysis of national surveys, 1987-2015

Version: 0 Date: 18 Dec 2018

Reviewer: Catherine Mercer

Reviewer’s report:

Thank you for inviting me to review this paper, which I read with interest. It examines the prevalence of heterosexual inexperience in Japan and changes over time in these, extending over a long time - period - almost 20 years. This is one of its strengths, along with using robust data from Japan's National Fertility Survey, which has achieved large samples and high response rates; addressing a neglected area of sexual health research in a country for which there is a dearth of literature (relative to eg the US).

The paper is well-written and so easy to follow and appropriate statistical methods are described. I have some suggestions which I hope will strengthen the paper and make it fit for publication. I present some comments that apply to the paper overall and then some points by section.

Overall:

1) Terminology: the paper refers to both 'sexually inactive' and 'sexually inexperienced'. The former implies that this includes those who have had sex at least once in their lives yet are not currently sexually active, while the latter implies those who are virgins. The paper's focus is on the latter so the authors should only use the term 'sexually inexperienced' to avoid confusion.

2) Related to point#1: if the paper does just consider sexual inexperience then this will underestimate the proportion sexual inactive (which will include those sexually-experienced just not sexually active), which is more of an issue for the fertility rate. Can the authors address this point - at least in the Discussion?

3) Analyses of factors associated with heterosexual inexperience: this analyses uses data from the 2010 survey, yet prevalence estimates are given for a more recent survey undertaken in 2015. While 2015 is not all that recent, it is more recent than 2010 and so it would be preferable for the authors to conduct the regression analyses using the 2015 data rather than the 2010 data.

4) In some places, I felt that there was too great a reliance on statistical significance. The sample size is very large so a difference of just a couple of % may turn out to be statistically significant but that they may not necessarily be really substantive different.
Abstract:

5) Conclusions: these seem rather repetitive of the Results section. Please revise paying more attention to the implications of the study, especially given "...and may have important demographic and public health implications" as stated on P.4, lines 73-74.

Background:

6) P.4, line 65: "more than 40% of never-married 18-34 yr old women..." - but what % of the total population were those never-married and how has this % changed over the study's time frame?

Methods:

7) Data sources: an important omission is whether the questionnaire wording was consistent in each round of the survey? Please address this point in this section.

8) P.6, lines 104-107: It wasn't clear to me the significance of these 2 sentences until later in the paper so could the authors address this?

9) P.6, lines 118-119: I assume the range of sample sizes correspond to different waves? Can the authors clarify this.

10) P.7, lines 127-128: "We assumed that all married participants and unmarried participants [...] had heterosexual experience". I am not very familiar with Japanese culture (like other readers perhaps) so I assume it's a reasonable assumption but can the authors provide any references to support this?

11) (a) P.8, lines 158-160: what was the basis for hypothesising that the variables list could be associated with sexual inexperience? Citing papers to support their authors' choices would be helpful.

(b) P.8, lines 158-160: were there other variables in the NFS that the authors could have looked at in their regression analyses but chose not to, or does their choice in part reflect data availability?

(c) Can the authors provide a reference to the actual surveys

Results:

12) P.9, lines 182-184: while I appreciate that the text is supposed to summarise the key findings shown in the tables, but I found this vague without any numbers to support the statements made. Also that the %s that were then given correspond to a different time
point (2015) to that just described. Similarly in lines 192-194. (Note that %s are fine so no need repeat the 95% CIs as well as these are in the tables)

13) P.9, lines 185-188: As per my 4th overall comment, I found there was too great a reliance on statistical significance, e.g. an increase from 6.2% to 11.9% - so almost a doubling - was described as "prevalence rose nominally", while a similarly sized increase - 4.0% to 8.9% - was described as "prevalence increased significantly".

14) P.10, lines 196-197: why are 95% CIs not given for the latter % cited, just the first %?

15) P.10, lines 201-202: it is insufficient to just say that the "characteristics of participants are shown in table X"; a paragraph summarising these should be given.

16) P.10, lines 215-216: for those readers unfamiliar with Japan's regions, these results aren't all that helpful. Could some context/explanation be provided?

Discussion:

17) P.11, line 11: "scarcity of national surveys on sexual behaviour..." - this isn't true, and although the authors cite data from the US and UK, it would be refreshing to consider data from surveys undertaken in other high-income countries, e.g. France, Sweden, Australia, New Zealand, ...

18) P.13: there is much reference to marriage in the Discussion, but what about cohabitation without marriage? Is that culturally-acceptable? Can the authors broaden the discussion to include reference to marriage as well as cohabitation?

19) P.14, lines 286-288: also, what about the Japanese government's subsidised speed dating efforts, e.g. from the media: www.bbc.co.uk/news/blogs-news-from-elsewhere-31869981

20) P.14, line 295: I don't think it's so much **in-person** interviews that are problematic, rather interviews that do not include a self-completion element. Can this be re-worded accordingly?

21) P.14, line 307: are there any data that can be presented on the prevalence of different types of sexual intercourse, specifically anal and oral? This would support the claim that most people in Japan consider intercourse to be mainly vaginal intercourse.

Tables:

22) Table 1: it would be helpful - in terms of understanding the characteristics of the population as a whole - if data could be presented for all women and all men in addition to stratifying by heterosexual experience.
23) Table 2: depending on journal style [editor please advise]: it would be helpful to provide global p-values for each factor (NB: not parameter) shown in Table 2 to highlight to the reader the overall 'effect' on heterosexual inexperience of each of the factors considered.

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

Yes

**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.

Yes

**Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?**
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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Acceptable

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