Reviewer's report

Title: Hepatitis B/C in the countries of the EU/EEA: a systematic review of the prevalence among at-risk groups

Version: 0 Date: 16 Oct 2017

Reviewer: Rachel Sacks-Davis

Reviewer's report:

This timely systematic review of hepatitis C synthesises recent HCV and HBV prevalence estimates for selected risk groups in Europe. The PWID estimates are derived from the EMCDDA repository and the estimates for prisoners and MSM are derived from a systematic review. The review provides a useful updated synthesis of data for prisoners and PWID in particular but is limited by the exclusion of migrants as a risk group for HBV and HIV infected MSM as a risk group for HCV. The paper would be improved by the inclusion of tables summarising the study characteristics, risk of bias and results.

Major revisions:

1. It is not clear why the particular risk groups (MSM, prisoners, PWID) were chosen relative to other potential risk groups. E.g., migrants for HBV, people living with HIV, HIV infected MSM, etc. Migrants are mentioned as a group in the Supplementary material but not in the main text. Migrants is a major exclusion as an HBV risk group and this should be acknowledged in the Discussion section. In the WHO testing guidelines, people living with HIV and HIV-infected MSM are mentioned as potential risk groups and prevalence of HCV at least is generally believed to be elevated in HIV-infected MSM but not necessarily in HIV-uninfected MSM.

2. Although I assume that studies of exclusively HIV-infected MSM were excluded, this should be stated explicitly.

3. The risk of bias data is not described in detail in the Results section - it would be useful to include a table of studies summarising the risk of bias in each study.

4. It would also be useful to include a Table summarising the study characteristics, number of participants and prevalence for each study.
5. In the Discussion the authors state that the findings don't support continuing to screen MSM for because prevalence is not >2% in that group but there is variation between countries and diseases. If the results from the studies were pooled, the point estimates for Estonia and Croatia would be>2% for HCV and for HBV all countries and some other countries for HCV, the confidence intervals include 2%. I don't think this conclusion is adequately supported by the findings.

6. In the Discussion section page 16, the "limitations of the study" are mentioned but they are not described explicitly. These should be described.

7. The conclusion is very general and doesn't specifically relate to the study findings.

Minor revisions:

8. In the Results section, in the section on prevalence in MSM, the authors suggest that there was an increase in HBsAg prevalence and decrease in HCV antibody in Estonia but it is not clear that time trends in the prevalence of either disease are supported by the data. It seems unlikely that HCV antibody prevalence could change so much in such a short time frame, and it is not clear that either "change" is statistically significant. Both studies are small and it is more likely that the different results reflect random variation.

9. Suggest including a completed Prisma Checklist in the Supplementary Material.

10. Suggest including the Prisma flowchart as a figure in the main text rather than the Supplementary Material.

11. I suggest reducing the number of acronyms, particularly ones such as MS and NFP that are not commonly used.

12. In the Discussion section, page 15, line 10, there is an unfinished sentence: "but that during their study…"

13. In the Discussion section, page 16, line 24, by "intra-population infection" do you mean "transmission"?
14. The paragraph "The limitations of this study...link to care people infected with chronic viral hepatitis" is quite general. What improvements in study design are required? How do the limitations of the study provide ideas for future research, or do you mean that the limitations of the included prevalence studies provide ideas?

15. In general, the data in the Supplementary material are not referenced in the main text so most readers will not realise that they are included.

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.

No

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.

Not relevant to this manuscript

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