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: 04 Aug 2017

Reviewer: Sarah Larney

Reviewer's report:

This paper presents estimates of the prevalence of HBsAg and anti-HCV in three risk groups - men who have sex with men, prisoner, and people who inject drugs - in the countries of the EU/EEA. Estimates are presented at the country level. No regional estimates are calculated.

I have several concerns with this paper, some overarching and some more specific.

Overarching concerns:

The rationale for the study is somewhat weak. Why this region in particular? It is said that current estimates need updating, but the reviewed studies go back to 2005, which doesn't seem up to date. As noted in the introduction, there have been multiple reviews of HBV/HCV in risk groups in various groups of countries (Europe, global). In the final paragraph of the introduction, the authors need to make a stronger case as to why this review, and why now (e.g. to provide baseline data towards elimination targets?)

The pooling of study findings seems to have been done by summing denominators and numerators. This is a bit simplistic - why wasn't a meta-analysis done where multiple studies were available for a country? Did the authors consider using meta-analysis to provide a summary estimate for the region?

I'm not clear why estimates for MSM and prisoners are presented as forest plots and those for PWID are in a table. It also may be useful to include all estimates for HBsAg in one forest plot, with sub-sections for each risk group, and all estimates for ant-HCV in another. It was difficult to compare across risk groups with data presented in different formats and plots.

The search was completed in March 2015 - nearly two and a half years ago. The review may be more useful if the searches and results are updated.
Specific issues:

Given there are a lot of acronyms in this paper, I wonder if some of the less frequently used acronyms could be spelled out at each use?

Abstract: Where the authors state that highest HBsAg was found in prisoners, followed by PWID and MSM (and similar statement for anti-HCV), it seems that this ranking is based on the highest point estimate in each risk group. I'm not sure it's valid to say one group has higher prevalence on this basis, given that there is considerable overlap between HBsAg/anti-HCV estimates between groups. This is where a regional summary estimate may be useful (although I do accept that there may be considerable/too much heterogeneity for this to be meaningful), to enable direct comparison of one estimate per risk group.

Introduction:

End of first paragraph - a reference for the statement regarding the threshold for a favourable cost effectiveness ratio would be good.

HBsAg and anti-HCV should be defined at first use.

As noted above, introduction should end with a stronger rationale for why this review is important.

Methods:

The authors should justify here their decision to use only EMCDDA data to assess prevalence in PWID.

The study flow chart is useful for understanding the search process - I suggest it be included in the main paper rather than appendix materials.

Under sub-heading 'Data extraction' - I may be confused, but I think the section that reads "one article may therefore include more than one study" is possibly supposed to be the other way around - should it be one study includes more than one article?

As noted above, the approach to pooling seems simplistic. Why wasn't meta-analysis used at the country level?
Results

The results section is quite dense and difficult to follow at times. Breaking up some of the long paragraphs may help. I would also argue that it's not necessary to present every estimate in text, but could just refer to the highest/lowest countries (and a regional summary estimate, if available) and refer to Figure.

Uncertainty around estimates is not always presented - recommend that 95% confidence intervals be presented with all estimates in text.

The sub-headings that being 'The prevalence of chronic viral hepatitis' need to be changed as the HCV results relate to antibody prevalence, not chronic infection.

Discussion

This is quite a long section and could be edited down.

One paragraph that strikes as somewhat extraneous is the discussion of Hungary's results. There is overlap in the confidence intervals of HBsAg estimates for prisoners and PWID, so I'm not convinced it is that much of a difference. I'm also not that convinced by the explanation given for this difference - current harm reduction service use does not necessarily relate to prevalence of infections that have been acquired sometimes many years ago.

The paragraph beginning "Our study seeks to contribute" (line 52 of p. 15 on reviewed manuscript) includes information that could be useful for improving the rationale for the study in the Introduction.
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.

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 recommend additional statistical review

Quality of written English
Please indicate the quality of language in the manuscript:

Acceptable

Declaration of competing interests
Please complete a declaration of competing interests, considering the following questions:

1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

2. Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

3. Do you hold or are you currently applying for any patents relating to the content of the manuscript?

4. Have you received reimbursements, fees, funding, or salary from an organization that holds or has applied for patents relating to the content of the manuscript?

5. Do you have any other financial competing interests?

6. Do you have any non-financial competing interests in relation to this paper?
If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

I declare that I have no competing interests

I agree to the open peer review policy of the journal. I understand that my name will be included on my report to the authors and, if the manuscript is accepted for publication, my named report including any attachments I upload will be posted on the website along with the authors' responses. I agree for my report to be made available under an Open Access Creative Commons CC-BY license (http://creativecommons.org/licenses/by/4.0/). I understand that any comments which I do not wish to be included in my named report can be included as confidential comments to the editors, which will not be published.

I agree to the open peer review policy of the journal