Reviewer’s report

Title: Modeling HIV-HCV coinfection epidemiology in the DAA era: the road to elimination

Version: 0 Date: 21 Apr 2017

Reviewer: Peter Vickerman

Reviewer's report:

This is an interesting study considering what treatment rates are needed to achieve considerable reductions in HCV prevalence amongst different HIV-HCV co-infected risk groups in France. I think the model produces some useful insights, but I feel some of the model assumptions should be adjusted. In summary, they use a similar model structure used in two previous papers considering the impact of scaling up HCV treatment amongst HIV infected men who have sex with men (MSM). These models did not consider the HCV transmission amongst HIV uninfected MSM because evidence suggests there is little transmission of HCV in this group, and any existing transmission is likely to result from and be driven by the HIV infected MSM. However, I don't think this same model structure can be used for the other HIV infected risk groups. For instance, for people who inject drugs (PWID), although HIV infected PWID generally have higher HCV prevalence than HIV uninfected PWID, the vast majority of HCV infection is still likely to be amongst HIV uninfected PWID and they usually have high prevalence and incidence. This means that you cannot assume that the level of transmission of HCV amongst these HIV infected risk groups (other than MSM) is solely dependent on the prevalence of HCV amongst the HIV infected sub-population. Indeed, it is likely that just scaling up HCV treatment amongst HIV infected PWID, heterosexuals or other risk groups will have very little effect on reducing the incidence of new HCV infections in the HIV-infected sub-groups. Therefore, I think it would be more realistic and conservative to assume no reduction in the incidence of HCV infection in these HIV infected risk-groups (except MSM) as you reduce the prevalence of HCV in the HIV infected sub-group. Linked to this, why do they then assume that the reinfection rate is independent of the prevalence of infection in each risk group. This is probably a more suitable assumption for the non-MSM risk groups, but should be dependent on HCV prevalence for the HIV infected MSM.

Otherwise, I have the following comments:

1. When the authors discuss and present the data used to fit the model, instead of presenting raw numbers it would be more useful if they presented the prevalence of HCV infection over time in each risk group, and a comparison of the model to this data. This would aid comparisons with data from other coinfected cohorts. Also, it would be interesting to present the primary and reinfection HCV incidence (per 100 person years) used to fit the model over
time and by risk group, and the treatment rate (instead of treatment number) over time to see how it changed over the years, and how it compares to elsewhere. It would be good if some of this data was then included in the main text with the model fits included in the figures.

2. The model fitting could be described better in terms of what parameters were varied to obtain fits and which varied over time to produce the inflexions in figure 2. Also, what do the authors think caused these abrupt changes in the level of transmission shown in Figure 2, or alternatively why does the data suggest large changes in prevalence?

3. Linked to this, Table 1 should also include data on the reinfection rate for non-MSM groups, how treatment duration varied in past, and why is there no uncertainty on some of the parameters.

4. Also, why has the number of HCV infections amongst HIV infected PWID decreased over time (mentioned in the discussion) - is it because HIV infected PWID are aging cohort with few of them now injecting?

5. In the methods, they say they assume the HCV prevalence amongst HIV undiagnosed members of each risk group is 2.9%? Surely, it would be much higher in PWID and MSM than in heterosexuals and other risk groups. I think this assumption needs adjusting.

6. More details on the cohort could be added at the start of the methods or results section, similar to what would be done for an epidemiological paper. It would also help the reader to make it clear what results presented in the first section of the results are model projections and which are data from the HIV cohort.

7. Why do they assume no increase in mortality rate amongst PWID and those with HCV co-infection - both are predictors of greater mortality.

8. They state in the discussion that the HCV epidemic in MSM cannot be controlled without behaviour change interventions, but their projections suggest it can be reduced massively with just treatment - maybe this statement should be cautioned a bit.

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:

Needs some language corrections before being published

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