Reviewer's report

Title: HIV treatment outcomes among people who inject drugs in Victoria, Australia

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Reviewer: Susana Monge

Reviewer's report:

This is a very interesting work exploring treatment outcomes for people who inject drugs (PWID) in Australia. This is an important and not sufficiently explored topic, and the main strength of the study is that authors were able to cross-match the patients recruited at two clinical sites with official registries to retrieve patient's information and life status. However, there are a number of issues I think merit revision:

Major Compulsory Revisions:

As death status was cross-matched with the registries, authors need to assume that patients not appearing in the registries are death-free up to the closing date of the registry. Otherwise they risk overestimating the event-rate by only using the registries to retrieve events but not the absence of the event. Therefore, I think it is important that free-of-event censoring date be considered as the closing date of the registry (or some months before if notification delay is usually present in that registry) and not the last contact with the clinic as it appears currently.

Similarly, risk onset should be clearly communicated. Was it the date of cART initiation? The date of first contact with the clinic? The date of HIV diagnosis? Interpretation may vary in each case.

The selection criteria for each analysis and patients that were used to construct each of the tables are not totally clear. For example, it should be clear since the beginning that only patients who ever started treatment AND had information on injecting status were included in the study. I guess these are the patients finally used in the analysis (the survival uni and multivariate models), but this needs to be clear and all tables and descriptive statistics should refer only to this population. Otherwise results are difficult to interpret. Authors could use a flow-diagram of selection of patients to make this more understandable.

As there are many time points in the study and risk onset is not clear, authors should further clarify at which time points refer "initial CD4 cell count" (lines 179, 191) or "initial viral load" (line 200), and maybe at other places through the document.

Finally, given the not-so-big number of deaths and the low statistical power (adjusted HR for PWID=0.76; CI95%: 0.17-3.30), I wonder why the authors
decided to build a predictive multivariate model instead of using an estimative approach which can be more efficient and more reliable to study the effect of one particular exposure after controlling for confounding.

Discretionary Revisions:

Given the potential for a different quality of information according to the source, it could be interesting for the reader to know if the clinical record completeness and data retrieved from registries was similar for non-injectors than for PWID. Which was the percentage of deaths not present in the clinical source and retrieved from official registries in each group?

In Methods, in the sentence “The final dataset consisted of 120,113 observations and 2,980 unique individuals” (line 137), I believe the number of observations per patient relates to data management aspects and do not need to be in the article. Similar comment for “Typically, multiple lines of clinical data were available for each individual and consisted of...” (line 146).

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests