Reviewer's report

Title: Physician experience and rates of plasma HIV-1 RNA suppression among illicit drug users: An observational study

Version: 1 Date: 21 October 2011

Reviewer: Alessandro Cozzi-Lepri

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This is an interesting analysis aiming at the evaluation of the association between physician experience and virological response to therapy in a group of IVDU people in Vancouver. The manuscript is overall clear and well written, a number of questions and issues that need to be addressed by the authors follows:

Major points

1. Data seem to be fairly old. Last enrolment in 2008 and viral load measured with assays with limit of detection of 500 copies/mL. Is ACCESS a closed cohort? This needs to be clarified. If not why not updating the analysis including patients starting ART more recently? For none of the included patients, follow-up viral load was measured with an assay with a lower threshold?

2. The fact that the outcome is time to a VL<=500 copies/mL should be clarified at the onset in the Methods, replacing the generic ‘viral suppression’ (page 6). At the moment it is mentioned only in the last sentence of the Discussion.

3. It is unclear how drug interruptions have been handled in this analysis. Stopping because of virological failure (after the first 3-6 months) is a competing risk event that needs to be dealt if not in the main analysis at least in a secondary analysis. How many people interrupted because of toxicity in the physician experience group vs. less experienced? Although not a competing risk it may be interesting to show whether physicians experience affected the probability of first treatment failure (i.e. counting interruption as failures, or at least stop due to failure as failures).

4. The rationale for using the Greenland a priori model building approach is unclear. Was there a specific reason for using this technique in this dataset? What was the sequence of variable dropping? Why not showing the fully adjusted model? With 267 participants and 65% incidence should be numerically possible to fit a model with 13 covariates without problems due to overfitting. Authors should at least present the estimates adjusted for the 3 main confounders shown in table 1 in addition to methadone use: DTES residence, daily cocaine use. The fact that were not significantly associated with the outcome in univariable analysis does not exclude that could confound the association. In particular, experienced physicians were also more likely to have treated people not using cocaine daily and outside of DTES. What are the conditions (education,
socio-economic factors) of IVDU residing outside DTES?

5. Physician experience was measured uniquely using number of patients previously treated. Did the author have other potential measurements e.g. number of HIV-infected IVDU previously treated, age of the treating physicians, months since first HIV clinic of treating physicians etc.? What was the rationale for fitting this covariate solely as continuous? I would have been interesting to show the association with a categorical variable using tertiles for example. Univariable analysis should include a Kaplan-Meier plot to illustrate the median time to suppression in the 3 groups. Stratification in tertiles should be also used for Table 1, in addition to showing the median values by main characteristics.

6. From reading Table 2 some of the variables were treated as time fixed and some time-varying. Reason for the choice is unclear. Because the authors made the point that physician experience can also vary with time and they have chosen to fit the covariate as time fixed the average reader would expect one of the fitted models to be one in which all variables were included as time-fixed (and possibly one in which they were all fitted as time-dependent)?

7. What was the comparator group for the PI variable? Were people starting a PI/r included in the PI group? What about controlling for nucleoside backbone? AIDS diagnosis?

8. Numbers of participants and how many achieved a VL<=500 copies/mL should be clearly stated in abstract and results section.

Other points

1. page 6. ‘per year increase’ does not read well. I would say ‘per more recent year’.

2. Page 8. Clarify that older age of the patients (not treating physician) was associated with higher chance of viral suppression. Although the latter may be interesting to evaluate also if available.

3. Page 8. We could not find Figure 1. Maybe there was a Figure 1 in an older version which was removed and results placed in table 2?

4. Page 9. The sentence: Although our measure of adherence has been previously validated to predict plasma HIV RNA responses [10], CD4 count responses [17] and patient survival [9], it is possible that residual confounding related....seems to acknowledge a limitation of the analysis. I would move it later after the paragraph starting with: Our study has some limitations to be noted.

5. Table 2. State in the title that these are the HR associated with the time to achieving a VL<=500 copies/mL. The effect of physician experience is ‘per 100 patients greater’, age ‘per 10 years older’, CD4 ‘per 100 cells higher’, etc.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests.