Author's response to reviews

Title: Highly active antiretroviral therapy and hospital readmission: Comparison of a matched cohort

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Version: 3 Date: 23 August 2006

Author's response to reviews: see over
Dear Dr. Phillips:

RE: Highly active antiretroviral therapy and hospital readmission: Comparison of a matched cohort

Thank you for your recent correspondence inviting us to revise and resubmit the above manuscript. In the current version of the paper, we have responded to each of the reviewers’ comments and the appropriate changes to the manuscript have been made (highlighted). We provide a point-by-point response to the comments of the editor and both referees below.

Responses to Comments of Referee 1: Ellie Shoenbaum

General Comments

1. The authors address many of the issues raised by the review. One major issue that still merits discussion is the extremely high readmission rate in a setting where HAART is available. The authors mention risk group and insurance status. Do drug users have access to the general insurance? One reason for the differential results may be the novel statistical analysis and lack of bias, present in other studies, as mentioned. This is an area that should be expanded on in the discussion. Left as is, the sense is that the novel statistical approach did not lead to a particularly new finding.

Done. We recognize that readmission rates are quite high in both cohorts; we have rewritten the discussion section (pg. 12, parag. 2). Furthermore, a greater emphasis within the discussion section has been placed on the role of propensity score matching in producing our strong, unbiased results (pg. 11, parag’s 1,2). Although our statistical methodology is not related to the high rates of readmission observed in our overall cohort – these were observed in the raw data, and we believe are the result of the factors we have listed above -- we believe the magnitude of the effect of HAART (a roughly 30% decrease in the probability of hospital readmission within one year) is large and the evaluative perspective is novel and adds to the body of knowledge regarding HAART for patients with HIV/AIDS. We hope our greater emphasis on this methodology will highlight these attributes of our study.
2. The paper’s approach is too broad, including the cost-benefit analysis, the HAART effects and lastly the methodology. Honing in on the methodology and how it may relate to differential results is the inherent potential strength of the paper.

Done. The reviewer’s point is well taken; to address this final point, we have removed discussion of the cost analysis and comparison of length of stay in pg. 2, parag. 3, pg. 7, parag. 1/2, pg. 10, parag. 3, pg. 11, parag. 3 (last submitted version), and added further discussion on the impact of the statistical methodology in the discussion section (pg. 11, parag 1). We believe this highlights the focal analysis of the paper – namingly, the propensity score matching methodology, and results of the matched cohort comparison – while not significantly taking away from the overall message of the paper.

Responses to Comments of Referee 2: Reto Neusch

Major Compulsory Revisions

1. One major issue is the lack of information on CD4 counts. The authors argue that the high correlation between CD4 count and HAART use makes them an inappropriate variable. However, hospital resource use, one of the main issues of this investigation, is certainly associated with CD4 counts. This is particularly so in this cohort with a high rate of readmission for AIDS diagnosis, also in the HAART group. I think that this limitation should be discussed with more details.

Done. We agree that this limitation does require more thorough treatment. After careful deliberation, we have revised the discussion to read as follows: “First, CD4 count at index admission was not controlled for in the baseline analysis. The causal relationship between CD4 and HAART initiation is unclear and time of HAART initiation in our population is unknown. In addition, the objective of HAART therapy is to increase CD4 count, and thus reduce the onset of AIDS-defining illnesses and other disease which require inpatient care. CD4 count is therefore an intermediate variable in the causal pathway between the exposure (HAART) and outcome (hospital readmission); inclusion of this covariate would constitute one type of overmatching, which is to be avoided [29]”; we refer to Rothman and Greenland’s text on this final point. We hope that this explanation is sufficiently clear to the reviewers and prospective readers.

Finally, we believe we have significantly improved the paper in making the changes suggested by the reviewers, and we thank them for their useful comments. We hope that you find our responses satisfactory and agree to publish our manuscript.

Best Regards,

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