Author's response to reviews

Title: Gender distribution of adult patients on highly active antiretroviral therapy (HAART) in Southern Africa: a systematic review.

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Responses to reviewer Carmen Zorilla’s comments

We have further explained how it may be that women may not be disadvantaged compared to men in accessing HAART in Africa. For instance, we have made references to the role that prevention of mother to child transmission could have on ensuring that women have access to voluntary HIV testing and counseling, care and support. We have also made some references to the usual access to health services that women, by default have, through maternity and family planning services as well as under five services.

The reviewer also mentioned the first sentence in the conclusion section was not clear. She had suggested an alternative. We have changed the sentence to make the though clearer and we have incorporated the suggestion.

Responses to reviewer Lalit Dandona

The reviewer reported that the paper was useful but rather too simplistic. We have improved to make the manuscript still clear and simple but also attend to this concern. We have for instance revised table 2 so that it is more informative by referencing it well. We have also spent more time on the discussion to suggest the possible explanations for the observed pattern. The discussion has therefore been revised to retain the portions that better explain the intention of the study while removing or clarifying the areas of concern. We have taken in the idea of incorporating the MOOSE Guidelines as suggested by the reviewer. A new reference has been included which is essentially an outline of the MOOSE guidelines: Stroup DF, Berlin JA, Morton SC, et al: Metanalysis of observational studies in epidemiology: a proposal for reporting. JAMA 2000; 283: 2008-12. We have clearly outlined our search strategy, exclusion criteria, and period of review (eligibility of studies) and the strengths and weaknesses of the data. However, we are not conducting a meta-analysis with the aim of calculating a single measure, forest plots etc; so some areas of the MOOSE guidelines do not strictly apply. We are thankful for this advice from the reviewer.

The reviewer suggested that there may be double counting of some of the studies as some were cited as located in a national setting. We would agree with this observation although strictly it is not double counting. We will illustrate what we did with the situation in South Africa. In this setting we have data from the provinces e.g. Eastern Cape and Northern Cape as well as National. The patients in the National data will include those in the provinces. We find these data useful in a sense that the pattern being shown at a provincial level is
maintained at the national level when other provinces are also included. However, as there was no tagging of patients and there is loss of follow-up, it is also clear that some patients in the provincial data done at a different time may not be alive or still in the cohort when the national data are compiled. In this regard therefore, the individual reports have been treated as separate when the gender pattern is the intention of analysis. However, if we were to calculate the total number of patients on HAART, we would need to find a way to separate the two. The reviewer also referred to Table 1 and stated that the number reported in Table 1 are not prevalence but “number of females and males with HIV”. We have now changed the table to reflect that these are the numbers of adult females and adult males with HIV rather than suggesting there are proportions. We have also changed the calculation error in the female: male proportion in the specific case of Namibia.

The estimates that are reported in Table 1 were obtained from the websites whose URL are indicated at the bottom of the Table. These are 2005 estimates and could be a closer reflection of the time the HAART numbers were obtained.

Table 2 has now being incorporated into the manuscript rather than being submitted as an extra file.

Responses to reviewer Lisa Hirschorn

The reviewer suggested that there was need for more background on history of disparities in accessing ART in women in resource richer settings, any data which may be available for gender and access in other health care systems in RLS, and more sophisticated use of the available data. We have since added reference to gender disparities in other settings to set the stage for the need to look also at gender disparity with regard to HAART.

The reviewer also suggested that in the abstract, we presented significant concerns about women accessing HAART, however in the published literature, it is men who seem to be having the potential challenges. Addressing this issue as gender equity rather than decreased access for women would strengthen the entire article. This could be further followed by exploring in the discussion potential reasons why this program as opposed to others have seen gender disparity in the opposite direction than expected or than seen in other programs in similar resourced settings.

We have addressed this concern in the Discussion section of the manuscript by suggesting what could be going on to obtain these disparities as we found. We have also strengthened the background section so as to set the scene for the motivation of the study.

The reviewer also suggested exploratory univariate analyses to determine where males have the least disparity in care, including national ratios, country age of program, program type (pay or not), etc. The number of programs would limit the ability for multivariate modeling, but this should be a potential future direction.
We agree that this would an important future line of work. The results to be obtained from a small sample size would be less stable and interpretations may be overstretched. At this time, we would suggest that we refrain from doing that with the data that we have.

The reviewer also suggested that the discussion would be strengthened by addressing other potential reasons for higher access by women, which may be due to the expansion of PMTCT programs which enable women to be tested and so more easily achieve the first step towards accessing HIV. Similarly any information about gender differences in stigma or other reasons of decreased male access would be important.

We thank the reviewer for these comments. We have attended to this concern in the Discussion section of the manuscript.

With reference to table 2, the comment was that “The table should be made more useful. There is no clear order beyond clustering by country in which the studies are presented (?by date of completion, but not consistent), and one date is missing. In addition the table would be considerably strengthened by adding columns describing if these were pay or free, adjusted ratio based on the national gender ratios. We have presented the studies in an orderly way. We have also reported on whether the treatment programs were for free or paying.

Table 2 was missing the dates for the study “Bisson et al” study: We have since corrected this error.

Adamson muula