Author's response to reviews

Title: Health and socioeconomic impact of HIV/AIDS on South African households: cohort study

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The Editors
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Dear Sir/Madam

Re: Health and socioeconomic impact of HIV/AIDS on South African households: cohort study

We wish to resubmit this paper, having revised it in the light of the referees' comments. Our amendments are as follows:

Tony Barnett's comments

We agree with Prof Barnett's comment that the sociological dimension of HIV impact has not been studied or theorised to the extent that economic aspects have. However, as this study has focused on the economic aspects of the impact of HIV on households, we feel it would be inappropriate here to try to advance the relevant sociological theory, which would lead us into speculation that is unsupported by our data. Therefore, we have focused more clearly on the economics, changing the title to "Health and economic impact of HIV/AIDS....", and using the term "economic" rather than "socioeconomic" throughout.

The possibility that the neighbouring households we call "unaffected" may indeed have had household members infected with HIV was explicitly stated in the Methods and Discussion. We have now stressed further in the Discussion that few, if any, households in South Africa are completely unaffected by the HIV/AIDS epidemic.

Mark Colvin's comments

1. Unfortunately we do not have a record of the number of potential subjects who were invited to take part in the study, and so cannot specify the response rates. We discuss this limitation of the study in the Discussion.
2. The sampling protocol had no more detail than to specify that the household that was physically closest to each affected household should be selected as a neighbouring household. In the
Discussion, we discuss how this limitation may have introduced sampling bias.

3. In the Discussion we say that excluding neighbouring households with a member currently or recently having TB may have exaggerated the differences between affected and unaffected households. However we believe this to be a lesser evil compared to probably misclassifying households with TB or pneumonia, that were very likely to have HIV, as unaffected. We have however taken up the referee's suggestion of performing secondary analyses after excluding all households reporting any TB or pneumonia at baseline. As we report in the Results and discuss in Discussion, this suggests that the comparisons of affected and unaffected households was indeed biased, but very slightly so, and does not substantially affect the results of the study.

4. We obtained verbal consent. One of us (F Booysen, in reference 6) has previously commented, in a review of 36 such studies, that only one of the 36 reported obtaining any consent at all from HIV-positive participants.

5. In the Discussion (under limitations) we explain why affected individuals within households were not identified.

6. In the Discussion, we point out that the sampling frame probably increased the proportion of HIV infected individuals who were symptomatic, compared to the total HIV positive population.

7. In the Discussion, we point out the similar distribution of morbidity to other published South African case series.

8a. This is clarified and referenced.
8b. The questionnaire was not back-translated, so we do not state this.
8c. This sentence has been reworded.
8d. This is stated more clearly.
8e. In the Results, we state that "we did found a higher dependency ratio in affected households, although this was not statistically significant".
8f. This is clarified.

We are grateful to and acknowledge the referees for their helpful comments, which have helped improve the paper.

Yours sincerely

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