Reviewer's report

Title: Delayed consultation among pulmonary tuberculosis patients: a cross sectional study of 10 DOTS districts of Ethiopia.

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Reviewer: Bernt Lindtjorn

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The main question I have raised is if selection bias in a study should have implications for how the researchers deal with their results.

The authors state the HIV prevalence in Tigrai is high, and that a TB-HIV co-infection rate of 45% might be representative for the region. They do not provide evidence for this statement. In 2006, The Ministry of Health in Ethiopia reported HIV prevalences of 13.31% in urban areas and 1.94% in the rural areas of Tigrai (http://www.etharc.org/tigray/aidsinethfindings.htm). We have shown that variances in HIV prevalence rates might result in different TB-HIV co-infection rates (See Datiko DG et al. BMC Public Health 2008, Jul 30;8(1):266).

The authors state, and write there is a selection bias. However, as the DOT programme advice passive case detection, selection bias may not be important.

I disagree with the authors’ statement. If they carry out epidemiological research, they should discuss the science, and not let their discussions be influenced by possible programmatic pragmatism.

I believe that this paper contains new information that is important for tuberculosis control. Unfortunately, the authors fail to discuss their important findings within the limitations of this study.

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