Reviewer's report

**Title:** Factors associated with the low cure rate of tuberculosis in the remote poor areas of ShaanXi Province, China: a case control study

**Version:** 1  **Date:** 22 October 2009

**Reviewer:** PENG WU

Reviewer's report:

The authors conducted an interesting study to investigate potential factors associated with relatively lower cure rate of tuberculosis in the remote areas of ShaanXi Province, China. The authors used a case-control design and multivariate analysis to explore the possible predictors of non-cure outcome from patient-related and treatment-related factors. They concluded that interruption of treatment and co-morbidity were the most important factors associated with non-cure of tuberculosis patients, and absence of a treatment supervisor was an independent risk factor.

Here are some concerns about the study, including some major and minor revisions suggested:

1. Major compulsory revisions

   (1) Many previous studies mentioned alcohol is also a risk factor affecting the outcome of tuberculosis treatment. Drinking should be popular in northern China where the study was carried out. Have the authors ever considered it in the study? And why was it not included in the study? In addition to that, would the clinical type of tuberculosis and/or the degree of relief in disease symptoms affect the outcome of treatment?

   (2) The authors mentioned in the “Methods (Sampling)”, “To increase the number if non-cured patients, we additionally included all uncured patients registered from June 1, 2006 to March 31, 2007”. Would they like to explain the reason to do this instead of choosing the exactly same time period for recruiting cases and controls? Would this extra recruitment of non-cured patients affect the whole study design and results?

   (3) The authors suggested “to make it possible for family members performing supervision in remote areas where logistically it is difficult to have village doctors perform treatment supervision” in the conclusions, I believe, which was resulted from numbers in Table 4. However, it’s not appropriate to say “no difference in cure rates were observed for patients with village doctors or family member as a supervisor” based on the numbers in Table 4. Please reconsider the interpretation of the results.

2. Minor essential revisions

   (1) There seems to be some mistakes or incorrect alignment of in Table 1, 2 and
especially 4, particularly the columns for P values. Please check them up and try to make the tables readable and understandable.

(2) For table 3, why did the authors only choose to test the relationship between co-morbidity and age and between side effects and age? And why the cut-off point for age grouping is different here from those in the table 1? Are there any special reasons for age group categorization?

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

**Quality of written English:** Needs some language corrections before being published

**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.