Reviewer's report

Title: Tuberculosis among health care workers in KwaZulu-Natal, South Africa: a retrospective cohort analysis

Version: 2  Date: 4 March 2014

Reviewer: Rodney Ehrlich

Reviewer's report:

Tuberculosis among health care workers in Kwa-Zulu-Natal, South Africa: a retrospective cohort analysis.

General

Given the size of the HIV-TB co-epidemic in South Africa and rising TB drug resistance, information which can contribute to defining risk among health care workers is valuable.

However, I have a few concerns about study design, analysis and measurement.

Major compulsory revisions

1. There seem to be two study objectives.

a. One objective was to estimate incidence of TB in this population and to compare it to that of the general population. Regarding the numerator, more detailed description is needed of how complete TB case ascertainment was in the employee occupational health records, and whether selection bias might operate at this level. For example, what proportion of the medical (doctor) staff report their TB to the occupational health service? Similarly, with regard to administrative and transport staff, etc.

   Regarding the denominator, more information is needed on how the total number of HCWs employed each year was calculated. Of additional interest would be staff turnover, as risk may vary by whether the staff establishment is stable or changing each year.

   Using the provincial incidence rates to calculate IRRs is a reasonable start, but it needs to be acknowledged that it is a crude comparison, unadjusted for potential confounding such as socioeconomic status.

b. The other objective was to model relative risks for TB disease, including having worked in at TB ward. I had some difficulty understanding the study design and Poisson modelling in this part of the study. As far as I interpret the study, there is no underlying cohort but rather the records collected by the occupational health service over 5 years (n = 1,313). Within this sample, 112 (9%) had a TB diagnosis during this period. This sounds more like a period prevalence. Cross-tabulating by ward would then be a cross-sectional analysis.
The authors need to present a clearer justification of why this is a cohort analysis qualifying for Poisson modelling. This would include a detailed description of how occupational health records are collected.

2. The authors do not mention any further objectives under their Introduction, but present information (e.g. Tables 2 and 3) on drug susceptibility and treatment outcomes. This information does not add anything to the first two objectives and could be dropped and made the subject of a separate communication.

3. Table 4 presents some difficulties of interpretation. The reference value (or counterfactual) for working in any ward is not having worked there, i.e. having worked in other wards. Yet all relative risks, including the presumably low risk stores/workshop, are elevated (except for the MDR-TB ward). The RRs would be easier to interpret if they were expressed in relation to a reference area known to be at low risk of nosocomial TB infection, which intuitively is the stores/workshop area.

Minor essential revisions (requiring action or consideration by the authors)

1. All the factors controlled in the regression should be listed.

2. “TB ward” should be defined, including whether it is the same as MDR-TB ward.

3. It is worth noting that the racial demography of the staff with occupational health records (Table 1) does not reflect provincial demography. Is there a reason for this?

4. The lower risk in MDR-TB wards may a reverse causation effect of high perceived risk inducing strict precautionary behaviour.

5. Recommendations

a. What is the practicality of all staff undergoing “routine symptom screening”?

b. Similarly, when would/should staff in general/paediatric wards wear N95s?

c. The authors should say more about the difficulties and likelihood of universal respiratory precautions in busy hospitals in this setting.

6. The findings support universal tuberculosis precautions in such hospitals as it is not possible to localise elevated risk. This is an important conclusion which needs a more explicit statement as risk stratification is being emphasised in local TB policies. The authors would contribute to this debate, and perhaps save misdirected effort, by entering it more explicitly.

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

**Quality of written English:** Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests.