Author’s response to reviews

Title: Current tuberculin reactivity of schoolchildren in the Central African Republic

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Author’s response to reviews: see over
The Editor

BMC Public Health

Dear Editor

Thank you for sending us the reviews of our manuscript 7934320161225266 “Current tuberculin reactivity in schoolchildren in the Central African Republic”, by Fanny Minime-Lingoupou, Rock Ouambita-Mabo, Aristide-Désiré Komangoya-Nzozo, Dominique Senekian, Lucien Bate, François Yango, Bachir Nambea and Alexandre Manirakiza.

We have taken account of all the comments in revising this manuscript, which is attached.

With best wishes

Alexandre Manirakiza
Reviewer's report

Title: Current tuberculin reactivity of schoolchildren in the Central African Republic

Version: 2 Date: 9 April 2015

Reviewer: Keren Middelkoop

Reviewer's report:

Major comments

Abstract:

1) Previous comment: Results: Authors should present the prevalence of TST positivity/TB infection for each of the cut-offs assessed. While the authors addressed this comment for TST positivity, they have not done so for the TST response by BCG status or ARI – while it may not be necessary to report these results for each cut-off, the authors should note the cut-off used for those values given.

Authors’ response: These questions have been answered.

2) The flow in the revised results section is confusing: I would present prevalence by TST cut-offs, then by BCG status.

Authors’ response: These changes have been made.

Introduction:

1) Previous comment: In the first sentence the authors state that children are the source of spread of TB in a community: given the pauci-bacillary nature of childhood TB disease, this is not accurate. It is generally accepted that adults are mainly responsible for transmission; children are a vulnerable population for infection and disease, and are a good sentinel for TB transmission rates in a community.

The authors have simply cut and paste my comment (which was meant to be explanatory, rather than a suggestion for the text) and attributed it to their original reference. This is not appropriate.

Authors’ response: These changes have been made.

Methods:

1) Previous comment: The sample size calculation information does not say what prevalence the authors were anticipating in the calculation. This comment referred to the expected prevalence of TB infection (the outcome measured in the study). This remains unaddressed by the authors.

Authors’ response: These changes have been made.

Discussion:

1) Paragraph 1: The authors state that “BCG vaccination status had no effect on tuberculin reactivity…”, but this was only true at #15mm cut-off.
Authors’ response: These questions have been answered.

2) Previous comment: The authors note that their reported TB infection rate is lower than that of the previous study in this country, but they have not attempted to explain why this is.

a. What cut-off was used in the 1988 study?

Authors’ response: These questions have been answered.

b. What is the confidence interval around this estimate and the 1988 estimate? The authors note that they have deleted this statement. I do not think that is an appropriate response to the comment. As the discussion now stands, there is no reflection on how the results may differ/or not from previous studies in the country, or in the region. There is no discussion on the factors that may be contributing to the study findings and any changes in prevalence/ARI from previous surveys. The discussion fails to contextualise or discuss any significance of the TB infection prevalence or ARI findings.

Authors’ response: These questions have been answered.

c. Previous comment: Given the Central African Republic status as a high burdened HIV country, what do the authors estimate may be the HIV prevalence in the study sample? Given the anergy to TST noted in HIV positive patients, may this have contributed to the lower than anticipated TB prevalence rate among the study sample? The authors’ response to this comment is “The table has been deleted”. I feel the authors’ may have misunderstood the comment: I am suggesting that it would be useful in interpreting the study results to understand how great a role HIV is likely to be playing in the TST responsiveness. Information on current MTCT programmes is not helpful in this regard; data on MTCT during the cohort’s birth years is required, and, based on that coverage and the HIV prevalence they report in pregnant women over that time, what proportion of this group of children is likely to be HIV positive? The data from co-infected children in hospital, as added by the authors, is not particularly helpful as this is a select, unrepresentative group. Do the authors think that HIV may have had a substantial impact on the study findings, or do they think this unlikely to have introduced a substantial bias?

Authors’ response: These questions have been answered.

d. Previous comment: Do the authors think the lower than anticipated TB infection prevalence/ARTI reflect the declining TB rates reported for the country by the WHO? See my comment above: this has not been addressed.

Authors’ response: These questions have been answered.

Minor comments

Methods:

1) Previous comment: A description of the two health districts should be provided– is HR1 a rural district, for example compared to the rest of the country? This comment has only been partially addressed by the authors. If no further information is available, they could let us know this in their response to comments.

Authors’ response: These questions have been answered.

2) “prior BCG vaccination as reported by parents or guardians and recorded on immunization cards” Do the authors mean “or as recorded on immunization cards”? As it current reads it seems
that documented proof in form of the immunization cards was required for each child. Is that correct?

Authors’ response: These questions have been answered.

3) Previous comment: The last sentence of the first paragraph of the Data Analysis section is very confusing (“The reaction induration size…..”). Please could the authors re-word this sentence. Despite the author’s comment that this has been amended, only the word “latent” has been included. I still find this sentence poorly written and confusing.

Authors’ response: These changes have been made.

Results:

1) p5, paragraph 1: I recommend the authors clarify the following sentence: “The proportions of children absent at TST administration were 8.5%....”

Authors’ response: These changes have been made.

2) p6, paragraph 3: At what cut-off were these findings noted: “Negative reactions to the TST were found in 71.7% (95% CI, 68.3–75.3%) of BCG-vaccinated children and 82.9% (95% CI, 74.1–91.4%) of non-vaccinated children.”

Authors’ response: These questions have been answered.

Discussion:

1) First sentence: again the authors have not stated which cut-off they are using for the ARI reported.

Authors’ response: These questions have been answered.

2) Previous comment: The authors do pick up on the lack of HIV testing as a limitation to this study, but more information could be provided: what has the PMTCT coverage been like over the birth years of this cohort in these areas; as mentioned above, some estimate of how many of the participants are likely to be infected would be helpful. - see my comment above (Discussion point 2c)

Authors’ response: These questions have been answered.

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