Author's response to reviews

Title: Health-service performance of TB treatment for indigenous and non-indigenous populations in Brazil: A cross-sectional study

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Author's response to reviews:

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Christopher Morrey
BioMed Central Editorial

Dear Editor:

We appreciate your consideration of our work. We are submitting the revised version of the manuscript, “Health-service performance of TB treatment for indigenous and non-indigenous populations in Brazil: A cross-sectional study” (MS: 2018415531852025). We have addressed the reviewers’ comments in the responses below.

Reviewers’ Critiques:

Reviewer: 1

Major compulsory revisions

1. Given the decentralization of TB services in Brazil, the research question posed in this manuscript (i.e., to evaluate the performance of two different TB services in Dourados) appears important. I would however like to have more detail provided in the Introduction section leading to why this particular study was needed. For instance, in the Introduction, paragraph 2, the authors state that: “This service must be improved to provide satisfactory access to early diagnosis, reduce the incidence of TB, and improve treatment compliance”. However, the evidence suggesting the current service is unsatisfactory is not explicit.

Author's reply: We provide more detail with regard to the decentralization of TB services in Brazil in the Introduction. The decentralization strategy aimed to offer...
health services related to disease diagnosis and to institute the DOT for all TB patients. In recent years, many efforts have been made to increase the coverage of PHC. Although 50% of Brazilians have coverage for primary care, the majority of diagnoses of tuberculosis are performed in the emergency department or in secondary and tertiary hospitals (Lines 59-68).

2. Further to this point, the aim of the paper is to assess performance, however what aspects of “performance” are needed to be assessed in this context? The authors state in paragraph 5 that the justification for this cross-sectional study was to overcome the shortcomings of the current retrospective study by Croda et al. However, as written, whether the two studies are measuring the same factors associated with health service “performance” assessment is not entirely clear.

Author’s reply: We have rewritten this paragraph in the introduction and have attempted to clarify the differences between the two studies (Lines 85-97). The secondary database used here is also used for epidemiological surveillance. Therefore, the results found in the initial study (Croda et al., 2012) are limited by the absence of important variables related to treatment default, as described in the literature, such as social characteristics, smoking, malnutrition, housing conditions, intravenous drug use, socioeconomic status, and access to health services. Another relevant limitation is the incompleteness of some of the information, such as the number of medical appointment and time required to establish the diagnosis of tuberculosis.

3. Can the authors please comment as to why the PCAT was chosen for this study, and how culturally appropriate this instrument is for the indigenous cohort?

Author’s reply: The instrument is the only one that has been validated in Brazil (Villa and Ruffino Neto, 2009) to assess the performance of health services in caring for tuberculosis. Therefore, it was chosen despite the cultural barriers related to the indigenous population (Lines 143-150). This limitation was also highlighted in the Discussion section (Lines 291-302).

4. Can the authors please comment as to whether the analysis of the PCAT performed in this study is the same/similar to the reference 14, and provide rationale for any deviation.

Author’s reply: We have included this information in the Methods section (Lines 181-185). The reference that supports this analysis was published in the journal BMC Health Service Research (2011).

5. Was the present study adequately powered to detect differences between the cohorts?

Author’s reply: The Methods section provides the calculation of the sample, with a statistical power of 0.9 (Lines 123-125).

6. The results section needs substantial rework. In particular it is not always clear where the results presented have come from and/or which table they pertain to.
For instance:

Results paragraph 3 states that “…diagnoses were mostly performed at specialized services…”

However, it is unclear where this information has come from.

Author’s reply: The data presented in paragraph 3 have been included in Table 1.

7. In this same paragraph, the results regarding delayed diagnosis (>3 appointments) and over 5 weeks do not match the results presented in Table 2.

Author's reply: We have reviewed the table numbers in the text (Lines 222-228).

8. Furthermore, the statement:

“… more likely to receive social support…when the performance of DOTS was emphasized (Table 2)” is not clearly represented in the tables.

Author's reply: We have corrected this information in the text. This information is presented in Table 4 and not in Table 2.

9. In the final paragraph, which data supports the statement:

“…Despite having better access to diagnosis and treatment, the indigenous patient…”

I.e., it is not clear how better access to what type of diagnosis and which type of treatment has been determined from this study.

Author’s reply: Revised as suggested. The sentence was deleted, and this finding has been introduced in the Discussion section (Lines 303-308).

9. Better reference to the tables throughout the text of the results section would improve this section substantially….”

Author’s reply: We have reviewed all references to the tables throughout the text.

10. The discussion section needs to be reworked. In particular, it is not always clear how the data is supports the statements made. For instance:

Paragraph 3, non-indigenous patients did not receive treatment directly observed, which may be responsible for the high default rates [ref10].

Which high default rates are the authors referring to and how do the results of this study explain high default rates?

In paragraph 3, the authors compare their study results to that of Amaral et al. In particular, the “expected results for the municipality”. It is not clear what was expected and how the current study is placed with that of Amaral et al’s..

Author’s reply: We have rewritten paragraph 3 and have attempted to clarify these two specific questions (Lines 252-262).

11. Paragraph 5 is not placed within the current literature and the interpretation of
this finding particularly with respect to health service delivery could be expanded.

Author’s reply: The paragraph was deleted, and the discussion was expanded with regard to health service delivery.

12. Paragraph 6 raises the point about “satisfactory” service. How is this judged?

Author’s reply: We have rewritten this sentence. In Brazil, it is recommended that the investigation of HIV should be provided to 100% of TB patients (Line 279-280).

13. Paragraph 7, how was service quality determined? What does a quality service entail, as judged in this study?

Author’s reply: This sentence was deleted.

14. Paragraph 8 refers to the cultural differences in medicine-disease perceptions between indigenous and non-indigenous groups. As stated above, to what extent would this be expected to influence the survey results, or asked another way, how relevant is the PCAT to this population?

Author’s reply: The instrument is the only one validated in Brazil (Villa and Ruffino Neto, 2009) to assess the performance of health services for tuberculosis. Therefore, it was chosen despite the cultural barriers related to the indigenous population (Lines 143-150). This limitation was also highlighted in the Discussion section (Lines 291-302).

15. Paragraph 9, a statement about statistical power is needed in the limitations.

Author’s reply: We have introduced this statement in the Methods section (statistical power of 0.9) (Lines 123-125).

16. Paragraph 10: it is not clearly stated how the results of this study can be extended to other “neglected” populations. For instance, what messages are most important, what could they take away from the study etc.

Author’s reply: We have deleted this sentence and modified the main conclusion at the end of the Discussion (Lines 303-308).

17. The conclusions are quite well written, but the results and discussion sections, as written, have not led convincingly to these conclusions.

Note: The abstract and title are clear.

Author’s reply: We have modified the Results and Discussion sections.

Minor compulsory revisions

18. Could the authors please clarify if abandonment is the same as default? I would suggest using one term throughout if they are the same.

Author’s reply: Revised as suggested. We have standardized the text to “default” (Lines 81, 84, 88, 202, 257, 259).
19. Data collection Tool, paragraph 1: could the authors please state the full term for PHC.

Author’s reply: Revised as suggested (Line 63).

20. Data collection tool, final paragraph: could the authors please confirm how many legal representatives answered the questionnaire and if there were any differences in the results between those self-responding versus those who didn’t.

Author’s reply: We have introduced this information (Lines 205-209). The internal consistency, checked by Cronbach’s alpha, showed a little difference between those who self-responded and those who did not (0.80 vs. 0.63 respectively).

21. Data analysis, paragraph 1: could the authors please clarify why the data was entered twice and what did this approach achieve or reveal?

Author’s reply: We have clarified this information (Lines 166-167).

22. Table 1: the term agglomeration needs better definition (eg: persons per room)

Author’s reply: We have introduced this information in the footnote of Table 1.

23. Discussion paragraph 2: Can this paragraph be expanded? It seems a little superficial, and the significance of the finding is not discussed, nor clear.

Author’s reply: We have expanded this paragraph (Lines 245-251).

24. Discussion paragraph 3: Presumable TDO is the same as DOT? Could the authors confirm and use one term only.

Author’s reply: We have corrected the term TDO to DOT (Lines 42, 45, 61, 228, 237, 308).

Reviewer: 2

1. Despite of the clear and well presented data, information related to the final outcome of patients is lacking, how many of the 109 patients abandon?, how many have a treatment fail? This information will give the manuscript a context related to the general performance of the tuberculosis control program in this area.

Author’s reply: Revised as suggested (Lines 204-207).

2. I may suggest to authors to add a map of Brazil and locate Dourados, this for potential readers of the manuscript non familiar to the country geography.

Author’s reply: Revised as suggested. We have introduced Figure 1.