PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	PROVIDER PERSPECTIVES OF THE INTRODUCTION AND IMPLEMENTATION OF CARE FOR DRUG RESISTANT TUBERCULOSIS PATIENTS IN DISTRICT LEVEL FACILITIES IN SOUTH AFRICA: A QUALITATIVE STUDY
AUTHORS	Vanleeuw, Lieve; Atkins, Salla; Zembe-Mkabile, Wanga; Loveday, Marian

VERSION 1 – REVIEW

REVIEWER	Rafael Van den Bergh Médecins Sans Frontières Belgium
REVIEW RETURNED	27-Jul-2019

GENERAL COMMENTS	Thank you for the opportunity to review this interesting paper, which identifies important concerns with the decentralisation and deinstitutionalisation process of DR-TB care in South Africa. The paper clearly situates the problem and research question, and the methodology is sound, and I recommend it for publication. I have a small number of suggestions which may help the authors improve the paper further. 1) Out of the two main changes in DR-TB care (decentralisation and deinstitutionalisation), the authors choose to focus on decentralisation. However, the examples given in the introduction speak more of deinstitutionalisation (such as the ambulatory treatment in Peru and Vietnam, and also the KZN and WC papers, which speak more of hospitalised care versus ambulatory or home-based care, with the exception of the Loveday paper). It may be helpful to highlight in the introduction why a focus was placed on decentralisation as research topic, to avoid raising incorrect expectations with the reader. 2) I would recommend to explain how the selection of provinces was conducted. It seems that the provinces of the study were those which were the least far in implementation of the new care model, and were still in semi-centralisation and/or in preparation of decentralisation. Was this deliberate, and may it have affected the results? 3) In the methods, it would be good to describe the relationship of the researchers to the participants: were there any power dynamics, or possible associations between the researchers and the management of the health programmes? Or could the researchers be seen as a neutral party?

	4) I would recommend to describe shortly where the interviews were conducted - conducting them in the concerned facilities, or in a more neutral setting can affect the responses. 5) The sampling process is not fully clear: purposive sampling was used, which is indeed appropriate. However, who performed the final selection of HCW - were lists of staff provided to the researchers, from which they selected participants, or was someone from the staff involved in designating specific participants? 6) Are there risks for identification of participants? Since the TB coordinators were involved in selecting the facilities, and some specific incidents are referred to (such as "death on arrival" of 3 referred cases), could this allow indirect identification of the participants? 7) It may be useful to clarify why repeat interviews were done with some participants - for clarification purposes, or simply for time management? 8) In the discussion, the authors frame the results largely in the light of "resistance" and of "the human factor" of change management. While this may well be true, I am not convinced this is fully supported by the observations. The quotes mainly suggest a lack of training and of engagement of the central management towards the decentralised sites; to label this as resistance may be somewhat reductive, and could mask genuine concerns about the training, management, supply, and general support to the new sites. While it is fair to speculate that the respondents may have shown resistance to change, it should be made clear that this is speculation on the side of the authors, and more detail should be provided on the provisions that were or were not made by the central management for support of the decentralised activities, in order to represent a more balanced and objective interpretation of the results. 9) The study limitations section focuses only on the generalisability of the findings, but I would suggest (depnding on the replies to my comments above) to also touch on issues such as possible bias in the sampling process.

REVIEWER	Kaspar Wyss Swiss TPH, Switzerland
REVIEW RETURNED	05-Sep-2019

GENERAL COMMENTS	Considering the persisting importance of MDR TB in South Africa, the manuscript investigates a relevant public health topic in selected provinces in South Africa. Consequently the theme of the manuscript is of general interest. This said, the manuscript has substantial shortcomings. 1. Key results emerging from the analysis, namely weak communication on changes in health delivery arrangements, have as the authors indicate in the introduction and discussion sections been document for other countries both for TB control and other disease control /health systems strengthening initiatives. Consequently what is new scientific evidence emerging from the study beyond the direct documentation of the specific Southern African setting? The manuscript would benefit if results are

	discussed and concluded in the light of new scientific evidence of importance when introducing new TB or disease control interventions. 2. The manuscript is limited to presenting the perspective of health workers and “facility staff”. At the same time the methods indicate that the study included TB coordinators and others cadres as this would dilute the focus of the manuscript. Given that a main conclusion of the manuscript is the need for improved communication and consultation with frontline providers it seems beneficial and important to include also the perspective (and the constraints) of those who are outlined to assure this. In other words the manuscript potentially gains in depth and richness if the perspective, opinion, and difficulties of managers and key staff of the DR-TB program and the general health service managers in delivering and assuring the warranted activities (triangulation of positions). Any possibility based on the available interviews / data to bring in the perspective of the DR-TB program and the general health service managers? 3. The state objective of the manuscript is among else to analysis effects of decentralisation and deinstitutionalisation on quality of care provided to DR-TB patients. At the same time the results do not present any data or evidence on effects of the decentralisation of DR-TB services. Quality of care only remerges as a theme in the discussion and conclusion sections where general statements are made around the impact of integration on quality of care. Consequently please either alter the stated objectives of the research or include data/information on changes in quality of care in the result section. If the second is done then it is also necessary to provide a distinct definition of quality of care given that different authors use different ways into analysing quality of care. In addition to these three major observations, the following comments: - In the introduction it would beneficial to be more specific on health service changes introduced going along the decentralisation of DR-TB services. For example in one quote it is indicated that “quick trainings” were offered to health workers. For the contextual under-standing of the reader it would be beneficial what activities have been outlined and implemented along the organisational changes for DR-TB services. - On page 9 it is indicated “Healthcare workers in all four study provinces remarked that communication and consultation had been”. Given that the main focus of the manuscript gravitates around this aspects, it would useful to better dissect the terms “communication” and “consultation”. Who is meant to communicate with whom, how, using which channels, when and how frequently? - Table 1 has the potential to be presented more concisely. For example the four provinces could be listed in the columns and the number of interviewees then could easily be summed up by professional category along the four provinces

VERSION 1 – AUTHOR RESPONSE

Comment	Response
Reviewer: 1 Reviewer Name: Rafael Van den Bergh Institution and Country: Médecins Sans Frontières Belgium	Dear Mr Van den Bergh, thank you for reviewing this manuscript and for your thoughtful and helpful comments.
1) Out of the two main changes in DR-TB care (decentralisation and deinstitutionalisation), the authors choose to focus on decentralisation. However, the examples given in the introduction speak more of deinstitutionalisation (such as the ambulatory treatment in Peru and Vietnam, and also the KZN and WC papers, which speak more of hospitalised care versus ambulatory or home-based care, with the exception of the Loveday paper). It may be helpful to highlight in the introduction why a focus was placed on decentralisation as research topic, to avoid raising incorrect expectations with the reader.	We have revised the last paragraph of the "background" section on page 5 of the clean and marked copy to more clearly reflect that the focus of our paper is on the introduction of DR-TB care in local, community-based facilities i.e. ambulatory healthcare instead of hospitalisation. Therefore, the change deals with both deinstitutionalisation of care for patients and decentralisation of care.
2) I would recommend to explain how the selection of provinces was conducted. It seems that the provinces of the study were those which were the least far in implementation of the new care model, and were still in semi-centralisation and/or in preparation of decentralisation. Was this deliberate, and may it have affected the results?	The provinces as well as facilities in each province were selected on the basis of representing different models of care across the country. As there was no uniform system of decentralisation across the country, each province adapted implementation according to their own needs, capacities and resources, resulting in different models and varying levels of progress with implementation. We felt it important to capture initial responses to the model, as it was rolled out, in order to ensure that the change was fresh in the minds of the study participants. We have now added a "Recruitment and sampling" subsection to the "Methods" section of the manuscript on page 6 and continuing on page 7 of the clean and marked copy detailing selection of provinces and districts. In addition, the last paragraph of the "context" section details the model of care for the selected provinces.

3) In the methods, it would be good to describe the relationship of the researchers to the participants: were there any power dynamics, or possible associations between the researchers and the management of the health programmes? Or could the researchers be seen as a neutral party?	We have now added a “researcher characteristics and reflexivity” section on page 7 and continuing on page 8 of the clean copy, page 7 of the marked copy. In this section we provide more detail on the two researchers that collected the data and their relationship to the participants.
4) I would recommend to describe shortly where the interviews were conducted - conducting them in the concerned facilities, or in a more neutral setting can affect the responses.	We agree that the interview space can affect responses. In the case of our study, all interviews by necessity had to be conducted in the participants’ place of work during work hours. The rooms where interviews took place were all private and sound proofed, and respondents seemed free to express themselves. We have given more detail about the interview space on page 7 of the clean copy and page 8 of the marked copy.
5) The sampling process is not fully clear: purposive sampling was used, which is indeed appropriate. However, who performed the final selection of HCW - were lists of staff provided to the researchers, from which they selected participants, or was someone from the staff involved in designating specific participants?	We have now added a “Recruitment and sampling” subsection to the “Methods” section of the manuscript on page 6 and continuing on page 7 of the clean and marked copy detailing the selection of participants.
6) Are there risks for identification of participants? Since the TB coordinators were involved in selecting the facilities, and some specific incidents are referred to (such as "death on arrival" of 3 referred cases), could this allow indirect identification of the participants?	Thank you for picking up the risk for identification of participants. Following a review of the participant list, we have removed all references to provinces or locations to reduce the risk of identification.
7) It may be useful to clarify why repeat interviews were done with some participants - for clarification purposes, or simply for time management?	We have added a clarification detailing the reasons for repeat interviews with two nurses on page 7 of the clean and marked copy.
8) In the discussion, the authors frame the results largely in the light of "resistance" and of "the human factor" of change management. While this	Thank you for this comment. After reading the results and discussion section as well as the transcripts of participants again we realized that “resistance” is not warranted by the observations made. It’s however a

may well be true, I am not convinced this is fully supported by the observations. The quotes mainly suggest a lack of training and of engagement of the central management towards the decentralised sites; to label this as resistance may be somewhat reductive, and could mask genuine concerns about the training, management, supply, and general support to the new sites. While it is fair to speculate that the respondents may have shown resistance to change, it should be made clear that this is speculation on the side of the authors, and more detail should be provided on the provisions that were or were not made by the central management for support of the decentralised activities, in order to represent a more balanced and objective interpretation of the results.	possible consequence so we have adapted the text in the "abstract", "background" and "discussion" sections to reflect this. In addition, following another review of the interviews with district and provincial coordinators, their perspectives have been added in the "results" section where available, to represent a more balanced interpretation of the results. This includes their perspectives on provisions made by the central management for support of decentralized activities.
9) The study limitations section focuses only on the generalisability of the findings, but I would suggest (depnding on the replies to my comments above) to also touch on issues such as possible bias in the sampling process.	We agree with your suggestion and have added the following on page 15 of the clean copy and page 17 of the marked copy: "we recognize that the sampling process where provinces and facilities were selected in agreement with national and provincial TB coordinators may have influenced the results."
Reviewer Name: Kaspar Wyss Institution and Country: Swiss TPH, Switzerland	Dear Mr Wyss, thank you for reviewing this manuscript and for your honest and constructive comments.
1. Key results emerging from the analysis, namely weak communication on changes in health delivery arrangements, have as the authors indicate in the introduction and discussion sections been document for other countries both for TB control and other disease control /health systems strengthening initiatives. Consequently what is new scientific evidence emerging from the study beyond the direct documentation of the specific Southern African setting? The	The evidence discussed in the introduction relates to decentralisation of DS-TB, an infectious disease that is fairly easy to treat and has high treatment success rates. While there are certainly similarities between decentralising DS-TB and DR-TB on an operational level, viewed from the perspectives of HCWs, the introduction of DR-TB has more impact on a personal and professional level as it is a much more frightening disease e.g. more difficult to treat, treatment has severe side-effects and the disease has high mortality rates. As such, the new scientific evidence is not so much the Southern African setting, as it is the introduction of care

manuscript would benefit if results are discussed and concluded in the light of new scientific evidence of importance when introducing new TB or disease control interventions.	for a highly infectious and deadly disease in primary healthcare services. We have revised text in the “background”, “discussion” and “conclusion” section to reflect this point.
2. The manuscript is limited to presenting the perspective of health workers and “facility staff”. At the same time the methods indicate that the study included TB coordinators and others cadres as this would dilute the focus of the manuscript. Given that a main conclusion of the manuscript is the need for improved communication and consultation with frontline providers it seems beneficial and important to include also the perspective (and the constraints) of those who are outlined to assure this. In other words the manuscript potentially gains in depth and richness if the perspective, opinion, and difficulties of managers and key staff of the DR-TB program and the general health service managers in delivering and assuring the warranted activities (triangulation of positions). Any possibility based on the available interviews / data to bring in the perspective of the DR-TB program and the general health service managers?	Thank you for this comment and suggestion. Following another review of the interviews with district and provincial coordinators, their perspectives have been added in the “results” section where available.
3. The state objective of the manuscript is among else to analysis effects of decentralisation and deinstitutionalisation on quality of care provided to DR-TB patients. At the same time the results do not present any data or evidence on effects of the decentralisation of DR-TB services. Quality of care only remerges as a theme in the discussion and conclusion sections where general statements are made around the impact of integration on quality of care. Consequently please either alter the stated objectives of the research or include data/information on changes in quality of care in the result section. If the second is done then it is also	Thank you for this comment. We agree that we don’t present evidence on the effect of decentralisation on patient care, rather healthcare workers’ perceptions of the effect on care provided by them to patients. We have adapted the text in the “discussion” and “conclusion” section to reflect this.

necessary to provide a distinct definition of quality of care given that different authors use different ways into analysing quality of care.	
In addition to these three major observations, the following comments: - In the introduction it would beneficial to be more specific on health service changes introduced going along the decentralisation of DR-TB services. For example in one quote it is indicated that “quick trainings” were offered to health workers. For the contextual under-standing of the reader it would be beneficial what activities have been outlined and implemented along the organisational changes for DR-TB services.	We have made an addition in the “results” section on page 10 of the cleaned copy and page 12 of the marked copy, following the quote containing ‘quick training’: “TB coordinators explained that NIMDR (nurse initiation of MDR-TB) training had been organized by the National Department of Health, as well as a readiness assessment of facilities earmarked for the initiation of MDR-TB, albeit with the necessary complications:”
- On page 9 it is indicated “Healthcare workers in all four study provinces remarked that communication and consultation had been”. Given that the main focus of the manuscript gravitates around this aspects, it would useful to better dissect the terms “communication” and “consultation”. Who is meant to communicate with whom, how, using which channels, when and how frequently?	Following a review of the data, we decided to replace “Communication and consultation” with “Introduction of DR-TB care in the facility” as this is a more accurate description on page 9 of the clean copy and page 11 of the marked copy.
- Table 1 has the potential to be presented more concisely. For example the four provinces could be listed in the columns and the number of interviewees then could easily be summed up by professional category along the four provinces	Thank you for the suggestion, we have adapted the table accordingly.

VERSION 2 – REVIEW

REVIEWER	Rafael Van den Bergh Médecins Sans Frontières-Operational Centre Brussels Belgium
REVIEW RETURNED	18-Nov-2019

GENERAL COMMENTS	Thank you, the author shave satisfied my earlier concerns, and I recommend the acceptance of the paper for publication.

REVIEWER	Kaspar Wyss Swiss TPH
REVIEW RETURNED	30-Oct-2019

GENERAL COMMENTS	We do not have further comments and consider that the authors have adequately dealt / responded to our concerns