Author’s response to reviews

Title: Implementing Collaborative Care to reduce depression for rural Native American/Alaska Native people

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Author’s response to reviews:

Lennart Reifels, Ph.D.
BMC Health Services Research
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RE: BHSR-D-19-01508

Implementing Collaborative Care to reduce depression for American Indian/Alaska Native people

Deborah Bowen, PhD; Diane Powers; Joan Russo, Psychiatry and Behavioral Sciences; Robert Arao;; Erin LePoire; Earl Sutherland; Anna Ratzliff

Dear Dr. Reifels:

Thank you for having our manuscript reviewed by the two external reviewers. They made very valuable points in the conceptualization and editing of this manuscript. The point by point response is below, with our responses in italics.

We hope the manuscript is now acceptable for publication in BMC Health Services Research. We look forward to your thoughts on the ms.

Sincerely,
Sincerely,

Deborah Bowen
Professor

Reviewer reports:

Stephen Harfield (Reviewer 1):

* Huria et al. Consolidated criteria for strengthening reporting of health research involving indigenous peoples: the CONSIDER statement. BMC Medical Research Methodology. 2019;19(1):173. It would provide great insight into what should be included in a manuscript that reports on research involving Indigenous populations. I'd suggest that the majority if not all of the criteria should be address in the manuscript.

Thanks, we will review this statement and use it as we can.

Additionally, the following should be addressed.

Methods
  * Should include statements on the following:
    o the significance level was set at 0.05, and data were reported with 95% confidence intervals (CIs)
    o missing and unknowns were excluded from calculations
    o logistic regression models, which values were chosen as referent and why.
  * What was methodology and analysis used for the survey?

We have added these details to the methods.

Line 30 & 31 - the use and consistency of native serving (native-serving), AI/AN serving is preferred and the full phrase should be used in the abstract.
Line 53 & 55 - AI/NA acronym and full phrase are inconsistently used.
Line 147 & 148 - change to, A secondary outcomes was defined as a change in suicidal… Lines 153-183 - should be moved to the results section, as the previous sentence is sufficient for the methods section.
Line 229 - reference for TIDieR checklist?
Line 280 - quote reference means nothing to the reader, BH?? And are the quotes from the same individual, need to be able to make a distinction.
Line 310 - "I think [CoCmis"
Line 317 & 318 - [CoCM] has, and at {clinic name} Line 401 - an extra "s"

Consistency and use of acronyms, need to ensure all are written out in full first and included in the acronyms list.

Thank you for these changes. We have edited the manuscript and made them.
Andrew Pomerville (Reviewer 2):

Overall:

The authors have provided a manuscript concerning an outcome study of Collaborative Care Management (CoCM). Although the paper is entitled "Implementing Collaborative Care to reduce depression for American Indian/Alaska Native people", the majority of participants in the study are White and a primary concern of the authors is the availability of rural mental health treatment.

We agree that we only recruited three clinics with enough AI/AN patients to analyze. However, the substantial number of AI/AN patients is rare in published articles and we feel that this adds to the value of this manuscript.

Having read the paper more than once I am unclear on what precisely CoCM is; the authors do not explain clearly whether it is a curriculum or simply a standard of care, whether any materials are provided, whether there are trainings, etc. Although it is understandable that the authors cannot outline the full program, it is unclear to me in any sense both what was provided to the clinics/clinicians and what was provided to the clients/patients as a result of the implementation of CoCM. The primary explanation is a list of 7 different responsibilities for the "care manager" role, which are largely already existing standards of evidence-based care in behavioral health, with some minimal duties for PCPs and Psychiatric Consultants also mentioned. This may explain why care managers rated the program least helpful and at least one reported that "this model of care does not work" according to the authors. It is also unclear what a psychiatric consultant is; this term is not defined nor is it clear what training they have in order to allow them to serve in this role.

We have clarified the roles and tasks of all three key clinicians in the process of COCM. We also have added a clearer definition of COCM.

In addition to finding the program of care inadequately defined, I also do not find the results encouraging. The clinic was a major determinant of the results of CoCM; this suggests that the programs available at the clinic aside from CoCM may be responsible for the improvement seen in the clients/patients who participated in this study; this is already a likely alternative explanation of the results given that the clients/patients are already interfacing with clinics which provide health and behavioral health care and that the authors did not include a control or waitlist comparison. Given this, the authors appear consistently overconfident in their findings in the abstract, discussion, and conclusion, especially given that the paper is highlighting the success of AI/AN clients/patients, but these clients/patients only differed in a single category from the other racial groups considered (i.e. there is no difference in remission of symptoms, only among those who improved but did not reach remission).

We have reduced the optimism in this paper, to not overstate the findings.

Although there is discussion of the value of tailoring evidence-based approaches for AI clients in the introduction, this is elided in the work itself and tailoring was not done (according to the authors as listed on the TIDieR form submitted). Authors may wish to consider dropping such references or giving greater consideration to what these studies they are citing mean for their own research.
We have dropped references to tailoring, and deemphasized this part of the manuscript.

Although I am recommending rejection, I wish to applaud the efforts of the researchers. Work with AI/AN people is limited, and although it appears that was not the primary purpose of this study, it does appear there is some evidence that CoCM may be an effective care model with this population. I would personally recommend that the authors spend time considering how to publish this work in a way that is clearer and more direct about what the results were, and more cautious in their interpretation of the findings given the limitations. I believe it may be possible to publish these results and highlight the success of AI/AN clients/patients in the CoCM system. However, I also believe that such a paper would require a complete reconsideration of how these findings are being presented, and as such I recommend a rejection at this time.

We hope that we have done this, in the new version.

A more detailed section-by-section review follows.

Abstract:

There are multiple typos in the abstract which need to be corrected. There are also grammatical issues which need corrected. For example; "Native patients reduced as much or more than did White patients." In addition to the typo that needs corrected, it should be explicit that this means Native patients' scores on the PHQ9 reduced. There are also several differences between the abstract submitted through the system and the abstract that appears on page one of the manuscript itself (but both contain multiple typos). The abstract should be cleaned up and the two versions reconciled so they are the same.

We have made the edits requested by this reviewer.

Introduction:

The introduction provides a brief but adequate summary of the issues of depression and the generally high rates of mental illness in AI/AN communities. However, the claim that depression is "a disability-producing health problem that is under-recognized and under-treated" (page 3) could use further elucidation. Although the manuscript does not say so, this appears to be specifically referring to rural areas, based on the citation. The majority of AI/AN people live in urban areas today, however, and it is not clear how this citation ties into the specific population (AI/AN people) as it is written. It is also unclear to me what is meant in saying that depression is "disability-producing"; does this refer to the fact that depression is a common reason for claiming disability status in the United States? If so it should be worded more directly. If this means instead that depression produces other forms of disability this should be clarified. Depression is also the most common mental health diagnosis in the United States, so claims that it is "underdiagnosed" need to be qualified (if only talking about diagnosis in certain populations such as people living in rural areas or AI/AN people) or backed up by some explanation or argument.

We have cleared up these issues in the introduction.

At the top of page 4 the claim is made that "recruiting more mental health specialists to rural America is not practical". Although I agree, no citation is given and no argument for why this is so is presented.
Further, this statement appears in a paragraph concerning the availability of psychiatrists and psychologists at FQHCs. FQHCs are not inherently rural and many exist in urban areas, and those in urban areas face similar difficulties in recruiting and maintaining mental health treatment staff, and the citations provided do not appear to reference only FQHCs operating in rural areas. Further, the shortage of psychiatrists at present is a problem nationwide, and not specifically only a challenge in rural areas. If the authors wish to specifically address rural provider shortages, they need to clarify and discuss the issue of rural providers throughout, and provide citations that are directly relevant to rural provider shortages. The authors may instead wish to drop references to availability of treatment in rural areas specifically if the primary purpose of the paper is to focus on treatment for AI/AN people. As written the two issues appear to be conflated. This is to me a very serious problem. If the proposed methods are intended specifically to target rural AI/AN people this is a perfectly legitimate aim, but this needs to be stated outright in the abstract and ideally in the title.

Good points. The study was conducted in rural areas, but certainly AI/AN patients are found in urban and rural areas. We have edited this part of the manuscript to make all this clear.

On page 4, an abbreviation is given for collaborative care (CoCM) but it is unclear what the abbreviation stands for as Collaborative Care has no M in it; as the authors mean Collaborative Care Management, the word Management should be added when the abbreviation is introduced.

We have worked on the abbreviation and the definition of the abbreviation to match.

The same paragraph also describes the CoCM program, which uses neither psychiatrists nor psychologists. Given that the previous paragraph suggests the value of CoCM is in providing psychological services without utilizing these types of mental health professionals, it would be appropriate to address the extent to which the types of mental health professionals that are utilized in the CoCM system already exist in the targeted settings, (and perhaps whether any other programs attempt to similarly solve provider shortage problems in such manner). Further, although the training of CMs and PCPs in the model is clear, it is not clear what degree is held by psychiatric consultants or what training they have in order to perform in this role.

We have added information about the psychiatric consultants, and have clarified these issues in the text.

The next paragraph also says without citation "reviews have documented successful adaptation of cognitive-behavioral therapy for AI/AN people". To the best of this reviewer's knowledge, no clinical trials of such an adaptation have been done, although some adaptations have been utilized in clinics and reported in studies. Reviews on the topic include Pomerville, Burrage, & Gone, 2016 and Gone & Alcantara, 2007. If there have been clinical trials on adaptations of CBT with AI/AN clients, that research should be cited. If the authors instead mean that adaptations have been implemented but not tested, this should be said in a way that indicates that they have not been tested for evidence of efficacy or effectiveness.

We have edited this text to be accurate.

Methods:

In general the methods of analyses proposed appear sound.

The authors may wish to remove some extraneous details regarding clinics C, F, and G in order to make
them less likely to be identified. For example it is not likely to be relevant to readers that one clinic "was started by a small group of providers who had all previously worked at the nearby Indian Health Service medical center" and opened 18 months prior to the study, but this detail combined with other details about the size and location of the clinic (an upper plains area surrounded by reservations) might make a site easily identifiable; however the mention of other recently introduced programs at the clinics is appreciated and helpful in considering this study and its results.

We have edited the text for the clinics.

On page 8, the authors refer to questions as "quantitative" and "qualitative,"; this would be more accurately written as "closed-ended" and "open-ended", as it is the analyses that are quantitative or qualitative, not the questions. No mention is made of how the qualitative / open-ended questions would be analyzed, or if they would be analyzed; if they are not analyzed in this study, it should be mentioned simply that this information was recorded but not analyzed or incorporated into the results. It is apparent no formal qualitative analysis has been done, and as such I would recommend rewording a description of such work as something like "open-ended questions were included on the survey, and some illustrative example responses are provided in the results."

We have added analysis text for the open ended questions and have edited this section.

Results:

Clinic is a major predictor in the results. This appears to be a troubling confounding variable because it suggests the improvements may have been site dependent, and thus that the demonstrated improvements could be a result of the other treatments available at the clinics as opposed to CoCM.

This finding is very similar to other kinds of implementation studies that have been published, in that clinic is usually a large source of variation in effect. We will add this text to the discussion section more strongly.

The open-ended responses are reported with no introduction or context. It is unclear what some of the responses mean without context (for example what is the question being responded to), and it is unusual to see quotes reported in this manner without any introduction. For example, the first BH provider quote starts with "all of them" and it is unclear what the "all of them" is referring to as the quote is not introduced or given any context as to what it might be in response to; without this it could be interpreted as "all of the primary care providers I worked with were helpful" or "all of the processes of CoCM are helpful," or "it takes all of these things working together to be effective" just to give a few examples. Quotations from participants should be introduced and given context.

We have added introductions and context to the qualitative data presentation.

There are typographical errors in some of the quotations.

We have left these, where they are about the speech patterns of the individuals. We have corrected those that are our typos.

Tables:
In multiple tables AINA or AI/NA appears instead of AI/AN or AIAN, as used in the rest of the paper.

We have corrected the table versions of this label.

In table 4a, percentages appear to be incorrectly reported.

We checked and corrected these percentages