PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Effect of a health literacy intervention trial on knowledge about cardiovascular disease medications among Indigenous peoples in Australia, Canada, and New Zealand
AUTHORS	Crengle, Sue; Luke, Joanne N.; Lambert, Michelle; Smylie, Janet; Reid, Susan; Harre-Hindmarsh, Jennie; Kelaher, Margaret

VERSION 1 – REVIEW

REVIEWER	Remo Ostini The University of Queensland, Rural Clinical School, Australia. The authors cite my research on health literacy and medication adherence.
REVIEW RETURNED	16-Aug-2017

GENERAL COMMENTS	This is a carefully designed and rigorously implemented study of an important clinical area. Intervention studies are rare in health literacy and this study is a good example of how to design an effective intervention. It is also a good example of how to conduct research in Indigenous communities. This study perhaps also points to some of the ongoing challenges of conducting intervention research in distinct and relatively small patient populations. In my view, the primary weakness of this study is that it is substantially underpowered for the original intent of the research. It is perhaps fortunate that the effect sizes from the intervention were as substantial as they were. Nevertheless, the much smaller than intended sample has restricted the depth of analysis that could be conducted in this study. The manuscript generally presents the study and its outcomes well and reasonably represents the strengths and weaknesses of the research. The authors note that a weakness of the study is that it did not assess the effect of the intervention on clinical outcomes – and provide a reasonable explanation of why this is so. The authors could however have also noted as a weakness the fact that they also did not measure any changes in health behaviours (e.g., improvements in medication adherence). This would have been much more practically achievable within the study design and would have gone a little of the way towards indicating whether clinical outcomes could be expected to improve.

	Specific manuscript comments: The last 4 sentences in the Introduction (p. 6, Lines 4-13, beginning "The intervention was implemented…") read more like a part of the Method and could fit quite well between the first two paragraphs of the Method section. The end of the Introduction would benefit from a statement of study aims/objectives, similar to that provided in the Abstract. The second sentence in the statistical analysis section (p. 8, Ln 42.5) says "All categorical data have been calculated…", when it might be more accurate to say "All categorical data analyses have been calculated …" It is excellent that [country and diabetes status?] have been controlled for in the multivariate analyses. Is there a reason that age and gender were not? Time with CVD may also have been an important covariate/potential confounder. A related question concerns which variables were actually controlled for. The Method section (p. 8) lists Country and Diabetes status while in Results, the model notes at the bottom of Table 5 (p. 13) list Site and Comorbidity as the covariates. Site is certainly preferable to country, given the between site differences noted. In any event, the reporting in the Method and Result sections should be consistent. P. 12, Ln 46 states that adjusted analyses are reported in Table 3. This should say Table 5. The Discussion, on p. 15, Ln 16, states that the study measured "health practices such as using health resources." It is not clear from the Method or Results whether or how this was done. This information is not obviously reported in Table 2 which seems to be entirely about measuring medication knowledge.

REVIEWER	Don Nutbeam University of Sydney Australia
REVIEW RETURNED	24-Aug-2017

GENERAL COMMENTS	This is an impressive study of a complex problem in hard-to-reach indigenous communities in three countries. The authors have described the study and results accurately and have provided a clear explanation of the limitations of the study design. The paper has great potential to add to our understanding of how to work effectively with indigenous populations, but currently falls short of reaching that potential. There are a number of issues that require attention to bring the paper to a publishable standard. Firstly, the paper rests heavily on a custom designed intervention that reflected the views and preferences of the targeted communities. Some additional information on the intervention can be found in the protocol paper, and this gives the impression that established methods of communication were used – a booklet, educational session(s) with a trained health worker, and an interactive "app" that provided customized information on medication use.

	One of the real sources of originality in the paper is in the engagement of the community, and the reported modification of the interventions to account for community preferences. This is referred to at the end of the introduction and implicit in the discussion of the results. Much more could be learned from this paper if it included more detailed information on this process of customization – what was learned, what was unique, and what might be generalizable. The results would be more meaningful, and aspects of the discussion better supported if this information were provided. Second, in the introduction the authors recognize that health literacy means more than simple knowledge improvement - most definitions, including the one used by the authors also include reference to critical understanding of new information, and capacity to act on the basis of improved knowledge. Improving knowledge is a necessary but not sufficient method for improving health literacy. This is a particularly important issue in relation to the discussion of results. In the first paragraph there are references to “supporting patients to improve their health literacy skills and capabilities”; and “developing participant’s knowledge and skill acquisition”. There is currently no evidence in the paper to support the acquisition of new skills and capabilities. This limitation should be acknowledged, or additional justification for the assertions should be provided. Third, the authors are sufficiently engaged and experienced in working with indigenous communities to know that the disadvantage they experience is a result of a complex set of historical, social and economic factors. Little account appears to have been taken of this wider context in the methodology and the presentation of results. This is surprising, and should be addressed more directly by the authors. As a minimum they should report of the assumptions they have they made about the homogeneity of the populations within and between countries, and on the impact that this might have on the observed results. Finally, the discussion lacked an obvious logic to the narrative. It would certainly be strengthened by better focus on the original/unique contributions to the literature. At the moment it has several assertions unsupported by the results presented , and fails to discuss the potential importance of the more original aspects of the intervention. More information on the customization of the intervention, and the role of community engagement in the design of the intervention would help here, and would enable a contextualized discussion of the importance of the observed results. The authors make a bold statement that “there are clear benefits to culturally appropriate and community specific interventions” without really offering the reader the information to verify that statement. It would strengthen the paper if that were possible. Currently the assertion is supported by material that would be better positioned in the introduction (paragraph 3 in the discussion). Overall, this study has great merit with some acknowledged limitations, but the paper doesn’t yet fully reflect the originality of the work.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Remo Ostini.

WE THANK THE REVIEWER FOR HIS COMMENTS.

The authors could however have also noted as a weakness the fact that they also did not measure any changes in health behaviours (e.g., improvements in medication adherence). WE HAVE ADDED

TO THE SENTENCE BEGINNING
'Finally, we have not assessed the effect of improved knowledge
on clinical outcomes..'

IN THE DISCUSSION (PG. 16). WE ALSO BELIEVE THAT THE LIMITED
FOLLOW UP PERIOD WOULD HAVE AFFECTED OUR ABILITY TO MEASURE WHETHER CHANGES IN BEHAVIOUR WERE SUSTAINED.

Specific manuscript comments:
The last 4 sentences in the Introduction (p. 6, Lines 4-13, beginning “The intervention was implemented…”) read more like a part of the Method and could fit quite well between the first two paragraphs of the Method section.

WE HAVE MOVED THESE SENTENCES TO BE A NEW PARA 2 OF THE METHODS SECTION.

The end of the Introduction would benefit from a statement of study aims/objectives, similar to that provided in the Abstract. SENTENCE ADDED AT END OF INTRODUCTION.
The second sentence in the statistical analysis section (p. 8, Ln 42.5) says “All categorical data have been calculated…”, when it might be more accurate to say “All categorical data analyses have been calculated …”

CORRECTION MADE

It is excellent that [country and diabetes status?] have been controlled for in the multivariate analyses. Is there a reason that age and gender were not? Time with CVD may also have been an important covariate/potential confounder.

AGE, SEX, TIME WITH CVD - DIFFERENCES ACROSS SITES WERE NOT SIGNIFICANT SO THESE VARIABLES WERE NOT INCLUDED IN MULTIVARIABLE MODEL.

The Method section (p. 8) lists Country and Diabetes status while in Results, the model notes at the bottom of Table 5 (p. 13) list Site and Comorbidity as the covariates. Site is certainly preferable to country, given the between site differences noted. In any event, the reporting in the Method and Result sections should be consistent. V

ARIABLES CONTROLLED FOR WERE SITE AND DIABETES. THIS HAS BEEN CLARIFIED WITH CORRECTION OF COUNTRY TO SITE IN THE METHODS SECTION.

P. 12, Ln 46 states that adjusted analyses are reported in Table 3. This should say Table 5.

CORRECTED.

The Discussion, on p. 15, Ln 16, states that the study measured “health practices such as using health resources.” It is not clear from the Method or Results whether or how this was done. This information is not obviously reported in Table 2 which seems to be entirely about measuring medication knowledge.

OTHER DATA EG USE OF HEALTH RESOURCES WAS COLLECTED IN THE TRIAL BUT IS NOT REPORTED IN THIS ARTICLE WHICH FOCUSES ON THE PRIMARY OUTCOME (MEDICATION KNOWLEDGE). WE HAVE DELETED THE PHRASE 'AND HEALTH LITERACY PRACTICES SUCH AS USING HEALTH RESOURCES'

Reviewer: 2
Reviewer Name: Don Nutbeam

WE THANK THE REVIEWER FOR HIS COMMENTS.

One of the real sources of originality in the paper is in the engagement of the community, and the reported modification of the interventions to account for community preferences. This is referred to at the end of the introduction and implicit in the discussion of the results. Much more could be learned from this paper if it included more detailed information on this process of customization – what was learned, what was unique, and what might be generalizable.

THE DEVELOPMENT OF THE INTERVENTION AND CUSTOMISATION WAS INFORMED BY INTERVIEWS WITH INDIGENOUS PEOPLES WITH CVD IN THE THREE COUNTRIES DURING PHASE 1 OF THIS PROJECT. THIS PAPER REPORTS THE RESULTS OF PHSASE 2 - THE IMPLEMENTATION OF THE TRIAL. WE HAVE ADDED FURTHER INFORMATION ABOUT THIS TO THE 4TH PARA OF THE INTRODUCTION.

Second, in the introduction... ...There is currently no evidence in the paper to support the acquisition of new skills and capabilities. This limitation should be acknowledged, or additional justification for the assertions should be provided.

WE HAVE CLARIFIED THAT THIS PAPER ONLY PRESENTS DATA ABOUT THE PRIMARY OUTCOME (MEDICATION KNOWLEDGE) AT THE END OF: THE NEWLY INSERTED PARA 2 OF METHODS; AT THE END OF DISCUSSION PARA 1; AND TO THE END OF THE DISCUSSION PARAGAPH (PG. 15) STARTING WITH 'MUCH OF THE CURRENT HEALTH LITERACY LITERATURE IS DESCRIPTIVE'.

Third, the authors are sufficiently engaged and experienced in working with indigenous communities to know that the disadvantage they experience is a result of a complex set of historical, social and economic factors. Little account appears to have been taken of this wider context in the methodology and the presentation of results. This is surprising, and should be addressed more directly by the authors. As a minimum they should report of the assumptions they have they made about the homogeneity of the populations within and between countries, and on the impact that this might have on the observed results.

THE REVIEWERS STATEMENT ABOUT THE HISTORICAL/POLITICAL FACTORS IS CORRECT. WE HAVE ADDED THE FOLLOWING TO THE INTRODUCTION (PARA 1) '

Although Māori (New Zealand), Aboriginal (Australia) and First Nations (Canada) peoples are distinct Indigenous populations, their shared history of colonisation, historically and in its contemporary expressions, has resulted in similar patterns of inequity in health and social outcomes, relative to the non-Indigenous populations in each country.'

HOWEVER, THIS RESEARCH TAKES A STRENGTH BASED APPROACH RATHER THAN A DEFICIT FOCUS. WE HAVE ADDED THE FOLLOWING PARA TO THE DISCUSSION PAGE 15 TO ADDRESS HIS QUESTION ABOUT DEALING WITH HETEROGENEITY. '

Furthermore, there has been a strong shift in Indigenous-led research towards strength based approaches rather than focusing on disparities and deprivation experienced by Indigenous people, accordingly the latter are not a focus of the research presented here. Communities in each country were engaged throughout the research process and their experiences, culture and values incorporated in the design of the intervention. Heterogeneity between the communities was accounted for by enabling communities to design an approach that was tailored to them.'

Finally... The authors make a bold statement that “there are clear benefits to culturally appropriate and community specific interventions” without really offering the reader the information to verify that statement. It would strengthen the paper if that were possible.

THE PARA THE REVEIWER IS REFERRING TO IN THE DISCUSSION IS OF RELEVANCE TO OUR RESULTS WHICH DEMONSTRATE THAT A CULTURALLY APPROPRIATE, COMMUNITY SPECIFIC INTERVENTION IS EFFECTIVE. WE HAVE MOVED THESE SENTENCES UP AND ADDED THEM TO THE SECOND PARAGRAPH OF THE DISCUSSION WHICH SUMMARISES OUR FINDINGS IN ORDER TO MAKE THIS CLEARER.

VERSION 2 – REVIEW

REVIEWER	Remo Ostini The University of Queensland, Rural Clinical School This manuscript cites research I have published
REVIEW RETURNED	18-Oct-2017

GENERAL COMMENTS	The authors have addressed comments and concerns expressed in the initial review very well. This is a very well presented manuscript on an important piece of work. I only have one (new) concern. In the Results on p. 9, line 33, the authors added age to the list of baseline characteristics that did not vary by site. However, a sentence on line 39 still says that participants at the NZ rural site were older than other sites. This seems contradictory and should be resolved or clarified before the paper is published.

REVIEWER	Don Nutbeam University of Sydney, Australia
REVIEW RETURNED	10-Oct-2017

GENERAL COMMENTS	I have reviewed the revised paper in the light of my comments on the original paper. Some of the concerns I raised have been adequately, if minimally addressed. There are two outstanding issues that need further attention. Firstly, the response to my comments on community engagement in the development of the interventions is inadequate. Arguing that this is phase 1 or phase 2 in a project is meaningless to the reader (or a reviewer). I repeat my comment that one of the more original and important elements of the paper is that it reports on an intervention that has been developed and customized to meet the needs of indigenous populations in different countries. In response, the authors make the point that these customizations varied from population to population. In order to understand the results presented in the paper (that combine results from different, customized forms of the intervention), and learn from this experience, it is important for the reader to have adequate information on the form of the intervention, and the nature of the customization. This is not available in the current paper. Second, the authors have provided a useful response to my concern that they did not adequately justify their claim that the results demonstrate "clear benefits to culturally appropriate and community specific interventions". I could not see the logic of including this in the discussion. More logically it should be positioned in the introduction as a "hypothesis" to be tested (with the arguments and references currently in the discussion re-positioned in the introduction), and then referred to in the discussion of the results as supporting the "hypothesis.

VERSION 2 – AUTHOR RESPONSE

Reviewer one: We have clarified the writing about age by amending the sentence (pg 9, results section, para 2) 'Baseline characteristics did not vary by site with regards to age, sex...' to 'Baseline characteristics did not vary significantly by site with regards to age, sex...'
We have deleted the sentence 'Participants at the NZ rural site were older than other sites.' in the same paragraph.

Reviewer two: We have asked the graphic designer who worked with us on the production of the booklets to provide us with electronic copies of each booklet which we will upload to the BMJ Open site when they are sent to us. We reiterate that the information itself was the same across all three countries. The images and Indigenous language used in the booklet/app were customised for each Indigenous group. We have not taken up the reviewers suggestion to move information regarding 'benefits of providing culturally appropriate and community specific interventions' to the introduction and make it a hypothesis of the study. The study was not designed with this hypothesis in mind. We did not have a non-culturally appropriate/community specific' intervention which we used to compare to our own culturally/community specific intervention in the study. Thus, we are unable to test the suggested hypothesis.