PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Exploring trust in religious leaders and institutions as a mechanism for improving retention in child malnutrition interventions in the Philippines: A retrospective cohort study
AUTHORS	Lau, Lincoln Leehang; Dodd, W; Qu, Han Lily; Cole, Donald

VERSION 1 – REVIEW

REVIEWER	Lynn Lieberman Lawry MD, MSPH, MSc USUHS, United States
REVIEW RETURNED	30-Jan-2020

GENERAL COMMENTS	Abstract: Line: 31 – The study is not designed to make a conclusion that [All] FBOs can promote adherence to high-quality child nutrition interventions among vulnerable populations. The study assess pre- and post- indicators from one NGO. It is appropriate to say ICM is associated with promoting adherence. Introduction Line 11-14 – Please clarify the following statement: Despite the critical role that faith-based organizations (FBOs) play in delivering healthcare in low resource settings, the capabilities and assets of these organization have been underused and underexplored. As key actors… FBOs are a mainstay for USAID funded projects and are used extensively in many areas of the world as both Primes and Subs and they are afforded 10% of the aid budget, at least in the US. Furthermore, some of the largest FBOs (World Vision, Mercy Corps, Samaritan's Purse, IMA World Health, Tearfund etc.) have access to funds other organizations do not and serve as key actors globally. FBOs have played important and sustained roles in Ebola, HIV, MNCH among others. Perhaps FBO should be defined here as smaller FBO or local FBOs as the statement made does not make sense. I suggest a better search for FBOs and health outcomes to supplement the discussion as this appears to be well studied. On quick review: https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1448385/ https://jliflc.com/wp-content/uploads/2014/06/Role-of-FBO-in-MNCH-Africa.Widmer.pdf https://www.researchgate.net/publication/8460649_Health_ Programs_in_Faith-Based_Organizations_Are_They_Effective

	Conclusions: The model is a bit thin based on the amount of data collected (appendix). There may be other factors such as sex of the child, number of household children, malaria, maternal health, transportation etc. that may have an impact on drop-out/adherence/improvement regardless of the trust of leaders. I would suggest adding in other factors into the model to see if the trust in religious leaders holds. Local leaders are many in rural/poor communities; not only faith leaders. Although the model included local government and neighbors, I wonder if the same results would have been seen if the model included other local (non-religious) leaders. And since the religious leaders were part of the program, it seems the assessment is a bit biased as there is no comparison (other than gov/neighbors who were not part of the program) with other leaders since they were not recruited in this program. A leader is a leader, religious or not.

REVIEWER	Jill Olivier University of Cape Town, South Africa
REVIEW RETURNED	03-Feb-2020

GENERAL COMMENTS	Thank you for this opportunity to review this article. I am an advocate of empirical articles that report on implementation of interventions, that open the 'black box' of international NGOs (INGOs), and which seek to redress the imbalance of evidence on faith-community or faith-based organisations in health and development – as well as addressing contexts such as the Philippines which are under-reported. For all those reasons, this is an interesting article that is worth pursuing. However, I do have some concerns/queries – clustered in three main (but related) areas: FIRSTLY, the article has some challenges in that it comes across as imbalanced or 'advertorial' towards the implementing organization of focus (International Care Ministries), which appears to have supported this study and paper (although this is not acknowledged in the main text). This imbalance is felt particularly strongly in the introduction and conclusion sections. >> For example PG4: "Faith-based organization (FBOs) play a critical role in delivering healthcare in low resource settings… etc" This introductory section is imbalanced. These arguments have indeed been made in those referenced articles, but those authors also describe the other/dark side of FB engagement, the potential 'negative effects' of faith-community/organization engagement. The authors do not acknowledge this at all, so this is very selective argument. >> For example PG 11: "…existing trust in local religious leaders and institutions offers an opportunity for high-quality care to be provided in partnership with local religious leaders and institutions" There is no evidence provided that this was a particularly 'high quality' intervention provided by the FBO-ICM (high quality compared to what? How was quality assessed?). As far as I can see, there was no quality assessment, and this therefore appears

	to be a judgement made by the authors – possibly about their own institution and intervention? >> For example: below, I also address some difficulties I have with conclusions made against data collected – in that the authors appear to overextend the data on trust in religious leaders to conclude that faith-based organisations should receive more financial support. I address this more later – but this is another example of a potential lack of impartiality. SECONDLY, related to the first concern, this paper does not have a limitations section – which could have addressed many questions I had about the study. >>For example: in the main messages, it states "the study could not account for all potential confounders", but this is not addressed in the main text at all. Where in the text do the authors consider potential confounders? >>For example, it is not clear what type of religious groups were surveyed – ie the word 'church' in the question to participants – although the Philippines is pre-dominantly Catholic, there are indeed multiple types of Christian groups, as well as other religions present in these areas. It would have been useful to understand how the data was limited in relation to this nuance? >>For example, I have been in some of these small communities in the Philippines. Asking a caregiver of an extremely malnourished child whether they trust the religious leader or the barangay (consisting of community leaders, usually inclusive of that same religious leader) is not without limitations. Freedom to respond openly is not a given in such small communities, in particular when that survey is connected to resource allocations from the INGO (I discuss the limitations of this distinction of barangay and religious leader more below). >>For example, the authors do not address the limitations of their own positionality (in relation to the object and funder ICM). >>For example, the study is limited by the fact that the evidence was gathered 7 years ago (2012-2013), and is now somewhat dated. This is not insurmountable, but needs to be addressed. Also, given the limitations of data collection, there is no way of knowing whether the trust that was seen and change that was assumed was sustainable, or whether those children fell back into malnutrition (or trust was maintained) after the 2014 intervention end? (this is perhaps beyond the scope of the study, but it would be good to see some reflection from the authors on this point). THIRDLY, there appears to be a major disconnect between the gathered data and the conclusions made – and some over-reaching of conclusions. >> Such as the disconnect between data on individual trust in local religious leaders and claims of organisational effectiveness of FBOs. For example, some of the argument and the data is about individual relationships (does an individual trust an individual local religious leader). But the conclusions that are made then suggests

	that this trust is equivalent to trust in all 'FBOs' (in this case, an international NGO). PG 11: "Our study confirmed that trust in religious leaders and institutions was a determinant of treatment adherence among participants attending a program administered by a faith-based organization in partnership with local religious leaders. Thus, this model of service delivery provides an opportunity for the public sector to support and partner with faith-based organizations to offer complementary care to address acute child malnutrition." This kind of statement is concerning for several reasons. - As far as I could see, the survey did not ask, 'do you trust the FB-INGO" – and it is simply NOT an automatic leap from trust in your local religious leader to trust in all FBOs. What is the logical bridge? This tension plays out in other places – for example, why is the introduction and conclusion about PPP, when the survey and data seems to be about individual-level trust mechanisms, not institutional-level PPP (which is a different field of enquiry? There is a substantial literature that would be more relevant than the PPP literature - in particular that which addresses individual/community trust in religious leaders as change agents (trusted individuals / key influencers), and whether this enhances intervention/behaviour change. None of this literature is acknowledged in this article at all, which is a substantial gap. The research question is not clear, but it seems to me that the question this survey addresses – is around whether local religious leaders work well as a mechanism for local intervention. This question, about religious leaders as key influencers, is already being asked widely in the broader literature – and it is rather odd that the authors do not connect to this. In this debate, there are some hard questions being asked, such as whether religious leaders can be effective key influencers in reproductive health (behaviour change) interventions. There is a large literature on this in the HIV/AIDS research arena. Trust in religious leaders is critically important for such conversations. So connecting to that literature would have made this small study more substantively relevant. - [PS: small note, but the authors reference L Gilson's work on trust in health systems to support this study - but as far as I am aware, Gilson's work does not apply the same framework for researching trust as is being applied in this study, so those references do not seem entirely appropriate]. >> A more concerning example, the individual level data on slightly elevated trust in religious leaders, does not seem to justify a strong conclusion that FBOs should receive more financial support from the public sector and multinationals (and this again is suggestive of bias). Pg 11 "Based on the promising findings from our study, complementary models of healthcare delivery between faith-based organizations of public health facilities that leverage public funding, in addition to bilateral and multi-lateral support should be further explored and evaluated" >>For example, in the abstract the first sentence suggests this is an evaluation of intervention 'effectiveness' – when in fact there is very little assessment of effectiveness at all? >>For example, the authors consistently use the term assessment of 'adherence' and 'treatment adherence' – but I do not see substantial data reported on 'adherence' per se (not as we would expect in a public health research)

	>>For example, in the key messages, Pg3, it claims that "The study was conducted in a unique setting for examining the underlying mechanisms associated with adherence to malnutrition treatment delivered by both government and faith-based organizations" – this suggests that some sort of comparative was conducted – whereas the main article does not address government provision (mechanisms or effectiveness) … (ie surely the question 'do you trust your barangay' is not being used as a proxy to compare all government interventions?) >>For example, PG 11: "Experiences of social exclusion (e.g. mistrust of public institutions) may influence health seeking behaviour and contribute to gaps in healthcare provision". I did not see evidence of mistrust in public institutions? There was a slightly elevated trust in religious leaders above barangay (confused by the fact that the religious leader is likely often in the barangay), but does this show mistrust in public institutions? All of this suggests that this article is worth developing further, but needs major revision. MINOR COMMENTS: this article requires some editing – including spelling out acronyms on first use etc.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name

Lynn Lieberman Lawry MD, MSPH, MSc

Institution and Country

USUHS, United States

Please state any competing interests or state 'None declared':

None declared

Please leave your comments for the authors below

<u>Abstract</u>

Line: 31 – The study is not designed to make a conclusion that [All] FBOs can promote adherence to high-quality child nutrition interventions among vulnerable populations. The study assess pre- and post- indicators from one NGO. It is appropriate to say ICM is associated with promoting adherence.

Response: Thank you for this comment. We have changed the conclusion in our abstract to read:

> "Leveraging pre-existing trust in religious leaders and institutions among households experiencing extreme poverty is one way that ICM, and potentially other FBOs, can promote participant retention in child nutrition interventions among vulnerable populations."

Introduction

Line 11-14 – Please clarify the following statement: "Despite the critical role that faith-based organizations (FBOs) play in delivering healthcare in low resource settings, the capabilities and assets of these organization have been underused and underexplored. As key actors…"

FBOs are a mainstay for USAID funded projects and are used extensively in many areas of the world as both Primes and Subs and they are afforded 10% of the aid budget, at least in the US. Furthermore, some of the largest FBOs (World Vision, Mercy Corps, Samaritan's Purse, IMA World Health, Tearfund etc.) have access to funds other organizations do not and serve as key actors globally. FBOs have played important and sustained roles in Ebola, HIV, MNCH among others. Perhaps FBO should be defined here as smaller FBO or local FBOs as the statement made does not make sense.

Response: Thank you for encouraging us to consider the role of larger FBOs and how findings on these organizations may differ from those focused on local FBOs. With that said, the papers we have cited here are referring to the role FBOs at large as opposed to just local FBOs. Thus, to better reflect the diversity of FBOs, we have changed the statement you identified to read:

> "Despite the critical role that FBOs play in delivering healthcare in low resource settings, the capabilities and assets of some FBOs have been underused and underexplored [1 ,5]."

I suggest a better search for FBOs and health outcomes to supplement the discussion as this appears to be well studied. On quick review:

- https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1448385/
- https://jliflc.com/wp-content/uploads/2014/06/Role-of-FBO-in-MNCH-Africa.Widmer.pdf
- https://www.researchgate.net/publication/8460649_Health_Programs_in_Faith-Based_Organizations_Are_They_Effective

Response: We appreciate your suggestion that we supplement our discussion with more work on FBOs and health outcomes. We found the recommended article by Widmer et al. (2011) particularly useful and have added it to our introduction. We have also added additional literature throughout our manuscript to improve our Discussion section.

Conclusions

The model is a bit thin based on the amount of data collected (appendix). There may be other factors such as sex of the child, number of household children, malaria, maternal health, transportation etc. that may have an impact on drop-out/adherence/improvement regardless of the trust of leaders. I would suggest adding in other factors into the model to see if the trust in religious leaders holds.

Response: We agree that relevant factors should be considered in the development of the model, in this study we grouped covariates relating to poverty into an 'intensity of poverty', A, measure that was developed and known as the Alkire-Foster method. This statistic comprises of 13 weighted variables that are measures of poverty from the survey and a detailed description is found in the Supplementary Materials. In addition to these measures, factors such as sex of the caregiver, number of household members, etc were explored but not included in the final models because they were not found to improve the model. We have clarified the relevant sentences in the Methods section to read:

> "For both outcomes, a series of covariates such as sex of caregiver, age of caregiver, household size, etc. were explored. The most parsimonious models that minimize deviance were chosen."

Local leaders are many in rural/poor communities; not only faith leaders. Although the model included local government and neighbors, I wonder if the same results would have been seen if the model included other local (non-religious) leaders. And since the religious leaders were part of the program, it seems the assessment is a bit biased as there is no comparison (other than gov/neighbors who were not part of the program) with other leaders since they were not recruited in this program. A leader is a leader, religious or not.

Response: We agree that local leaders - beyond simply religious leaders - can have a great deal of influence in rural/poor communities. Although a full discussion of the role of other leaders is beyond the scope of this paper, we think future research on this topic is needed. To illustrate the specific importance of local religious leaders, and why we have focused our study in this way, we have added existing literature on the impact religious leaders can have on healthcare delivery into our introduction. Specifically in our introduction, we state:

> "Collaboration with local religious leaders and their institutions is one factor contributing to the success of health interventions implemented by FBOs. Many communities view religious leaders and institutions as a trustworthy and credible source of health advice and information, with research finding that religious leaders' opinions can strongly influence social and behavioural norms (e.g., Rivera-Hernandez 2015; Kanda et al. 2013; Adedini et al. 2018; Cohen-Dar and Obeid 2017). As embedded members of their communities, local religious leaders frequently have intimate knowledge of existing histories, networks, and sociocultural dynamics influencing individual and community health and wellbeing, which positions them as important resources for health interventions (Powell 2014). As such, local religious leaders have been identified as change agents key to promoting health awareness, disseminating health education, developing and implementing health interventions, and influencing health seeking behaviour (e.g., Sidibé et al. 2019; Rivera-Hernandez 2015; Kanda et al. 2013; Adedini et al. 2018, Ruark et al. 2019; Cohen-Dar and Obeid 2017)."

Reviewer: 2

Reviewer Name

Jill Olivier

Institution and Country

University of Cape Town, South Africa

Please state any competing interests or state ‘None declared’:

None declared

Please leave your comments for the authors below

Thank you for this opportunity to review this article. I am an advocate of empirical articles that report on implementation of interventions, that open the ‘black box’ of international NGOs (INGOs), and which seek to redress the imbalance of evidence on faith-community or faith-based organisations in health and development – as well as addressing contexts such as the Philippines which are under-reported. For all those reasons, this is an interesting article that is worth pursuing.

Response: Thank you for your insights, we have found them very useful in strengthening our article.

However, I do have some concerns/queries – clustered in three main (but related) areas:

FIRSTLY, the article has some challenges in that it comes across as imbalanced or ‘advertorial’ towards the implementing organization of focus (International Care Ministries), which appears to have supported this study and paper (although this is not acknowledged in the main text). This imbalance is felt particularly strongly in the introduction and conclusion sections.

Response: Thank you for pointing out the imbalanced tone of this article, we have made efforts to address this issue throughout, which we highlight below.

>> For example PG4: “Faith-based organization (FBOs) play a critical role in delivering healthcare in low resource settings… etc”

This introductory section is imbalanced. These arguments have indeed been made in those referenced articles, but those authors also describe the other/dark side of FB engagement, the potential ‘negative effects’ of faith-community/organization engagement. The authors do not acknowledge this at all, so this is very selective argument.

Response: We appreciate you bringing our attention to this imbalance, we have added to the introduction in order to discuss the potential ‘negative effects’ of faith-community/organization engagement. The second paragraph of the introduction now reads:

> “Despite the critical role that FBOs play in delivering healthcare in low resource settings, the capabilities and assets of local FBOs have been underused and underexplored [1 ,5]. This

> underutilization may be influenced by concerns that the religious underpinnings of FBOs contradict human rights and associated health outcomes, such as in the case of sexual and reproductive health or resistance to vaccinations (Tomkins et al. 2015; Duff and Buckingham 2015). Challenges also exist with the alignment of health priorities between FBOs and national health systems (De Gruchy et al. 2006; Karam et al. 2015), inconsistent funding and governance of local FBOs (Karam et al. 2015), and their limited capacity to adapt to changing health systems (Olivier et al. 2015)."

>> For example PG 11: "…existing trust in local religious leaders and institutions offers an opportunity for high-quality care to be provided in partnership with local religious leaders and institutions"

There is no evidence provided that this was a particularly 'high quality' intervention provided by the FBO-ICM (high quality compared to what? How was quality assessed?). As far as I can see, there was no quality assessment, and this therefore appears to be a judgement made by the authors – possibly about their own institution and intervention?

Response: *We appreciate you noting this potentially biased statement. We have removed the word 'high-quality' from this sentence, which now reads:*

> "Our study suggests that for households experiencing extreme poverty that do not have an established trusting relationship with their local government, existing trust in local religious leaders and institutions (i.e., local pastor and church) offers an opportunity for <u>healthcare</u> to be provided in partnership with <u>these actors"</u>

>> For example: below, I also address some difficulties I have with conclusions made against data collected – in that the authors appear to overextend the data on trust in religious leaders to conclude that faith-based organisations should receive more financial support. I address this more later – but this is another example of a potential lack of impartiality.

Response: *Again, thank you for highlighting this as an area of concern. We have made changes to address your concerns with these conclusions, which we expand on below.*

SECONDLY, related to the first concern, this paper does not have a limitations section – which could have addressed many questions I had about the study.

Response: We have added a paragraph on limitations to the discussion section of our paper, and hope that this section will address the issues you have highlighted.

> "This study faced several a number of limitations. The findings represent the outcomes of one program implemented by a specific Christian FBO in the Philippines and its partnership with Protestant religious leaders and churches, which might not be readily generalizable to other settings. The data collected was limited to surveyors directly asking caregivers of children in

income poor communities to respond to questions about their trust in individuals and groups connected to the organization providing them with services or resources. Additionally, the models presented were restricted to exploring the covariates included in the baseline survey. It is also important to disclose that two of the authors received funding from ICM, which informs our view on the importance of partnerships with religious leaders and institutions in delivering health programs in income poor settings, and could be biased toward protecting the faith-based partnerships of ICM. Finally, the sustainability of these results over time could be questioned as the data were collected up to and including May 2013. To address this limitation, future replication studies are planned."

>>For example: in the main messages, it states "the study could not account for all potential confounders", but this is not addressed in the main text at all. Where in the text do the authors consider potential confounders?

Response: We have clarified in the methods section that a number of covariates were explored in the model, and we have added a sentence explaining that we are not able to address all potential confounders within the limitations paragraph added to the discussion.

> "Additionally, the models presented were restricted to exploring the covariates included in the baseline survey."

>>For example, it is not clear what type of religious groups were surveyed – ie the word 'church' in the question to participants – although the Philippines is pre-dominantly Catholic, there are indeed multiple types of Christian groups, as well as other religions present in these areas. It would have been useful to understand how the data was limited in relation to this nuance?

Response: We have added two sentences to the methods section, explaining what kind of religious groups were represented in our study, and have discussed this as a potential limitation in our discussion section. In the methods we state:

> "Pastors, who were associated with various Protestant denominations, consulted a list of malnourished children kept by local health centers, followed by house-to-house visits to complete enrollment. All malnourished children were eligible for enrollment, regardless of religious affiliations."

In the limitations section, we state:

> "The findings represent the outcomes of one program implemented by a specific Christian FBO in the Philippines and its partnership with Protestant religious leaders and churches, which might not be readily generalizable to other settings."

>>For example, I have been in some of these small communities in the Philippines. Asking a caregiver of an extremely malnourished child whether they trust the religious leader or the barangay (consisting of community leaders, usually inclusive of that same religious leader) is not without limitations. Freedom to respond openly is not a given in such small communities, in particular when that survey is connected to resource allocations from the INGO (I discuss the limitations of this distinction of barangay and religious leader more below).

Response: Thank you for highlighting this limitation, as we acknowledge that it was a potential concern for us with this study. We describe the potential bias of our data collection method in the added sentence:

> "The data collected were limited to surveyors directly asking caregivers of children in income poor communities to respond to questions about their trust in individuals and groups connected to the organization providing them with services or resources."

>>For example, the authors do not address the limitations of their own positionality (in relation to the object and funder ICM).

Response: We agree that it is important to include this in the main text, we have added a sentence in the limitations section in the discussion in which we reflect on our positions in relation to the object and funder of the study:

> "It is also important to disclose that two of the authors received funding from ICM, which informs our view on the importance of partnerships with religious leaders and institutions in delivering health programs in income poor settings."

>>For example, the study is limited by the fact that the evidence was gathered 7 years ago (2012-2013), and is now somewhat dated. This is not insurmountable, but needs to be addressed. Also, given the limitations of data collection, there is no way of knowing whether the trust that was seen and change that was assumed was sustainable, or whether those children fell back into malnutrition (or trust was maintained) after the 2014 intervention end? (this is perhaps beyond the scope of the study, but it would be good to see some reflection from the authors on this point).

Response: We agree that the data set is somewhat dated and we are unable to address whether the change was sustainable after the 2014 intervention ended. We reflect on this as a limitation of our study in the discussion section:

> "Finally, the sustainability of these results over time could be questioned as the data were collected up to and including May 2013. To address this limitation, future replication studies are planned."

THIRDLY, there appears to be a major disconnect between the gathered data and the conclusions made – and some over-reaching of conclusions.

Response: We appreciate that you have provided such specific examples of disconnect between the gathered data and the conclusions made. We have carefully edited these sentences to address these concerns, which we highlight below.

>> Such as the disconnect between data on individual trust in local religious leaders and claims of organisational effectiveness of FBOs. For example, some of the argument and the data is about individual relationships (does an individual trust an individual local religious leader). But the conclusions that are made then suggests that this trust is equivalent to trust in all 'FBOs' (in this case, an international NGO).

Response: We have edited these sentences to make it clearer that the data collected was about individual trust in local religious leaders and institutions (i.e., local pastors and churches), and that in our study this trust was associated with retention to the program run by one specific NGO (International Care Ministries).

PG 11: "Our study confirmed that trust in religious leaders and institutions was a determinant of treatment adherence among participants attending a program administered by a faith-based organization in partnership with local religious leaders. Thus, this model of service delivery provides an opportunity for the public sector to support and partner with faith-based organizations to offer complementary care to address acute child malnutrition."

This kind of statement is concerning for several reasons.

- As far as I could see, the survey did not ask, 'do you trust the FB-INGO" – and it is simply NOT an automatic leap from trust in your local religious leader to trust in all FBOs. What is the logical bridge?

Response: We have reworked this sentence to be more specific in the discussion of our results. It now reads:

> "Using quantitative analysis of survey data, our study confirmed that trust in religious leaders and institutions was a determinant of retention among participants attending the Malnourished Child Outreach program administered by ICM in partnership with local religious leaders. This model of service delivery provides an example of a potential strategy FBOs in the Philippines, and elsewhere, can use to contribute to addressing acute child malnutrition – namely, leveraging pre-existing trust in religious leaders and institutions among households experiencing extreme poverty to promote health program retention among vulnerable populations."

This tension plays out in other places – for example, why is the introduction and conclusion about PPP, when the survey and data seems to be about individual-level trust mechanisms, not institutional-level PPP (which is a different field of enquiry? There is a substantial literature that would be more relevant than the PPP literature - in particular that which addresses individual/community trust in religious leaders as change agents (trusted individuals / key influencers), and whether this enhances intervention/behaviour change. None of this literature is acknowledged in this article at all, which is a substantial gap. The research question is not clear, but it seems to me that the question this survey addresses – is around whether local religious leaders work well as a mechanism for local intervention. This question, about religious leaders as key influencers, is already being asked widely in the broader literature – and it is rather odd that the authors do not connect to this. In this debate, there are some hard questions being asked, such as whether religious leaders can be effective key influencers in reproductive health (behaviour change) interventions. There is a large literature on this in the HIV/AIDS research arena. Trust in religious leaders is critically important for such conversations. So connecting to that literature would have made this small study more substantively relevant.

Response: Thank you for suggesting we engage with the literature on trust with religious leaders, we have found that this greatly contributed to re-framing our paper. We have added a paragraph to the introduction about the role of religious leaders in healthcare delivery, as well as incorporated this literature throughout our discussion. In the introduction, we have added the following paragraph:

> "Collaboration with local religious leaders and their institutions is one factor contributing to the success of health interventions implemented by FBOs. Many communities view religious leaders and institutions as a trustworthy and credible source of health advice and information, with research finding that religious leaders' opinions can strongly influence social and behavioural norms (e.g., Rivera-Hernandez 2015; Kanda et al. 2013; Adedini et al. 2018; Cohen-Dar and Obeid 2017). As embedded members of their communities, local religious leaders frequently have intimate knowledge of existing histories, networks, and sociocultural dynamics influencing individual and community health and wellbeing, which positions them as important resources for health interventions (Powell 2014). As such, local religious leaders have been identified as change agents key to promoting health awareness, disseminating health education, developing and implementing health interventions, and influencing health seeking behaviour (e.g., Sidibé et al. 2019; Rivera-Hernandez 2015; Kanda et al. 2013; Adedini et al. 2018, Ruark et al. 2019; Cohen-Dar and Obeid 2017)."

Additionally, we have removed much of our discussion on PPP as we found the new framing on trust in religious leaders to be better suited for our discussion.

- [PS: small note, but the authors reference L Gilson's work on trust in health systems to support this study - but as far as I am aware, Gilson's work does not apply the same framework for researching trust as is being applied in this study, so those references do not seem entirely appropriate].

Response: Although we agree that Gilson's work on trust in health systems does not apply the same framework for researching trust as we do in this study, we believe that her work is important to reference as it discusses the different levels at which trust can impact health systems (including both individual relationships and broader structures).

>> A more concerning example, the individual level data on slightly elevated trust in religious leaders, does not seem to justify a strong conclusion that FBOs should receive more financial support from the public sector and multinationals (and this again is suggestive of bias). Pg 11 "Based on the promising findings from our study, complementary models of healthcare delivery between faith-based organizations of public health facilities that leverage public funding, in addition to bilateral and multi-lateral support should be further explored and evaluated"

Response: To address this concern, we have reworked the conclusion of our discussion, which now reads:

> "Community-based care offered by FBOs in partnership with local religious leaders and institutions presents an opportunity to engage with and support income poor households with weak social networks. Based on this finding, we suggest that the delivery of healthcare through FBOs that build on pre-established trusting relationships with local religious leaders and institutions, should be further explored and evaluated.".

>>For example, in the abstract the first sentence suggests this is an evaluation of intervention 'effectiveness' – when in fact there is very little assessment of effectiveness at all?

Response: To be more reflective of our manuscript findings, we have changed this sentence to read:

> "In the context of persistent child malnutrition in the Philippines, the objective of this study was to understand <u>the role</u> of one faith-based organization (FBO) in addressing moderate and severe acute malnutrition among children from households experiencing extreme poverty."

>>For example, the authors consistently use the term assessment of 'adherence' and 'treatment adherence' – but I do not see substantial data reported on 'adherence' per se (not as we would expect in a public health research)

Response: Thank you for highlighting that you did not find that the term "adherence" adequately reflected our data. We have altered the description of our findings to use the term "retention" as we feel it more adequately reflects our results.

>>For example, in the key messages, Pg3, it claims that "The study was conducted in a unique setting for examining the underlying mechanisms associated with adherence to malnutrition treatment delivered by both government and faith-based organizations" – this suggests that some sort of comparative was conducted – whereas the main article does not address government provision (mechanisms or effectiveness) … (ie surely the question 'do you trust your barangay' is not being used as a proxy to compare all government interventions?)

Response: We have reworked this sentence in our key message so that it does not suggest a comparative was conducted, it now reads:

> “The study was conducted in a unique setting for examining the underlying mechanisms associated with the retention of participants in malnutrition interventions delivered by a faith-based organization to children in households experiencing extreme poverty.”

>>For example, PG 11: “Experiences of social exclusion (e.g. mistrust of public institutions) may influence health seeking behaviour and contribute to gaps in healthcare provision”. I did not see evidence of mistrust in public institutions? There was a slightly elevated trust in religious leaders above barangay (confused by the fact that the religious leader is likely often in the barangay), but does this show mistrust in public institutions?

Response: Thank you for noting that our evidence does not fully support that there was “mistrust of public institutions.” To support this claim, we reference existing work that has found this to be the case. This section now reads:

> “Experiences of social exclusion (e.g. limited trust in public institutions) have been found to influence health seeking behaviour and contribute to gaps in healthcare provision (Duckett et al. 2016). We suggest that when individuals feel socially excluded from public health services or institutions, providing health services through collaborations between FBOs and trusted religious leaders and institutions can act as a critical alternative.”

All of this suggests that this article is worth developing further, but needs major revision.

MINOR COMMENTS: this article requires some editing – including spelling out acronyms on first use etc.

Response: The article was carefully edited, including spelling out acronyms on first use (e.g. UNICEF, PPI and ICC were each spelled out).

FORMATTING AMENDMENTS (if any)

Required amendments will be listed here; please include these changes in your revised version:

List of Added References:

Adedini, S. A., Babalola, S., Ibeawuchi, C., Omotoso, O., Akiode, A., & Odeku, M. (2018). Role of religious leaders in promoting contraceptive use in Nigeria: evidence from the Nigerian Urban Reproductive Health Initiative. Global Health: Science and Practice, 6(3), 500-514.

Cohen-Dar, M., & Obeid, S. (2017). Islamic Religious Leaders in Israel as Social Agents for Change on Health-Related Issues. Journal of religion and health, 56(6), 2285-2296.

Duckett, J., Hunt, K., Munro, N., & Sutton, M. (2016). Does distrust in providers affect health-care utilization in China?. Health policy and planning, 31(8), 1001-1009.

Hung, N., & Lau, L. L. (2019). The relationship between social capital and self-rated health: a multilevel analysis based on a poverty alleviation program in the Philippines. *BMC Public Health*, *19*(1), 1641.

Kanda, K., Jayasinghe, A., Silva, K. T., Priyadarshani, N. G. W., Delpitiya, N. Y., Obayashi, Y., ... & Tamashiro, H. (2013). Religious leaders as potential advocates for HIV/AIDS prevention among the general population in Sri Lanka. Global public health, 8(2), 159-173.

Lipsky, A. B. (2011). Evaluating the strength of faith: Potential comparative advantages of faith-based organizations providing health services in sub-Saharan Africa. *Public Administration and Development*, *31*(1), 25-36.

Powell, C. L. (2014). Working together for global health goals: The United States Agency for International Development and faith-based organizations. Christian Journal for Global Health, 1.

Rivera-Hernandez, M. (2015). The role of religious leaders in health promotion for older Mexicans with diabetes. Journal of religion and health, 54(1), 303-315.

Ruark, A., Kishoyian, J., Bormet, M., & Huber, D. (2019). Increasing family planning access in Kenya through engagement of faith-based health facilities, religious leaders, and community health volunteers. Global Health: Science and Practice, 7(3), 478-490.

Sidibé, B., Pelster, A. K., Noble, J., & Dinkel, D. (2019). Health promotion needs in faith-based organizations: Perceptions of religious leaders in Bamako. Journal of religion and health, 58(2), 639-652.

Tomkins, A., Duff, J., Fitzgibbon, A., Karam, A., Mills, E. J., Munnings, K., ... & Yugi, P. (2015). Controversies in faith and health care. The Lancet, 386(10005), 1776-1785.

Widmer, M., Betran, A. P., Merialdi, M., Requejo, J., & Karpf, T. (2011). The role of faith-based organizations in maternal and newborn health care in Africa. International Journal of Gynecology & Obstetrics, 114(3), 218-222

VERSION 2 – REVIEW

REVIEWER	Lynn Lieberman Lawry MD, MSPH, MSc Associate Professor Division of Global Health Department of Preventive Medicine and Biostatistics F. Edward Hebert School of Medicine Uniformed Services University Bethesda, Maryland, USA
REVIEW RETURNED	31-Mar-2020

GENERAL COMMENTS	This is much better than the first review and addressed most of my comments. Please define Barangay for the reader. Under Discussion (lines 45-47) is a mix of disclosures and limitations. A limitations section is needed. The final concluding paragraph: you might consider just starting after line 54. It would make it more succinct and focus the reader on the key message.

REVIEWER	Jill Olivier University of Cape Town, South Africa
REVIEW RETURNED	15-Apr-2020

GENERAL COMMENTS	Thank you, the revision of this paper has improved it substantially, and most of my earlier concerns have been addressed. I do still have some remaining suggestions: Please look closely at the stated Objectives of the Study (in the abstract) an (“the objective of this study was to understand the role of one faith-based organization (FBO) in addressing moderate and severe acute malnutrition”) - and reflect on whether that is actually what is done and reported on in this paper. It does not seem to me that the discussion or conclusion really "describe a role". There is some unresolved confusion in the new version. This was originally presented as an intervention evaluation report (eg abstract organised by setting and intervention participants etc) – but is now (appropriately) reframed as a research article reporting on a retrospective cohort STUDY. However, it is not organised as a study. For example, in the abstract, the first two sections speak about the study objectives and study setting … but then the rest is talking about the INTERVENTION participants not the STUDY participants. They are not the same thing – and the whole abstract should address the cohort study. The same for how the main text in the article is framed (eg you might have a short background/method section describing the intervention, but the bulk of the argument on objectives, method etc need to be about the STUDY – the study and intervention are not the same thing. For example:

	- "Participants" (in abstract) – is participants IN THE INTERVENTION – not participants in the STUDY – or so I am guessing, as you can't get 6month children to respond to your survey … so I would imagine the "STUDY participants" are 1,192 (not 1219) "adult caregivers of …."? (note, the appropriate number here is the number of respondents surveyed, not number of participants in the intervention) - Results – it is the 'CAREGIVER'S level of trust right? Not the CHILDREN'S level of trust? (please give clarity on this throughout) - The Strengths and Limitations section keeps talking about 'the participants' – do you mean intervention participants, or study participants? - In a report of a cohort study, you would expect to see explanation of sampling. I understand why there was no sampling (that you utilised data on all intervention participants you could is fine) – but I would expect a more 'cohort study-type' explanation then of why there was no purposive/statistical sampling. - I think it would be useful to more clearly explain to the reader that this was an 'OPPORTUNISTIC' retrospective cohort study – ie the intervention was designed and conducted first – the study was designed later, and opportunistically used the evaluation data from the intervention. This has important ramification for validity of the study – and would give you more leeway (with the critical reader) as they would understand better that the data doesn't perfectly match/answer some of your questions – because it wasn't designed to do so. Please take care with your strong claim: "The retrospective cohort design of this study is a novel approach for exploring the role of religious leaders in health interventions". Actually, this is not 'novel' at all – there are a LOT of evaluations conducted with FBOs using a retrospective cohort design (especially in the HIV/AIDS terrain) – I would ask that this claim is more carefully framed/caveated. Under method (intervention and study design) I would more clearly have a sub-section on intervention design, and then a section on study-design – this would help with this clarification between the two I was suggesting earlier. Pg12: Your main argument "We suggest that when households experiencing extreme poverty trust local religious leaders and institutions (i.e., local pastors and churches) an opportunity exists for these actors to provide them with needed healthcare services. This may be especially true for households with the greatest intensity of poverty, who were the least likely to drop out of the program offered by ICM in partnership with local religious leaders." Please could you unpack this argument, I am afraid I am not following it. Are you arguing that households in extreme poverty need to trust local religious leaders/inst to access healthcare service opportunities? Or need to trust for those opportunities to be provided? Sorry, I don't follow. [And I would suggest that if a household in extreme poverty was offered needed healthcare services from a Lithuanian heavy metal band, they would take advantage of that opportunity, trust or no trust … and yes, it seems logical that those most in poverty are least likely to drop out … perhaps this might be framed as a confirmation of what you would expect, not a new finding? Pg12: "the trusting relationships that FBOs as well as religious leaders and institutions" – this demonstrates my earlier review

	comments, that has not been addressed in this version. There is a lack of clarity on whether you are focusing on trust in individuals or institutions (and surely an FBO is an institution, making this sentence particularly odd?). You note in your review response that you want to look at all types of trust, and that is why the Gilson reference is appropriate. That Gilson ref (as well as the broader field of research on trust in public health) usually distinguishes between "interpersonal" and "institutional" trust, and there is also a whole literature on the relationship between individual and institutional trust. I would still prefer better alignment with this global literature – and a clearer distinction between interpersonal and institutional trust throughout the paper (so relating to your introduction, your methods section, how your data/results are presented, and how you nuance your discussion and findings – eg are you speaking about 'pre-existing' trust-relationships with embedded local religious leaders (individuals), or trust in 'foreign' FBO-institutions. Pg12: "our study confirmed that trust in religious leaders and institutions was a determinant of retention among participants attending the Malnourished Child Outreach program administered by ICM in partnership with local religious leaders" Please can you unpack in more detail how this conclusion was made. I am not clearly seeing the evidence of this in the results, nor the explanation for how those results provide the evidence for this conclusion in the discussion. [I am not saying this is not true, I am suggesting it is not sufficiently evidenced/argued for the reader to believe this claim]. Conclusion: speaks of "build on pre-ESTABLISHED trusting relationships" – in other places, you talk of pre-EXISTING trust. I think these are quite different (are we talking about the pre-existing trust between religious leaders and local community, or some sort of trust that an FBO establishes with the community before intervention by involving the religious leaders? Please could you carefully consider and clarity? It is your main finding, so needs to be very specific. (It would be useful for this 'pre-existing' issue to be addressed more clearly in the intro/methods section … as it suddenly appears in the discussion and conclusion (and I can only assume is linked to 'baseline' survey results? – although that is not clearly explained).

VERSION 2 – AUTHOR RESPONSE

Reviewer 1

This is much better than the first review and addressed most of my comments.

Response: Thank you

Reviewer 1: Please define Barangay for the reader.

Response: We have now defined the 'barangay' within the paper when it is first used. The definition added to the table reads:

"A 'barangay' is the smallest administrative division in the Philippines, and represents the local government"

Reviewer 1: Under Discussion (lines 45-47) is a mix of disclosures and limitations.

Response: We agree that we should not mix the disclosures with limitations. We have now moved the disclosure to the competing interest declaration:

Competing Interests: Dr. Lau reports that Lau and Han's were paid salaries by ICM as research staff which informs our view on the importance of partnerships with religious leaders and institutions in delivering health programs in income poor settings. They were both given full freedom to publish positive and/or negative results.

Reviewer 1: A limitations section is needed.

Response: Yes, we agree that a limitations section is important. The edited limitations are found in the second last paragraph and states:

This study faced several limitations. The findings represent the outcomes of one program implemented by a specific Christian FBO in the Philippines and its partnership with Protestant religious leaders and churches, which might not be readily generalizable to other settings. The data collected were limited to enumerators directly asking caregivers of children in income poor communities to respond to questions about their trust in individuals and groups connected to the organization providing them with services or resources. Additionally, the models presented were restricted to exploring the covariates included in the baseline survey. An important limitation in the survey was the lack of distinction between interpersonal trust in religious leaders and trust in religious institutions. As a result, we were not able to distinguish between these within this study and will aim to explore these different types of trust in future work. Finally, the sustainability of these results over time could be questioned as the data were collected up to and including May 2013. To address this limitation, future replication studies are planned.

Reviewer 1: The final concluding paragraph: you might consider just starting after line 54. It would make it more succinct and focus the reader on the key message.

Response: Thank you for this comment. We have re-structured the concluding paragraph to improve the flow and highlight the key message.

Reviewer 2

Thank you, the revision of this paper has improved it substantially, and most of my earlier concerns have been addressed. I do still have some remaining suggestions:

Response: Thank you for the comments.

Reviewer 2: Please look closely at the stated Objectives of the Study (in the abstract) an ("the objective of this study was to understand the role of one faith-based organization (FBO) in addressing moderate and severe acute malnutrition") - and reflect on whether that is actually what is done and reported on in this paper. It does not seem to me that the discussion or conclusion really "describe a role".

Response: We agree that this study did not really address the 'role' of an FBO, and have reframed the stated objectives to read:

In the context of persistent child malnutrition in the Philippines, the objective of this study was to understand the mechanisms at play when a faith-based organization (FBO) addressed moderate and severe acute malnutrition among children from households experiencing extreme poverty.

Reviewer 2: There is some unresolved confusion in the new version. This was originally presented as an intervention evaluation report (eg abstract organised by setting and intervention participants etc) – but is now (appropriately) reframed as a research article reporting on a retrospective cohort STUDY. However, it is not organised as a study. For example, in the abstract, the first two sections speak about the study objectives and study setting … but then the rest is talking about the INTERVENTION participants not the STUDY participants. They are not the same thing – and the whole abstract should address the cohort study. The same for how the main text in the article is framed (eg you might have a short background/method section describing the intervention, but the bulk of the argument on objectives, method etc need to be about the STUDY – the study and intervention are not the same thing.

For example:
- "Participants" (in abstract) – is participants IN THE INTERVENTION – not participants in the STUDY – or so I am guessing, as you can't get 6month children to respond to your survey … so I would imagine the "STUDY participants" are 1,192 (not 1219) "adult caregivers of …."? (note, the appropriate number here is the number of respondents surveyed, not number of participants in the intervention)

Response: Thank you for pointing this out, we need to be clear that in the abstract we are referring to study participants. The abstract now reads:

Study Participants: Caregivers of 1,192 children experiencing moderate acute malnutrition and severe acute malnutrition between the ages of 6 to 60 months

We have reviewed the use of the word 'participant' in the rest of the article. We have either replaced the word for clarity, or if used, we believe it is clear within the sentence that we are referring to participants in the intervention.

Reviewer 2: - Results – it is the 'CAREGIVER'S level of trust right? Not the CHILDREN'S level of trust? (please give clarity on this throughout)

Response: Yes, that is correct, we apologize for this and have clarified the following sentences:

…the caregiver's trust in religious leaders or church was negatively associated with dropout (-0.87; [95CI: -1.43, -0.26]), suggesting that each increased level of satisfaction or trust was associated with a decreased proportion of dropouts from the treatment program.

The caregiver's trust in the local barangay was associated with dropout in the reverse direction…

Neither measure of caregiver trust (in religious leaders and church, or local government) was found to be significantly correlated with WHZ2.

The logistic mixed effects model showed a significant negative association (-0.87; [95CI: -1.43, -0.26]) between dropout a caregiver's trust in religious leaders and institutions, confirming that this type of trust was a determinant of retention among participants attending the Malnourished Child Outreach program administered by ICM in partnership with local religious leaders.

Reviewer 2: - The Strengths and Limitations section keeps talking about 'the participants' – do you mean intervention participants, or study participants?

Response: We apologize for the lack of clarity, and the confusion between intervention participants and study participants. We have rewritten these two points to read:

• The study was conducted in a unique setting for examining the underlying mechanisms associated with the retention of children enrolled in malnutrition interventions delivered by a faith-based organization to children in households experiencing extreme poverty

• While multiple factors were considered to examine treatment outcomes in child malnutrition interventions, the study could not account for all potential confounders within the complex social settings where the study was conducted

Reviewer 2: - In a report of a cohort study, you would expect to see explanation of sampling. I understand why there was no sampling (that you utilised data on all intervention participants you could is fine) – but I would expect a more 'cohort study-type' explanation then of why there was no purposive/statistical sampling.

Response: Yes, we agree that sampling should be explained, and we have re-organized the methods section in light of other suggestions below. The relevant section in the methods now reads:

This was an opportunistic study, retrospectively designed to utilize household surveys that ICM administered to the caregivers of the children enrolled in the MCO program and treatment outcome data. As a result, all households with complete data were included as study participants within the retrospective cohort.

Reviewer 2: - I think it would be useful to more clearly explain to the reader that this was an 'OPPORTUNISTIC' retrospective cohort study – ie the intervention was designed and conducted first – the study was designed later, and opportunistically used the evaluation data from the intervention. This has important ramification for validity of the study – and would give you more leeway (with the critical reader) as they would understand better that the data doesn't perfectly match/answer some of your questions – because it wasn't designed to do so.

Response: We agree and please the previous response, which includes clarification on the opportunistic nature of this study.

Reviewer 2: Please take care with your strong claim: "The retrospective cohort design of this study is a novel approach for exploring the role of religious leaders in health interventions". Actually, this is not 'novel' at all – there are a LOT of evaluations conducted with FBOs using a retrospective cohort design (especially in the HIV/AIDS terrain) – I would ask that this claim is more carefully framed/caveated.

Response: We apologize for the strength of the claim, and have re-written the sentence:

The retrospective cohort design of this study is an opportunistic and practical approach for exploring the role of religious leaders in health interventions

Reviewer 2: Under method (intervention and study design)
I would more clearly have a sub-section on intervention design, and then a section on study-design – this would help with this clarification between the two I was suggesting earlier.

Response: Yes, thank you for this suggestion. The Methods section now includes a sub-section on Intervention Design, followed by a sub-section on Study Design that has been expanded.

Reviewer 2: Pg12: Your main argument "We suggest that when households experiencing extreme poverty trust local religious leaders and institutions (i.e., local pastors and churches) an opportunity exists for these actors to provide them with needed healthcare services. This may be especially true for households with the greatest intensity of poverty, who were the least likely to drop out of the program offered by ICM in partnership with local religious leaders." Please could you unpack this argument, I am afraid I am not following it. Are you arguing that households in extreme poverty need to trust local religious leaders/inst to access healthcare service opportunities? Or need to trust for those opportunities to be provided? Sorry, I don't follow. [And I would suggest that if a household in extreme poverty was offered needed healthcare services from a Lithuanian heavy metal band, they would take advantage of that opportunity, trust or no trust … and yes, it seems logical that those most in poverty are least likely to drop out … perhaps this might be framed as a confirmation of what you would expect, not a new finding?

Response: We see how this argument was not clear and have rewritten these sentences. Instead of suggesting that there is a need for trust to access health care service opportunities, we want to highlight how trust can improve adherence, and the recognition of this mechanism is an 'opportunity' to improve these types of interventions. These are the rewritten sentences:

We suggest that when households experiencing extreme poverty trust local religious leaders and institutions (i.e., local pastors and churches), they are more likely to adhere to services provided through these networks, and an opportunity exists for these actors to improve the delivery of health and social services. In addition to trust, we recognize that need also drives adherence as households with the greatest intensity of poverty were the least likely to drop out of the program offered by ICM.

Reviewer 2: Pg12: "the trusting relationships that FBOs as well as religious leaders and institutions" – this demonstrates my earlier review comments, that has not been addressed in this version. There is a lack of clarity on whether you are focusing on trust in individuals or institutions (and surely an FBO is an institution, making this sentence particularly odd?). You note in your review response that you want to look at all types of trust, and that is why the Gilson reference is appropriate. That Gilson ref (as well as the broader field of research on trust in public health) usually distinguishes between "interpersonal" and "institutional" trust, and there is also a whole literature on the relationship between individual and institutional trust. I would still prefer better alignment with this global literature – and a clearer distinction between interpersonal and institutional trust throughout the paper (so relating to your introduction, your methods section, how your data/results are presented, and how you nuance your discussion and findings – eg are you speaking about 'pre-existing' trust-relationships with embedded local religious leaders (individuals), or trust in 'foreign' FBO-institutions.

Response: We agree that it is important to distinguish between interpersonal and institutional trust. As a retrospective study, we did not have an active role in survey design, and regrettably the question asked to the respondents did not distinguish between these two types of trust. It is for this reason that we cannot split the two in this study, and have added this in the limitations:

Fourth, there was the lack of distinction between interpersonal trust in religious leaders and trust in religious institutions in the survey. As a result, we were unable to distinguish between these types of trust within this study.

Reviewer 2: Pg12: "our study confirmed that trust in religious leaders and institutions was a determinant of retention among participants attending the Malnourished Child Outreach program administered by ICM in partnership with local religious leaders"
Please can you unpack in more detail how this conclusion was made. I am not clearly seeing the evidence of this in the results, nor the explanation for how those results provide the evidence for this conclusion in the discussion. [I am not saying this is not true, I am suggesting it is not sufficiently evidenced/argued for the reader to believe this claim].

Response: To unpack this conclusion further and provide the basis of the statement, we have included the coefficients from the statistical models in this part of the discussion.

The logistic mixed effects model showed a significant negative association (-0.87; [95CI: -1.43, -0.26]) between dropout a caregiver's trust in religious leaders and institutions, confirming that this type of trust was a determinant of retention among participants attending the Malnourished Child Outreach program administered by ICM in partnership with local religious leaders.

Reviewer 2: Conclusion: speaks of "build on pre-ESTABLISHED trusting relationships" – in other places, you talk of pre-EXISTING trust. I think these are quite different (are we talking about the pre-existing trust between religious leaders and local community, or some sort of trust that an FBO establishes with the community before intervention by involving the religious leaders? Please could you carefully consider and clarity? It is your main finding, so needs to be very specific. (It would be useful for this 'pre-existing' issue to be addressed more clearly in the intro/methods section … as it suddenly appears in the discussion and conclusion (and I can only assume is linked to 'baseline' survey results? – although that is not clearly explained).

Response: Thank you for pointing this out. We used pre-established and pre-existing interchangeably to signify that it was trust measure at baseline, prior to the intervention. We can see the confusion this causes, and have removed the term 'pre-established' edited other uses of pre-existing to read:

leveraging trust in religious leaders and institutions prior to the intervention among households experiencing extreme poverty to promote health program retention among vulnerable populations.

We have also clarified in the methods, when referring to the baseline survey:

Indicators of pre-existing social capital were also explored including group membership, trust in local religious leaders and institutions, and trust in local public healthcare facilities