Dear Editor:

We are pleased to resubmit for publication the revised version of “Religiosity and sexual abstinence among Nigerian youths: Does parent religion matter?” We appreciate the comments of the Editor and the reviewers and it has helped improve the manuscript greatly. We have addressed each of their concerns as outlined below:

Reviewer reports:

(Reviewer 1):

This paper uses data from Nigerian youths to study the association between religiosity and sexual abstinence among youth. My main concerns with the paper have to do with the sample and the measure for religiosity:

What were the study exclusion criteria, and did the study exclude married youth? If married youth are included, could a separate set of analysis that excludes married youth be conducted? If highly religious youth get married earlier/later, that would bias the association between religiosity and abstinence.

The study was for unmarried youth. The inclusion criteria was youth aged 15-24 who have never been married and not currently married. This has been noted in manuscript (pg 5, line 26).
Was the measure for religiosity adapted from previous studies, or was it based on a previously validated instrument? If so, please cite the studies. If an original measure was developed, please state why that was preferred over existing measures.

This was adapted from other studies which have been cited (pg8, line 16-17).

The use of different cut-offs of frequency of religious behavior based on the respondent's religion (i.e., defining religiosity for christians and muslims differently) needs more justification. Is this commonly done in the literature?

It's not commonly done in literature because of the different context and majority of studies in SSA have focused on affiliation alone. However, Nigeria’s religion is nearly equally divided between Christianity and Islam, while people who practice other forms of religion are less than 2% in the population. The frequency of attendance differs for Muslims and Christians which is why we chose to have different cut-offs before combining them into a single construct.

Also, it appears that the author equates praying 5 times a day to attending religious services 5 times a day. Is this how the respondents would have interpreted the survey question? I.e., would a Muslim respondent who is praying at home consider that a religious service?

The question specified how many times they attended religious service which is why we created different measures for each religious sect (pg 9, line 20).

How did the author classify frequency of religious behaviour for respondents of other faiths (i.e., not Muslim or Christian)?

We dropped this from the sample as they were less than 1%.

How was salience of religion measured?

Religious salience is defined as the relative importance of religion in one’s personal life and this response ranged from “very important”, “important” and “not important”. (pg 8, line 22).

Please describe in more detail how the dimensions of religiosity were scored and combined.

This has been described (pg 9, line 4-6)

A large majority (88%) of the sample is highly religious, which likely reflects the source population. This makes the 12% who are not highly religious a very select group that is able to go against the societal norms by not being highly religious. Could their low sexual abstinence be an extension of their tendency to against societal norms? What can the author tells from the data and from previous literature on why this small group of youth are not religious? Without this information, it is difficult to say that the low religiosity group has low rates of sexual abstinence because of their low religiosity and not because of other factors that may have brought about their low religiosity, e.g., exposure to media, risk taking, etc.
I have provided a table showing sample characteristics by religiosity of respondents. I have also referred to existing literature that explains religiosity in Nigeria in the theoretical framework section (pg 4, ln 28). This has also been explained on page 12, line 26 (Wusu, 2011).

In addition, I have the following comments listed in roughly chronological order:

P 1 line 45. I recommend that the author remove "important"

This has been removed.

P 2 line 6. Please clarify what forms of abuse are referred to here.

This has been corrected.

P 2 line 10. Please reword "low percentage of females using condoms" so that it is clear what the issue is. E.g., is the concern that females are engaging in unprotected sex? Or that females are not engaging in sex (protected or unprotected)? Or that it is males who are using condoms and not females?

This has been corrected.

P 2 line 11. Please clarify which behaviours you are referring to.

This has been corrected.

P 2 line 13. The sentence beginning with "These factors..." needs to be made more clear. E.g., what factors is the author referring to?

This has been corrected.

P 3 lines 35-44. There are two different studies cited which are reported as if it was one study. The use of "the authors" is problematic.

This has been corrected.

P 3 lines 44-51. Please clarify what was the reference category for the cited study

This was stated as delayed sexual debut and condom use at last sex. Delayed sexual debut was a continuous variable so there was no reference category.

The Hypotheses section outlines the mechanisms through which youth religiosity affects sexual abstinence. The author writes, "This association will operate, in part, through parental religion and presence of parents in the household. It will also be mediated by family and community social capital." The author should provide evidence to support these statements. Also, please explain how youth religiosity can affect these hypothesized mediators (parental religion, co-residence of parents, social capital).
This claim has been referenced in the introduction and theoretical framework (pg 4, line 13-15; 31-32) and hypothesis section has been reworked.

P 5 line 35. Please clarify what "positive sexual behaviours" are.

This has been explained (pg 5, line 21).

Please explain why selection of states was done based on ethnicity rather than religion.

The main study was interested in examining prevalence and why youth engage in protective sexual behaviours. This aim of this paper was to examine association between religiosity and abstinence so the data was not collected solely for measuring religion alone. In addition, there is a description of states by religiosity in the study area paragraph.

Why were states with the highest prevalence of "positive sexual behaviors" selected?

This study is part of a larger study that sought to examine the prevalence of protective sexual behaviours (PSB) among youth in Nigeria and also examine why youth choose to engage in PSB in the same environment as their counterparts who do not. Therefore, the selected states were selected based on the first objective that used a nationally representative dataset. In addition, selecting these states informed us about the factors that contribute to PSB behaviours so they can be strengthened.

In the sample size section, please state directly what effect or difference the sample size was aimed at detecting.

The current manuscript is a secondary analysis of data collected with the primary aim of examining prevalence of protective sexual behaviours and accordingly, the sample size for the original study was calculated to estimate this outcome with stated precision.

Why did the author decide to sample 571 youths in each state? Was the original plan to do analysis stratified by state? Also, why did the author decide to sample more than the necessary number youths from the same states rather than add more/new states to the study?

The sample size calculation was done to estimate protective sexual behaviours at state level with adequate precision. The purpose of the study was to focus on the 4 purposefully selected states because they met the inclusion criteria for the study which is why the study didn’t explore the entire country. Also, sample size was calculated separately for the selected states which has now been included in appendix 1.

Please state the total number of wards selected.

The states had different number of wards but this study selected 36 wards in each state.

Were any efforts done to ensure adequate numbers of both males and females were recruited? If so, what were the efforts?
There was no special effort made to ensure gender balance of respondents but research assistants were trained to ensure that sample should be representative.

What language(s) were used to interact with respondents?

English was used to interact with the respondents as most of them had basic understanding of English. The research assistants were proficient in English and the local languages in each of the states. They were trained to help clarify issues to respondents who presented in any difficulties (pg 7, line 18-21).

How was "sex" defined in the study (vaginal?, oral?, anal?) and what efforts were undertaken to ensure a consistent understanding of "sex" by respondents? E.g., across ethnic and age groups?

Sex was defined as vaginal sex and this was explained to the research assistants and respondents as vaginal sex.

P 9 line 5. Please explain why the author "expect[s] an individual to respond yes to about two of the measures."

This has been corrected to “highly religious”

How was Presence of a role model measured?

A question “is there someone who you know and interact with on a regular basis that you aspire to be like, either in the present or in the future?” was asked. Responses ranged from aunty, brother, family friend, grandparent, other, religious leader, sister, teacher and uncle. Youth who mentioned “other” listed some celebrities as their role models while some of them said their boyfriends were their role models.

Which parent's religion was measured? Mother's or father's? Please clarify.

We used the religion of head of household as a proxy for parental religion.

P 9 lines 8-13. Please explain why the variables listed were included in the analysis? Was it to control for confounding?

The variables listed were included in the analysis based on the reviewed literature and are also confounders. The unadjusted and adjusted results have been presented to examine the independent and net effect.

Please state how the complex survey design was accounted for in the analysis

The study was conducted among youth recruited using non-probability sampling methods from different locations where youth congregate and we could not obtain cluster sizes for youth in these locations. This has been included as a limitation of the study.
The author's literature review suggests that gender is a moderating variable. Why did the author not stratify the analysis by gender?

Although literature has suggested gender differences in the sexual behaviour of youth, the descriptive analysis did not show significant differences in the religiosity of youth. Also at the unadjusted level, there were no differences in the sexual behaviour of youth which is why we didn’t go on to stratify by gender in this present study.

It would be interesting to see if there is an interaction between religion and religiosity.

This is not the focus of this study and will be looked into for another study.

Policy recommendations (e.g., to provide youth with religious events) based on the evidence provided seems premature. I recommend that the author removes the policy implications.

I recommend that the author use sexual abstinence rather than sexual behavior or positive sexual behavior in the paper wherever possible. E.g., in the titles of the tables.

This has been done.

(Reviewer 2):

This is an important article with relevant findings and rigorous methods. A few edits and clarification are needed, as detailed below.

Abstract:

In the background section of the abstract, the research question addresses interests in mechanisms and moderation, but these findings are not touched on in the findings section of the abstract. Additional information in the findings section would be helpful.

These results have been presented in model 2 of table 3 and discussed (pg 11, line 26-27).

Background:

Line 2: Spelling: believe (should be "belief")

This has been corrected.

Line 8: Grammar: A research (should be "Research")

Sentence has been reworded.
Line 13: youth people?
This has been corrected.

Line 13: put youth at risk of what? Affect them in what way?
Sentence has been reworded.

The second paragraph ("despite this grim") would benefit from citations supporting the various claims.

References have been included (Fatusi & Blum, 2008; Odimegwu & Somefun, 2017).

The discussion of resilience and religion in the introduction is helpful, but it is not clear until later in the introduction that the focus is on HIV risk. It may be helpful to introduce language around HIV earlier, and give additional information regarding contexts of HIV risk, including prevalence and incidence, among young people in Nigeria.

p. 4 line 24 Perhaps refer to "the" religious landscape" in Nigeria rather than "her."
This has been changed.

Methods:

General editing is needed. There is unclear phrasing, incorrect capitalization throughout the paper.

The paper has been edited.

Study design: Rigorous sampling strategy and approach to data collection. Approximately how long did it take youths to complete the survey?
This is included on page 5, line 28.

Are other sexual/HIV risk outcomes reported elsewhere? The authors justify the focus on abstinence as the key indicator, but other outcomes are also certainly important to examining HIV risks among youth.

Yes, there are other sexual outcomes reported in some other papers that would be published from the bigger study.

The examination of separate religiosity scales across religions appears useful and appropriate, but it would be helpful to perhaps describe or cite that this conceptualization was based on consultation/context or other sources.
The statement at the end of the religiosity description that, "This is because we expect an individual to respond yes to about two of the measures" is not clear. Why would the researchers expect people to respond in a certain way? Was there variability in the sample?

This has been reworded. Based on the scores created from the measures of religiosity, highly religious youth are expected to respond “yes” to two of the measures.

The findings mention that youth living with their mother were more likely to abstain. Does this mean they lived only with their mother, or they lived with their mother and potentially a father as well? Later the non-significance of living with the father was mentioned, but it might be useful to mention this right after living with mother is mentioned.

This has been done.

On the role models variable, do we know what type of role model they had?

Yes, this has been included (pg 9, line 20-23)

Results of interactions, moderation, mediation are not explained in full detail in the findings section. It would be helpful to more deliberately point to how these questions (role of parents) were examined in the models and what the findings demonstrate.

This interaction effects were not significant but we have now presented in table 4 and discussed it.

Discussion

Has religion become important, or it remains important?

This statement has been corrected.

Are there any findings available regarding risks among those who are sexually active, which comprise a relatively large minority? It would be interesting to consider how these same variables mitigate their risks, although this would likely be a separate paper.

Thanks for this direction. This would be added as a frontier for further studies.

The authors mention the presence of parents, but it seems that only the presence of mothers is significant, in both protective/risk directions. It would be useful to explore these dynamics in more detail.