PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Factors associated with incomplete immunization in children aged 12 to 23 months at sub-national level, Nigeria – a cross-sectional study
AUTHORS	Eze, Paul; Agu, Ujunwa; Aniebo, Chioma; Agu, Sergius; Lawani, Lucky; Acharya, Yubraj

VERSION 1 – REVIEW

REVIEWER	Lim, Kuang Hock Institute of Public Health, Centre for Occupational Health Research
REVIEW RETURNED	18-Jan-2021

GENERAL COMMENTS	COMMENT FORM TO AUTHOR Manuscript Number: bmjopen-2020-047445 Title: Determinants of incomplete immunization in children aged 12 to 23 months at sub-national level, Nigeria – a cross sectional study In this manuscript, the authors analyzed the 12-23 months children immunization coverage and determinants in Enugu State. Cross sectional analysis was performed and several significant factors such as children of single mothers, children delivered without skilled birth attendant present, children of mothers who did not receive postnatal care, children of mothers with poor knowledge of routine immunization, dwelling in rural district, low-income families, and living further than 30 minutes from the nearest vaccination facility. The total coverage rate 78.9% is lower than the World Health Organization recommended of 80% in all districts and 90% nationally by 2020 It is helpful that the authors identified some critical factors associated with the immunization coverage, facilitating the upcoming intervention in order to increase the immunization of newborn babies to WHO recommended coverage. Nevertheless, I have a few questions about the study. The paper is generally well written, and straight forward to follow. I have some comments for improvement: Introduction: 1. The author highlighted that the EPI in Nigeria, created in 1979, had a significant impact during the first few years with immunization coverage peaking at 81.5% in 1990. Immunization coverage plummeted to 12.3% in 2003. Therefore it would be helpful to state the reasons for this drastic reduction. 2. It is advisable for the author to highlighted the compulsory vaccination schedule for newborn in Nigeria . Is the Vitamin A

	supplement and yellow fever antigens compulsory in determine the complete immunization ? Methodology: 3. The author mentioned that the questionnaire was constructed from a review of the available literature. Are there any validation and reliability test done for this questionnaire before it is administered to the respondents ?. Results: 4. Household income was believed as the significant factor for immunization coverage. In table 1, the income was divided into 2 groups. Why did the cutoff line set to RMN80,000 ? Is it necessary to further divide the group? 5. The authors mentioned that only 48.7% possessed immunization cards. In order to get more clear picture and to solve one of the limitation, it is recommended to further regroup those with card only and without card. 6. The authors collected the reasons of non-vaccination. However, it is not highlighted in the outcome of the study. Suggested to include this in result section. General speaking, even though there are some minor problems, the authors successfully identified a few immunization coverage associated factors, which could help to further improve the vaccination coverage rate based on the current findings.

REVIEWER	Cockcroft, Anne CIET Trust
REVIEW RETURNED	31-Jan-2021

GENERAL COMMENTS	The paper deals with childhood immunization. This remains an important challenge, even more so with the early evidence of disruption of routine childhood immunization during the recent COVID-19 pandemic. Main comments 1. This was a cross-sectional study. The authors should avoid statements implying causality of associations and conclusions based on assumptions of causality. For example, the association between use of maternal health services and full immunization of children might be because more advantaged women are both able to use the services and to have their children immunized. One cannot assume from a cross-sectional study that something that happened in the services (such as health education) led to the women immunizing their children. 2. The conclusions and policy implications are not supported by the study and results presented in the paper. The authors should re-consider what they can reasonably conclude from their study. > The authors cannot conclude from their study that providing immunization services in market places would improve immunization rates. Their study did not examine this. > The authors should not suggest that “strategies aimed at improving MHC utilization, especially in underserved rural communities, could be effective”. This may or may not be true. But it is an inappropriate conclusion about causality from a cross-sectional

	study. > The authors should not state that "comprehensive sensitization campaigns on immunization programmes should strengthened in rural communities to improve the effectiveness of immunization programme". They have shown, along with many other authors, that immunization coverage was lower in rural communities. Their study provides some evidence that children of mothers with less knowledge about immunization had lower rates of immunization. But they have not shown that campaigns to sensitize rural communities about immunization would work. > The authors should not state as a policy implication that "educational and reminder interventions that leverage on existing mobile phone technology obtainable in almost all rural communities in Nigeria could improve immunization awareness, timeliness, and coverage". This could potentially be a method of increasing immunization coverage but the authors did not study this and it is not an implication of their findings. 3. There are some important discrepancies between the findings of this study about maternal education and childhood immunization rates in Enugu and the findings in the 2018 Nigeria DHS. The authors report that 93.9% of the responding mothers in their survey had secondary education or higher. In the 2018 DHS, 53.4% of women in Enugu aged 15-49 had completed secondary education or above (35.9% + 17.5%). The figure increases to 78.3% if one includes those with incomplete secondary education. The authors report an overall complete immunization rate of 78.9% in their sample of children aged 12-23 months, whereas the 2018 DHS reports a rate of 36.4% with all basic immunizations for Enugu. The authors need to explain these discrepancies. Was their sample intended to be representative of Enugu State? Why do they think their findings for maternal education and childhood immunization are so much higher than those reported in the 2018 DHS? 4. The survey data collection took place in July 2020. What was the COVID-19 situation in Enugu at that time? It seems surprising that there were no pandemic-related restrictions on data collection and travelling between communities. The authors need to explain what effect, if any, pandemic restrictions had on their study. Probably most of the recorded immunizations in the survey took place before the pandemic, but it would be useful to confirm this. Other comments 5. The authors should use active rather than passive voice throughout their paper. 6. There are too many acronyms. This is annoying for the reader and many of them are unnecessary.

REVIEWER	Utazi, C. Edson University of Southampton
REVIEW RETURNED	03-Mar-2021

GENERAL COMMENTS	This paper studied the determinants of incomplete immunization coverage among children aged 12-23 months in Enugu state Nigeria. The study was well-conceived and carefully implemented. It demonstrates the importance of synthesizing evidence at the district/subnational level to uncover factors associated with un-vaccination and under-vaccination, which is key for identifying and targeting zero-dose children.

	My comments to the authors are given as follows: 1. The range of socio-demographic characteristics considered is quite limited. There are many more variables that can be obtained from DHS and MICS surveys – e.g. data on access to the media. I wonder why the authors chose to select their own sample instead of using these readily available data sets. 2. I think that the authors have not done due diligence in terms of comparing their results with those obtained using other surveys (e.g. DHS and MICS). For example, the estimated DHS 2018 FIC for Enugu is 36.4%. This study estimated 78.9%. Why is there a huge difference between these estimates given that there is only an interval of two years between both surveys? Does this indicate an inadequate sampling procedure for the study? 3. In the data analysis section, the authors mentioned that "T-test was used to assess for statistical difference in the mean scores for knowledge of RI." The results of this t-test are neither discussed nor shown in the tables included in the paper. 4. No model assessment statistics such as AUC or goodness-of-fit statistics are reported. It is difficult to assess the discriminatory power of the multivariate model. 5. I was wondering whether a multinomial regression framework would suit the analysis carried out by the authors better as it appears that the outcome variable could be categorized as fully immunized, partially immunized and zero dose. I am not asking the authors to do this, but they can perhaps comment on it. 6. Was the survey sample self-weighting? If not, did the authors include sampling weights in the logistic regression models? 7. Page 3 Line 12 – The RED strategy expired last year. Perhaps the authors can also reference the SDGs or the newly launched Immunization Agenda 2030 (https://www.who.int/teams/immunization-vaccines-and-biologicals/strategies/ia2030) both of which emphasize district-level evaluation of programmatic performance. 8. Page 3 Line 57: Is this figure the full immunization coverage according to the DHS? 9. Table 1: Education – Include a "no education" category if there is enough data. This is often of interest. 10. Table 1: How was walking distance to the nearest health facility measured during the survey? 11. The authors should include a sample questionnaire in the supplementary file to help readers understand how data were coded.

VERSION 1 – AUTHOR RESPONSE

REVIEWER 1: Mr. Kuang Hock Lim, Institute of Public Health

Comments to the Author:

In this manuscript, the authors analyzed the 12-23 months children immunization coverage and determinants in Enugu State. Cross sectional analysis was performed and several significant factors such as children of single mothers, children delivered without skilled birth attendant present, children of mothers who did not receive postnatal care, children of mothers with poor knowledge of routine immunization, dwelling in rural district, low-income families, and living further than 30 minutes from the nearest vaccination facility. The total coverage rate 78.9% is lower than the World Health Organization recommended of 80% in all districts and 90% nationally by 2020

It is helpful that the authors identified some critical factors associated with the immunization coverage, facilitating the upcoming intervention in order to increase the immunization of newborn babies to WHO recommended coverage. Nevertheless, I have a few questions about the study.

The paper is generally well written, and straight forward to follow. I have some comments for improvement:

Introduction:

1. The author highlighted that the EPI in Nigeria, created in 1979, had a significant impact during the first few years with immunization coverage peaking at 81.5% in 1990. Immunization coverage plummeted to 12.3% in 2003. Therefore, it would be helpful to state the reasons for this drastic reduction.

Authors response
Thanks very much for the suggestion. We have provided an explanation for this drastic reduction.

2. It is advisable for the author to highlight the compulsory vaccination schedule for newborn in Nigeria . Is the Vitamin A supplement and yellow fever antigens compulsory in determine the complete immunization ?

Authors response
Thanks very much for the suggestion. We have clarified this – Vitamin A and Yellow fever vaccines were not included in determining complete immunization status.

Methodology:

3. The author mentioned that the questionnaire was constructed from a review of the available literature. Are there any validation and reliability test done for this questionnaire before it is administered to the respondents ?.

Authors response
Yes, we validated the questionnaire with a sample of 20 mothers to assess logical sequence and reliability of the questionnaire before we administered it to study participants.

Results:

4. Household income was believed as the significant factor for immunization coverage. In table 1, the income was divided into 2 groups. Why did the cutoff line set to RMN80,000 ? Is it necessary to further divide the group?

Authors response

Household income was initially categorized into four categories; less than N40,000 (Approx. US $100), N40,000 to N79,999, N80,000 to N119,999, and N120,000 and above. Since there were only a small number of respondents in one of the categories; above N119,000 (n = 38), we merged the groups. We did not want our analysis to be driven by a group with a relatively small number, so we collapsed the four groups into two groups.
However, dichotomizing this variable did not change the result of the analysis in any substantive way.

5. The authors mentioned that only 48.7% possessed immunization cards. In order to get clearer picture and to solve one of the limitations, it is recommended to further regroup those with card only and without card.

Authors response
Thanks for this suggestion. The issue of whether mothers have vaccination cards or not is an important issue that requires a more complete analysis than simply presenting in as a supplement in this paper. We are presently working on another manuscript that looks at the broad determinants of missing vaccination cards, the quality of the vaccination cards observed, the validity of maternal recall for assessing vaccination coverage, and differential accuracy with the timing of vaccination (vaccines administered shortly after birth versus those administered later).

6. The authors collected the reasons of non-vaccination. However, it is not highlighted in the outcome of the study. Suggested to include this in result section.

Authors response
Thanks for this suggestion. We have included the reasons for non-vaccination in the results section.

General speaking, even though there are some minor problems, the authors successfully identified a few immunization coverage-associated factors, which could help to further improve the vaccination coverage rate based on the current findings.

REVIEWER 2: Dr. Anne Cockcroft, CIET Trust

Comments to the Author:

The paper deals with childhood immunization. This remains an important challenge, even more so with the early evidence of disruption of routine childhood immunization during the recent COVID-19 pandemic.

Main comments

1. This was a cross-sectional study. The authors should avoid statements implying causality of associations and conclusions based on assumptions of causality. For example, the association between use of maternal health services and full immunization of children might be because more advantaged women are both able to use the services and to have their children immunized. One cannot assume from a cross-sectional study that something that happened in the services (such as health education) led to the women immunizing their children.

Authors response

Thanks for this very critical advice. We have reviewed our discussion and conclusion to expunge any suggestion of causality.

2. The conclusions and policy implications are not supported by the study and results presented in the paper. The authors should re-consider what they can reasonably conclude from their study.

   a. The authors cannot conclude from their study that providing immunization services in marketplaces would improve immunization rates. Their study did not examine this.

Authors response
We have deleted this from manuscript.

   b. The authors should not suggest that "strategies aimed at improving MHC utilization, especially in underserved rural communities, could be effective". This may or may not be true. But it is an inappropriate conclusion about causality from a cross-sectional study.

Authors response
We have reviewed these sentences and expunged any suggestion of causality.

   c. The authors should not state that "comprehensive sensitization campaigns on immunization programmes should strengthened in rural communities to improve the effectiveness of immunization programme". They have shown, along with many other authors, that immunization coverage was lower in rural communities. Their study provides some evidence that children of mothers with less knowledge about immunization had lower rates of immunization. But they have not shown that campaigns to sensitize rural communities about immunization would work.

Authors response
We have reviewed and appropriately reworded the sentence.

   d. The authors should not state as a policy implication that "educational and reminder interventions that leverage on existing mobile phone technology obtainable in almost all rural communities in Nigeria could improve immunization awareness, timeliness, and coverage". This could potentially be a method of increasing immunization coverage, but the authors did not study this, and it is not an implication of their findings.

Authors response
We have deleted this from the policy implication.

3. There are some important discrepancies between the findings of this study about maternal education and childhood immunization rates in Enugu and the findings in the 2018 Nigeria DHS. The authors report that 93.9% of the responding mothers in their survey had secondary education or higher. In the 2018 DHS, 53.4% of women

in Enugu aged 15-49 had completed secondary education or above (35.9% + 17.5%). The figure increases to 78.3% if one includes those with incomplete secondary education. The authors report an overall complete immunization rate of 78.9% in their sample of children aged 12-23 months, whereas the 2018 DHS reports a rate of 36.4% with all basic immunizations for Enugu. The authors need to explain these discrepancies. Was their sample intended to be representative of Enugu State? Why do they think their findings for maternal education and childhood immunization are so much higher than those reported in the 2018 DHS?

Authors response
The discrepancy between this study's coverage estimates and the 2018 DHS estimates could be due to the following reasons;

a. It is possible that there some improvements in routine immunization coverage utilization likely occurred over the period (recall the 2018 DHS was conducted in 2017). This is could possibly be due to recent State Government efforts to boost vaccination coverage in the state (See https://www.afro.who.int/news/enugu-state-inaugurates-task-force-immunization-close-gaps-immunization-coverage)
b. Secondly, this could also be due to difference in the sampling approach between the two surveys. Our sampling procedure strictly adhered to the steps recommended in the WHO Vaccination Coverage Cluster Surveys Reference Manual 2019.
c. Also, that DHS 2018 did not provide the estimates at each local government area (LGA) level and as we know estimates do vary from LGA to LGA with some having higher estimates than others. The 2018 DHS report only presented average estimates for the state. It is possible that the population studied in our work may have higher figures than the state average.

In any case, we are confident in our study's estimates and it is a true reflection of the time and district/localities we sampled. We recommend that the figures for each locality which sums up to the state or national average be reported in updated DHS and MICS assessments.

4. The survey data collection took place in July 2020. What was the COVID-19 situation in Enugu at that time? It seems surprising that there were no pandemic-related restrictions on data collection and travelling between communities. The authors need to explain what effect, if any, pandemic restrictions had on their study. Probably most of the recorded immunizations in the survey took place before the pandemic, but it would be useful to confirm this.

Authors response
The restrictions were mainly inter-state (between the states), and not intra-state (within the state) and did not restrict access to and uptake of health services such as vaccination. These restrictions did not limit nor impact our data collection activities. However, our CHWs were also trained on COVID-19 safety guidelines and ensured safety protocols during the interviews. Also, the current study did not assess the impact of Covid 19 on immunization. However, we did not notice any noticeable negative impact of the pandemic on recorded immunization, based on anecdotal assessment of a sample of immunization cards during the data collection process.

5. The authors should use active rather than passive voice throughout their paper.

Authors response
We have reviewed the manuscript to active voice.

6. There are too many acronyms. This is annoying for the reader and many of them are unnecessary.

Authors response
We have deleted the unnecessary acronyms.

Reviewer 3: Dr. Chigozie Edson Utazi, University of Southampton

Comments to the Author:

This paper studied the determinants of incomplete immunization coverage among children aged 12-23 months in Enugu state Nigeria. The study was well-conceived and carefully implemented. It demonstrates the importance of synthesizing evidence at the district/subnational level to uncover factors associated with un-vaccination and under-vaccination, which is key for identifying and targeting zero-dose children.

My comments to the authors are given as follows:

1. The range of socio-demographic characteristics considered is quite limited. There are many more variables that can be obtained from DHS and MICS surveys – e.g. data on access to the media. I wonder why the authors chose to select their own sample instead of using these readily available data sets.

Authors response
The DHS and MICS survey data certainly contains more variables than our study. However, we sought to understand the factors driving the very low immunization coverage reported for Enugu State in the latest DHS 2018 report. The study was originally motivated by the stark difference between our experience with the routine vaccine delivery service in Enugu state and the immunization coverage estimate reported in the latest DHS (2018) for the state.

2. I think that the authors have not done due diligence in terms of comparing their results with those obtained using other surveys (e.g. DHS and MICS). For example, the estimated DHS 2018 FIC for Enugu is 36.4%. This study estimated 78.9%. Why is there a huge difference between these estimates given that there is only an interval of two years between both surveys? Does this indicate an inadequate sampling procedure for the study?

Authors response
Reviewer 2 (Professor Anne Cockcroft) raised a similar comment which we addressed above. We have included some explanations for this difference. Our sampling procedure strictly adhered to the steps recommended in the WHO Vaccination Coverage Cluster Surveys Reference Manual 2019 (already referenced in the manuscript).

3. In the data analysis section, the authors mentioned that “T-test was used to assess for statistical difference in the mean scores for knowledge of RI.” The results of this t-test are neither discussed nor shown in the tables included in the paper.

Authors response
The result of the t-test was presented in Table 3 as the mean difference between the mean score of knowledge of routine immunization. We have revised the results section to discuss this result in the discussion.

4. No model assessment statistics such as AUC or goodness-of-fit statistics are reported. It is difficult to assess the discriminatory power of the multivariate model

Authors response
Thank you very much for this suggestion. We have included the goodness-of-fit statistics for the multivariate model.

5. I was wondering whether a multinomial regression framework would suit the analysis carried out by the authors better as it appears that the outcome variable could be categorized as fully immunized, partially immunized and zero dose. I am not asking the authors to do this, but they can perhaps comment on it.

Authors response
Thank you very much for the suggestion. We strongly considered using a multinomial regression framework to analyze our data. However, we reasoned that since our primary objective was to identify determinants of incomplete immunization, we did not consider including the distinction between partially immunized and 'zero dose' children offered any additional benefits. As such, we concluded on the binary logistics regression framework.

6. Was the survey sample self-weighting? If not, did the authors include sampling weights in the logistic regression models?

Authors response
The survey sample was not self-weighted. Our sampling was designed to give every eligible child aged 12 – 23 months in the state an equal chance of being selected in the sample. Small pockets of children in Fulani settlements within the states were not reached during data collection due to security concerns. However, the number of children was small enough to not alter the random probability sampling design of the study and affect the main findings.

7. Page 3 Line 12 – The RED strategy expired last year. Perhaps the authors can also reference the SDGs or the newly launched Immunization Agenda 2030 (https://www.who.int/teams/immunization-vaccines-and-biologicals/strategies/ia2030) both of which emphasize district-level evaluation of programmatic performance.

Authors response
Thanks very much for this very important suggestion. We have revised the relevant sentences and references too.

8. Page 3 Line 57: Is this figure the full immunization coverage according to the DHS?

Authors response
Yes, this figure for full immunization coverage was according to the DHS Nigeria 2018.

9. Table 1: Education – Include a “no education” category if there is enough data. This is often of interest.

Authors response
Yes, we agree that this “no education” category is interesting. However, there was no sufficient data to include this category in the table.

10. Table 1: How was walking distance to the nearest health facility measured during the survey?

Authors response
Our CHWs used Google® Map mobile app on smartphones to estimate the walking distance from each study participant’s house to the nearest vaccination center in all but four clusters, all four clusters in Ezeagu LGA. In these four clusters, the CHWs first identified the nearest routine childhood vaccination point in each cluster and then estimated the walking distance from this nearest vaccination facility to each household included in the study.

11. The authors should include a sample questionnaire in the supplementary file to help readers understand how data were coded.

Authors response
Yes, we have included the study questionnaire in the supplementary file.

VERSION 2 – REVIEW

REVIEWER	Lim, Kuang Hock Institute of Public Health, Centre for Occupational Health Research
REVIEW RETURNED	19-Apr-2021

GENERAL COMMENTS	COMMENT FORM TO AUTHOR Manuscript Number: bmjopen-2020-047445.R1 Title: Determinants of incomplete immunization in children aged 12 to 23 months at sub-national level, Nigeria – a cross-sectional study In this manuscript, the authors analyzed the 12-23 months children immunization coverage and determinants in Enugu State. Cross sectional analysis was performed and several significant factors such as children of single mothers, children delivered without skilled birth attendant present, children of mothers who did not receive postnatal care, children of mothers with poor knowledge of routine immunization, dwelling in rural district, low-income families, and living further than 30 minutes from the nearest vaccination facility. The total coverage rate 78.9% is lower than the World Health Organization recommended of 80% in all districts and 90% nationally by 2020. The paper is generally well written, and straight forward to follow. In addition, all the comments from reviewer had been rectified. Suggested it to be accepted.

REVIEWER	Cockcroft, Anne CIET Trust

REVIEW RETURNED	16-Apr-2021

GENERAL COMMENTS	The authors have made good efforts to address my comments on their manuscript. I have some further comments about the revised manuscript. Main comments 1. Conclusions about causality from a cross-sectional study. The authors have removed from the Discussion most of the statements based on a causal interpretation of the associations they found in their study and modified their policy suggestions. > But they still use language in the Results and Discussion that implies causality. They use the terms “predictors” and “determinants” many times. I realise that sometimes these terms are used loosely, but they do imply causality. It would be better to talk about “associations” with the outcome of full immunisation. > In the Discussion, they include a sentence: “This is consistent with several studies in other LMICs that demonstrate that increased health communications on immunization during MHC utilization significantly impacts childhood immunization [51,53].” But the systematic review (ref 51) covered almost entirely cross-sectional studies (and a few case-control studies) and ref 53 was a cross-sectional study. One cannot conclude from these studies that health communication during MHC impacts childhood immunization. It could be that more health-conscious or more advantaged mothers are both more likely to attend MHC and more likely to have their children immunized. The authors’ causal interpretation of the cited articles is inappropriate. >Even in the title of the paper, the word “determinants” implies causality, although the mention of a cross-sectional study allows readers to know that the study cannot, in fact, allow conclusions about causality. 2. Differences between the study and DHS findings. In their response to comments, the authors propose several reasons for the difference between their findings for vaccination coverage in Enugu and the DHS figures for vaccination coverage in Enugu. But in the revised manuscript Discussion, they simply state the fact of the difference without proposing any possible reasons for the discrepancy. They do not address reasons for the difference in maternal education between their study and the DHS. There is one other likely reason for the difference between their vaccination and maternal education figures and the DHS figures that the authors do not mention. They write that there are 17 LGAs in Enugu, 4 predominantly urban and 13 predominantly rural. So urban LGAs are about 24% of the LGAs in the state. But in their sample, they included 2 urban LGAs and 2 rural LGAs; so urban LGAs are 50% of their sample. Because vaccination rates and maternal education rates are higher in urban communities, their over-sampling of urban LGAs could partly explain why their study gives higher vaccination and maternal education rates than the DHS for Enugu state, assuming the DHS sample had a balance of urban and rural sites closer to the actual population of Enugu. It is clearly not the whole reason, because even in their rural sites the authors

	report higher vaccination coverage than the DHS figures for the whole of Enugu State. The authors need to explain in their paper the reasons for the difference between their findings and the DHS findings for vaccination coverage. They also need to make clear specifically their over-sampling of urban sites. For their main finding of much lower vaccination rates in rural communities, it does not really matter that urban sites were over-sampled. But they cannot reasonably claim that their vaccination coverage rates can be taken as representative of the State, unless they first apply weights to allow for the over-sampling of urban sites. Other comments 3. The meaning of a sentence in the Discussion (first paragraph of p7) is not clear: “The low immunization dropout rate amidst low DPT-1 coverage in rural communities suggests that access to routine immunization services RI in these contexts remains a problem [50], as previous studies have argued[49].” 4. Patient and public involvement. I believe the information requested by the journal is about patient or public involvement. For a public health study, the question would be about involvement of the public in the areas concerned. The journal will be able to confirm about this interpretation of their requirement. 5. There are still many sentences in passive voice.

REVIEWER	Utazi, C. Edson University of Southampton
REVIEW RETURNED	25-Apr-2021

GENERAL COMMENTS	None.

VERSION 2 – AUTHOR RESPONSE

REVIEWER 2: Dr. Anne Cockcroft, CIET Trust

Comments to the Author:

The authors have made good efforts to address my comments on their manuscript. I have some further comments about the revised manuscript.

Main comments

1. Conclusions about causality from a cross-sectional study.

The authors have removed from the Discussion most of the statements based on a causal interpretation of the associations they found in their study and modified their policy suggestions.

But they still use language in the Results and Discussion that implies causality. They use the terms “predictors” and “determinants” many times. I realize that sometimes these terms are used loosely, but they do imply causality. It would be better to talk about “associations” with the outcome of full immunization.

Authors response
Thanks very much for this suggestion. We have removed every mention of “predictors” and “determinants” from the manuscript.

In the Discussion, they include a sentence: “This is consistent with several studies in other LMICs that demonstrate that increased health communications on immunization during MHC utilization significantly impacts childhood immunization [51,53].” But the systematic review (ref 51) covered almost entirely cross-sectional studies (and a few case-control studies) and ref 53 was a cross-sectional study. One cannot conclude from these studies that health communication during MHC impacts childhood immunization. It could be that more health-conscious or more advantaged mothers are both more likely to attend MHC and more likely to have their children immunized. The authors’ causal interpretation of the cited articles is inappropriate.

Authors response
We have revised this sentence to expunge any appearance of casual relationship between MHC utilization and childhood immunization. Although both cited articles demonstrated that MHC utilization is significantly associated with childhood immunization, we believe that by using the word “impacts” to describe this relationship, we introduced ‘causality’ in the relationship and give a yet-to-be-proven impression that MHC utilization has a causal relationship with childhood immunization.

Even in the title of the paper, the word “determinants” implies causality, although the mention of a cross-sectional study allows readers to know that the study cannot, in fact, allow conclusions about causality.

Authors response
Thanks for this very critical advice. We have revised the title and removed “Determinants” from the title.

2. Differences between the study and DHS findings.

In their response to comments, the authors propose several reasons for the difference between their findings for vaccination coverage in Enugu and the DHS figures for vaccination coverage in Enugu. But in the revised manuscript Discussion, they simply state the fact of the difference without proposing any possible reasons for the discrepancy. They do not address reasons for the difference in maternal education between their study and the DHS.

There is one other likely reason for the difference between their vaccination and maternal education figures and the DHS figures that the authors do not mention. They write that there are 17 LGAs in Enugu, 4 predominantly urban and 13 predominantly rural. So urban LGAs are about 24% of the LGAs in the state. But in their sample, they included 2 urban LGAs and 2 rural LGAs; so urban LGAs are 50% of their sample. Because vaccination rates and maternal education rates are higher in urban communities, their over-sampling of urban LGAs could partly explain why their study gives higher vaccination and maternal education rates than the DHS for Enugu state, assuming the DHS sample had a balance of urban and rural sites closer to the actual population of Enugu. It is clearly not the whole reason, because even in their rural sites the authors report higher vaccination coverage than the DHS figures for the whole of Enugu State.

The authors need to explain in their paper the reasons for the difference between their findings and the DHS findings for vaccination coverage. They also need to make clear specifically their over-sampling of urban sites. For their main finding of much lower vaccination rates in rural communities, it does not really matter that urban sites were over-sampled. But they cannot reasonably claim that their vaccination coverage rates can be taken as representative of the State, unless they first apply weights to allow for the over-sampling of urban sites.

Authors response
Thank you very much for persistently seeking to understand differences between our study and the 2018 DHS findings. This has made us understand DHS better and appreciate the nuanced differences between our study and DHS. Thank you. We suspect there are three possible reasons for this difference.

1. The first reason relates to the definition of FIC: FIC was defined for DHS as having received one dose of BCG, one dose of measles, three doses of DPT, and three doses of OPV vaccines (pg. 224). FIC for this study was likewise defined as in DHS, but for OPV, we defined as three doses of polio vaccine instead, that is either three doses of OPV or two doses of OPV and one dose of IPV, in line with the Polio Endgame Strategy 2019-2023. To illustrate how the difference in FIC definition drives the overall rates, we calculated FIC by applying our definition to the DHS data which shows that FIC rates in the current study and DHS are within 11 percentage points when our definition of FIC is used.
2. Secondly, difference in the sampling approaches used in our study and DHS, and the resulting differences in the characteristics of the sample could explain some of the difference. A comparison of demographic characteristics of our sample with that of the DHS sample (in Enugu state) shows that mothers in our sample are more educated and more likely to be working. It is reasonable to expect a higher FIC among these mothers.
3. Thirdly, a portion of the difference could be due to the State Government's recent efforts to boost vaccination coverage in the state since the 2018 Nigeria DHS.

Thank you very much again.

3. The meaning of a sentence in the Discussion (first paragraph of p7) is not clear: "The low immunization dropout rate amidst low DPT-1 coverage in rural communities suggests that access to routine immunization services RI in these contexts remains a problem [50], as previous studies have argued[49]."

Authors response
We have revised this sentence to make its meaning clearer. .

4. Patient and public involvement. I believe the information requested by the journal is about patient or public involvement. For a public health study, the question would be about involvement of the public in the areas concerned. The journal will be able to confirm about this interpretation of their requirement.

Authors response
Thank you. We have revised this in line with your suggestion.

5. There are still many sentences in passive voice.

Authors response
We have revised all the sentences to active voice.

REVIEWER 1: Mr. Kuang Hock Lim, Institute of Public Health

Comments to the Author:

The paper is generally well written, and straight forward to follow. In addition, all the comments from reviewer had been rectified. Suggested it to be accepted.

> **Authors response**
> Thanks very much

REVIEWER 3: Dr. C. Edson Utazi, University of Southampton

Comments to the Author:

None

> **Authors response**
> Thank you very much.

VERSION 3 – REVIEW

REVIEWER	Cockcroft, Anne CIET Trust
REVIEW RETURNED	26-May-2021

GENERAL COMMENTS	Thank you for addressing my comments.