## PEER REVIEW HISTORY

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## ARTICLE DETAILS

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| **TITLE (PROVISIONAL)** | What prevents pregnant women from adhering to the continuum of maternal care? A prospective cohort study in Kenya |
| **AUTHORS** | Aksünger, Nursena; de Sanctis, Teresa; Waiyaiya, Emma; van Doeveren, Rianne; van der Graaf, Mark; Janssens, Wendy |

## VERSION 1 – REVIEW

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| **REVIEWER** | Singh, Jitendra<br>Janaki Medical College, Community Medicine and Public Health |
| **REVIEW RETURNED** | 11-May-2021 |

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| **GENERAL COMMENTS** | The study is dealing with important issue of maternal health. The study aimed to evaluate the continuum of maternal care from an integrated perspective, focusing on how key components of an adequate journey are interrelated. However, some of the questions still remain unanswered and need clarification.<br><br>Abstract<br>Pls mention full form of abbreviation first time.<br><br>Methodology<br>Study Setting<br>The sentence “The chance of live birth in Kenya doubles if the mother attended more than one ANC visit during pregnancy” is confusing to the reader. It needs to clarify.<br><br>Authors mentioned “25 health facilities across three counties of Kenya: Nairobi, Kisumu and Kakamega” in the abstract, however, it is not described in the methods section.<br><br>Research design and sampling methodology<br>The sampling technique employed to approach the study participants is not clearly explained? Would you explain what sampling technique did you use? Adequacy of sample size for the different outcome variables need to be clarified.<br><br>Data collection<br>“.....Medical records in the MomCare analytics engine contain information about the week of enrolment.....”. This seems the data is retrieved from the MomCare projects and it make confusion to the reader. Would you highlight process of data collection more clearly? It is primary data or secondary data is used for analysis purpose.<br><br>Outcome Variables<br>Authors mentioned “The analyses are based on three primary outcome variables” but here, this sentence is confusing to the |

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| | readers as it seems 4 outcome variables as :the week of enrolment and first ANC visit at a MomCare clinic, the total number of ANC visits at a MomCare clinic and having a skilled delivery at a MomCare clinic.<br>This need to clarify.<br><br>Statistical methods<br>There are 3 outcome variables which are 3 types; (i) continuous variable (ii) count based data and (iii) Categorical variable. My concern is for (ii) type. What is the ground to select OLS for this outcome variable? Is all the assumptions are fulfilled for this outcome variable to use OLS or it is better to use poisson regression model for count based data. This needs clarification.<br>Likewise, for logistic regression model, authors say nothing about confounders, which is the main statistical assumption need to be considered in the logistic regression model. I think a big threat that your data is highly vulnerable to confounder as you missed to gather important variables. Have you assessed that? If yes, at what stage and how?<br>Results<br>Authors presented; observation, mean, SD, min. max in Table 1. Is it appropriate to present mean and SD for categorical data? However, authors mentioned percentage of categorical data in the description of Table 1.<br>Authors mentioned about 49 per cent of them choosing to come on foot. however, I did not find it in the table.<br>Moreover, authors mentioned we examine the determinants of the continuum of maternal care from an integrated perspective, seeking to identify how the three key components of an adequate journey are interrelated. But I did not find inter-relationships between these three; as authors described the models between (i) total number of ANC visits and the week of enrolment (ii) utilisation of SBA and total number of ANC visits. If possible, Structural Equation Modelling (SEM) analysis can be employed to observed the interrelationships between 3 dependent variables<br><br>Discussion<br>Authors discussed “To gain further insights, we collected qualitative data from a randomly selected proportion of MomCare mothers at the end of their journey”, Therefore, methods and findings of the qualitative study should be mentioned in methods and result section as appropriate.<br><br>The discussion is a bit too long for the results you found. The discussion section should consider the aforesaid points beyond comparing the findings with relevant and non-relevant prior findings in the field. I suggest to tighten the discussion and just focus on your key findings.<br><br>Additionally, authors mentioned Figure 1, Figure 2 and Figure 3 in the text but not mentioned in the figures itself.<br>The structure of the manuscript can be improved. |

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| **REVIEWER** | Fukuda, Yoshiharu<br>Yamaguchi University, Community Health and Medicine |
| **REVIEW RETURNED** | 07-Jun-2021 |

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| **GENERAL COMMENTS** | Major comments.<br>1. The headings could follow IMRAD (Introduction, Methods, Results and Discussion). |

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| | 2. The statistic and the description were not suitable. (1) Table 1: The categorical variables should be showed by Mean and SD but by number and %. (2) The results of enrollment week, number of ANC visit and SBA should be showed by separated tables. The tables could show not only adjusted values also descriptive statistics and crude values. (3) OLS was used for ANC and SBA. They could be categorical variables using cut-off, and the logistic regression. The authors should consider change of the analysis.<br><br>Minor comments<br>Abstract<br>1. OLS and ANC should be firstly spelled out.<br>2. Last two sentences seemed to be overstatement.<br>Introduction<br>3. SBA should be firstly spelled out.<br>4. Sub-headings could be omitted.<br>Methods<br>5. 27 health facilities were enrolled in the MomCare. But the Abstract descried 25 facilities.<br>6. The number of subjects differed by outcome. The subjects without any missing should be analyzed.<br>7. The formulas of regression analyses could be omitted. But, I am afraid that the formula of SBA was not wrong, since it was logistic regression.<br>Results<br>8. Sub-heading could be omitted.<br>Discussion<br>9. P15, L9-: The results not including methods should not be mentioned.<br>10. P16: The last paragraph could be omitted or shortened to combined the prior description.<br>11. Limitations should be mentioned. |

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| **REVIEWER** | Bonfrer, IEJ<br>Erasmus Universiteit Rotterdam |
| **REVIEW RETURNED** | 16-Jun-2021 |

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| **GENERAL COMMENTS** | Thank you for the opportunity to review this manuscript which sets out to answer an important question: what prevents Kenyan pregnant women from attending antenatal care?<br><br>Major suggestions<br>1. This study can be placed within a large existing literature that identifies (modifiable) factors affecting antenatal care and skilled birth attendance utilization in low- and middle- income countries (LMICs). We know from among others Andersen's model of health services utilization, as also cited by the authors, that education, wealth and access to health care facilities (travel time), are important determinants of health care utilization. The authors claim to add to this literature by not studying these factors separately but by applying an "integrated perspective".<br><br>Resulting from this "integrated perspective", the authors' main finding is that "early initiation of the care journey directly creates positive externalities for an adequate number of ANC visits". In other words, those who start ANC early on in their pregnancy are likely to have had more ANC visits during that pregnancy than those who start later. With an average first ANC visit taking place about half way through the pregnancy, it seems indeed to be expected that women who start earlier have had more visits |

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| | overall. What are other findings that the authors can share based on their “integrated perspective”, in addition to what we already know about determinants of health care utilization from existing literature? Can the authors identify, through their integrated perspective, relevant components of what they call the “journey” that affect ANC and/or SBA use in ways we did not know about before? Can the authors provide Kenyan policy makers with suggestions to support Kenyan women in using ANC and SBA, other than early initiation of ANC? Currently the study objective does not yet seem to have been fully achieved, raising doubts about the added value of this study.<br><br>2. The authors describe a novel intervention called “MomCare” which “incentivises access and adherence to care-journeys through a digitally enabled smart contract”, enrolling pregnant women in a subsidised health insurance program via a health wallet on their mobile phone, using tailored behavioural nudges. This lengthy and insightful description of the program suggests that the reader will subsequently be treated to insights about the implementation and effectiveness of the described program. However, that is unfortunately not the case. Why are so many insights shared about an intervention which main characteristics are not subsequently included in the analyses? What are the benefits of this separate study over an integration of this study into an impact evaluation of the described program?<br><br>Minor suggestions<br>1. A strength of this study that seems to be underexposed currently, is the way in which data are collected. The earlier described MomCare allows for more timely data collection through an in app survey which will reduce recall bias as is often a problem with surveys like the Demographic and Health Survey, asking mothers to report about their ANC utilization up to five years prior to the survey. Authors could make this more clear in section 3.4 Data collection.<br><br>2. While abbreviations are provided in a list at the end, it would be helpful if each abbreviation is written out upon first use (for example SBA).<br><br>3. The Study Setting describes that data were collected in three counties: Nairobi, Kisumu and Kakamega. Why are these three counties the most relevant when trying to understand what prevents pregnant Kenyan women from attending ANC?<br><br>4. Is the subscript i for the beta’s in the equations under 3.5 Statistical methods correct? Do the beta’s indeed vary by individual i? I would expect these to vary by explanatory variable i.e. not having a subscript i but a different one, for example p.<br><br>5. Both the introduction and discussion start from maternal deaths worldwide to signal the societal importance of the study. While I agree on the societal importance, the study does not look (for understandable reasons) at maternal mortality, but at antenatal care and skilled birth attendance. The underlying assumption seems to be that increased use of ANC and SBA will reduce maternal mortality. However, the authors do not cite evidence for that assumption which I find important given that quality of ANC in LMICs has been reported to be below standard in various settings so it is questionable whether that will help save maternal lives. The |

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| | authors do cite studies about the link between ANC and awareness, diagnosis and emergency preparedness which is important but is not about a reduction in the health outcome they use to signal the importance of the study i.e. maternal mortality. It would be important to add that evidence or use different outcomes to signal the importance of this study.<br><br>6. The “need” for ANC is diversified into “no”, “medium” and “high” risk, suggesting that women with no risk have a different or no need for ANC. I would argue that all pregnant women have a need for ANC even when their pregnancy is low risk. I therefore suggest to present this explanatory variable as an indicator of risk, not as an indicator of need and change the wording in the manuscript accordingly.<br><br>7. The third paragraph of the discussion indicates that delivery at a MomCare clinic is lower for women who live in more educated and wealthier households. The authors showed earlier in the manuscript that higher education (of the household head) leads to more ANC visits. Could the authors explore and explain in more detail why those who used more ANC are less likely to use skilled birth attendance? Do they have lower expectations of SBA, potentially through their experiences with low quality ANC, preventing them from choosing to deliver at MomCare clinics? Or are there other explanations for the lower SBA among better educated women? |

**VERSION 1 – AUTHOR RESPONSE**

Response document to Manuscript ID: bmjopen-2021-050670

*Reviewer: 1*
*Dr. Jitendra Singh, Janaki Medical College, MedSpirit Alliance*

*Comments to the Author:*

*The study is dealing with important issue of maternal health. The study aimed to evaluate the continuum of maternal care from an integrated perspective, focusing on how key components of an adequate journey are interrelated. However, some of the questions still remain unanswered and need clarification.*

Response: Thank you for taking the time to read and comment on our paper. Your recommendations have been very helpful to us in revising and strengthening the paper.

*Abstract*
*1. Pls mention full form of abbreviation first time.*

Response: Thank you for this note. We have now spelled out all abbreviations in the abstract as well.

*Methodology*
*Study Setting*

*2. The sentence "The chance of live birth in Kenya doubles if the mother attended more than one ANC visit during pregnancy" is confusing to the reader. It needs to clarify.*

Response: We have clarified the reference as follows: "Attending at least two ANC visits has been shown to reduce the probability of a stillbirth by half in Kenya.". Please see pg. 7.

*3. Authors mentioned "25 health facilities across three counties of Kenya: Nairobi, Kisumu and Kakamega" in the abstract, however, it is not described in the methods section.*

Response: We thank the reviewer for pointing this out. The methods section (3.2

Program Description) mentioned 27 health facilities. However, two of the clinics

were not connected to the MomCare program until after the end of the study

period; hence they are not part of the study sample. We have changed the

information in section 2.2 Study setting to reflect this more clearly, and now also refer to the 25 health facilities in section 2.3 Research design and sampling methodology. Please see pg. 7 and pg. 8.

Research design and sampling methodology

*4. The sampling technique employed to approach the study participants is not clearly explained? Would you explain what sampling technique did you use? Adequacy of sample size for the different outcome variables need to be clarified.*

Response: The study sampling frame includes all pregnant women who presented at one of the MomCare clinics since program inception in November 2017, and who were eligible for MomCare. The primary program eligibility criterion was gestation of less than 26 weeks. This criterion did not apply to pregnant teenagers who could enroll at any moment in their pregnancy. In total, 11,538 eligible women enrolled in MomCare since inception. The study period starts from February 2019 onwards, because the baseline questionnaire was not standardized before that moment. As a result, 856 women (7.4 percent) who enrolled before February 2019 were excluded from the study sample. To be able to conduct the analyses of enrolment week, ANC visits and SBA on the same sample of women, we excluded 4,799 women (40.5 per cent) for whom SBA data were missing (whose pregnancy was less than 42 weeks by the end of the study period in August 2020, and who had not yet delivered in a MomCare clinic). The final study sample includes 5,879 women. The sampling strategy was

already described in the original Figure 2 (which is now Appendix Figure A.1), but we have adjusted the text on pg. 8 to make the sampling technique clearer.

In line with your suggestion, we have added a discussion on the adequacy of the sample size to the manuscript. Specifically, our analysis is powered (at $\beta$ = .80 and $\alpha$ = .05) to correctly estimate with 95% confidence the week of enrolment within 0.2 weeks of the true population average, the number of ANC visits within 0.05 visits of the true population average, and SBA within 4,0 percent of the true population average. We have added this in the Methods section on pg. 8.

*Data collection*

*5. ".....Medical records in the MomCare analytics engine contain information about the week of enrolment.....". This seems the data is retrieved from the MomCare projects and it make confusion to the reader. Would you highlight process of data collection more clearly? It is primary data or secondary data is used for analysis purpose.*

Response: We use the data as collected through the MomCare baseline survey and the medical information from the MomCare analytics engine for the analysis. To clarify this for the reader, we have adjusted section 2.4 Data collection to explicitly mention that the analyses are based on the data collected in MomCare (see pg. 8). As the MomCare program is still a pilot project, we consider this data to be primary data that we used in order to better understand the maternal healthcare-seeking behavior of the target population in the study areas.

*Outcome Variables*
*6. Authors mentioned "The analyses are based on three primary outcome variables" but here, this sentence is confusing to the readers as it seems 4 outcome variables as :the week of enrolment and first ANC visit at a MomCare clinic, the total number of ANC visits at a MomCare clinic and having a skilled delivery at a MomCare clinic.*
*This need to clarify.*

Response: Thank you for pointing out that this formulation was confusing. The 'week of enrolment and first ANC visit' pertain to the same outcome. We have now deleted '… and first ANC visit…' from the sentence (pg. 9).

*Statistical methods*
*7. There are 3 outcome variables which are 3 types; (i) continuous variable (ii) count based data and (iii) Categorical variable. My concern is for (ii) type. What is the ground to select OLS for this outcome variable? Is all the assumptions are fulfilled for this outcome variable to use OLS or it is better to use poisson regression model for count based data. This needs clarification. Likewise, for logistic regression model, authors say nothing about confounders, which is the main statistical assumption need to be considered in the logistic regression model. I think a big threat that your data is highly vulnerable to confounder as you missed to gather important variables. Have you assessed that? If yes, at what stage and how?*

Response: Thank you for these important comments. Following your advice, we use a Poisson regression model for the number of ANC visits in the revised manuscript, and we have adjusted Sections 2.6 Statistical methods and 3. Results accordingly. We note that this does not qualitatively change the results (shown in revised Table 2).

We have now also added in Section 3 that the analyses introduce subsets of explanatory variables (predisposing, enabling, and need factors) in a stepwise manner to control for potential confounding effects. The original Table 3 with full regression results has been split in multiple columns per outcome to explicitly investigate the effect of confounders in Table 2 of the revised manuscript. As shown in Table 2, the estimated associations between week of enrolment (Columns 1-3), number of ANC visits (Columns 4-7), and SBA (Columns 8-11) are robust to the consecutive inclusion of predisposing factors (age, education of household head), enabling factors (wealth tercile, county, travel distance) and need factors (multigravida, pregnancy risk level). We have added a more elaborate discussion on potential confounders throughout the Results section (pages 12-13).

In a previous stage (before the start of the study period in February 2019), the MomCare analytics engine collected less data on potential confounders. Additional variables were subsequently added to the baseline survey, yielding the dataset as currently used in the manuscript. In Section 4. Discussion we highlight additional variables that we recommend including in future studies of adherence to the continuum of maternal care, amongst others on detailed obstetric history and maternal education (pg. 15).

*Results*

*8. Authors presented; observation, mean, SD, min. max in Table 1. Is it appropriate to present mean and SD for categorical data? However, authors mentioned percentage of categorical data in the description of Table 1.*

Response: This is well-noted. We have changed the table such that the categorical variables are now presented with their percentages and excluding their standard deviation. Please see Table 1.

*9. Authors mentioned about 49 per cent of them choosing to come on foot. however, I did not find it in the table.*

Response: Thank you for noticing this omission. We have now added the means of transportation to Table 1.

*10. Moreover, authors mentioned we examine the determinants of the continuum of maternal care from an integrated perspective, seeking to identify how the three key components of an adequate journey are interrelated. But I did not find inter-relationships between these three; as authors described the models between (i) total number of ANC visits and the week of enrolment (ii) utilisation*

*of SBA and total number of ANC visits. If possible, Structural Equation Modelling (SEM) analysis can be employed to observed the interrelationships between 3 dependent variables*

Response: This is a very good suggestion. After estimating the independent regressions in Table 2 (as in the original manuscript), we have now added a SEM analysis as well to investigate both the direct effects of delayed week of enrolment on skilled delivery as well as the indirect effects through a reduced number of ANC visits. Findings are summarized in the Results section on pages 13 and in the new Figure 3. The discussion section elaborates on them on pg. 14, highlighting both an (indirect) domino-effect from late enrolment to reduced ANC visits to a lower probability of SBA, as well as a counteracting direct effect from late enrolment to a higher probability of SBA. The latter partly reflects the late enrolment of pregnant teenagers in combination with their higher likelihood of SBA. It might also be related to a selection effect, if women who enroll late are also at greater risk of complications, and hence of skilled delivery. However, more research is needed to provide conclusive evidence in this respect.

*Discussion*

*11. Authors discussed "To gain further insights, we collected qualitative data from a randomly selected proportion of MomCare mothers at the end of their journey", Therefore, methods and findings of the qualitative study should be mentioned in methods and result section as appropriate.*

Response: We have carefully considered your comment, also in view of a similar comment from another reviewer who recommends omitting these findings from the discussion as they were not described in the methods and results section. We have decided to delete this paragraph from the discussion, as we feel that the qualitative data collection was too limited to merit a full-fledged description in the Methods and Results section.

*12. The discussion is a bit too long for the results you found. The discussion section should consider the aforesaid points beyond comparing the findings with relevant and non-relevant prior findings in the field. I suggest to tighten the discussion and just focus on your key findings.*

Response: Thank you for this recommendation. Apart from deleting the paragraph on the qualitative insights, we have now also further tightened the discussion such that it concentrates more strongly on the key findings, and less on the other explanatory variables. See pages 14-15.

*13. Additionally, authors mentioned Figure 1, Figure 2 and Figure 3 in the text but not mentioned in the figures itself. The structure of the manuscript can be improved.*

Response: We are sorry that the reviewer did not have access to the figures within the main text. The journal requires figures to be uploaded separately, however.

*Reviewer: 2*
*Dr. Yoshiharu Fukuda, Yamaguchi University*
*Comments to the Author:*

Response: Thank you for taking the time to read and comment on our manuscript, and for your very helpful suggestions to revise and improve it.

*Major comments.*
*1. The headings could follow IMRAD (Introduction, Methods, Results and Discussion).*

Response: Thank you for this suggestion. Our main headings followed the IMRAD structure except for "Conceptual Framework", which we had added separately to keep it distinct from the empirical methodology. In line with your suggestion, we have now merged it under Methods. Also, we deleted the subheadings in the Introduction and the Results sections. The subheadings under Methods have been kept in for readability. If the editorial rules require a strict and sparse IMRAD structure, we are happy to delete those as well.

*2. The statistic and the description were not suitable.*

*(1) Table 1: The categorical variables should be showed by Mean and SD but by number and %.*

Response: This is well-noted. We have changed Table 1 accordingly, showing only

the number of observations and the percentage for categorical variables.

*(2) The results of enrollment week, number of ANC visit and SBA should be showed by separated tables. The tables could show not only adjusted values also descriptive statistics and crude values.*

Response: We appreciate your suggestion and we have worked on making the results table clearer for the reader. Please note that the descriptive statistics were already included in panel D of Table 1 and crude values were in the original Appendix. We have now highlighted panel D more clearly in the text (on pg. 11) and following your suggestion, we moved the Appendix Figure to the main manuscript (new Figures 2a and 2b).

We did not split Table 2 into three separate tables because the submission guidelines recommend approximately five figures and tables in total. Instead, to accommodate your concerns, we have reformatted Table 2 such that it now more clearly shows the regression results pertaining to each of the three outcome variables. We hope that this change allows for easier reading.

*(3) OLS was used for ANC and SBA. They could be categorical variables using cut-off, and the logistic regression. The authors should consider change of the analysis.*

Response: We have taken your recommendation on board. In the revised manuscript, we have used Poisson regression for the number of ANC visits (a count variable) and logistic regressions for SBA (a binary variable), see Table 2. The Methods section and the Results section have been adjusted accordingly on pg. 10 and 12-13, respectively.

*Minor comments*
*Abstract*
*1. OLS and ANC should be firstly spelled out.*

Response: We have changed the abstract accordingly.

*2. Last two sentences seemed to be overstatement.*

Response: We agree with the Reviewer that the last two sentences do not fully align with the results and discussion of the analyses. We have therefore deleted them from the conclusion in the revised abstract. We have added instead a reflection on the direct versus indirect effects of the continuum of maternal care, to capture the new SEM results.

*Introduction*
*3. SBA should be firstly spelled out.*

Response: We have changed the text accordingly (pg. 2).

*4. Sub-headings could be omitted.*

Response: Thank you for this suggestion, we have deleted the sub-headings in the introduction.

*Methods*
*5. 27 health facilities were enrolled in the MomCare. But the Abstract descried 25 facilities.*

Response: This is well-noted. The reason for this discrepancy in numbers is that *at the time of writing*, there were 27 health facilities connected to MomCare but for the analysis we only include the 25 facilities that were connected before the end of the study period in August 2020. The remaining two facilities did not connect to MomCare until after that date. To avoid confusion, we have adjusted the text such that we now only refer to the 25 connected facilities during the study period (pg. 7 and pg. 8).

*6. The number of subjects differed by outcome. The subjects without any missing should be analyzed.*

Response: Thank you for this comment. Following your advice, we have adjusted our analysis sample to include only those women without any missing observation in any of the outcome variables. We have adjusted our discussion of the sampling strategy accordingly (pg. 7-8). The reduction of the total sample from 5,883 to 5,879 does not change the main results.

*7. The formulas of regression analyses could be omitted. But, I am afraid that the formula of SBA was not wrong, since it was logistic regression.*

Response: This is a good observation. We have corrected the formula for the logistic regression. We have kept the other regression notations in as well, because the number of ANC visits is now estimated with a Poisson regression model. Showing the formulas makes this more explicit (pg. 10).

*Results*
*8. Sub-heading could be omitted.*

Response: In line with your suggestion, we have omitted the sub-headings in the Results section of the revised manuscript.

*Discussion*
*9. P15, L9-: The results not including methods should not be mentioned.*

Response: We agree with your comment, and we have deleted the qualitative results from the discussion section of the revised manuscript.

*10. P16: The last paragraph could be omitted or shortened to combined the prior description.*

Response: Thank you for this suggestion. We have now shortened the last paragraph, while also linking it more clearly to the specific outcomes of the analyses (see pg. 15).

*11. Limitations should be mentioned.*

Response: We have revised the Discussion such that the limitations are now more clearly mentioned and grouped in the third paragraph of the section. Most notably, this concerns the limitation that the data do not capture maternal care sought at facilities that were not connected to MomCare. Therefore, our results might underestimate the actual adherence to maternal care. The paragraph also highlights the need for a more comprehensive recording of maternal education and obstetric history to facilitate more detailed analyses (see pg. 15).

*Reviewer: 3*
*Dr. IEJ Bonfrer, Erasmus Universiteit Rotterdam*
*Comments to the Author:*
*Thank you for the opportunity to review this manuscript which sets out to answer an important question: what prevents Kenyan pregnant women from attending antenatal care?*

Response: Thank you for taking the time to read and comment on our paper. We very much appreciate your recommendations, they have been of great help to us in revising and strengthening the paper.

*Major suggestions*
*1. This study can be placed within a large existing literature that identifies (modifiable) factors affecting antenatal care and skilled birth attendance utilization in low- and middle- income countries (LMICs). We know from among others Andersen's model of health services utilization, as also cited by the authors, that education, wealth and access to health care facilities (travel time), are important determinants of health care utilization. The authors claim to add to this literature by not studying these factors separately but by applying an "integrated perspective".*

*Resulting from this "integrated perspective", the authors' main finding is that "early initiation of the care journey directly creates positive externalities for an adequate number of ANC visits". In other words, those who start ANC early on in their pregnancy are likely to have had more ANC visits during that pregnancy than those who start later. With an average first ANC visit taking place about half way through the pregnancy, it seems indeed to be expected that women who start earlier have had more visits overall. What are other findings that the authors can share based on their "integrated perspective", in addition to what we already know about determinants of health care utilization from existing literature? Can the authors identify, through their integrated perspective, relevant components of what they call the "journey" that affect ANC and/or SBA use in ways we did not know about before? Can the authors provide Kenyan policy makers with suggestions to support Kenyan women in using ANC and SBA, other than early initiation of ANC? Currently the study objective does not yet seem to have been fully achieved, raising doubts about the added value of this study.*

Response: We thank the reviewer for raising these questions, as they clearly highlight that our original analyses were not making full use yet of the richness of our data. As we describe in the introduction, and as opposed to most existing studies, our data allow for a fully integrated analysis of the continuum of care. The revised manuscript takes our analyses one step further in this respect by estimating a Structural Equations Model (SEM) in addition to the

original, separate regressions. To the best of our knowledge, we are the first to simultaneously model the determinants of the three main outcome variables as well as the direct and indirect relationships between them.

The SEM identifies a number of systematic patterns within the journey of maternal care, as described in the Results section on pg. 13. First, an increased number of ANC visits has a direct and significant positive effect on the probability of delivery in the presence of skilled birth attendant. Second, we find significant evidence of a domino-effect within the continuum of care. Early enrolment significantly increases the number of ANC visits, which in turn increase SBA, as described above. This documents the indirect effect of timely enrolment on skilled delivery. We also uncover a direct association between enrolment and SBA, which is positive rather than negative. Our data allow us to explore some potential mechanisms leading to this unexpected finding. First, this picks up the significantly later enrolment of pregnant teenagers compared to adult women in MomCare, in combination with the significantly higher probability of SBA for pregnant teenagers. Second, we hypothesize that the direct association between late enrolment and SBA might be due to a selection effect, if women who enroll late are also more prone to have an increased risk of complications (leading to greater use of SBA). Since our data do not allow for conclusive evidence in this respect, we recommend future research to better measure obstetric history at enrolment, including details on (potential) complications of previous and current pregnancies (pg. 15).

A key policy recommendation pertains to the importance of enhancing adherence to the full continuum of care because of the identified domino-effect: Indeed, early initiation of ANC is a stepping stone to more ANC visits; however, a higher number of ANC visits also improves the likelihood of SBA independently. Pregnant teenagers are a particularly vulnerable target population who require specific attention, as they tend to enrol late in maternal care, while at the same time being more prone to complications during pregnancy and childbirth, resulting in adverse maternal and child health outcomes. These recommendations have been added to the discussion section on pg. 14-15.

*2. The authors describe a novel intervention called "MomCare" which "incentivises access and adherence to care-journeys through a digitally enabled smart contract", enrolling pregnant women in a subsidised health insurance program via a health wallet on their mobile phone, using tailored behavioural nudges. This lengthy and insightful description of the program suggests that the reader will subsequently be treated to insights about the implementation and effectiveness of the described program. However, that is unfortunately not the case. Why are so many insights shared about an intervention which main characteristics are not subsequently included in the analyses? What are the benefits of this separate study over an integration of this study into an impact evaluation of the described program?*

Response: Upon reading your comment, we understand that the lengthy description of the MomCare program in the original manuscript might have raised unwarranted expectations about a study of its effectiveness. Unfortunately, we are not able to conduct an impact evaluation because we use data collected through the MomCare analytics engine and we do not have comparable data for non-MomCare clinics. This effectively precludes evaluating the effectiveness of the program compared to other clinics that provide similar maternal care

services in the region. To manage reader expectations, we have now substantially shortened the description of the program. We have kept in the insights about the program that are necessary to understand the context (pg. 7), sampling frame (pg. 8) and data collection (pg. 8), moving text accordingly to the respective subsections.

As described in our response to your major and minor comments #1, we believe that the benefits of our study are related to its ability to measure and analyse in detail the full continuum of care from an integrated perspective for a large sample of low-income pregnant women based on high-quality real-time data. A randomized control trial design would be most appropriate for conducting a subsequent rigorous impact evaluation, but as of yet, such a study is not ongoing.

*Minor suggestions*

*1. A strength of this study that seems to be underexposed currently, is the way in which data are collected. The earlier described MomCare allows for more timely data collection through an in app survey which will reduce recall bias as is often a problem with surveys like the Demographic and Health Survey, asking mothers to report about their ANC utilization up to five years prior to the survey. Authors could make this more clear in section 3.4 Data collection.*

Response: This is a very helpful suggestion. To underscore this point, we have now added to the Data Collection section (section 2.4 in the revised manuscript) that the main advantages of using real-time data collected through the analytics engine are the reduced recall bias and increased probability of accurate reporting (pg. 8). We also mention the strength of our data in the final paragraph of the introduction (pg. 5), and we have slightly adjusted the first bullet point of the article summary to emphasize that the data were collected in real-time.

*2. While abbreviations are provided in a list at the end, it would be helpful if each abbreviation is written out upon first use (for example SBA).*

Response: Thank you for this observation. We have now spelled out all abbreviations the first time we use them, both in the abstract and in the main text.

*3. The Study Setting describes that data were collected in three counties: Nairobi, Kisumu and Kakamega. Why are these three counties the most relevant when trying to understand what prevents pregnant Kenyan women from attending ANC?*

Response: These three counties are a representation of urban, peri-urban and rural areas. Nairobi is the capital, fully urban with substantial areas of informal settlements, and the largest county of Kenya. Kakamega in contrasts represents a largely rural population, with 90 percent of its population living in rural villages. Lastly, in Kisumu, 61.8 percent of the population lives in rural areas. As such, the combination of these three counties provides a

diverse overview of the maternal care-seeking behavior of pregnant women in both urban as well rural locations. We have made this more explicit on pg. 6.

*4. Is the subscript i for the beta's in the equations under 3.5 Statistical methods correct? Do the beta's indeed vary by individual i? I would expect these to vary by explanatory variable i.e. not having a subscript i but a different one, for example p.*

Response: Thank you for remarking this mistake in the notation. We have changed the formulas accordingly (see pg. 10).

*5. Both the introduction and discussion start from maternal deaths worldwide to signal the societal importance of the study. While I agree on the societal importance, the study does not look (for understandable reasons) at maternal mortality, but at antenatal care and skilled birth attendance. The underlying assumption seems to be that increased use of ANC and SBA will reduce maternal mortality. However, the authors do not cite evidence for that assumption which I find important given that quality of ANC in LMICs has been reported to be below standard in various settings so it is questionable whether that will help save maternal lives. The authors do cite studies about the link between ANC and awareness, diagnosis and emergency preparedness which is important but is not about a reduction in the health outcome they use to signal the importance of the study i.e. maternal mortality. It would be important to add that evidence or use different outcomes to signal the importance of this study.*

Response: We appreciate this advice to strengthen the motivation in our introduction. We have expanded the paragraphs on the importance of the study (pg. 3-4), adding references on the effectiveness of ANC to reduce anaemia, pregnancy-induced hypertension, pre-eclampsia, and infections during pregnancy, as well as improving neonatal and infant child outcomes; on the impact of skilled delivery on maternal mortality through the reduction of the four main obstetric complications (obstetric haemorrhage, eclampsia obstructed labor and sepsis); and on the association between a linkage of maternal care services (early enrolment, ANC, delivery) and reduced risk of combined neo-natal, perinatal and maternal mortality.

*6. The "need" for ANC is diversified into "no", "medium" and "high" risk, suggesting that women with no risk have a different or no need for ANC. I would argue that all pregnant women have a need for ANC even when their pregnancy is low risk. I therefore suggest to present this explanatory variable as an indicator of risk, not as an indicator of need and change the wording in the manuscript accordingly.*

Response: We believe that our terminology might have led to confusion, because we fully agree with the reviewer that all pregnant women need ANC regardless of their pregnancy risk. To redress this misunderstanding, we (i) explicitly mention that the recommended minimum of 4 visits are for uncomplicated and low risk-pregnancies (pg. 9), (ii) have changed the labels of the pregnancy risk-level from 'no, medium, and high risk' to 'low, medium, and high risk' (pg. 9 and Tables 1 and 2), and (iii) reformulate the description of 'need'-factors to emphasize that the need-factors capture health-related determinants of women's decision to seek maternal

care, encompassing both subjectively perceived needs and needs as evaluated by a health professional (pg. 6).

*7. The third paragraph of the discussion indicates that delivery at a MomCare clinic is lower for women who live in more educated and wealthier households. The authors showed earlier in the manuscript that higher education (of the household head) leads to more ANC visits. Could the authors explore and explain in more detail why those who used more ANC are less likely to use skilled birth attendance? Do they have lower expectations of SBA, potentially through their experiences with low quality ANC, preventing them from choosing to deliver at MomCare clinics? Or are there other explanations for the lower SBA among better educated women?*

Response: The reviewer raises a pertinent issue. In our regression of SBA on potential determinants (Table 2 columns 9-11), we find that – conditional on the number of ANC visits – women from better educated and wealthier households were less likely to have a skilled delivery at a MomCare clinic. Unfortunately, based on the quantitative data collected within the study, we cannot infer whether women of higher socio-economic status choose to deliver at home instead (potentially in the presence of a skilled birth attendant) or whether they deliver in another clinic not connected to MomCare.

As mentioned in the original manuscript, we also conducted brief phone interviews after delivery. These qualitative data confirm that women who were relatively wealthier were more likely to deliver elsewhere. Yet, the reasons they reported for their choice of clinic were mostly unrelated to financial constraints: (a) unexpected onset of labour causing the woman to be rushed to the nearest clinic, (b) relocation to another county during the pregnancy period, (c) unexpected complications, (d) and external influences in choosing the place of delivery. It may have been that better-off and better-educated households were more likely to relocate or to be persuaded to deliver in a different clinic. Unfortunately, based on the limited qualitative data, we cannot draw strong conclusions in this respect.

Since one of the reviewers urged us to shorten the discussion and focus on the main results, while another reviewer recommended taking out the qualitative survey results all together, we have decided not to delve deeper into this issue in the current study, but rather to indicate that this result merits further research on pg. 15.

**VERSION 2 – REVIEW**

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|---|---|
| **REVIEWER** | Fukuda, Yoshiharu<br>Yamaguchi University, Community Health and Medicine |
| **REVIEW RETURNED** | 11-Oct-2021 |

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| **GENERAL COMMENTS** | The manuscript had been revised according to our review comments. But some problems remained to be revised.<br>1. There were many subjects had missing for explanatory variables. They could be omitted. |

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| | 2. Regarding to Table 2, OR and CI were not shown. But, the text mentioned CI including other variables not shown in Table 2. Please rewire Table 1. |

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| **REVIEWER** | Bonfrer, IEJ<br>Erasmus Universiteit Rotterdam |
| **REVIEW RETURNED** | 18-Oct-2021 |

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|---|---|
| **GENERAL COMMENTS** | Thank you for addressing both my major and minor suggestions for revision. |

## VERSION 2 – AUTHOR RESPONSE

**Response document to Manuscript ID: bmjopen-2021-050670**

**Response to Reviewers:**

***Reviewer: 2***

***Dr. Yoshiharu Fukuda, Yamaguchi University***

*Comments to the Author:*

*The manuscript had been revised according to our review comments. But some problems remained to be revised.*

Response: Thank you for taking the time to re-read and comment on our manuscript and your constructive suggestions to revise and improve it.

1. *There were many subjects had missing for explanatory variables. They could be omitted.[Editor's note: only do so if appropriate and/or comment on how you deal with the missing data]*

Response: Thank you for your comment. We use the same set of control variables ($X_i$) in all three regressions on page 10. The utilized software program (STATA) automatically omits an individual X from all three regressions if the explanatory variables are missing. Therefore, missing values in control variables do not change the sample for the three analyses and do not bias the regression results.

*2. Regarding to Table 2, OR and CI were not shown. But, the text mentioned CI including other variables not shown in Table 2. Please rewire Table 1.*

Response: This is well-noted. We have changed Table 2 accordingly, showing OR and CI for each variables.

***Reviewer: 3***
***Dr. IEJ Bonfrer, Erasmus Universiteit Rotterdam***

*Comments to the Author:*

*Thank you for addressing both my major and minor suggestions for revision.*

Response: Thank you for taking the time to read and comment on our paper. We very much appreciate your recommendations. They have been of great help to us revising and strengthening the paper.