PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Seasonality and predictors of childhood stunting and wasting in drought-prone areas in Ethiopia: A cohort study
AUTHORS	Kabalo, Bereket; Lindtjørn, Bernt

VERSION 1 – REVIEW

REVIEWER	Anna Dimitrova UCSD, Climate, Atmospheric Science, and Physical Oceanography
REVIEW RETURNED	14-Feb-2022

GENERAL COMMENTS	Using survey data collected in two villages in the Wolaita area of Ethiopia in four consecutive seasons, the authors investigate factors associated with child undernutrition. Special attention is paid to household participation in a Productive Safety Net Programme (PSNP), their access to safe drinking water and their exposure to food insecurity. While the study focuses on a very important issue, I am not convinced that it makes an important contribution to the literature. The title of the study implies that certain intervention strategies will be investigated, however, to my knowledge the study is not based on random assignment and no causal interpretations can be made. A large part of the analysis is descriptive, and it is unclear how it relates to the overall objective of the study. I recommend a major revision of the study, removing some redundant parts of the analysis, making clear what the aims of the study are and interpreting the results with caution. Below are provide more detailed comments on specific sections of the study. Some parts of the analysis seem disconnected and other parts are redundant or misleading. For example, on page 12 lines 5-7, the authors state that "Most (73.9%) households relied exclusively on farming income…". However, in Table 2 the opposite statistic is shown. The authors need to double check the statistics reported in the study and address such inconsistencies. In the Methods section, the variables 'household food insecurity', 'dietary diversity', and 'diarrheal illness' and included in two different sections 'Outcome' and 'Exposure variables'. It is confusing whether these variables are treated as outcome or exposure measures? The authors should explain better their conceptual framework. Perhaps the authors can include a schematic diagram of their conceptual framework. On p. 11 where the authors describe their models, diarrheal illness is treated both as 'background characteristic' and as an 'intermediary variable'. The authors should be consistent throughout the paper. I do not understand why diarrheal illness would be treated as a background characteristic, in the same category as child's sex? In sections "Baseline data" (page 12) and "Household wealth and participation in PSNP" (page 13), the authors compare the sample

	populations in the two villages where the data were collected (lowland and midland areas). It is unclear what the purpose of this comparison is and how representative the two samples are of the general populations in these areas. What do we learn from comparing of these two sample populations? It seems that the population of the lowland area is wealthier and more likely to participate in the PSNP program. What could be the underlying reasons and why is this relevant for the purpose of this study? It is unclear what the sections "Baseline data" and "Household wealth and participation in PSNP" add to the study. It is even more confusing that no comparison between the two villages is made with regards to other indicators like stunting/wasting, food insecurity and dietary diversity. The statistics reported in these sub-sections seem disconnected and redundant. More interesting would be to show how PSNP participation differs by socio-economic status, education, dependence on farming income, and also to relate it to child nutritional status. Are food insecure households more or less likely to benefit from such programs and what are target population for these programs in general. Are households participating in this program more or less likely to experience food insecurity and to have undernourished children? Enrolment in PSNP is unlikely to be random, so what are the implications for the conclusion you draw? The authors show some interesting statistics in sub-section "Intermediary factors". It seems that food insecurity increases during the dry pre-harvest season and as households experience more food insecure seasons, their dietary diversity seems to decline. However, these findings are to be expected since both indicators (dietary diversity and food insecurity) relate to poverty. Households that are continuously reporting food insecurity in all 4 seasons are probably the poorest households, who also consume less diverse food. I suggest that the authors improve the first part of the results section by removing redundant statistics, correcting inconsistent statistics, and focusing on the most relevant findings. For example, more focus can be placed on the seasonal variation in wasting, food insecurity and diarrhea prevalence. Throughout the analysis, the authors repeatedly switch between measuring HAZ/WHZ and stunting/wasting. Why did the authors decide to switch from one measure to another? This confuses the reader unnecessarily. I suggest that the authors remain consistent. Moreover, in the Introduction section on p. 5 the authors discuss in detail the disadvantage of using dichotomized measures of undernutrition such as stunting/wasting and the advantage of using continuous measures such as HAZ/WHZ. Therefore, it was surprising to see that the authors still decided to use stunting/wasting measures in their analysis. Participation in PSNP seems like a relevant factor for child nutrition, however, since the study is to my knowledge not based on a random assignment, no causal link with child nutrition can be deduced. The same applies to safe water access. It may be that households participating in PSNP (having access to safe water) are fundamentally different from households not participating in PSNP (without access to safe water) and this will bias the results, unless assignment into PSNP (clean water access) is random? Did some households enroll in PSNP and/or receive clean water in the course of the survey and was this based on random assignment or not? If not, there is likely to be unobserved confounding. Moreover, it is unclear how the results presented in Figures 3 and 4 were obtained? Did the authors used a regression model and were confounders

	included? Or are the results purely descriptive, in which case no causal interpretation can be made? On page 17, the results reported for Table 5 are confusing. The authors claim that PSNP participation reduced the risk of wasting, however, the coefficient in Table 5 shows that NOT participating in PSNP reduces the risk of wasting (beta = - 0.168). The authors should double check the results reported in Table 5. Same applies to their interpretation of safe drinking water. Why in Tables 5 and 6, different reference values are used for some variables such as PSNP and clean water access? In Table 5, the reference category for PSNP is participation and in Table 6, NOT participation. This is confusing for the reader; I suggest that the authors remain consistent.

REVIEWER	Sugeng Wiyono Politeknik Kementerian Kesehatan Jakarta II, Nutrition
REVIEW RETURNED	19-Feb-2022

GENERAL COMMENTS	The reviewer provided a marked copy with additional comments. Please contact the publisher for full details.

VERSION 1 – AUTHOR RESPONSE

Author's point-by-point responses to specific questions and comments the reviewers forwarded.
Reviewer's comments and author's responses are in a dialogue form (our responses to each question or comment are outlined below the specific comment)**.**
Reviewer: Did some households enroll in PSNP and/or receive clean water in the course of the survey and was this based on random assignment or not? If not, there is likely to be unobserved confounding. Enrolment in PSNP is unlikely to be random, so what are the implications for the conclusion you draw? Participation in PSNP seems like a relevant factor for child nutrition, however, since the study is to my knowledge not based on a random assignment, no causal link with child nutrition can be deduced. The same applies to safe water access. It may be that households participating in PSNP (having access to safe water) are fundamentally different from households not participating in PSNP (without access to safe water) and this will bias the results, unless assignment into PSNP (clean water access) is random? Are food insecure households more or less likely to benefit from such programs and what are target population for these programs in general? Are households participating in this program more or less likely to experience food insecurity and to have undernourished children?
Author: We did this observational study (in a cohort design) among a random sample of households with children aged below 5 years. We refer to the details on our sampling procedures in the revised manuscript (Page 6, Lines: 15-24). However, we regret for some misleading terms we used in this work (e.g., interventions in the title) as we did not implement any intervention. As such, household PSNP participation and drinking water access were among the background factors we considered in this study. Given our random selection of the households, PSNP participation and protected drinking water access we reported could indicate the coverage of PSNP and clean water access in the study area.

Reviewer: In sections “Baseline data” (page 12) and “Household wealth and participation in PSNP” (page 13), the authors compare the sample populations in the two villages where the data were collected (lowland and midland areas). It is unclear what the purpose of this comparison is and how representative the two samples are of the general populations in these areas. What do we learn from comparing of these two sample populations? It seems that the population of the lowland area is wealthier and more likely to participate in the PSNP program. What could be the underlying reasons and why is this relevant for the purpose of this study? It is unclear what the sections “Baseline data” and “Household wealth and participation in PSNP” add to the study. It is even more confusing that no comparison between the two villages is made with regards to other indicators like stunting/wasting, food insecurity and dietary diversity. The statistics reported in these sub-sections seem disconnected and redundant. More interesting would be to show how PSNP participation differs by socio-economic status, education, dependence on farming income, and also to relate it to child nutritional status.
Author: We entirely accepted the above comments and summarised our baseline data in Table 1 (Page 11), as our study was based on one sample estimation. In our revised submission, we cleared out all misleading texts starting from our very title throughout the document.
Reviewer: In the Methods section, the variables ‘household food insecurity’, ‘dietary diversity’, and ‘diarrheal illness’ and included in two different sections ‘Outcome’ and ‘Exposure variables’. It is confusing whether these variables are treated as outcome or exposure measures?
Author: We considered household food insecurity, dietary diversity, and child diarrhoeal illness as time-varying exposures. We also realised some terms we used (e.g., intermediary factors vs. intermediary outcomes) in our previous submission that could confuse our readers and edited all such texts throughout the document in our revised submission (Page 6, Lines: 1-7).
Reviewer: The authors should explain better their conceptual framework. Perhaps the authors can include a schematic diagram of their conceptual framework.
Author: We kindly accepted these comments. In our revised submission, we included a conceptual framework for our work (Figure 1). Our work was based on a systematic review paper by Phalkey et al., suggesting complex pathways from climate variability to undernutrition in subsistence communities (Phalkey, Aranda-Jan et al. 2015)., but we adapted their work to the scope of our study and we focused on the human nutrition (Page 9, Lines: 4-5). Table 1 in our previous submission (units of analysis) is now replaced by a flow chart of measurements for this cohort study (Figure 2).
Reviewer: On p. 11 where the authors describe their models, diarrheal illness is treated both as ‘background characteristic’ and as an ‘intermediary variable’. The authors should be consistent throughout the paper. I do not understand why diarrheal illness would be treated as a background characteristic, in the same category as child’s sex?
Author: We kindly accepted this comment and corrected such editorials all across the document. We considered childhood diarrhoeal illness as a time-varying exposure (Page 8, Lines: 3-5) and assessed if seasonal variations in child diarrhoeal illness (Table 2) would affect our outcome estimates. However, the observed seasonality of diarrhoeal illness showed no association with child undernutrition in our study area (bivariate analysis). As such, we considered child diarrhoeal illness as covariate with other time-varying exposures (Table 4, 5, and 6). More details on our time-varying data considerations are included in our revised submission (Page 9, Lines: 7-16) and (Page 10, Lines: 1-9).
Reviewer: The authors show some interesting statistics in sub-section “Intermediary factors”. It seems that food insecurity increases during the dry pre-harvest season and as households experience more food insecure seasons, their dietary diversity seems to decline. However, these findings are to be expected since both indicators (dietary diversity and food insecurity) relate to

poverty. Households that are continuously reporting food insecurity in all 4 seasons are probably the poorest households, who also consume less diverse food.
Author: We kindly accepted these comments and described on how these two correlated factors (Household food insecurity and dietary diversity) were handled as exposure variables (Page 8, Lines: 5-16). We refer to the details in our revised manuscript (Page 7, Lines: 11-31 and Page, Lines: 8 1-16).
Reviewer: On page 12 lines 5-7, the authors state that "Most (73.9%) households relied exclusively on farming income…". However, in Table 2 the opposite statistic is shown. The authors need to double check the statistics reported in the study and address such inconsistencies.
Author: We kindly accepted your comments and made relevant revisions. In our revised manuscript, all our baseline data are summarised in Table 1(Page 11).
Reviewer: I suggest that the authors improve the first part of the results section by removing redundant statistics, correcting inconsistent statistics, and focusing on the most relevant findings. For example, more focus can be placed on the seasonal variation in wasting, food insecurity and diarrhea prevalence.
Author: We kindly accepted these comments. As such, we have made major revisions in our revised submission (e.g., removed redundant statistics, corrected inconsistent statistics, and focused on seasonality and baseline risk factors for child malnutrition). (Page 11-24).
Reviewer: Throughout the analysis, the authors repeatedly switch between measuring HAZ/WHZ and stunting/wasting. Why did the authors decide to switch from one measure to another? This confuses the reader unnecessarily. I suggest that the authors remain consistent. Moreover, in the Introduction section on p. 5 the authors discuss in detail the disadvantage of using dichotomized measures of undernutrition such as stunting/wasting and the advantage of using continuous measures such as HAZ/WHZ. Therefore, it was surprising to see that the authors still decided to use stunting/wasting measures in their analysis.
Author: We accepted your comments and edited the outcome data definitions and results. Our main outcome measures were height-for-age and weight-for-height indices (Z-scores), measured at each season for one year and defined based on the World Health Organization (WHO) 2006 child growth standards (Bloem 2007). Our main outcome definitions were based on HAZ and WHZ data distributions (i.e., decreased HAZ was defined as increased risk of stunting and vice-versa and decreased WHZ was defined as increased risk of wasting and vice-versa) (Wells, Briend et al. 2019). We refer to our revisions in the methods section (Page 7, Lines 1-10). We used categorical classifications only to describe the magnitude of child undernutrition (stunting and wasting rates) in the study area.
Reviewer: It is unclear how the results presented in Figures 3 and 4 were obtained? Did the authors used a regression model and were confounders included? Or are the results purely descriptive, in which case no causal interpretation can be made?
Author: We thank you for these comments. As Figure 3 in our previous submission does not add any information and as we have many tables and figures, we deleted Figure 3 in our revised submission. We also made major revision on Figure 4 to explore the effects of PSNP participation and protected drinking water access on child nutritional status (WHZ). As to your technical question on how to draw such figures (3 and 4 in old submissions) we refer to 'plofileplot' command in Stata for data with repeated measurements.
Reviewer: On page 17, the results reported for Table 5 are confusing. The authors claim that PSNP

participation reduced the risk of wasting, however, the coefficient in Table 5 shows that NOT participating in PSNP reduces the risk of wasting (beta = - 0.168). The authors should double check the results reported in Table 5. Same applies to their interpretation of safe drinking water. Why in Tables 5 and 6, different reference values are used for some variables such as PSNP and clean water access? In Table 5, the reference category for PSNP is participation and in Table 6, NOT participation. This is confusing for the reader; I suggest that the authors remain consistent.
Author: We entirely accepted your comments and made major revisions on our model specifications and results. We refer to Tables 4, 5, and 6 in our revised submission where we cleared out the above issues you forwarded (Page 14-22).

VERSION 2 – REVIEW

REVIEWER	Anna Dimitrova UCSD, Climate, Atmospheric Science, and Physical Oceanography
REVIEW RETURNED	06-Sep-2022

GENERAL COMMENTS	I congratulate the authors for the extensive revision of the manuscript. The paper is much improved, the methods and the narrative are much more clear and the results are very informative. I have no further comments and suggestions.