

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Child Malnutrition and Recurrent Flooding in Rural Eastern India: A Community-Based Survey
AUTHORS	Rodriguez-Llanes, Jose Manuel; Ranjan-Dash, Shisir; Degomme, Olivier; Mukhopadhyay, Alok; Guha-Sapir, Debarati

VERSION 1 - REVIEW

REVIEWER	Sari Kovats Senior Lecturer Department of Social and Environmental Health Research Faculty of Public Health and Policy London School of Hygiene and Tropical Medicine 15 - 17 Tavistock Place London WC1H 9SH sari.kovats@lshtm.ac.uk
REVIEW RETURNED	21-Mar-2011

THE STUDY	<p>The paper would benefit from a clear hypothesis and clear focus of on the main question. The discussion of the general determinants of child malnutrition should be limited to only where there is evidence of effect modification of the flood-malnutrition association.</p> <p>A more detailed critique of studies that attribute health effects to floods in similar settings (rural populations in low income countries) would be useful.</p>
RESULTS & CONCLUSIONS	<p>This is an important paper with a valuable dataset. The paper should be published, but more care should be taken in the reporting and interpretation of the results.</p> <p>There is clearly some post-hoc rationalisation as the main exposure appears to have changed from the 2008 floods to the 2006 floods. In addition, the lack of evidence for wasting</p> <p>A key issue for this study is selection bias and whether the control villages are really sufficiently similar to the flood villages. The assessment of being a flooded village (in 2006 and 2008) needs more attention in the methods.</p>
GENERAL COMMENTS	<p>See general comments above. This is an interesting and potentially very useful research paper.</p> <p>Minor comments</p> <ul style="list-style-type: none"> * The prevalence of stunting, underweight and wasting should be included with other baseline data in Table 1. * the adjusted ratios for flood-exposed for all 3 malnutrition outcomes should be included in the same table. Not all the ratios for the non-flood determinants of malnutrition need to be shown as they

	<p>are not directly relevant to the research question.</p> <p>* Table 4. I do not think that this is the best way to look at age as a modifier for flood health effects.</p> <p>* Surely disaster aid was also available immediately after the 2006 flood and so may have also avoided any acute malnutrition effects.</p> <p>* page 14 23-26. This statement is too strong - there are other causes of malnutrition than disasters.</p> <p>* In the survey, were any questions asked about the impact of the flood on the household? e.g. loss of food, loss of access to water, loss of livelihood or evacuation from house? This information would have been very useful for your study.</p> <p>* A more detailed time line regarding the causes of stunting vs wasting and the flood exposures would be useful, and clarify the proposed mechanisms.</p> <p>* The methods section could be shortened by removing excess detail, and information that is not directly relevant (e.g. background on Microdis project).</p>
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REVIEWER	P. Gregg Greenough, MD, MPH School of Public Health, Harvard University, Harvard Humanitarian Initiative
REVIEW RETURNED	06-May-2011

THE STUDY	<p>Please see my comments in the attachment -</p> <p>At the end of the introduction, the research question is vague and needs clarification - I recommend stating that "this study seeks to explore the strengths of association between an extreme weather event (flooding) and the prevalence of undernutrition while taking other variables that directly affect nutrition into account "</p> <p>Please see my attached comments -- the description of the sampling strategy also needs clarification - it is unclear whether the sampling stages are using a simple (or stratified) random sampling approach or a cluster sampling approach.</p>
GENERAL COMMENTS	<p>General comments:</p> <p>1. The author's exploration of weather variability and what that portends for vulnerable populations is laudable and opens the doors for more detailed studies on the associations between climate issues and health. That this study is regionally specific to a place of high climatic vulnerability also argues for its place in the published literature.</p> <p>Specific comments:</p> <p>2. Introduction:</p> <p>a. A brief discussion of what the climate models predict for Orissa's future would add significant weight to the paper's premise that future health effects at the hands of climate change is not only an awareness issue but one for which tools exist to study the implications.</p> <p>b. The research objective that concludes the introduction is vague and needs sharper clarity: i.e., state that this study seeks to explore the strengths of association between an extreme weather event (flooding) and the prevalence of undernutrition while taking other variables that directly affect nutrition into account such as poverty and food insecurity – or something to that effect.</p> <p>c. Use of prevalence ratios adds clarity to the interpretation—</p>

good choice.

3. Sample selection:

a. The fact that the authors chose villages that were repeatedly flooded and those that remained not flooded in each of the preceding years minimizes any cohort related chronic malnutrition (a longer term indicator) – good!

b. The description of the multi-stage sampling needs clarification – the authors obtained lists of children (as would be needed for a simple or stratified random sample) but then used a modified EPI method (as would be done in a cluster sample) at the second stage to select households rather than children. If simple or stratified random sampling was not used then the researchers must include the design effect and inflate the sample size. This clarification is paramount for the study's credibility – if indeed it's a cluster sample (first stage sampling of villages as clusters, second stage households), then the study is inadequately powered, the precision is less, the variance increased, and the entire dataset inappropriately analyzed (as if it was a simple or stratified random sample when in reality it was a cluster sample). My hunch is that it truly is a stratified random sample – the description of the sampling strategy needs clarification so as not to confuse it with a cluster sample.

c. As a follow on to the previous comment, if simple random sampling (from lists of potential respondents) was used, the sampling design is really a stratified (by flood exposure) random sample – why did the authors not characterize it thus?

d. The rationale for 10% of households selected in each village is unclear – if the villages varied in size a uniform selection of a blanket 10% for each would technically make the sample less random (variable chances of being selected).

e. The authors should mention the specific reason why the children of three villages couldn't be measured – otherwise the reader is left to wonder if this group could have biased the sample in any way – an explanation would be helpful here or in the discussion.

4. Outcome measures

a. Maternal education is a critical variable for undernutrition – why wasn't this included?

b. Since there is no explanation given for defining the age categories they seem quite arbitrary. Why these intervals – at 26 months, 46 months, etc? I suggest running age as a continuous variable in the regression to determine which method provides a more accurate adjustment.

5. Power analysis

a. Is there a reference for the stunting assumption in Orissa?

6. Results and Discussion

a. While the authors rightly claim that this study was not designed to determine causality, the strengths of association with income and flood exposure are still quite marginal and the suspicion for residual confounding by other variables not in the analysis should be addressed as significant design limitations in the discussion. For instance:

i. The poor tend to live on marginal land such as floodplains, prone to flooding;

ii. The rural household's state of food insecurity (as defined by sufficient access to food) could be highly influenced by floods – blocking their ability to get to their fields, degrading quality of land from recurrent flooding over the long term, etc

iii. Maternal education (mentioned above), a strong confounder

	<p>for wasting and stunting, was not even considered in the model.</p> <p>iv. Communities given to recurrent flooding and food insecurity often develop coping mechanisms which in this study could have had a mitigating effect – but these markers were not taken into account.</p> <p>b. An acknowledgement of the complexities of rural household food insecurity in the discussion section would give the interested reader the understanding that while flood exposure demonstrates an association with underweight and stunted children, the further question of causality is a multi-layered, complex undertaking for future studies.</p> <p>Overall, the paper and study is a helpful “shot across the bow” to inspire and prompt greater epidemiologic thought on vulnerable populations and future climate events.</p>
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REVIEWER	
REVIEW RETURNED	

THE STUDY	
RESULTS & CONCLUSIONS	
REPORTING & ETHICS	
GENERAL COMMENTS	

VERSION 1 – AUTHOR RESPONSE

Authors - The authors are grateful for the insightful comments of both reviewers, which have helped improve the manuscript.

Reviewer: Sari Kovats

Senior Lecturer

Department of Social and Environmental Health Research

Faculty of Public Health and Policy

London School of Hygiene and Tropical Medicine

15 - 17 Tavistock Place

London WC1H 9SH

SK - The paper would benefit from a clear hypothesis and clear focus of on the main question.

Authors - We have focused our main research question. See changes at the end of the introduction and at the first paragraph of the methods.

SK - The discussion of the general determinants of child malnutrition should be limited to only where there is evidence of effect modification of the flood-malnutrition association.

Authors - Following your suggestion, we have limited our discussion to variables modifying this association.

SK - A more detailed critique of studies that attribute health effects to floods in similar settings (rural populations in low income countries) would be useful.

Authors - We agree with this comment and we have expanded our introduction along these lines.

SK - This is an important paper with a valuable dataset. The paper should be published, but more care should be taken in the reporting and interpretation of the results.

There is clearly some post-hoc rationalisation as the main exposure appears to have changed from the 2008 floods to the 2006 floods. In addition, the lack of evidence for wasting

Authors - Despite 2006 and 2008 were different events and clearly were different in total extent, the exposed villages selected in this study were inundated during both 2006 and 2008 according to OSDMA. This is also consistent with our data collected at the household level. The type of disaster (eg, flood, cyclone), perceived severity of the disaster (i.e. very severe, severe, moderate and mild) and date (month-year) were recorded for the last three events suffered. All 757 respondents interviewed the 14 flooded villages confirmed their household to be exposed to only these two floods and the cyclone Paradip in October 1999. Most respondents considered the 2008 floods 'very severe' (84.6%) compared to the 2006 floods which they mostly categorized as 'mild' (79.5%).

SK - A key issue for this study is selection bias and whether the control villages are really sufficiently similar to the flood villages. The assessment of being a flooded village (in 2006 and 2008) needs more attention in the methods.

Authors – We also think selection bias is a very important issue to interpret the findings. We have detailed possible implications of selecting villages with accessibility after the floods (see limitations in the discussion section).

Regarding the control villages, we have given all details available in the census on the characteristics of the two subpopulations studied (see changes highlighted in yellow in Table S1). Both subpopulations are very similar for the variables compared, except that in the non-flooded villages a greater percentage of the population is working (4.2% difference). Village size was also comparable among the two groups.

We have given more details in the methods about the assessment of being a flooded village in 2006 and 2008.

SK - This is an interesting and potentially very useful research paper.

Minor comments

* The prevalence of stunting, underweight and wasting should be included with other baseline data in Table 1.

Authors - We have corrected this. These prevalences are included in Table 1. Table S2 is not necessary anymore.

* the adjusted ratios for flood-exposed for all 3 malnutrition outcomes should be included in the same table. Not all the ratios for the non-flood determinants of malnutrition need to be shown as they are not directly relevant to the research question.

Authors – We think the way you suggested to show the results is much clearer and compact. We have followed your suggestions and only included significant determinants. We have kept all full

tables (2 and 3) as supplementary information, as these may be of interest for further analyses (e.g. meta-analysis).

* Table 4. I do not think that this is the best way to look at age as a modifier for flood health effects.

Authors – Here the aim was to show that the cohort of children that were younger than 1 year when they were exposed to floods in 2006 have the largest difference in prevalence of chronic malnutrition compared to the same age group in the non-exposed. Our analysis was not correct in the previous manuscript, because we compared each cohort with a reference cohort. We have conducted new analyses (see methods and results).

* Surely disaster aid was also available immediately after the 2006 flood and so may have also avoided any acute malnutrition effects.

Authors - We agree thought this is difficult to assess in our study.

* page 14 | 23-26. This statement is too strong - there are other causes of malnutrition than disasters.

Authors – A more appropriate sentence has been used here.

* In the survey, were any questions asked about the impact of the flood on the household? e.g. loss of food, loss of access to water, loss of livelihood or evacuation from house? This information would have been very useful for your study.

Authors - Yes, but they were only asked in the flooded villages. Thus we could not use these variables in this study. This impact data may be of interest to understand how it can affect health outcomes, though the sample size will be smaller for analysis on malnutrition. In later surveys we have emphasized the need to collect this data also in the non-flooded villages.

* A more detailed time line regarding the causes of stunting vs wasting and the flood exposures would be useful, and clarify the proposed mechanisms.

Authors - More detailed studies would be helpful to untangle these mechanisms. As suggested in your comments and those of the other reviewer, important factors remain unknown and thus a more deep understanding is necessary. Further cohort studies would be helpful to provide a time line.

* The methods section could be shortened by removing excess detail, and information that is not directly relevant (e.g. background on Microdis project).

Authors – The background on Microdis project has been removed and we have tried to shorten the methods section as much as possible.

Reviewer: P. Gregg Greenough, MD, MPH

School of Public Health, Harvard University, Harvard Humanitarian Initiative

Please see my comments in the attachment -

GG - At the end of the introduction, the research question is vague and needs clarification - I recommend stating that "this study seeks to explore the strengths of association between an extreme weather event (flooding) and the prevalence of undernutrition while taking other variables that directly affect nutrition into account "

Authors - We absolutely agree with this comment and we have used your suggestion at the end of the introduction.

Please see my attached comments -- the description of the sampling strategy also needs clarification - it is unclear whether the sampling stages are using a simple (or stratified) random sampling approach or a cluster sampling approach.

Thank you for the opportunity to review this manuscript.

General comments:

GG - 1. The author's exploration of weather variability and what that portends for vulnerable populations is laudable and opens the doors for more detailed studies on the associations between climate issues and health. That this study is regionally specific to a place of high climatic vulnerability also argues for its place in the published literature.

Specific comments:

2. Introduction:

GG - a. A brief discussion of what the climate models predict for Orissa's future would add significant weight to the paper's premise that future health effects at the hands of climate change is not only an awareness issue but one for which tools exist to study the implications.

Authors – We have included specific climate model predictions for Orissa and its vulnerability to weather variability at the end of the introduction.

b. The research objective that concludes the introduction is vague and needs sharper clarity: i.e., state that this study seeks to explore the strengths of association between an extreme weather event (flooding) and the prevalence of undernutrition while taking other variables that directly affect nutrition into account such as poverty and food insecurity – or something to that effect.

Authors - We fully agree with this comment and we have used your suggestion at the end of the introduction.

c. Use of prevalence ratios adds clarity to the interpretation—good choice.

3. Sample selection:

GG - a. The fact that the authors chose villages that were repeatedly flooded and those that remained not flooded in each of the preceding years minimizes any cohort related chronic malnutrition (a longer term indicator) – good!

b. The description of the multi-stage sampling needs clarification – the authors obtained lists of children (as would be needed for a simple or stratified random sample) but then used a modified EPI method (as would be done in a cluster sample) at the second stage to select households rather than children. If simple or stratified random sampling was not used then the researchers must include the design effect and inflate the sample size. This clarification is paramount for the study's credibility – if indeed it's a cluster sample (first stage sampling of villages as clusters, second stage households), then the study is inadequately powered, the precision is less, the variance increased, and the entire

dataset inappropriately analyzed (as if it was a simple or stratified random sample when in reality it was a cluster sample). My hunch is that it truly is a stratified random sample – the description of the sampling strategy needs clarification so as not to confuse it with a cluster sample.

c. As a follow on to the previous comment, if simple random sampling (from lists of potential respondents) was used, the sampling design is really a stratified (by flood exposure) random sample – why did the authors not characterize it thus?

d. The rationale for 10% of households selected in each village is unclear – if the villages varied in size a uniform selection of a blanket 10% for each would technically make the sample less random (variable chances of being selected).

Authors – We agree that as stated the design of the study is not clear and we have attempted to clarify this in the methods.

We think it is important to answer some of your comments here:

b-d. There is no random selection of villages. Fourteen flooded villages and 18 non-flooded villages were selected. The limiting factor of this study was the accessibility to the villages. Thus 14 accessible villages were chosen. The 18 non-flooded villages were selected by proximity to those flooded. The sample was stratified by village. In each village, the number of households was obtained from the census and a modified EPI 'random walk' method used to sample 10% of the households in each village. Those surveyed households containing children was the data analyzed in this study. A stratified (by exposure) random sample would have been a better design. Because we could not get the list of households due to time constraints, the EPI method was used and stratifying by exposure was not a plausible solution.

The list of children was obtained to cross check the information given by the households and also to get an idea of the final number of children we may get. The lists of children are much easily obtained as this information is centralized in the ICDS centers available in each village.

A 10% of the households selected in each village give an equal probability of selection (0.1) to any households in the population. In a cluster survey, however, the number of households is fixed, thus the chances are different at the village level, but these are compensated by the PPS selection of villages (clusters) at the first stage to give equal probabilities of selection of the primary sampling unit.

GG - e. The authors should mention the specific reason why the children of three villages couldn't be measured – otherwise the reader is left to wonder if this group could have biased the sample in any way – an explanation would be helpful here or in the discussion.

Authors – This is correct. We have included this information in Figure 1 and the methods.

4. Outcome measures

GG - a. Maternal education is a critical variable for undernutrition – why wasn't this included?

Authors – There was a problem with the questionnaire design. The original household questionnaire collected information on education for all members in the household, but it did not include a variable identifying the mothers of the children. Thus the information could not be used.

GG - b. Since there is no explanation given for defining the age categories they seem quite arbitrary. Why these intervals – at 26 months, 46 months, etc? I suggest running age as a continuous variable in the regression to determine which method provides a more accurate adjustment.

Authors – We agree that this is not clear in the text. A more detailed explanation has been now included.

5. Power analysis

GG - a. Is there a reference for the stunting assumption in Orissa?

Authors – The reference is given in the first version of the manuscript (#30). In the new version this is #36. This section needed to be better explained.

6. Results and Discussion

GG-

a. While the authors rightly claim that this study was not designed to determine causality, the strengths of association with income and flood exposure are still quite marginal and the suspicion for residual confounding by other variables not in the analysis should be addressed as significant design limitations in the discussion. For instance:

i. The poor tend to live on marginal land such as floodplains, prone to flooding;

ii. The rural household's state of food insecurity (as defined by sufficient access to food) could be highly influenced by floods – blocking their ability to get to their fields, degrading quality of land from recurrent flooding over the long term, etc

iii. Maternal education (mentioned above), a strong confounder for wasting and stunting, was not even considered in the model.

iv. Communities given to recurrent flooding and food insecurity often develop coping mechanisms which in this study could have had a mitigating effect – but these markers were not taken into account.

b. An acknowledgement of the complexities of rural household food insecurity in the discussion section would give the interested reader the understanding that while flood exposure demonstrates an association with underweight and stunted children, the further question of causality is a multi-layered, complex undertaking for future studies.

Authors – We agree with your comments and these limitations are acknowledged in the discussion.

Overall, the paper and study is a helpful “shot across the bow” to inspire and prompt greater epidemiologic thought on vulnerable populations and future climate events.