PEER REVIEW HISTORY

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

ARTICLE DETAILS

TITLE (PROVISIONAL)	Double burden of malnutrition in children aged 24-59 months by socioeconomic status in five South Asian countries: evidence from Demographic and Health Surveys
AUTHORS	Hossain, Fariha Binte; Shawon, Md Shajedur Rahman; Al-Abid, Md Shehab Uddin; Mahmood, Sultan; Adhikary, Gourab; Bulbul, Md. M. Islam

VERSION 1 – REVIEW

REVIEWER	Mark McGovern Queen's University Belfast, Northern Ireland
REVIEW RETURNED	21-Aug-2019

| GENERAL COMMENTS | Review of bmjopen-2019-032866 "Double burden of malnutrition in children aged 24-59 months by socioeconomic status in five South Asian countries: evidence from Demographic and Health Surveys"

21st August 2019

Comments to author(s)

This is a mainly descriptive paper that measures the proportion of child of who are underweight and overweight in a selection of countries in South Asia (Bangladesh, India, Pakistan, Maldives, and Nepal). The data used are from the Demographic and Health Surveys. I have the following comments.

Major Comments

1. Understanding the prevalence of being underweight among children is certainly important from a policy perspective, as is identifying the prevalence of overweight among children. For the reasons the authors outline, it is also important to identify those countries and locations that experience high rates of both of these at the same time. Therefore, the motivation for focusing on the topic presented seems clear.

However, I wonder if the analysis warrants a research article of this length. The analysis is very descriptive, and does not go beyond providing regression-adjusted estimates of prevalence by group (e.g. household education). I would not say this is an issue in every context, often this type of analysis can be very informative and useful, but in this case the data presented seem to be more suited to a report rather than a research article. For instance, it appears to be possible to obtain estimates of the prevalence of both underweight and overweight among children, including by |

	country and group, directly from the DHS (https://www.statcompiler.com/en/).
	I am not sure how the authors could draw out their contribution better than it is currently represented. Instead, my suggestion would be to either substantially shorten the paper, or else consider some additional analysis that could help further our understanding of the double burden issue. Perhaps one of the recent review papers on this subject could be useful for pointing to what this could be, e.g. :
	Haddad, L., Cameron, L., Barnett, I., 2015. The double burden of malnutrition in SE Asia and the Pacific: priorities, policies and politics. Health Policy Plan 30, 1193–1206.
	2. There are many typos and grammatical errors in the paper (e.g. the first sentence), it needs to be thoroughly checked.
	3. There is a lot of repetition in the discussion section.
	4. The use of the IOTF cut-off requires further justification, ideally with evidence on robustness of results to alternative definitions, and discussion of the potential limitations of cross-country comparisons.

REVIEWER	Ivan Mejia Stanford University, USA
REVIEW RETURNED	31-Aug-2019

GENERAL COMMENTS	Overall notes
	Abstract
	- Which are the 'latest' years? (line 7, p2) - Define 'ORs' (line 20, p2). - 'richest vs poorest households' (line 20, p2). The unit of analysis is children or households? - 'household education level'. What does this mean? Education of the mother, father, household head? (line 1, p3) - Revise 'Conclusions'. It is not clear in what sense the associations between SES and underweight 'were consistent' (line 4-5); 'tackle double burden'? (line 8, p3)
	Strengths and limitations of this study
	- 'information on dietary and lifestyle factors that could modify those 13 associations.' (lines 12-13, p4). Information on dietary is available in DHS surveys, perhaps not in all survey waves, but the authors can take a closer look at the information available in each country.
	Intro
	The main problem of this section is the motivation behind the 'need to understand the socioeconomic inequalities in nutritional outcomes' (lines 6-7, p6). It is not clearly stated in my opinion. I also do not see a clear hypothesis about trying to explain the coexistence of the two burdens of malnutrition.

- 'overnutrition (overweight or obesity) within individuals' (line 3, p5). What does 'within individuals' mean?
- I do not understand the following phrase 'While South Asian countries have highest numbers of underweight children due to higher prevalence rates and large populations in younger age groups' (lines14-16, p5).
- 'South Asians children living in developed countries also have a much higher prevalence of overweight than any other ethnic groups - a recent study suggests'. Why this is relevant? Are the authors suggesting that South Asian children are predisposed to be overweight?
- 'using the latest nationally-representative surveys' (line 16, p6). Which surveys?

Methods

The main issue of this section is that stunting, and wasting were not included as indicators of undernutrition. This requires further explanation based on previous evidence.

- 'DHS' (line 3, p7). 'Demographic and Health Surveys' if this is the first mention.
- 'Year of survey for each included country is given in Table 1' (line 7, p7). The corresponding years for each country should be added in the main text.
- The authors provide many details about the characteristics and structure of DHS data (e.g., lines 5-8, p 8). The authors can summarize and cite relevant sources.
- 'We excluded children aged less than 24 months because there is no available classification system for defining overweight for them'. A citation is needed here.
- Underweight is a composite definition which can encompass stunting, wasting, or both (lines 1-2, p9). The authors are correct in that underweight is a compositive measure of wasting and stunting. However, this does not seem to justify their exclusion from the analysis in my opinion. Particularly stunting, which reflect chronic status of undernutrition, it is related to environmental and socioeconomic circumstances, and may have lasting consequences. It seems to be relevant for this study. In addition, previous studies have found that stunting and overweight can occur concurrently (see Bates et al. 2017, JECH). Ultimately, what can we say of children who may be wasted/stunted and overweight, but not underweight?
- What does it mean 'Household's highest education level'? (Table 1). Is that the education of the mother, father, household head or other hh member? Please clarify.
- Important predictors of undernutrition have been omitted in this analysis (e.g., mother's height, poor dietary intake, mother's BMI, household air quality, sanitation).
- 'we applied Stata's survey estimation procedures ("svy" command) for regression analyses' (line 15-16, p10). The 'svy' need to be used for the estimation of prevalence, proportions, means too. Survey weights are not enough to estimate the precision of those indicators.

Results

Some statements are difficult to follow or understand in this section. There are also issues with interpretation of results and consistency with the statements in the abstract.

- 'Pakistan has significant' (line 9, p11). 'significant' may not be used in this context.
- 'the better part of burden for malnutrition in all countries was due to undernutrition (Figure 1)'. I do not understand what 'the better part of burden for undernutrition' means.
- 'Pakistan and Maldives had much higher prevalence, 7% and 9% respectively.' (line 19, p11). In the abstract this difference was characterized as a 'slightly higher prevalence'.

Discussion

- The authors cite relevant work from the region that describe important determinants for undernutrition (lines 20-23, p14), but the authors did not include those determinants in their own analysis (e.g., initiation of complementary feeding). It would be important to control for other factors and see if the results stand.
- In addition, the author did not discuss how this double burden has evolved across time. Many publications in the region should be available presenting trends of undernutrition and overweight separately. The authors can also estimate trend using DHS surveys from previous years.
- Finally, the authors omit to discuss the existence and effectiveness of policies/programs trying to address both malnutrition problems. Is there evidence on this regard from other countries, or policies that can address both problems simultaneously?

Other minor comments
Proof reading is needed. There are several grammatical issues along the text
e.g., 'implies to the presence' (line 2, p5)
'South Asian countries have highest numbers' (line 15, p5)
'South Asians children' (line 17, p5)
'are of residence' (line 13, p10)
'in compared to' (line 21, p11; line 19, p12)
And others, this list is not exhaustive.

REVIEWER	Mustafa Mahfuz Nutrition and Clinical Services Division, icddr,b Bangladesh One of the authors is from my institute. However, I do not work with him with any research. I do not have any competing interest.
REVIEW RETURNED	07-Sep-2019

GENERAL COMMENTS	This paper presents analysis of DHS survey data of five South Asian countries to explore the double burden of malnutrition among young children by household's socioeconomic status. These countries housed the highest burden of childhood undernutrition in the world. However, following the recent socio-economic transitions, double burden of malnutrition becomes evident in these countries with slower reduction rate of undernutrition with a faster increasing rate of overnutrition. This is very important topic and the paper is well-written. The authors have mentioned that this is the first study to explore double burden in South Asian countries using nationally representative sample size. Double burden of malnutrition can occur at individual level, household level and even at population level. The authors have used the population level data to explore prevalence of over nutrition and under nutrition among children 24

months to 60 months of age. However, there is a recent publication using the same DHS 2014 data of Bangladesh to explore double burden at household level (Das S, PHN 2019). Although Das et al explored the coexistence of overweight or obese mother and undernourished children.

However, the findings were nothing new or interesting. Except for wealth index and education level there were no other variables that were used to explain the double burden of malnutrition. I do not know what message from this study will be useful to the nutrition researchers or program-implementers. There are many other variables collected in DHS surveys that could have used (example: mothers age, mothers' nutritional status, number of siblings, exposure to media etc). I think these should be adjusted. Some typos need to be corrected. Example: Page 10, line 13, area of residence?

Table 1. age category should be redefined (rather than 2 year, 3 year, 4 year, 1-2 year, 2-3 years etc should be used).

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
This is a mainly descriptive paper that measures the proportion of child of who are underweight and overweight in a selection of countries in South Asia (Bangladesh, India, Pakistan, Maldives, and Nepal). The data used are from the Demographic and Health Surveys. I have the following comments.
Major Comments
1. Understanding the prevalence of being underweight among children is certainly important from a policy perspective, as is identifying the prevalence of overweight among children. For the reasons the authors outline, it is also important to identify those countries and locations that experience high rates of both of these at the same time. Therefore, the motivation for focusing on the topic presented seems clear.
However, I wonder if the analysis warrants a research article of this length. The analysis is very descriptive, and does not go beyond providing regression-adjusted estimates of prevalence by group (e.g. household education). I would not say this is an issue in every context, often this type of analysis can be very informative and useful, but in this case the data presented seem to be more suited to a report rather than a research article. For instance, it appears to be possible to obtain estimates of the prevalence of both underweight and overweight among children, including by country and group, directly from the DHS (https://www.statcompiler.com/en/).
I am not sure how the authors could draw out their contribution better than it is currently represented. Instead, my suggestion would be to either substantially shorten the paper, or else consider some additional analysis that could help further our understanding of the double burden issue. Perhaps one of the recent review papers on this subject could be useful for pointing to what this could be, e.g. : Haddad, L., Cameron, L., Barnett, I., 2015. The double burden of malnutrition in SE Asia and the Pacific: priorities, policies and politics. Health Policy Plan 30, 1193–1206.

Our response: We understand the reviewer's concern regarding the value added by our paper but with the latest revision, our paper now provides novel analyses not only on the associations between socioeconomic status and childhood nutritional outcomes but also on other factors that can influence such outcomes (i.e. additional analysis as the reviewer suggested). It also shows cross-country comparisons on such analyses. It is true that DHS website provides prevalence by socioeconomic group for every country. But our paper shows the associations after adjustment for a wide range of child, household and maternal factors that could modify these associations. We used the IOTF classification for defining overweight in this age group, but the DHS uses WHO cut-offs, so this can

also be considered as an added value of the study. So, we are confident that this paper offers enough value, originality and relevance to be published in BMJ Open.

2. There are many typos and grammatical errors in the paper (e.g. the first sentence), it needs to be thoroughly checked.
Our response: We have taken much care of this issue in this revision.

3. There is a lot of repetition in the discussion section.
Our response: We have almost re-written the discussion in this revision and much care about not to repeat things has been taken.

4. The use of the IOTF cut-off requires further justification, ideally with evidence on robustness of results to alternative definitions, and discussion of the potential limitations of cross-country comparisons.
Our response: We have added this information in the discussion section. The main text now reads: "We used the IOTF reference to define childhood overweight instead of the WHO or Centers for Disease Control (CDC) references [22,23,25]. The IOTF classification system is based on large datasets from six regions covering different ethnicities, therefore more suitable for international comparisons [23,25]. When compared with other references, the IOTF reference yielded similar estimates for overall overweight prevalence but different estimates for obesity [41,42]. It was also found to be more specific in identifying children with overweight and obesity than other references [43]." (Page 19 lines 4-11)

Reviewer: 2

Abstract
-	Which are the 'latest' years? (line 7, p2)
-	Define 'ORs' (line 20, p2).
-	'richest vs poorest households' (line 20, p2). The unit of analysis is children or households?
-	'household education level'. What does this mean? Education of the mother, father, household head? (line 1, p3)
-	Revise 'Conclusions'. It is not clear in what sense the associations between SES and underweight 'were consistent' (line 4-5); 'tackle double burden'? (line 8, p3)

Our response: The abstract has been re-written, and the mentioned issues has been dealt with. The unit of analysis is children here. Operational definition of household education level is given in the method section (page 9, lines 15-16)

Strengths and limitations of this study
-	'information on dietary and lifestyle factors that could modify those 13 associations.' (lines 12-13, p4). Information on dietary is available in DHS surveys, perhaps not in all survey waves, but the authors can take a closer look at the information available in each country.

Our response: We have added some new variables into our analysis and also looked at their associations with the nutritional outcomes. Although some of the DHS data has dietary information for example, complementary feeding, breast feeding etc., they were either missing for children aged 24-59 months or had lots of missing values. Therefore, we did not include them in our analysis.

Intro
The main problem of this section is the motivation behind the 'need to understand the socioeconomic inequalities in nutritional outcomes' (lines 6-7, p6). It is not clearly stated in my opinion. I also do not see a clear hypothesis about trying to explain the coexistence of the two burdens of malnutrition.

- 'overnutrition (overweight or obesity) within individuals' (line 3, p5). What does 'within individuals' mean?
- I do not understand the following phrase 'While South Asian countries have highest numbers of underweight children due to higher prevalence rates and large populations in younger age groups' (lines14-16, p5).
- 'South Asians children living in developed countries also have a much higher prevalence of overweight than any other ethnic groups - a recent study suggests'. Why this is relevant? Are the authors suggesting that South Asian children are predisposed to be overweight?
- 'using the latest nationally-representative surveys' (line 16, p6). Which surveys?

Our response: We have re-written the introduction to state the rationale of the study more clearly. We have rephrased the lines about double burden of malnutrition at the individual level. The confusing line about the number of underweight children in South Asia has been removed. We agree that the line about South Asian children living in developed country is kind of irrelevant, so we deleted the sentence. We added a paragraph about DHS survey in the introduction section to explain which surveys were used in this study. We also revised the aim of the study.

Methods
The main issue of this section is that stunting, and wasting were not included as indicators of undernutrition. This requires further explanation based on previous evidence.

Our response: As mentioned in the earlier version, underweight is a composite definition which can encompass stunting, wasting, or both. Our aim in this study is to understand the overall burden of undernutrition, so we think use of underweight as an indicator of undernutrition is justified here. However, we understand the reviewer's concerns about double counting of children due to children who may be wasted/stunted and overweight. While we acknowledge the concerns, we think this specific issue is out of scope of this study. However, we have added this issue as a potential limitation of this study in the discussion section. (page 19, lines 13-17)

- 'DHS' (line 3, p7). 'Demographic and Health Surveys' if this is the first mention.
Our response: Updated accordingly
- 'Year of survey for each included country is given in Table 1' (line 7, p7). The corresponding years for each country should be added in the main text.
Our response: Updated accordingly
- The authors provide many details about the characteristics and structure of DHS data (e.g., lines 5-8, p 8). The authors can summarize and cite relevant sources.
Our response: We kept the part because we think it will give the readers who are not well-acquainted with DHS data a better understanding of the data used in this study.
- 'We excluded children aged less than 24 months because there is no available classification system for defining overweight for them'. A citation is needed here.
Our response: Updated accordingly

- Underweight is a composite definition which can encompass stunting, wasting, or both (lines 1-2, p9). The authors are correct in that underweight is a compositive measure of wasting and stunting. However, this does not seem to justify their exclusion from the analysis in my opinion. Particularly stunting, which reflect chronic status of undernutrition, it is related to environmental and socioeconomic circumstances, and may have lasting consequences. It seems to be relevant for this study. In addition, previous studies have found that stunting and overweight can occur concurrently (see Bates et al. 2017, JECH). Ultimately, what can we say of children who may be wasted/stunted and overweight, but not underweight?
Our response: We explained this issue in a previous comment.

- What does it mean 'Household's highest education level'? (Table 1). Is that the education of the mother, father, household head or other hh member? Please clarify.

Our response: We have updated the operational definitions of the variables used in this study.

- Important predictors of undernutrition have been omitted in this analysis (e.g., mother's height, poor dietary intake, mother's BMI, household air quality, sanitation).

Our response: We have added factors like maternal age at first birth, maternal BMI, access to improved sanitation and drinking water in our current analysis. However, some variables mentioned by the reviewer here are not available in all dataset and to keep consistently across countries, we did not include them.

- 'we applied Stata's survey estimation procedures ("svy" command) for regression analyses' (line 15-16, p10). The 'svy' need to be used for the estimation of prevalence, proportions, means too. Survey weights are not enough to estimate the precision of those indicators.

Our response: Updated accordingly. The estimates did not vary in the updated results.

Results

Some statements are difficult to follow or understand in this section. There are also issues with interpretation of results and consistency with the statements in the abstract.

- 'Pakistan has significant' (line 9, p11). 'significant' may not be used in this context.
- 'the better part of burden for malnutrition in all countries was due to undernutrition (Figure 1)'. I do not understand what 'the better part of burden for undernutrition' means.
- 'Pakistan and Maldives had much higher prevalence, 7% and 9% respectively.' (line 19, p11). In the abstract this difference was characterized as a 'slightly higher prevalence'.

Our response: We have updated the results section almost completely and taken care of the issues mentioned here.

Discussion

- The authors cite relevant work from the region that describe important determinants for undernutrition (lines 20-23, p14), but the authors did not include those determinants in their own analysis (e.g., initiation of complementary feeding). It would be important to control for other factors and see if the results stand.

Our response: We have added some new variables into our analysis and also looked at their associations with the nutritional outcomes. Although some of the DHS data has dietary information for example, complementary feeding, breast feeding etc., they were either missing for children aged 24-59 months or had lots of missing values. Therefore, we did not include them in our analysis.

- In addition, the author did not discuss how this double burden has evolved across time. Many publications in the region should be available presenting trends of undernutrition and overweight separately. The authors can also estimate trend using DHS surveys from previous years.

Our response: We added a line on how the prevalence of underweight has declined over time. The main text reads now "Trends in the prevalence of childhood underweight have been declining in these countries, with almost 25-30% reduction between 2004 and 2014 in Bangladesh, India, Pakistan, and Nepal [31]." (page 16, lines 20-23)

However, the prevalence estimates for overweight in children of this age group are limited. DHS survey reports do not report the prevalence of overweight in this age group.

- Finally, the authors omit to discuss the existence and effectiveness of policies/programs trying to address both malnutrition problems. Is there evidence on this regard from other countries, or policies that can address both problems simultaneously?

Our response: We have added this line in the discussion – "Previous studies suggested that a multi-sectoral approach is needed to alleviate poverty and other social inequalities related to the double burden of malnutrition in South Asia and beyond [40]." (page 18, lines 17-19)

Other minor comments
Proof reading is needed. There are several grammatical issues along the text
e.g., 'implies to the presence' (line 2, p5)
'South Asian countries have highest numbers' (line 15, p5)
'South Asians children' (line 17, p5)
'are of residence' (line 13, p10)
'in compared to' (line 21, p11; line 19, p12)
And others, this list is not exhaustive.

Our response: These issues have been taken care of.

Reviewer: 3

This paper presents analysis of DHS survey data of five South Asian countries to explore the double burden of malnutrition among young children by household's socioeconomic status. These countries housed the highest burden of childhood undernutrition in the world. However, following the recent socio-economic transitions, double burden of malnutrition becomes evident in these countries with slower reduction rate of undernutrition with a faster increasing rate of overnutrition. This is very important topic and the paper is well-written.

The authors have mentioned that this is the first study to explore double burden in South Asian countries using nationally representative sample size. Double burden of malnutrition can occur at individual level, household level and even at population level. The authors have used the population level data to explore prevalence of over nutrition and under nutrition among children 24 months to 60 months of age. However, there is a recent publication using the same DHS 2014 data of Bangladesh to explore double burden at household level (Das S, PHN 2019). Although Das et al explored the coexistence of overweight or obese mother and undernourished children.

Our response: We have now cited the above-mentioned paper. As the reviewer said, the aim of our study is different from this study and our study is the first study to look at the issue of double burden of malnutrition particularly in under-five children.

However, the findings were nothing new or interesting. Except for wealth index and education level there were no other variables that were used to explain the double burden of malnutrition. I do not know what message from this study will be useful to the nutrition researchers or program-implementers. There are many other variables collected in DHS surveys that could have used (example: mothers age, mothers' nutritional status, number of siblings, exposure to media etc). I think these should be adjusted.

Our response: We understand the reviewer's concern regarding the value added by our paper but with the latest revision, our paper now provides novel analyses not only on the associations between socioeconomic status and childhood nutritional outcomes but also on other factors that can influence such outcomes (i.e. additional analysis as the reviewer 1 suggested). It also shows cross-country comparisons on such analyses. In this version, we have newly adjusted for a wide range of factors (for example, maternal age at first birth, no. of household members, maternal BMI, access to

improved sanitation and drinking water) that could modify the observed associations. We also examined their individual associations with the outcomes. So, we are confident that this paper offers enough value, originality and relevance to be published in BMJ Open.

Some typos need to be corrected. Example: Page 10, line 13, area of residence?
Table 1. age category should be redefined (rather than 2 year, 3 year, 4 year, 1-2 year, 2-3 years etc should be used).
Our response: Updated accordingly

VERSION 2 – REVIEW

REVIEWER	Mark McGovern Rutgers University, USA
REVIEW RETURNED	30-Oct-2019

GENERAL COMMENTS	Review of bmjopen-2019-032866.R1
	Double burden of malnutrition in children aged 24-59 months by socioeconomic status in five South Asian countries: evidence from Demographic and Health Surveys
	October 29th 2019
	Comments to authors
	I am grateful to the authors for their revisions, in terms of the writing and structure the paper has improved substantially. I have no major concerns with the analysis. However, my view about the contribution of the paper has not changed. I appreciate that the authors have made some modifications to their analysis, but from my perspective they are minimal and this submission still would be better suited to a short descriptive report. Having said that, I recognize that others may disagree with this view. If the paper is to be published I have the following comments:
	1. Information on missing data should be reported. For example, how many children were not measured for height and/or weight? How many were excluded because of implausible measurements? What was the extent of missing data for covariates? The STROBE guidelines should be followed here, for instance giving a more complete description of the sample size and excluded observations at each stage of data preparation and analysis. Where rates of missing data are high the potential implications of this should be discussed in the text.
	2. Again following the STROBE guidelines, the title should include information about the study design. I suggest adding "observational" or similar.
	3. Table 1 – I would try to keep the column heading "Bangladesh" on one line.
	4. Like with the graphs, notes to tables should also state when CIs were adjusted to account for survey design, and/or data were weighted. In the note to the graphs, "Sampling weight" should be plural.

| | 5. Figures 1-3: I would try not to have the CI bars covering the numbers on the graph, and I would also consider changing the colors so they are more suited to viewing in black and white.

6. A small number of typos remain, for example:

P8, line 14: Is this a subsection heading? Should it be bolded?
P12, line 8: "A" or "The" majority?
P12, line 11: "except"
P14, line22: "households are less" |
|---|---|

| REVIEWER | Ivan Mejia
Stanford University |
|---|---|
| REVIEW RETURNED | 22-Oct-2019 |

| GENERAL COMMENTS | The article has improved substantially. The authors have addressed previous comments adequately regarding the inclusion of new predictors of undernutrition, as well as the readability of the manuscript. Perhaps the only remaining issues on this specific regard are:

- Explain why maternal height was excluded from the analysis. See, for example, the work from Kim et al. 2017 (Social Science & Medicine), a study of stunting for Afghanistan, Bangladesh, India, Nepal, and Pakistan showed that maternal height was a very important predictor of child stunting after controlling for 13 different covariates.
- The authors included 'No. of household member' and 'No. of children under five' as predictors in the household in the same model. I wonder if they tested for collinearly, as they may be correlated.

However, I believe the response regarding the inclusion of stunting/wasting was not addressed adequately and their assessment of undernutrition remains incomplete. The authors are right that underweight is a composite measure of stunting and wasting, but that also represents a disadvantage of this indicator because it is difficult to interpret. See, for example, trends from three indicators of undernutrition (stunting, wasting, underweight) in India and two Indian states (Maharashtra and Tripura) between 2005-6 (from NFHS-3) and 2015-16 (from NFHS-4) that I show in the table below. Different conclusions can be made depending on the indicator selected and in different contexts (e.g., in Maharashtra, we observe a very important improvement in chronic undernutrition as measured by stunting, an important increase in the prevalence of acute undernutrition measured by wasting, but underweight would indicate nearly no improvement – the story is quite different in India and Tripura). Stunting and wasting are very different constructs of undernutrition and reflect very distinct states of child undernutrition and underlying factors that underweight does not fully capture. (for more details see also the paragraph below from 'Nutrition Landscape Information System (NLIS) country profile indicators: interpretation guide', WHO 2010, available online at: https://www.who.int/nutrition/nlis_interpretation_guide.pdf).

	India		Maharashtra		Tripura	
Indicator	2005	2015	2005	2015	2005	2015
Stunting	48	38	46	36	36	24
Wasting	20	21	17	26	25	17
Underweight	43	36	37	36	40	24

Source: DHS reports NFHS-4. |
|---|---|

> "The percentage of children with a low height for age (stunting) reflects the cumulative effects of undernutrition and infections since and even before birth. This measure can therefore be interpreted as an indication of poor environmental conditions or long-term restriction of a child's growth potential. The percentage of children who have low weight for age (underweight) can reflect 'wasting' (i.e. low weight for height), indicating acute weight loss, 'stunting', or both. Thus, 'underweight' is a composite indicator and may therefore be difficult to interpret."

VERSION 2 – AUTHOR RESPONSE

Reviewer: 2

Reviewer Name: Ivan Mejia

The article has improved substantially. The authors have addressed previous comments adequately regarding the inclusion of new predictors of undernutrition, as well as the readability of the manuscript. Perhaps the only remaining issues on this specific regard are:

- Explain why maternal height was excluded from the analysis. See, for example, the work from Kim et al. 2017 (Social Science & Medicine), a study of stunting for Afghanistan, Bangladesh, India, Nepal, and Pakistan showed that maternal height was a very important predictor of child stunting after controlling for 13 different covariates.

Our response: Thank you for pointing out this. We have added maternal height in our analyses.

- The authors included 'No. of household member' and 'No. of children under five' as predictors in the household in the same model. I wonder if they tested for collinearly, as they may be correlated.

Our response: We have excluded no. of household member in the analyses and included only no. of children under five (because we think this is a more important predictor than no. of household members).

However, I believe the response regarding the inclusion of stunting/wasting was not addressed adequately and their assessment of undernutrition remains incomplete. The authors are right that underweight is a composite measure of stunting and wasting, but that also represents a disadvantage of this indicator because it is difficult to interpret. See, for example, trends from three indicators of undernutrition (stunting, wasting, underweight) in India and two Indian states (Maharashtra and Tripura) between 2005-6 (from NFHS-3) and 2015-16 (from NFHS-4) that I show in the table below. Different conclusions can be made depending on the indicator selected and in different contexts (e.g., in Maharashtra, we observe a very important improvement in chronic undernutrition as measured by stunting, an important increase in the prevalence of acute undernutrition measured by wasting, but underweight would indicate nearly no improvement – the story is quite different in India and Tripura). Stunting and wasting are very different constructs of undernutrition and reflect very distinct states of child undernutrition and underlying factors that underweight does not fully capture. (for more details see also the paragraph below from 'Nutrition Landscape Information System (NLIS) country profile indicators: interpretation guide', WHO 2010, available online at: https://www.who.int/nutrition/nlis_interpretation_guide.pdf).

India   Maharashtra   Tripura

Indicator	2005	2015	2005	2015	2005	2015
Stunting	48	38	46	36	36	24
Wasting	20	21	17	26	25	17
Underweight	43	36	37	36	40	24

Source: DHS reports NFHS-4.

"The percentage of children with a low height for age (stunting) reflects the cumulative effects of undernutrition and infections since and even before birth. This measure can therefore be interpreted as an indication of poor environmental conditions or long-term restriction of a child's growth potential. The percentage of children who have low weight for age (underweight) can reflect 'wasting' (i.e. low weight for height), indicating acute weight loss, 'stunting', or both. Thus, 'underweight' is a composite indicator and may therefore be difficult to interpret."

Our response: We have conducted additional analyses for childhood stunting and wasting. Results from these analyses are mostly presented as supplements, while relevant information is added in different sections of the main text, where appropriate.

Reviewer: 1

Reviewer Name: Mark McGovern

I am grateful to the authors for their revisions, in terms of the writing and structure the paper has improved substantially. I have no major concerns with the analysis. However, my view about the contribution of the paper has not changed. I appreciate that the authors have made some modifications to their analysis, but from my perspective they are minimal and this submission still would be better suited to a short descriptive report. Having said that, I recognize that others may disagree with this view.

Our response: Thank you for your comment about how the paper was improved in the last revision. We tried our best to improve the scope of the paper but sorry to hear that you are still not convinced. However, in the current revision, we added analysis on stunting and wasting too (as the other reviewer suggested), which we think improved the contribution of the paper further.

If the paper is to be published I have the following comments:

1. Information on missing data should be reported. For example, how many children were not measured for height and/or weight? How many were excluded because of implausible measurements? What was the extent of missing data for covariates? The STROBE guidelines should be followed here, for instance giving a more complete description of the sample size and excluded observations at each stage of data preparation and analysis. Where rates of missing data are high the potential implications of this should be discussed in the text.

Our response: We added a flowchart of study participants included in the study as a supplementary figure. The flowchart shows exclusions of children at each stage of data preparation and analysis.

Regarding the missing data for covariates, we put percentage of missing value in each variable as a footnote to Table 1. It says – "There was less than 1% missing value for variables: received vitamin A in last 6 months, received deworming drug in last 6 months, mother's height, and mother's BMI in all countries except Maldives. For Maldives, there were around 5% missing values in mother's height and mother's BMI. There was no missing value in other variables listed in this table."

To explain how missing data were dealt in the analysis, we also added this statement to the method section – "Missing data in the adjustment variables (usually less than 5%) were considered as separate categories so that the same children were compared in all analyses."

2. Again following the STROBE guidelines, the title should include information about the study design. I suggest adding "observational" or similar.

Our response: The current title has the word "surveys" which we think is sufficient to inform the study design.

3. Table 1 – I would try to keep the column heading "Bangladesh" on one line.

Our response: Updated accordingly

4. Like with the graphs, notes to tables should also state when CIs were adjusted to account for survey design, and/or data were weighted. In the note to the graphs, "Sampling weight" should be plural.

Our response: This statement has been added to the footnote of table 2 and table 3 – "Analyses were conducted using sampling weights provided by the Demographic and Health Survey (DHS) and Stata's survey estimation procedures."

5. Figures 1-3: I would try not to have the CI bars covering the numbers on the graph, and I would also consider changing the colors so they are more suited to viewing in black and white.

Our response: We updated all graphs to black and white as per suggestion. Since this paper is about double burden of malnutrition, we now change the graphs to stacked columns to show the prevalence of underweight and overweight together. To avoid overlapping of confidence intervals (CIs), we put all the prevalence estimates with corresponding CIs in supplementary tables. We also added an extra statement to the figure legends saying – "Corresponding 95% confidence intervals of prevalence estimates are given in Supplementary Table S2."

6. A small number of typos remain, for example:

P8, line 14: Is this a subsection heading? Should it be bolded?

P12, line 8: "A" or "The" majority?

P12, line 11: "except"

P14, line22: "households are less"

Our response: Corrected in the current revision

VERSION 3 – REVIEW

REVIEWER	Ivan Mejia Stanford University / USA
REVIEW RETURNED	07-Feb-2020

GENERAL COMMENTS	The authors have addressed my previous comments adequately. A few minor issues remain before being considered for publication. Please see below.

| | Please revise the following sentences/phrases and modify if necessary. The sound odd or incomplete.
Abstract, line 18: Erase 'As expected,' (people who are not familiar with this topic may not find this fact obvious).
Page 5, line 24: 'Understanding the socioeconomic inequalities in nutritional outcomes is essential.' Essential for what?
Page 6, lines 10-11: "studying both outcomes together in a population will be more useful to the relevant stakeholders." More useful for what?
P 6, 24: "taking account."
P 9, l 23: "those taller than this" --> "taller children".
P 16, line 2: "households less likely" --> households are less likely
P19, lines 6-10: The authors may tone down the following statements or indicate that their results are in line with previous studies.
"Our study also showed that factors like low maternal height and maternal underweight could significantly increase the likelihood of undernutrition in children, while other factors like older age of mother at birth, and access to improved sanitation were also associated with lower odds of childhood underweight"

Page 19, line 14: The following statement of the authors seems to be incomplete:
"To have a better insight on the assessment of childhood undernutrition, we additionally explored the burden and the underlying factors of childhood stunting and wasting."
In what sense it was a better assessment or what they conclude from this additional analysis?

Table 3: Results in this table correspond to fully-adjusted models or just included the factors shown in the table? Please clarify.
Table 1: Mother's age at first birth, n(%). The fist category reads "Less than 250 years" |

VERSION 3 – AUTHOR RESPONSE

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Response to reviewer's comments:
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The authors have addressed my previous comments adequately. A few minor issues remain before being considered for publication. Please see below.

Please revise the following sentences/phrases and modify if necessary. The sound odd or incomplete.

Abstract, line 18: Erase 'As expected,' (people who are not familiar with this topic may not find this fact obvious).

Our response: Deleted "As expected"

Page 5, line 24: 'Understanding the socioeconomic inequalities in nutritional outcomes is essential.' Essential for what?

Our response: Edited. The text now reads - "Understanding the socioeconomic inequalities in nutritional outcomes in LMICs is essential to seize programme and policy opportunities to address malnutrition in both forms."

Page 6, lines 10-11: "studying both outcomes together in a population will be more useful to the relevant stakeholders." More useful for what?

Our response: Edited. The text now reads - "...studying both outcomes together in a population can inform the relevant stakeholders on seizing intervention and policy opportunities to tackle childhood malnutrition in more holistic ways."

P 6, 24: "taking account."

Our response: Deleted by before taking account.

P 9, l 23: "those taller than this" --> "taller children".

Our response: Edited accordingly.

P 16, line 2: "households less likely" --> households arless likely

Our response: Corrected.

P19, lines 6-10: The authors may tone down the following statements or indicate that their results are in line with previous studies.

"Our study also showed that factors like low maternal height and maternal underweight could significantly increase the likelihood of undernutrition in children, while other factors like older age of mother at birth, and access to improved sanitation were also associated with lower odds of childhood underweight"

Our response: Edited. The text now reads - "In line with previous studies, our study also showed that factors like low maternal height and maternal underweight could significantly increase the likelihood of undernutrition in children, while other factors like older age of mother at birth, and access to improved sanitation were also associated with lower odds of childhood underweight."

Page 19, line 14: The following statement of the authors seems to be incomplete:
"To have a better insight on the assessment of childhood undernutrition, we additionally explored the burden and the underlying factors of childhood stunting and wasting."
In what sense it was a better assessment or what they conclude from this additional analysis?

Our response: We have added this line to the text: "These additional analyses showed that although the burden of childhood undernutrition varied widely by the indicator of interest, the determinants of childhood undernutrition were similar."

Table 3: Results in this table correspond to fully-adjusted models or just included the factors shown in the table? Please clarify.

Our response: The results in this table are adjusted for socioeconomic variables only (i.e. household's wealth index and highest education level), mainly because we wanted to see the socioeconomic

status-adjusted assocations between other factors and double burden of malnutrition. The adjustment profile is given as a footnote to the table.

Table 1: Mother's age at first birth, n(%). The fist category reads "Less than 250 years"

Our response: Corrected.