

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Gender-specific prevalence and associated factors of hypertension among elderly Bangladeshi people: findings from a nationally representative cross-sectional survey
AUTHORS	Hanif, Abu Abdullah Mohammad; Shamim, Abu Ahmed; Hossain, Md Mokbul; Hasan, Mehedi; Khan, Md Showkat Ali; Hossaine, Moyazzam; Ullah, Mohammad Aman; Sarker, Samir Kanti; Rahman, S M Mustafizur; Mitra, Dipak; Mridha, Malay Kanti

VERSION 1 – REVIEW

REVIEWER	Michihiro Satoh Tohoku Medical and Pharmaceutical University
REVIEW RETURNED	06-Apr-2020

GENERAL COMMENTS	<p>Thank you for giving me the opportunity to review this article. This study indicated that the prevalence and associated factors of hypertension in the Bangladesh population. They indicated the higher prevalence rate of hypertension than before, therefore, they emphasized public health policy for the prevention of hypertension is immediately needed. Their findings are meaningful to know the situation of hypertension management in Bangladesh. However, I have concerns about their results and discussion as indicated below.</p> <p>Major comments</p> <ol style="list-style-type: none">1. My main concern is on the results regarding the smoking status and low education status. In general, smoking status and low education levels are positively associated with a higher prevalence rate of hypertension while their results are completely opposite. Although they explain the reason in the discussion, it is unsuitable to show these results without any detailed information. I believe that several strong confounding factors affected their results despite the statistical adjustments. I suggest they perform stratification analyses according to sex, age, and other factors.2. Why was age included as a categorical variable? Age is a very strong factor of hypertension; it should be included as a continuous variable.3. In relation to “Our reported prevalence is higher than the prevalence estimated in Bangladesh demographic and health survey 2011, which was 35% and 40% for the age groups 60-69 and 70+ years, respectively” in the discussion section, was the study design of this previous study the same to the present study? In the introduction, the prevalence rate of hypertension in those aged ≥ 60 was 40% in the 2011 survey; why was the prevalence rate different from that in the discussion?4. They mentioned that “the increase in the prevalence of hypertension may be due to recent advancements in the economy
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	<p>and infrastructure of the country, along with rapid urbanization, sedentary lifestyles, and stress". However, their results do not support this. for example, living in an urban city was not significantly associated with hypertension.</p> <p>5. It is important to compare their findings with prevalence rates in other countries. In the U.S, Europe, and Japan, the higher prevalence rates have been observed.</p> <p>6. They better mention the short discussion regarding the factors associated with hypertension in the conclusion, in both of the abstract and the main text.</p> <p>7. "insufficient physical activity and obesity" are well-known factors of hypertension. A stronger novel point is needed.</p> <p>Minor comments</p> <p>8. I cannot understand how they calculate "design effect (DEF) = 1.61".</p> <p>9. The authors should show the results of multivariable analysis, in which BMI is included instead of waist circumference.</p> <p>10. Does adjusted odds ratios mean odds ratios from the model including all variables shown the table?</p> <p>11. Results regarding Table 1 can be shortened since we can understand them by reading Table 1.</p> <p>12. Instead of Table 2 (probably also Table 1), the characteristics stratified according to sex as well as hypertension status should be shown.</p> <p>13. What is SSC?</p>
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REVIEWER	Muhammad A.B. Chowdhury University of Florida
REVIEW RETURNED	08-Apr-2020

GENERAL COMMENTS	This is a well-executed study and well written manuscript that addresses a significant issue by the author's analyzed nationally representative household survey data. The potential for inferences is clearly consistent and the findings can be relevant in the perspective of national public health.
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REVIEWER	Shaun Scholes University College London, United Kingdom.
REVIEW RETURNED	24-Jun-2020

GENERAL COMMENTS	<p>I was asked just to comment on the statistical aspects of the study. My comments are:</p> <p>Abstract: Please report reference categories where not obvious (e.g. central obesity versus whom? Those with normal weight?).</p> <p>Introduction: The authors quote various hypertension statistics in the 3rd paragraph. Prevalence levels are affected by the definitions: therefore, the authors should define each term precisely as used in the study cited (e.g. define hypertension prevalence; controlled hypertension; undiagnosed hypertension).</p> <p>Sampling: Describe the procedure used to randomly select 1 person per household.</p> <p>Methods: Was there a time frame regarding when participants were told they were hypertensive (e.g. last year)? It is better to state the actual survey question used.</p>
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	<p>The authors give no detail about the weighting. Did the weight reflect just the sampling design or was there an additional adjustment for non-response (e.g. post-stratification?). There is also no mention of accounting for any geographical clustering of participants in the calculation of sampling variability (e.g. p-values and 95% CI). The authors need to specify that all appropriate features of the survey design were accounted for in the analysis. Define the risk factors in a legend to Figure 2. The authors state that risk factor prevalence was higher in females versus males. Table 1 contains p-values: but no mention was given in the methods section that the authors were conducting such statistical tests (e.g. which statistical tests were used and was account made for the complex survey design). The same point applies to Table 2. The footnote implies a significance test for gender differences in the prevalence of hypertension: the estimate for females is clearly higher than that for males (non-overlapping CIs) but there is no indication of this in the table.</p> <p>The authors report an “unexpected” finding that hypertension was lower among current smokers. As reported in our paper: https://academic.oup.com/ije/article/47/3/860/4831068 smokers may have been advised to quit smoking because they were hypertensive.</p> <p>Please include the reference category when discussing results (e.g. education up to SSC versus no formal education). Describing results as lower odds of hypertension is preferable to using terms such as negative association.</p> <p>Finally, could the authors discuss the issue of treated and controlled hypertension. Is there an assumption that participants with controlled hypertension (e.g. BP <140/90) due to medication use would be classed as hypertensive in this study due to being told that they were hypertensive by a trained health care provider? Did the survey ask about medication use? Could you use this information as well to shed light on factors associated with management of hypertension?</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Michihiro Satoh

Institution and Country: Tohoku Medical and Pharmaceutical University

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below:

Thank you for giving me the opportunity to review this article. This study indicated that the prevalence and associated factors of hypertension in the Bangladesh population. They indicated that the higher prevalence rate of hypertension than before, therefore, they emphasized public health policy for the prevention of hypertension is immediately needed. Their findings are meaningful to know the situation of hypertension management in Bangladesh. However, I have concerns about their results and discussion as indicated below.

Response: Thank you so much for your kind words! Please, see our response to your comments and suggestions.

Major comments

1. My main concern is on the results regarding the smoking status and low education status. In

general, smoking status and low education levels are positively associated with a higher prevalence rate of hypertension while their results are completely opposite. Although they explain the reason in the discussion, it is unsuitable to show these results without any detailed information. I believe that several strong confounding factors affected their results despite the statistical adjustments. I suggest they perform stratification analyses according to sex, age, and other factors.

Response: We revisited the variables "smoking" and "smokeless tobacco," and we think that as we have data only on the current smoking and smokeless tobacco consumption status, it does not give us enough scope of explaining the association of these two variables with hypertension among elderly people. Moreover, the tobacco industry lobby can misguide the people using this conclusion based on incomplete information. Considering the above scenario and the review comment, we have dropped the variables smoking and smokeless tobacco from our revised analysis. In the future round of the survey, we will work on collecting further details on smoking and smokeless tobacco.

Regarding the education status and hypertension, while it is true for the high-income countries that low education status is positively associated with a higher prevalence of hypertension, the situation is often reverse in the low-income countries (Al Kibria, Burrowes, Choudhury, Sharmeen, & Swasey, 2019). In Bangladesh, people with higher education status are most likely in the higher income quintiles, and they lead a sedentary lifestyle, go through stressful life, and eat more junk food compared to the people from low education status or in the lower socioeconomic tier. Therefore, we would like to report this association.

In terms of stratified analysis, we already performed separate analyses for both males and females. However, as per advice from the reviewer, we also conducted an analysis, including age, and this did not change the association between education and hypertension.

2. Why was age included as a categorical variable? Age is a very strong factor of hypertension; it should be included as a continuous variable.

Response: Based on the review comments, we also performed bivariate and multivariable logistic regression with age as a continuous variable and for both sexes and found that odds of having hypertension significantly increased as age increased. Below is the result of bivariate and multivariable logistic regression. [we tried to paste the table here but it was broken. Please, see the table in the word file attached herewith. Sorry for the inconvenience]

However, we used the categorized variables for age in the analysis to facilitate action by the policymakers as the policies are taken based on different age groups for prioritizing screening as well as management of hypertension. If we use age as a continuous variable in the analysis, it might be difficult for policymakers to interpret the results also, as our target population is elderly people. The age variable is not normally distributed, and it is highly skewed to the right, even after log transformation, which also encouraged us to categorize the variable.

3. In relation to "Our reported prevalence is higher than the prevalence estimated in Bangladesh demographic and health survey 2011, which was 35% and 40% for the age groups 60-69 and 70+ years, respectively" in the discussion section, was the study design of this previous study the same to the present study?

Response: The study design was not exactly the same. There were some similarities and dissimilarities. Also, the time frame for the studies was different. Data of BDHS 2011 in Bangladesh was collected in 2011, and we collected data between 2018-19. Both BHDS 2011 and the one we conducted were nationally representative, but our study was also representative in terms of the divisions (entire Bangladesh is divided into 8 administrative divisions). In the BDHS 2011, primary

sampling units were selected based on the enumeration units created by the Bangladesh Bureau of Statistics (BBS) for a 10-yearly national census. But in our survey, we created primary sampling units dividing the selected unions (the smallest administrative of Bangladesh) into several segments with at least 250 households and the randomly selected 2 clusters for each selected union from those segments. Finally, in our study, sampling for the elderly population was done independently as a separate age group while in BDHS, elderly people were included as a general adult age group.

In the introduction, the prevalence rate of hypertension in those aged ≥ 60 was 40% in the 2011 survey; why was the prevalence rate different from that in the discussion?

Response: The study mentioned in this sentence was different from the Bangladesh Demographic and Health Survey 2011 (Khanam et al., 2015). However, we deleted the sentence as it may create confusion.

4. They mentioned that "the increase in the prevalence of hypertension may be due to recent advancements in the economy and infrastructure of the country, along with rapid urbanization, sedentary lifestyles, and stress." However, their results do not support this. For example, living in an urban city was not significantly associated with hypertension.

Response: Although the prevalence of hypertension was higher in a non-slum urban area compared to the rural area. But based on the reviewer comments, we revised the text as it was not significant in multivariable logistic regression. Revised texts are as follows –

"The increase in the prevalence of hypertension may be due to recent advancements in the economy and infrastructure of the country, leading the people into sedentary lifestyles and stress."

5. It is important to compare their findings with prevalence rates in other countries. In the U.S, Europe, and Japan, the higher prevalence rates have been observed.

Response: We compared the findings with the South-Asian countries as Bangladesh is a South-Asian country and also, with the Low- and Middle-Income countries, rather than comparing with developed countries. However, as per the reviewer's suggestion, we added a comparison with the prevalence of hypertension in high-income countries:

"The prevalence is still lower than the prevalence of hypertension in high-income countries. In a systematic analysis, Katherine et al. demonstrated that, in 2010, the prevalence of hypertension among the high-income countries was 60.8% (male: 55.3%; female: 60.9%) and 73.6% (male: 65.6%; female: 77.5%) in the age groups 60-69 years and 70+ years, respectively".

Please, see the first paragraph of the discussion section.

6. They better mention the short discussion regarding the factors associated with hypertension in the conclusion, in both of the abstract and the main text.

Response: Noted with thanks. We had to keep the abstract within a word limit of 300 words. But we modified the conclusion section of the main text according to the suggestion from the reviewer. The texts of the modified conclusion are as follows –

"As per the findings of our study, about half of the Bangladeshi elderlies were hypertensive, and hypertension was more prevalent among elderly females in terms of socio-demographic, behavioral, and biological characteristics. Extreme old age (≥ 70 years), education above 10th grade, insufficient physical activity, abdominal obesity (higher waist circumference), and self-reported diabetes was associated with increased odds of hypertension in Bangladeshi elderly population. Additionally, living in slums had lower odds of hypertension among elderly males. The government of Bangladesh should take a multisectoral approach involving health, economic, education, and social welfare sectors to promote healthy lifestyles among the elderly people and their families and provide emphasis on early

diagnosis and treatment to prevent complications of hypertension among the elderly population."

7. "insufficient physical activity and obesity" are well-known factors of hypertension. A stronger novel point is needed.

Response: The uniqueness of our study was that it was conducted among the elderly population, and this is a first-ever nationally representative study among the elderly population.

Minor comments

8. I cannot understand how they calculate "design effect (DEF) = 1.61". :

Response: We calculated the design effect using the formula:

Design Effect = $1 + (n-1) \cdot icc$

In our study, the cluster sample size was 62 and we assumed the intra-cluster correlation as 0.01. (ref)

9. The authors should show the results of multivariable analysis, in which BMI is included instead of waist circumference.

Response: We have used waist circumference (WC) instead of BMI in the final multivariable logistic regression considering statistical procedure (high correlation), interpretability, and policy implication. BMI and Waist circumference were highly correlated ($r = 0.8705$, $P\text{-value} < 0.001$). Also, in Bangladesh, measuring scale for the height and/or training to measure height is often unavailable in the primary health care facilities, while waist circumference can be measured with minimal training with a low-cost measuring tape. Moreover, height measurement is often difficult in the elderly population as they cannot stand in the proper posture required for height measurement. Additionally, there is also evidence that height decreases with age, especially after middle age (Wannamethee, Shaper, Lennon, & Whincup, 2006).

However, based on the reviewers' comments, we ran the model with BMI instead of WC and added the result in a supplementary table. Please, see supplementary table 1 at the end of this document. [we tried to paste the supplementary table here but it was broken. Please, see the table in the word file attached herewith. Sorry for the inconvenience]

10. Does adjusted odds ratios mean odds ratios from the model including all variables shown the table?

Response: Yes. We mentioned this in the footnotes of Table 3.

11. Results regarding Table 1 can be shortened since we can understand them by reading Table 1.

Response: Noted with thanks. We tried to shorten the result sections for both tables 1 and 2 as much as possible. Please, see paragraphs 1 and 2 of the result sections.

12. Instead of Table 2 (probably also Table 1), the characteristics stratified according to sex as well as hypertension status should be shown.

Response: Based on the suggestion from the review, we modified table 1, where we provided the characteristics stratified by hypertension status (no/yes). But for table 2, as we performed sex-segregated analysis and conducted multiple logistic regression with hypertension as a dichotomous variable, thought to show the difference between the prevalence of hypertension between males and

females.

13. What is SSC?

Response: It is the 10th-grade education in Bangladesh. We replaced the term with 10th grade. Please, see tables 1, 2, and 3 in rows allocated for educational status variable.

Reviewer: 2

Reviewer Name: Muhammad A.B. Chowdhury

Institution and Country: University of Florida

Please state any competing interests or state 'None declared': None Declared

Please leave your comments for the authors below

This is a well-executed study and well written manuscript that addresses a significant issue by the author's analyzed nationally representative household survey data. The potential for inferences is clearly consistent and the findings can be relevant in the perspective of national public health.

Response: Thank you for your comments.

Reviewer: 3

Reviewer Name: Shaun Scholes

Institution and Country: University College London, United Kingdom.

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

I was asked just to comment on the statistical aspects of the study. My comments are:

Abstract:

Please report reference categories where not obvious (e.g. central obesity versus whom? Those with normal weight?).

Response: Noted with thanks. We did not mention the reference category in the abstract due to the word limit. But we mentioned it in the result section of the main text.

Introduction:

The authors quote various hypertension statistics in the 3rd paragraph. Prevalence levels are affected by the definitions: therefore, the authors should define each term precisely as used in the study cited (e.g., define hypertension prevalence; controlled hypertension; undiagnosed hypertension).

Response: Noted with thanks. We added the definition of the mentioned terms in parenthesis. Please, see the 3rd paragraph of the introduction section.

Sampling:

Describe the procedure used to randomly select 1 person per household.

Response: We have elaborated it as following:

"After selecting the households from a cluster, if there was any household with more than one person aged ≥ 60 years, we randomly selected one of them using a simple random sampling method."

Methods:

Was there a time frame regarding when participants were told they were hypertensive (e.g., last year)? It is better to state the actual survey question used.

Response: No, there was no time frame for this question. Below is the question we asked the participants: "Has a health care provider ever told you that you have high blood pressure, also called hypertension (other than during pregnancy)?"

We are also adding it to the method section. Please, see the first sentence of the second paragraph in the data collection section in the method part.

The authors give no detail about the weighting. Did the weight reflect just the sampling design, or was there an additional adjustment for non-response (e.g. post-stratification?).

Response: We used only sampling design to construct the weight but did not adjust for the non-response rate. However, we encountered a very low non-response rate throughout the study (1.59% for the elderly age group).

There is also no mention of accounting for any geographical clustering of participants in the calculation of sampling variability (e.g. p-values and 95% CI). The authors need to specify that all appropriate features of the survey design were accounted for in the analysis.

Response: We considered population density for selecting the number of clusters in rural, non-slum urban, and slum areas. The surveillance has been taken place in 89 clusters (64 rural, 15 urban and 10 slum clusters) throughout the country. Multistage cluster sampling was used for surveillance site selection to make the estimates from the surveillance as divisionally representative. Clusters in rural, urban, and slum areas were selected randomly.

Define the risk factors in a legend to Figure 2. The authors state that risk factor prevalence was higher in females versus males. Table 1 contains p-values: but no mention was given in the methods section that the authors were conducting such statistical tests (e.g. which statistical tests were used and was account made for the complex survey design). The same point applies to Table 2. The footnote implies a significance test for gender differences in the prevalence of hypertension: the estimate for females is clearly higher than that for males (non-overlapping CIs) but there is no indication of this in the table.

Response: Noted with thanks. We defined the risk factors in the method part of the manuscript. Please, see the 3rd and 4th paragraphs of the operational definition sub-section of the method part. We revisited the table 1 and realized the redundancy of the p-values given. We dropped the column with the p-values from table 1. For table 2, it was a copying error. We dropped the footnote related to the test of significance from table 2.

The authors report an "unexpected" finding that hypertension was lower among current smokers. As reported in our paper:

<https://academic.oup.com/ije/article/47/3/860/4831068>

smokers may have been advised to quit smoking because they were hypertensive.

Response: We revisited the variables "smoking" and "smokeless tobacco," and we think that as we have data only on the current smoking and smokeless tobacco consumption status, it does not give us enough scope of explaining the association of these two variables with hypertension among elderly people. Moreover, the tobacco industry lobby can misguide the people using this conclusion based on

incomplete information. Considering the above scenario, we have dropped the variables of smoking and smokeless tobacco from our current analysis. In the future round of the survey, we will work on collecting further detail on smoking and smokeless tobacco.

Regarding the education status and hypertension, while it's true for the high-income countries that low education status is positively associated with a higher prevalence of hypertension, the situation often reverses in low-income countries. In Bangladesh, persons with higher education status are most likely well off, and they lead a sedentary lifestyle, go through stressful life and eat more junk food compared to the people from low education status or lower socioeconomic tier (Al Kibria et al., 2019; Almogbel, Aladhadh, Almotyri, Alhumaid, & Rasheed, 2019; Chowdhury, Uddin, Haque, & Ibrahimou, 2016).

Please include the reference category when discussing results (e.g. education up to SSC versus no formal education). Describing results as lower odds of hypertension is preferable to using terms such as negative association.

Reference: Noted with thanks. We modified the discussion section accordingly. Please, see lines 9-17 of the third paragraph of the discussion section.

Finally, could the authors discuss the issue of treated and controlled hypertension. Is there an assumption that participants with controlled hypertension (e.g. BP <140/90) due to medication use would be classed as hypertensive in this study due to being told that they were hypertensive by a trained health care provider? Did the survey ask about medication use? Could you use this information as well to shed light on factors associated with management of hypertension?

Response: Noted with thanks. Irrespective of medication use, if a trained health care provider ever told that the patient had hypertension, we classified them as hypertensive along with them who were diagnosed as hypertensive by measurement.

We did not collect data on Hypertension medication.

VERSION 2 – REVIEW

REVIEWER	Michihiro Satoh Tohoku Medical and Pharmaceutical University
REVIEW RETURNED	31-Aug-2020

GENERAL COMMENTS	<p>I thank the authors for modifying their manuscript according to my comments. However, I have still several concerns.</p> <ol style="list-style-type: none"> 1. As I mentioned, Age is one of the strongest risk factors and should be included as a continuous variable in the statistical model. I understood the authors' intentions (about policymakers). However, to include age as a continuous variable is necessary for the calculation of the accurately adjusted odds ratios for other variables; it is not only for the odds ratio for age itself. Similarly, BMI and waist circumference can be treated as continuous variables. The authors should show the adjusted odds ratio using the model, in which the factors are treated as continuous variables as much as possible, at least in the supplementary data section. Categorization can decrease statistical power. 2. Although I mentioned "it is unsuitable to show these results without any detailed information.", it does not mean remove all results regarding smoking from the analysis. If anything, smoking status should be included though more detailed results are needed. For example, stratification analysis according to age, BMI, other confounding factors. Although the authors mentioned in the
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	<p>response latter that the stratification analyses have been done, it seems to be for education status.</p> <p>3. I found the following publication: “Clin Hypertens doi: 10.1186/s40885-020-00143-1. eCollection 2020. Hypertension prevalence and its trend in Bangladesh: evidence from a systematic review and meta-analysis. Mohammad Ziaul Islam Chowdhury, et al. DOI: 10.1186/s40885-020-00143-1”</p>
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REVIEWER	Shaun Scholes University College London, United Kingdom.
REVIEW RETURNED	02-Sep-2020

GENERAL COMMENTS	I have read the authors responses to my comments on the earlier manuscript and I am happy with the arguments made. I hope my comments were useful. It appears the authors have done a nice job in the revised paper. I encourage the authors to proof-read the manuscript carefully. I hope the article helps to inform policy and future data collection efforts in Bangladesh.
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Michihiro Satoh

Institution and Country:

Tohoku Medical and Pharmaceutical University

Competing interests: None

Please leave your comments for the authors below

I thank the authors for modifying their manuscript according to my comments. However, I have still several concerns.

1. As I mentioned, Age is one of the strongest risk factors and should be included as a continuous variable in the statistical model. I understood the authors’ intentions (about policymakers). However, to include age as a continuous variable is necessary for the calculation of the accurately adjusted odds ratios for other variables; it is not only for the odds ratio for age itself. Similarly, BMI and waist circumference can be treated as continuous variables. The authors should show the adjusted odds ratio using the model, in which the factors are treated as continuous variables as much as possible, at least in the supplementary data section. Categorization can decrease statistical power.

Author response:

Response: Thank you so much for the valuable suggestions! As per your suggestion, we conducted additional analysis with age, BMI and waist circumference as continuous variables and provided the results in supplementary table 1. However, we did not see any substantial difference between the results both in terms of Odds Ratio (OR) or Significance levels.

2. Although I mentioned “it is unsuitable to show these results without any detailed information.”, it does not mean remove all results regarding smoking from the analysis. If anything, smoking status should be included though more detailed results are needed. For example, stratification analysis according to age, BMI, other confounding factors. Although the authors mentioned in the response latter that the stratification analyses have been done, it seems to be for education status.

Response: We reinserted the results containing the variables ‘current smoker’ and ‘current smokeless tobacco user’. However, as we performed multivariable logistic regression analysis to adjust the

effects of confounders, we have not shown additional results from stratification analysis.

3. I found the following publication:

“Clin Hypertens doi: 10.1186/s40885-020-00143-1. eCollection 2020.

Hypertension prevalence and its trend in Bangladesh: evidence from a systematic review and meta-analysis. Mohammad Ziaul Islam Chowdhury, et al. DOI: 10.1186/s40885-020-00143-1”

Response: Thank you so much for referring the publication. It was really helpful.

Reviewer: 3

Reviewer Name: Shaun Scholes

Institution and Country: University College London, United Kingdom.

Competing interests: None declared.

Please leave your comments for the authors below

I have read the authors responses to my comments on the earlier manuscript and I am happy with the arguments made. I hope my comments were useful. It appears the authors have done a nice job in the revised paper. I encourage the authors to proof-read the manuscript carefully. I hope the article helps to inform policy and future data collection efforts in Bangladesh.

Response:

Thank you so much again for your valuable time and comments!

VERSION 3 – REVIEW

REVIEWER	Michihiro Satoh Tohoku Medical and Pharmaceutical University
REVIEW RETURNED	04-Jan-2021
GENERAL COMMENTS	I have no further comments.