

## PEER REVIEW HISTORY

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## ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	A Cross-sectional Analysis of Ethnic Differences in Falls Prevalence in Urban-Dwellers Aged 55 Years and Over in the Malaysian Elders Longitudinal Research Study
<b>AUTHORS</b>	Alex, Deepa; Khor, Hui Min; Chin, Ai-Vyryn; Hairi, Noran; Othman, Sajaratulnisah; Khoo, Selina Phaik Kin; Bahyah Kamaruzzaman, Shahrul; Tan, Maw Pin

## VERSION 1 – REVIEW

<b>REVIEWER</b>	Saliu Balogun Menzies Institute for Medical Research, University of Tasmania, Australia
<b>REVIEW RETURNED</b>	10-Oct-2017

<b>GENERAL COMMENTS</b>	<p>This is a valuable study that examined ethnic differences in the prevalence of falls among older urban dwellers in Malaysia. The authors found that the prevalence of falls was higher in the Chinese and Indian ethnic groups compared to the Malays. Please see below for my comments on the manuscript.</p> <ol style="list-style-type: none"> <li>1. Please provide some information about the response rate of the survey? Was it similar among the three ethnic groups?</li> <li>2. Was there any evidence of effect modification? Table 3 should examine not only the association between ethnicity and falls but also the interactions of age, gender, education and marital status with ethnicity.</li> <li>3. The mean age of the Indians and the Chinese ethnic groups were similar, did the authors examined whether the odds of falls was similar between this two groups? It is possible that Indians had a higher odds of falls compared to the Malays but their odds of falls may not be significantly different from the Chinese.</li> <li>4. Odds ratios were used to describe the association between falls and ethnicity. However, the authors interpreted odds ratios in the manuscript as though they were a relative risk. For instance, on page 12 line 58: 'risk of falls increases' rather than 'odds of falls'. Odds ratio does not approximate risk ratio when the outcome of interest &gt;10%. The authors may consider estimating risk ratio to prevent misinterpretation of their finding. See for example Davies, et al. (1998). When can odds ratios mislead?. BMJ, 316(7136), 989-991.</li> </ol> <p>Ranganathan, P., Aggarwal, R. and Pramesh, C.S., 2015. Common pitfalls in statistical analysis: Odds versus risk. Perspectives in Clinical Research, 6(4), p.222.</p> <p>Knol, M.J., Le Cessie, S., Algra, A., Vandenbroucke, J.P. and Groenwold, R.H., 2012. Overestimation of risk ratios by odds ratios in trials and cohort studies: alternatives to logistic regression. Canadian Medical Association Journal, 184(8), pp.895-899.</p>
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	<p>5. The reference list contains a number of references that are very old (6 of 19 are more than 10 years old). Please update the references.</p> <p>Minor comments:</p> <p>6. Please provide age range in Table 1</p> <p>7. P. 13 line 13: I assume that instead of 'falls rate' you mean 'falls' as you did not examine the frequency of falls.</p>
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<b>REVIEWER</b>	Jaspreet Bhangu Boston University School of Medicine, U.S.A
<b>REVIEW RETURNED</b>	16-Oct-2017

<b>GENERAL COMMENTS</b>	<p>Thank you for allowing me to read this study. It focuses on the reported falls prevalence in a longitudinal cohort and reports differences in falls prevalence between ethnic populations. Overall I think this study has merit as it has reported on a novel population with a unique demographic make-up which may have impacted on falls prevalence. Unfortunately the granularity required to account for the differences in reported falls prevalence is lacking. In the models reported there are very few demographic variables included which are not known to be traditional risk factors for falls prevalence and may have little impact, if any, on reported falls rates. Additionally the authors do not provide any further data or hypothesis regarding the observed ethnic differences (are Indian Malays a poorer population and have less access to services?) . Perhaps this is beyond the scope of the study but it would be useful to include more traditional risk factors such as self reported gait unsteadiness, memory dysfunction , cardiac disease, etc into a model in order to provide a full picture of these patients and allow for further hypothesis to be tested in the future. I would recommend a major revision to include this and provide more data from the study. I have also provided feedback on results and discussion sections below.</p> <p>Results:</p> <p>1. In Table 1 it is unclear to me how the p-value was obtained for each of the categories. was this done via logistic regression or via ANOVA for continuous variables? In these tables perhaps the total number should be in the last column as per standard convention. Also there was no p value given for the age category differences. This may be relevant as there was a significant p value reported in the mean age row and it would be important to know which of the age categories accounted for this difference. Again I would suggest running an ANOVA for continuous variables to help to look into this further as it is unclear from these tables what the p-value is in relation to (i.e. whether there were between group differences as opposed to a difference within the entire dataset).</p> <p>When reporting the prevalence of falls you are comparing the 55-64 age group with the over 65 age group. Although your figure does show a linear regression model I cannot see what the R squared value was for the analysis which makes this a difficult figure to understand. Perhaps another statistical comparison (consider T-test) between the age groups reported falls prevalence would be appropriate to allow for a full evaluation of the effect of age on falls within the MELOR cohort. This is especially important as you make a long mention of it in your discussion but provide very little in the results to allow for comparison with other cohorts,.</p> <p>In Table 3 it is unclear what your column labelled B is reporting. It does not correspond to a p value and I cannot find what exactly it is reporting. I do not see p-values reported in the models as well.</p>
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	<p>Lastly it is unclear to me from the table which of the odds ratios reported were made using a univariate analysis and which were made using a multivariate analysis.</p> <p>Discussion: Please state whether this was overall prevalence or population adjusted prevalence in your opening sentence. Within your discussion it would be nice to have more of a biological explanation or hypothesis for why the observed differences exist in Indian Malays. Also it would be helpful to place these differences in a clinical context -perhaps a small discussion on how these differences may impact clinical care/pathways would be appropriate. Lastly There was no mention made about the type of fall which was suffered and whether multiple falls were experienced. This may not have been included in the MELOR study, but as these are major risk factors for higher risk cohorts this should be addressed in the limitations if the results cannot be published.</p>
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### VERSION 1 – AUTHOR RESPONSE

#### Reviewers' Comments to Author:

Reviewer: 1

Reviewer Name: Saliu Balogun

Institution and Country: Menzies Institute for Medical Research, University of Tasmania, Australia

Competing Interests: None Declared

This is a valuable study that examined ethnic differences in the prevalence of falls among older urban dwellers in Malaysia. The authors found that the prevalence of falls was higher in the Chinese and Indian ethnic groups compared to the Malays. Please see below for my comments on the manuscript.

1. Please provide some information about the response rate of the survey? Was it similar among the three ethnic groups?

Response: The response rate of the survey has been published elsewhere (Lim et al 2017). We have included the reference in our manuscript.

2. Was there any evidence of effect modification? Table 3 should examine not only the association between ethnicity and falls but also the interactions of age, gender, education and marital status with ethnicity.

Response: We have adjusted for all the above factors using multivariate analyses, but the ethnic differences remained.

3. The mean age of the Indians and the Chinese ethnic groups were similar, did the authors examine whether the odds of falls was similar between these two groups? It is possible that Indians had a higher odds of falls compared to the Malays but their odds of falls may not be significantly different from the Chinese.

Response: We have examined this effect, and adjusted for age differences. We apologize for this lack of clarity and amended the main manuscript accordingly.

4. Odds ratios were used to describe the association between falls and ethnicity. However, the authors interpreted odds ratios in the manuscript as though they were a relative risk. For instance, on page 12 line 58: 'risk of falls increases' rather than 'odds of falls'. Odds ratio does not approximate risk ratio when the outcome of interest >10%. The authors may consider estimating risk ratio to prevent misinterpretation of their finding. See for example Davies, et al. (1998). When can odds ratios mislead?. BMJ, 316(7136), 989-991.

Ranganathan, P., Aggarwal, R. and Pramesh, C.S., 2015. Common pitfalls in statistical analysis: Odds versus risk. *Perspectives in Clinical Research*, 6(4), p.222.

Knol, M.J., Le Cessie, S., Algra, A., Vandenbroucke, J.P. and Groenwold, R.H., 2012. Overestimation of risk ratios by odds ratios in trials and cohort studies: alternatives to logistic regression. *Canadian Medical Association Journal*, 184(8), pp.895-899.

Response: Thank you for bringing this to our attention. We have now calculated the risk ratios to avoid being misleading.

5. The reference list contains a number of references that are very old (6 of 19 are more than 10 years old). Please update the references.

Response: Thank you once again for bringing this to our attention. We have made the necessary adjustments.

Minor comments:

6. Please provide age range in Table 1

7. P. 13 line 13: I assume that instead of 'falls rate' you mean 'falls' as you did not examine the frequency of falls.

Response: Thank you for the above. We have addressed these accordingly.

Reviewer: 2

Reviewer Name: Jaspreet Bhangu

Institution and Country: Boston University School of Medicine, U.S.A

Competing Interests: None declared

Thank you for allowing me to read this study. It focuses on the reported falls prevalence in a longitudinal cohort and reports differences in falls prevalence between ethnic populations. Overall I think this study has merit as it has reported on a novel population with a unique demographic make-up which may have impacted on falls prevalence. Unfortunately the granularity required to account for the differences in reported falls prevalence is lacking. In the models reported there are very few demographic variables included which are not known to be traditional risk factors for falls prevalence and may have little impact, if any, on reported falls rates. Additionally the authors do not provide any further data or hypothesis regarding the observed ethnic differences (are Indian Malays a poorer population and have less access to services?). Perhaps this is beyond the scope of the study but it would be useful to include more traditional risk factors such as self reported gait unsteadiness, memory dysfunction, cardiac disease, etc into a model in order to provide a full picture of these patients and allow for further hypothesis to be tested in the future. I would recommend a major revision to include this and provide more data from the study. I have also provided feedback on results and discussion sections below.

Results:

1. In Table 1 it is unclear to me how the p-value was obtained for each of the categories. was this done via logistic regression or via ANOVA for continuous variables? In these tables perhaps the total number should be in the last column as per standard convention. Also there was no p value given for the age category differences. This may be relevant as there was a significant p value reported in the mean age row and it would be important to know which of the age categories accounted for this difference. Again I would suggest running an ANOVA for continuous variables to help to look into this further as it is unclear from these tables what the p-value is in relation to (i.e. whether there were between group differences as opposed to a difference within the entire dataset).

Response: Thank you for your suggestions. We have made the necessary amendments.

When reporting the prevalence of falls you are comparing the 55-64 age group with the over 65 age group. Although your figure does show a linear regression model I cannot see what the R squared value was for the analysis which makes this a difficult figure to understand. Perhaps another statistical comparison (consider T-test) between the age groups reported falls prevalence would be appropriate to allow for a full evaluation of the effect of age on falls within the MELOR cohort. This is especially important as you make a long mention of it in your discussion but provide very little in the results to allow for comparison with other cohorts,.

Response: Thank you for bringing this to our attention. We acknowledge the potential source of confusion. We have therefore removed the figure, and just added p-value with Chi-squared for the three ethnic groups.

In Table 3 it is unclear what your column labelled B is reporting. It does not correspond to a p value and I cannot find what exactly it is reporting. I do not see p-values reported in the models as well. Lastly it is unclear to me from the table which of the odds ratios reported were made using a univariate analysis and which were made using a multivariate analysis.

Discussion: Please state whether this was overall prevalence or population adjusted prevalence in your opening sentence. Within your discussion it would be nice to have more of a biological explanation or hypothesis for why the observed differences exist in Indian malays. Also it would be helpful to place these differences in a clinical context -perhaps a small discussion on how these differences may impact clinical care/pathways would be appropriate. Lastly There was no mention made about the type of fall which was suffered and whether multiple falls were experienced. This may not have been included in the MELOR study, but as these are major risk factors for higher risk cohorts this should be addressed in the limitations if the results cannot be published

Response: Our study has exposed for the first time ethnic differences in a novel population. We have attempted to hypothesize on potential explanations, but as this was a longitudinal cohort study, rather than a specific falls study we were unable to evaluate risks any further. This would be a topic for future studies. The type of falls and number of falls were measured, but is beyond the scope of the paper.

## VERSION 2 – REVIEW

<b>REVIEWER</b>	Saliu Balogun University of Tasmania, Australia
<b>REVIEW RETURNED</b>	09-Jan-2018
<b>GENERAL COMMENTS</b>	<p>Deepa et al. present an improved version of their article "Ethnic Differences in Falls Prevalence in Urban-Dwellers Aged 55 Years and Over in the Malaysian Elders Longitudinal Research (MELoR) Study". Although the article has improved, some concerns remain.</p> <p>1. The authors did not address the comment as to whether age, sex, and education modify the relationship between ethnicity and falls. This is important to investigate. For instance, although Indians had a higher risk of falls compared to Malay, it is possible that Indians with a higher secondary/tertiary education has a lower falls risk compared to Malays with lower levels of education. Hence, Table 3 should examine the interaction between ethnicity and age, sex, and education.</p> <p>2. Although the authors mentioned that they have now calculated risk ratios in Table 3, 'odds ratio' rather than 'risk ratio' is presented in Table head of Table 3. Please clarify.</p>

## VERSION 2 – AUTHOR RESPONSE

### Reviewer's Comments to Author:

Reviewer: 1

Reviewer Name: Saliu Balogun

Institution and Country: University of Tasmania, Australia

Competing Interests: None declared

Deepa et al. present an improved version of their article "Ethnic Differences in Falls Prevalence in Urban-Dwellers Aged 55 Years and Over in the Malaysian Elders Longitudinal Research (MELoR) Study". Although the article has improved, some concerns remain.

1. The authors did not address the comment as to whether age, sex, and education modify the relationship between ethnicity and falls. This is important to investigate. For instance, although Indians had a higher risk of falls compared to Malay, it is possible that Indians with a higher secondary/tertiary education has a lower falls risk compared to Malays with lower levels of education. Hence, Table 3 should examine the interaction between ethnicity and age, sex, and education.

We apologies for not addressing this previously. We believe that the influence of age, sex and education on the relationship between ethnicity and falls had been addressed in Table 3 in our logistic regression models. While age, gender and education were significantly associated with falls, we had examined interaction terms for ethnicity and educational level and ethnicity and age in the logistic regression equations, and found that they were not significantly different. We had therefore elected not to report them previously. However, we do recognize that it's important to report any potential interaction and have now added to the statistical analysis, results and discussion accordingly.

2. Although the authors mentioned that they have now calculated risk ratios in Table 3, 'odds ratio' rather than 'risk ratio' is presented in Table head of Table 3. Please clarify.

We apologize for this omission. We had calculated risk ratios separately, but had overlooked their inclusion within the main manuscript. As risk ratios can only be calculated for 2 by 2 tables, it is not possible to obtain risk ratios using logistic regression with dummy variables. We have therefore compared risk ratios using multiple comparisons. However, as this is considered multiple comparison we have applied Bonferroni's correction to the level of significance to adjust for multiple comparisons.

## VERSION 3 – REVIEW

REVIEWER	Saliu Balogun University of Tasmania, Australia
REVIEW RETURNED	19-Mar-2018
GENERAL COMMENTS	1. The statistical analysis of the interaction and the presentation of the results of the interaction (on Page 13) is unclear. In practice, interactions are often assessed using statistical models by including a product term for the two exposures (for instance, ethnicity and age) in the model. If the p-value for the interaction is not significant, there is no point in performing a sub-group analysis. Please clarify whether there is a statistically significant interaction between ethnicity and the



	<p>three socio-demographic variables (age, sex and education).</p> <p>2. Regarding the risk ratio, the authors argue that it is not possible to obtain risk ratios using logistic regression with dummy variables. While you may elect not to report risk ratios in your manuscript, please note that it is possible to estimate risk ratio for the relationship between ethnicity and falls. See the help section of SPSS from the link below.  <a href="https://www.ibm.com/support/knowledgecenter/SSLVMB_sub/spss/tutorials/xtab_demo_oddsratio.html">https://www.ibm.com/support/knowledgecenter/SSLVMB_sub/spss/tutorials/xtab_demo_oddsratio.html</a></p> <p><a href="https://www.ibm.com/support/knowledgecenter/SSLVMB_sub/spss/tutorials/xtab_demo_layer_var_howto.html">https://www.ibm.com/support/knowledgecenter/SSLVMB_sub/spss/tutorials/xtab_demo_layer_var_howto.html</a></p>
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### VERSION 3 – AUTHOR RESPONSE

#### Point-by-point rebuttal to reviewers' comments

Reviewer: 1

Reviewer Name: Saliu Balogun

Institution and Country: University of Tasmania, Australia

Competing Interests: None declared

1. The statistical analysis of the interaction and the presentation of the results of the interaction (on Page 13) is unclear. In practice, interactions are often assessed using statistical models by including a product term for the two exposures (for instance, ethnicity and age) in the model. If the p-value for the interaction is not significant, there is no point in performing a sub-group analysis. Please clarify whether there is a statistically significant interaction between ethnicity and the three socio-demographic variables (age, sex and education).

*Response: 1. We acknowledge the lack of clarity in our interpretation of the interaction effect analysis. We had included the product terms age\*ethnicity and education\*ethnicity into the logistic regression model, and found no significant interaction effects. Hence these were not included in the tables. We have mentioned the method of analysis within the data analysis (Page 8) and stated the lack of interaction effects within the results (Page 13), and revised our wordings to attempt to add clarity. We had not attempted any subgroup analysis, as this was not the objective of our study. Instead the main objective of our study was to report the prevalence of falls for the overall Malaysian older population and the prevalence of falls among Malaysian according to ethnicity and age group as stipulated in our manuscript. The mechanisms underlying the relationship between ethnicity and falls is outside the scope of this article. We hope the reviewer is satisfied with this explanation.*

2. Regarding the risk ratio, the authors argue that it is not possible to obtain risk ratios using logistic regression with dummy variables. While you may elect not to report risk ratios in your manuscript, please note that it is possible to estimate risk ratio for the relationship between ethnicity and falls. See the help section of SPSS from the link below.

[https://www.ibm.com/support/knowledgecenter/SSLVMB\\_sub/spss/tutorials/xtab\\_demo\\_oddsratio.html](https://www.ibm.com/support/knowledgecenter/SSLVMB_sub/spss/tutorials/xtab_demo_oddsratio.html)

[https://www.ibm.com/support/knowledgecenter/SSLVMB\\_sub/spss/tutorials/xtab\\_demo\\_layer\\_var\\_howto.html](https://www.ibm.com/support/knowledgecenter/SSLVMB_sub/spss/tutorials/xtab_demo_layer_var_howto.html)

*Response: Risk ratios have been provided within the results section (Page 13) with an explanation within the data analysis section about their derivation (Page 8).*

## VERSION 4 – REVIEW

REVIEWER	Saliu Balogun The University of Tasmania, Australia
REVIEW RETURNED	18-May-2018

GENERAL COMMENTS	<p>Thank you for clarifying the analysis of the interaction term. The authors have answered my comments satisfactorily and I think the manuscript would make a good contribution to BMJ Open.</p> <p>The only concern that I have left is the relative risk estimation. The statistical analysis of the relative risk (Page 8, lines 41 – 50) and the presentation of the results (Page 13, lines 9 – 22) is unclear. Please delete the following sentences as these are not necessary.</p> <ul style="list-style-type: none"> <li>- “Risk ratios for falls according to each ethnic group compared to the other ethnic groups (Malays vs. non-Malays, Chinese vs non-Chinese, Indians vs non-Indians) were also estimated. However, as this will be considered multiple comparisons, Bonferroni’s correction was applied, and a p-value of less than 0.017 was therefore considered the level of statistical significance. For other statistical analyses,” (Page 13, lines 9 – 22)</li> <li>- “The risk ratios for falls according to ethnic groups were calculated separately. The ethnic Malays were significantly less likely to fall compared to the non-Malays (Risk Ratio, RR= 0.763, 95% confidence interval, CI= 0.620 to 0.938; p=0.009). The ethnic Chinese were not significantly less likely than the non-Chinese to fall (RR=0.974, 95%CI=0.804 to 1.18; p=0.791). The ethnic Indians were significantly more likely than the non Indians to experience at least one fall in the previous 12 months (RR=1.319, 95%CI=1.096 to 1.587; p=0.004).” (Page 8, lines 41 – 50)</li> <li>- The authors only need to present the result in Table 3 as prevalence ratio/relative risk rather than odds ratio. While the odds ratio is not technically wrong, I would prefer that the authors estimate relative risk/prevalence ratio mainly because odd ratios are frequently misinterpreted as though they were a relative risk. Besides, odds ratio does not approximate risk ratio when the outcome of interest &gt;10% (in your study, the prevalence of falls is 18.9%)</li> </ul> <p>Minor comment: Please remove intervention from the abstract as there are no interventions in the study.</p>
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## VERSION 4 – AUTHOR RESPONSE

Responses to Reviewer 1:

Thank you for clarifying the analysis of the interaction term. The authors have answered my comments satisfactorily and I think the manuscript would make a good contribution to BMJ Open.

-Response: Thank you for recommending our article for publications. We acknowledge that your contributions to our article have been substantial.

The only concern that I have left is the relative risk estimation. The statistical analysis of the relative risk (Page 8, lines 41 – 50) and the presentation of the results (Page 13, lines 9 – 22) is unclear. Please delete the following sentences as these are not necessary.



- "Risk ratios for falls according to each ethnic group compared to the other ethnic groups (Malays vs. non-Malays, Chinese vs non-Chinese, Indians vs non-Indians) were also estimated. However, as this will be considered multiple comparisons, Bonferroni's correction was applied, and a p-value of less than 0.017 was therefore considered the level of statistical significance. For other statistical analyses," (Page 8, lines 41 – 50)

Response: We have deleted this as suggested, instead we have added a reference to describe how we have managed to calculate relative risk using modified logistic regression . (Page 8, Para 1, line 12-17)

- "The risk ratios for falls according to ethnic groups were calculated separately. The ethnic Malays were significantly less likely to fall compared to the non-Malays (Risk Ratio, RR= 0.763, 95% confidence interval, CI= 0.620 to 0.938; p=0.009). The ethnic Chinese were not significantly less likely than the non-Chinese to fall (RR=0.974, 95%CI=0.804 to 1.18; p=0.791). The ethnic Indians were significantly more likely than the non Indians to experience at least one fall in the previous 12 months (RR=1.319, 95%CI=1.096 to 1.587; p=0.004)." (Page 13, lines 9 – 22)

Response: This has also been removed. (Page 13, lines 1-8). The paragraph has now been amended to present relative risk (Page 13, line 1)

- The authors only need to present the result in Table 3 as prevalence ratio/relative risk rather than odds ratio. While the odds ratio is not technically wrong, I would prefer that the authors estimate relative risk/prevalence ratio mainly because odd ratios are frequently misinterpreted as though they were a relative risk. Besides, odds ratio does not approximate risk ratio when the outcome of interest >10% (in your study, the prevalence of falls is 18.9%)

Response: Thank you for your comments. We acknowledge that odds ratios do overestimated risk as our outcome of interest exceeded 10%. We have now calculated relative risk using a previously published method of modified logistic regression as described earlier and references in the methods. (Page 13, Table 3). The overall results remain unchanged and therefore, we have changed the corresponding odds ratios to relative risks and left the remainder of the manuscript unchanged.

Minor comment:

Please remove intervention from the abstract as there are no interventions in the study.

Response: We have removed "intervention" in the abstract. (Page 3)