

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Association of objective and subjective socioeconomic markers with cognitive impairment among older adults: cross-sectional evidence from a developing country
AUTHORS	Muhammad, T; Sekher, T.V; Srivastava, Shobhit

VERSION 1 – REVIEW

REVIEWER	Tomo Takasugi Hamamatsu University School of Medicine, Department of Community Health and Preventive Medicine
REVIEW RETURNED	21-Jul-2021

GENERAL COMMENTS	The title should be included a research design.
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REVIEWER	Aayush Khadka UCSF, cognitive outcomes
REVIEW RETURNED	07-Dec-2021

GENERAL COMMENTS	<p>The authors have written an interesting article in which the investigate the association between objective and subjective measures of socioeconomic status (SES) with cognitive impairment among adults ≥ 60 years in India. The authors argue that this is a novel contribution to the India-specific literature on cognitive impairment. Another strength of their analysis is the data they use, which is nationally representative.</p> <p>I have the following major and minor comments on the paper. I hope these comments can help the authors improve their manuscript.</p> <p>Major comments:</p> <ol style="list-style-type: none">1. Use of SES measures: The authors need to justify in greater detail their construction of both the objective and subjective SES measures. <p>In terms of objective SES, the authors initially point to reference 17 which suggests that education and wealth are key measures of objective SES; however, in their own construction of objective SES, they only use expenditure data. Why do the authors initially suggest that both education and wealth are part of objective SES, but then use something completely different in their own construction of the measure? In addition, it is not clear to me that reference 17 – which is about the Whitehall II study – is 100% relevant in the Indian context. Perhaps the authors can follow studies that have created such objective measures of SES, if not in India, then in South Asia?</p>
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In terms of the subjective SES measure, the authors need to explain whether the ladder method is a valid way of capturing subjective SES in India. Furthermore, it seems a bit strange that best off are described only in terms of money, education, and jobs, and not in terms of other key hierarchies in Indian society such as caste. As the authors themselves argue on page 12: “The Scheduled Tribes (STs) and Scheduled Castes (SCs) are among the most disadvantaged and discriminated socio-economic groups in India.”

I am also a bit surprised that the authors did not construct alternate measures of objective/subjective SES to test the sensitivity of their results to their primary construction of these measures. In general, SES is very difficult to measure in the data, so having a few different definitions of the measure may be a useful way of examining the relationship between SES and cognitive functioning.

2. Use of cognitive impairment measure: Like with objective/subjective SES, more motivation is needed on why the authors construct the cognitive impairment score in the way they do so. In particular, the authors need to discuss the validity of the cognitive battery used to construct the measure and why simply adding the test-specific scores is a valid way of constructing the cognitive impairment score. I’m also a bit surprised by the fact that the authors did not use the continuous score as an outcome in their analysis – even if this continuous measure would not capture the binary nature of cognitively impaired/not impaired, it could potentially provide interesting insights into the relationship between SES and cognitive functioning.

3. Control variable constructions: I did not fully understand why the authors categorize so many of their control variables. For example, why has the continuous variable “age” been divided into three categories? Similarly, why was self-rated health, a categorical measure already, re-categorized as a binary variable?

On a slightly separate issue: does LASI not collect data on type of morbidity? In the context of cognition, certain diseases – e.g., cardiovascular diseases – may be more important than others – e.g., respiratory diseases. I suppose I understand why this variable may not be used even if it exists, as it is likely that there is a fair bit of measurement error here. Nevertheless, it would be useful for the authors to discuss this.

4. Interpreting results: There are a couple of issues here: first, I noted some incorrect interpretation of odds ratios. For example, on page 13, the authors write “Moreover, older adults who belonged to lower objective SES had 32% significantly higher likelihood to suffer from cognitive impairment [AOR: 1.32; $p < 0.05$]”. An adjusted odds ratio of 1.32 would imply 32% higher odds of cognitive impairment and not a 32% increase in the probability of cognitive impairment, which is what is currently implied by the highlighted sentence. A similar error exists in the following paragraph and in the results section of the abstract.

Second, I would encourage the authors to not focus on p-values and rather on confidence intervals. If possible, it would be good to interpret the bounds of the confidence interval and how meaningful the magnitude of these bounds are in the context of their research question. This comment is motivated by the following paper: <https://www.nature.com/articles/d41586-019-00857-9>

	<p>Minor comments:</p> <ol style="list-style-type: none"> 1. It may be useful to have a figure for the sampling strategy as opposed to writing it all out in the manuscript. 2. Authors need to justify why they focus only on those 60 years or older in their study as opposed to using the entire sample of individuals 45 years and older.
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REVIEWER	Noémie Letellier Unité 1061 Inserm
REVIEW RETURNED	07-Dec-2021

GENERAL COMMENTS	<p>Thank you for giving me the opportunity to review this paper. Using a large representative survey of older adults aged 60 and above in India, the authors found that the participants who belonged to lower subjective and objective SES had significantly higher odds for cognitive impairment, even after adjustment for individual characteristics, health factors and household factors. The associations were especially high for subjective SES. The topic is important, and I found particularly interesting to add subjective SES measurements to the classical objective measurements. I have the following comments.</p> <p>Major comments:</p> <ul style="list-style-type: none"> - In the abstract, it would be helpful to provide more information on the assessment of cognitive impairment. - I suggest rephrasing the aim in the abstract and introduction, for example: "explore whether objective and subjective SES status were associated with late-life cognitive impairment". - This study was restricted to participants aged 60 years and above. However, the authors had access to participants aged 45 and older, and given that residential environment characteristics might be a source of differences in cognitive function in older adults, but probably also earlier in life, it may be of interest to study such association in middle-aged and young-old individuals. Can the authors explain why they limited the analyses to participants aged 60 years and older and did not use the all sample? I suggest that this point be added in discussion, it could be an idea for future analyses. - Generally, SES is evaluated by income, education level and occupation. In this study, objective SES variable was only based on monthly per capita expenditure whereas the authors had access to other indicators such as educational level or Caste. Please, justify the choice of this specific variable. - In the models, among the covariates, the authors used two similar variables: marital status and living arrangements. I wonder if there is a problem of collinearity. Did you test multicollinearity for these two variables specifically? - In methods, the authors should add the list of covariates used to fit the models as well as in the Figure 3 legend. - In the results section, it should be interesting to have the number of participants with cognitive impairment and to specify which number corresponds to the 10th on the scale from 0 to 43. - In the results section, I strongly encourage the authors to add a few sentences about the dose-response relationship observed for both subjective and objective SES and provide p trend. I think this is an important finding. The author should highlight (in the abstract, results and discussion) the fact that the lower the socioeconomic level, the
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	<p>higher the risk of cognitive impairment.</p> <ul style="list-style-type: none"> - In the abstract and in the method section, the authors talk about “Chi square test was used to evaluate the significance level of differences in cognitive impairment for subjective and objective SES”. If I am not mistaken, no result corresponds to this analysis, so please remove this sentence from the method and abstract. <p>Minor comments:</p> <ul style="list-style-type: none"> - I advise the authors to pay attention to punctuation when proofreading, for example there are a lot of commas missing, which makes reading the manuscript sometimes complicated. - In the abstract and the manuscript, replace “AOR” by “aOR”, and “UOR” by “OR” - Page 2, line 41, in the abstract, delete “It was also revealed that” - Page 2, line 48, add a comma after “in comparison to their counterparts” - Page 3, line 3, delete “also” - Page 6, line 44: we in this study aim to explore -> in this study, we aim to explore - Page 7, line 56: “on the different cognitive measures”, please delete “the” in this sentence - Page 8, in methods, I recommend removing the “Control variables” heading and separate SES variables from other variables: one paragraph for SES exposures and then another paragraph for covariates (individual characteristics, health factors and household factors). - Introduce and define in the statistical section the terms OR and IC95%. - Page 12, line 29: “Table-1 represents socio-economic and demographic profile of older adults in India” replace by “Table-1 represents socio-economic and demographic profile of Indian older adults included in this study”. - Page 14, line 15, I think this following sentence is not correct: “which found that older individuals with higher SES experience cognitive impairment compared with people with lower SES”, did the authors reverse higher and lower? - In addition to including multiple SES groups from the Indian population, this study also includes participants living in both rural and urban areas. I suggest the authors add this as a strength to reinforce the generalizability of their results.
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Dr. Tomo Takasugi, Hamamatsu University School of Medicine

Comments to the Author:

The title should include a research design.

Response: Dear reviewer, thank you for the comment. After incorporating the comments from other reviewers, the revised title is read as ***“Association of objective and subjective socioeconomic markers with cognitive impairment among older adults: cross-sectional evidence from a developing country*”**

Reviewer: 2

Dr. Aayush Khadka, UCSF

Comments to the Author:

The authors have written an interesting article in which they investigate the association between objective and subjective measures of socioeconomic status (SES) with cognitive impairment among adults ≥ 60 years in India. The authors argue that this is a novel contribution to the India-specific literature on cognitive impairment. Another strength of their analysis is the data they use, which is nationally representative.

I have the following major and minor comments on the paper. I hope these comments can help the authors improve their manuscript.

Major comments:

1. Use of SES measures: The authors need to justify in greater detail their construction of both the objective and subjective SES measures.

In terms of objective SES, the authors initially point to reference 17 which suggests that education and wealth are key measures of objective SES; however, in their own construction of objective SES, they only use expenditure data. Why do the authors initially suggest that both education and wealth are part of objective SES, but then use something completely different in their own construction of the measure? In addition, it is not clear to me that reference 17 – which is about the Whitehall II study – is 100% relevant in the Indian context. Perhaps the authors can follow studies that have created such objective measures of SES, if not in India, then in South Asia?

Response: Dear reviewer, many thanks for the comment. The manuscript is substantially revised now by adding the education and caste status into the key explanatory variables as part of objective SES measures. The citation #17 is replaced by multiple references that have considered wealth status, education and caste status as measures of SES in Indian context. The statement is modified accordingly.

In terms of the subjective SES measure, the authors need to explain whether the ladder method is a valid way of capturing subjective SES in India. Furthermore, it seems a bit strange that best off are described only in terms of money, education, and jobs, and not in terms of other key hierarchies in Indian society such as caste. As the authors themselves argue on page 12: “The Scheduled Tribes (STs) and Scheduled Castes (SCs) are among the most disadvantaged and discriminated socio-economic groups in India.”

Response: Dear reviewer, the LASI survey used the MacArthur scale of measuring subjective SES which is also used in other studies in India and other countries. The only available subjective measure of SES was this ladder SES which is used in the current study. The citations are provided for the ladder SES in the revised version. Also, as per the suggestions, caste status as an objective measure of SES is considered as a key variable in the revised manuscript. The paper is revised accordingly. Thank you for raising the concern.

I am also a bit surprised that the authors did not construct alternate measures of objective/subjective SES to test the sensitivity of their results to their primary construction of these measures. In general, SES is very difficult to measure in the data, so having a few different definitions of the measure may be a useful way of examining the relationship between SES and cognitive functioning.

Response: Dear reviewer, we agree with your concern. The authors have tried to elaborate on the available subjective SES marker and different objective SES markers in the LASI survey data which were used as key variables in the revised manuscript.

2. Use of cognitive impairment measure: Like with objective/subjective SES, more motivation is needed on why the authors construct the cognitive impairment score in the way they do so. In particular, the authors need to discuss the validity of the cognitive battery used to construct the measure and why simply adding the test-specific scores is a valid way of constructing the cognitive impairment score. I'm also a bit surprised by the fact that the authors did not use the continuous score as an outcome in their analysis – even if this continuous measure would not capture the binary nature of cognitively impaired/not impaired, it could potentially provide interesting insights into the relationship between SES and cognitive functioning.

Response: The authors created the variable, cognitive impairment as binary because as per the objective, the authors intend to see the association of subjective and objective SES with cognitive impairment and make the results more easily interpretable. However, as per the reviewer's suggestion, the authors have provided the standardized beta coefficients of cognitive impairment (cognitive functioning score of 0-43 was reversed during the analysis) used in continuous form (on a scale of 43-0) to provide more insight into the relationships. Since LASI is the sister study of the Health and Retirement Study (HRS), the cognition module of HRS was followed to measure the cognitive impairment, which is mentioned in the manuscript. Also, a pilot survey was conducted before the main survey of LASI to ensure the validity of the scales and questions. The lowest tenth percentile as proxy measure of cognitive impairment was validated in India and the study is cited now.

3. Control variable constructions: I did not fully understand why the authors categorize so many of their control variables. For example, why has the continuous variable "age" been divided into three categories? Similarly, why was self-rated health, a categorical measure already, re-categorized as a binary variable?

Response: Thank you for raising the concern. The age and self-rated health were categorized as per the literature. However, as per the comment, the authors have now reanalysed by keeping age in continuous form and self-rated health in its original five categories.

On a slightly separate issue: does LASI not collect data on type of morbidity? In the context of cognition, certain diseases – e.g., cardiovascular diseases – may be more important than others – e.g., respiratory diseases. I suppose I understand why this variable may not be used even if it exists, as it is likely that there is a fair bit of measurement error here. Nevertheless, it would be useful for the authors to discuss this.

Response: The authors controlled the analysis for the morbidity status in the revised manuscript. The variable morbidity was created using the chronic diseases which include hypertension, chronic heart diseases, stroke, any chronic lung disease, diabetes, cancer or malignant tumor, any bone/joint disease, neurological/psychiatric disease, or high cholesterol. This is now included in the method section.

4. Interpreting results: There are a couple of issues here: first, I noted some incorrect interpretation of odds ratios. For example, on page 13, the authors write "Moreover, older adults who belonged to lower objective SES had 32% significantly higher likelihood to suffer from cognitive impairment [AOR: 1.32; $p < 0.05$]". An adjusted odds ratio of 1.32 would imply 32% higher odds of cognitive impairment and not a 32% increase in the probability of cognitive impairment, which is what is currently implied by the highlighted sentence. A similar error exists in the following paragraph and in the results section of the abstract.

Response: Thank you for the comment. The authors have now removed the word "likelihood" and replaced it with "odds".

Second, I would encourage the authors to not focus on p-values and rather on confidence intervals. If possible, it would be good to interpret the bounds of the confidence interval and how meaningful the magnitude of these bounds are in the context of their research question. This comment is motivated by the following paper: <https://www.nature.com/articles/d41586-019-00857-9>

Response: Thank you for the suggestion. The paper was really helpful for understanding the importance of focusing on confidence interval rather than p value. Accordingly, the authors have modified statements in some places that mentioned the statistical significance in the results.

Minor comments:

1. It may be useful to have a figure for the sampling strategy as opposed to writing it all out in the manuscript.

Response: Dear reviewer, many thanks for the suggestion. Since details of the survey sampling strategy are published elsewhere, some of the statements have been removed and the reference has been added.

2. Authors need to justify why they focus only on those 60 years or older in their study as opposed to using the entire sample of individuals 45 years and older.

Response: Dear reviewer, the authors aimed to analyze the data for older adults who are defined in Indian context as those who are aged 60 years and above, therefore authors feel that including the data for those who are aged 45 years and above would lead to confusion in defining the study population.

Reviewer: 3

Dr. Noémie Letellier, Unité 1061 Inserm

Comments to the Author:

Thank you for giving me the opportunity to review this paper. Using a large representative survey of older adults aged 60 and above in India, the authors found that the participants who belonged to lower subjective and objective SES had significantly higher odds for cognitive impairment, even after adjustment for individual characteristics, health factors and household factors. The associations were especially high for subjective SES. The topic is important, and I found particularly interesting to add subjective SES measurements to the classical objective measurements. I have the following comments.

Major comments:

- In the abstract, it would be helpful to provide more information on the assessment of cognitive impairment.

Response: Dear reviewer, thank you for the suggestion. More detail on outcome variable is provided in the abstract now.

- I suggest rephrasing the aim in the abstract and introduction, for example: "explore whether objective and subjective SES status were associated with late-life cognitive impairment".

Response: Many thanks for the suggestion. The sentence is modified accordingly. It now reads as "study explored how various markers of objective and subjective socio-economic statuses (SES) are associated with cognitive impairment among older Indian adults."

- This study was restricted to participants aged 60 years and above. However, the authors had access to participants aged 45 and older, and given that residential environment characteristics might be a source of differences in cognitive function in older adults, but probably also earlier in life, it may be of interest to study such association in middle-aged and young-old individuals. Can the authors explain why they limited the analyses to participants aged 60 years and older and did not use the all sample? I suggest that this point be added in discussion, it could be an idea for future analyses.

Response: Dear reviewer, the authors aimed to analyze the data for older adults who are defined as those who are aged 60 years and above. The future studies on middle aged and older adults are suggested and elaborated in the discussion section as per your suggestion.

- Generally, SES is evaluated by income, education level and occupation. In this study, objective SES variable was only based on monthly per capita expenditure whereas the authors had access to other indicators such as educational level or Caste. Please, justify the choice of this specific variable.

Response: Dear reviewer, many thanks for the comment. The manuscript is substantially revised now by adding the education and caste status into the key explanatory variables as part of objective SES measures. Multiple references have been provided in the introduction of the revised manuscript that have considered wealth status, education and caste status as measures of SES in Indian context. The statements in places are modified accordingly.

- In the models, among the covariates, the authors used two similar variables: marital status and living arrangements. I wonder if there is a problem of collinearity. Did you test multicollinearity for these two variables specifically?

Response: Dear reviewer, the authors agree with your point. The authors did check the problem of multicollinearity and found no multicollinearity between the specific variables.

- In methods, the authors should add the list of covariates used to fit the models as well as in the Figure 3 legend.

Response: Comment is incorporated in the revised version.

- In the results section, it should be interesting to have the number of participants with cognitive impairment and to specify which number corresponds to the 10th on the scale from 0 to 43.

Response: Thank you for the comment. The authors added the same in the results section. The numbers are also provided.

- In the results section, I strongly encourage the authors to add a few sentences about the dose-response relationship observed for both subjective and objective SES and provide p trend. I think this is an important finding. The author should highlight (in the abstract, results and discussion) the fact that the lower the socioeconomic level, the higher the risk of cognitive impairment.

Response: Dear reviewer the authors have already shown the dose-response relationship observed for both subjective and objective SES in the result section. However, p-values are now provided in the result section.

- In the abstract and in the method section, the authors talk about "Chi square test was used to evaluate the significance level of differences in cognitive impairment for subjective and objective SES". If I am not mistaken, no result corresponds to this analysis, so please remove this sentence from the method and abstract.

Response: Thank you for the observation. The authors have now removed it from the method section.

Minor comments:

- I advise the authors to pay attention to punctuation when proofreading, for example there are a lot of commas missing, which makes reading the manuscript sometimes complicated.

Response: The punctuations are appropriated in the revised version.

- In the abstract and the manuscript, replace “AOR” by “aOR”, and “UOR” by “OR”

Response: Comment is incorporated in the revised manuscript. Thanks for the comment.

- Page 2, line 41, in the abstract, delete “It was also revealed that”

Response: Comment is incorporated in the revised manuscript.

- Page 2, line 48, add a comma after “in comparison to their counterparts”

Response: Thanks for noticing. Comment is incorporated in the revised manuscript.

- Page 3, line 3, delete “also”

Response: Comment is incorporated in the revised manuscript. The word “also” is deleted from the respective place.

- Page 6, line 44: we in this study aim to explore -> in this study, we aim to explore

Response: Comment is incorporated in the revised manuscript. The statement is modified accordingly.

- Page 7, line 56: “on the different cognitive measures”, please delete “the” in this sentence

Response: Comment is incorporated in the revised manuscript.

- Page 8, in methods, I recommend removing the “Control variables” heading and separate SES variables from other variables: one paragraph for SES exposures and then another paragraph for covariates (individual characteristics, health factors and household factors).

Response: Comments are incorporated now.

- Introduce and define in the statistical section the terms OR and IC95%.

Response: Comment is incorporated. It now reads as “*The results were presented in the form of odds ratio (OR) with 95% confidence interval (CI)*”.

- Page 12, line 29: “Table-1 represents socio-economic and demographic profile of older adults in India” replace by “Table-1 represents socio-economic and demographic profile of Indian older adults included in this study”.

Response: Comment is incorporated.

- Page 14, line 15, I think this following sentence is not correct: “which found that older individuals with higher SES experience cognitive impairment compared with people with lower SES”, did the authors reverse higher and lower?

Response: Many thanks for noticing. Yes. It was mistakenly reversed. The statement is revised now.

- In addition to including multiple SES groups from the Indian population, this study also includes participants living in both rural and urban areas. I suggest the authors add this as a strength to reinforce the generalizability of their results.

Response: Thank you for the suggestion. This is mentioned in the strengths of the study in discussion section now.

VERSION 2 – REVIEW

REVIEWER	Tomo Takasugi Hamamatsu University School of Medicine, Department of Community Health and Preventive Medicine
REVIEW RETURNED	08-Feb-2022

GENERAL COMMENTS	It is an interesting topic with a large amount of data. Congratulations on your work.
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REVIEWER	Aayush Khadka UCSF, cognitive outcomes
REVIEW RETURNED	09-Feb-2022

GENERAL COMMENTS	<p>Dear authors,</p> <p>Thank you for revising your manuscript and taking into consideration the comments previously made by the reviewers. I believe the topic you are trying to address is important, especially in the Indian context from where we do not have much evidence. However, I still have some concerns about the study, which I try to outline below. I hope my comments are helpful in making your manuscript more robust.</p> <p>Major comments:</p> <ol style="list-style-type: none"> 1. The authors still need to provide additional justification for simply adding the scores of the various cognitive tests that are used to create the overall outcome variable. There are a few algorithms which have been designed using the HRS data to estimate cognitive functioning and dementia probability. For example, see the Wu et. al. (2013) paper discussing the creation of one such algorithm. Perhaps the authors could justify why it is okay to weight all tests equally and why an additive measure for overall cognitive functioning makes sense. If this was done for simplicity's sake or because no such algorithm for combining cognitive tests exists in the Indian context, then perhaps the authors could note this under the "Discussion" section as a limitation. 2. Unfortunately, I still don't understand the reason behind some of the choices the authors made in coding their variables. For example, in estimating the MPCE, the authors first estimate a continuous measure for monthly expenditure, after which they convert this into a 5-level categorical variable (quintiles), and then they again recode the quintiles as a 3-level categorical variable. I appreciate the transparency with which the authors describe their process, but why was this final recoding from 5- to 3-categories necessary? <p>Relatedly, could the authors clarify how caste was coded for individuals who are not Hindu? Also, the "other" caste category appears to be a very large, catch-all category. It would be good if the authors could justify the choice of creating this particular caste category.</p> <ol style="list-style-type: none"> 3. I apologize for not noting this in the previous version of my review, but I also think additional justification is needed for the choice of covariates. Should we think of the covariates listed by the authors as
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confounders of the SES-cognition relationship? Should we think of them as variables that the authors are controlling for in order to improve the precision of their outcomes?

In addition, there is some danger of some of these covariates potentially being on the pathway from, say, objective SES to cognition. For example, if we view objective SES as caste, this is generally determined at birth, which could then influence your life course in multiple ways, including how often you partake in physical activity or social activity (both of which are covariates, as per the manuscript). If some of these covariates are on the pathway from the exposure to the outcome, then the authors may be getting attenuated estimates or worse, their results may be subject to collider stratification bias – the direction of the latter bias is difficult to determine a priori.

This is a difficult issue, but potentially an important one given the fact that it can really affect the estimates from the regression models. I would urge the authors to provide a justification for their choice of covariates. If this is not possible to do in the text (due to word count limitations, say), then it may be useful for the authors to include an appendix table where they mention why they are including each of the covariate they use in their analysis.

4. Some of these results make me skeptical about whether the model has been correctly specified (see point #3 above as well). For example, the authors find that “Older adults who were not educated/with minimum education had significantly higher odds of cognitive impairment in reference to older adults with higher education [aOR: 22.4; $p < 0.05$]” (p 14, lines 6-10). An approximately 2140% increase in odds of cognitive impairment seems rather improbable, right? And, while the authors note that this result is statistically significant, the confidence interval on this estimate goes from 10.58 to 47.41, which is very wide.

It's unclear to me what exactly is going on here to lead to a result which seems rather hard to believe. It could be that the sample of higher education older adults is very different from those without education, even after controlling for the several covariates. It could also be that there is some sort of collider stratification bias affecting these estimates as a result of controlling for covariates which could potentially be on the pathway from SES to education (see point #3 above). It could also be that the correct specification for the model should not include subjective SES and objective SES in the same model at the same time (for example, perhaps the regression should just be of the outcome on one of the objective SES measures at one time?). It could be that the functional form is mis-specified in some way. In any case, I think this is worth investigating further from the authors' side.

Perhaps the authors could begin by comparing their point estimates against the literature? I do not believe I saw this being done explicitly, even if the authors do argue that their results on the education exposure are broadly in line with published studies. I would also ask the authors to fit models where they have separate models for each aspect of objective SES and subjective SES. That is, they have four models in total:

M1: cognitive functioning ~ subjective SES + control variables
M2: cognitive functioning ~ MPCE quintiles + control variables

	<p>M3: cognitive functioning ~ education + control variables M4: cognitive functioning ~ caste + control variables</p> <p>Perhaps the authors could also try to estimate a latent variable model, where they use the three measures of objective SES to construct one latent variable, and then use this latent variable as their primary exposure variable in their regressions.</p> <p>Minor comments:</p> <ol style="list-style-type: none"> 1. Some suggestions for making the abstract read more crisply: <ol style="list-style-type: none"> a. In general, use past tense when describing actions that have already taken place. For example, in the Setting and participant section, "...used data from the Longitudinal Aging Study in India" as opposed to "...uses data from..." b. It may be better to use active voice in the Abstract rather than passive voice. For example, in the Primary and secondary outcome measures section, instead of "Descriptive statistics and cross-tabulations were presented", the authors could say, "We estimated descriptive statistics and presented cross-tabulations of the outcome". Similarly, in the Conclusion section, instead of "The findings suggest...", the authors could write, "Our findings suggest..." c. Replace "The present study" from the Objective section with "This study". Similarly, replace "The present study" from the Setting and participant section with "This study". d. Add the word "nationally" when talking about the representative nature of the survey under the "Design" section of the Abstract 2. It may be preferable to use the active voice in the main text of the manuscript as well. 3. Although understandable, the acronym UT has not been previously defined in page 7 of the manuscript under the "Data Source" section
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REVIEWER	Noémie Letellier Unité 1061 Inserm
REVIEW RETURNED	09-Feb-2022

GENERAL COMMENTS	The authors did a good job for addressing the reviewers' comments. I just have one more comment, it seems that some research ethics information (e.g., participant consent, ethics approval) is missing from the Data Source paragraph.
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Dr. Tomo Takasugi, Hamamatsu University School of Medicine

Comments to the Author:

It is an interesting topic with a large amount of data. Congratulations on your work.

Response: Thank you for the recommendation.

Reviewer: 2

Dr. Aayush Khadka, UCSF

Comments to the Author:

Dear authors,

Thank you for revising your manuscript and taking into consideration the comments previously made by the reviewers. I believe the topic you are trying to address is important, especially in the Indian context from where we do not have much evidence. However, I still have some concerns about the study, which I try to outline below. I hope my comments are helpful in making your manuscript more robust.

Major comments:

1. The authors still need to provide additional justification for simply adding the scores of the various cognitive tests that are used to create the overall outcome variable. There are a few algorithms which have been designed using the HRS data to estimate cognitive functioning and dementia probability. For example, see the Wu et. al. (2013) paper discussing the creation of one such algorithm. Perhaps the authors could justify why it is okay to weight all tests equally and why an additive measure for overall cognitive functioning makes sense. If this was done for simplicity's sake or because no such algorithm for combining cognitive tests exists in the Indian context, then perhaps the authors could note this under the "Discussion" section as a limitation.

Response: Dear reviewer, many thanks for the insightful comment. The adding of various tests for measuring cognition was done for simplicity's sake and because of lack of evidence on any algorithm for combining cognitive tests that exists in the Indian context. This is added as a limitation in the Discussion section of the revised manuscript with details.

2. Unfortunately, I still don't understand the reason behind some of the choices the authors made in coding their variables. For example, in estimating the MPCE, the authors first estimate a continuous measure for monthly expenditure, after which they convert this into a 5-level categorical variable (quintiles), and then they again recode the quintiles as a 3-level categorical variable. I appreciate the transparency with which the authors describe their process, but why was this final recoding from 5- to 3-categories necessary?

Response: Dear reviewer, the MPCE quintile was available in the LASI survey data with details of measurement which are provided in the methods section. Further, the variable was recoded into 3 categories for easy interpretability and better understanding while applying interaction term. Keeping the actual categories would produce large number of categories during the interaction analysis.

Relatedly, could the authors clarify how caste was coded for individuals who are not Hindu? Also, the "other" caste category appears to be a very large, catch-all category. It would be good if the authors could justify the choice of creating this particular caste category.

Response: Dear reviewer, thanks for raising the concern. In the Indian context, the Other Backward Classes which also include non-Hindus occupy positions in the middle. And "other" caste category denotes to a large number of "forward" caste groups of all religions which occupy the high position in caste hierarchy in India. Thus, the terms like Scheduled Caste and Tribe, Other Backward Classes and others are used to identify people and referred in government documents. Citations which discuss on these are provided in the manuscript. Several modifications are made for better understanding.

3. I apologize for not noting this in the previous version of my review, but I also think additional justification is needed for the choice of covariates. Should we think of the covariates listed by the authors as confounders of the SES-cognition relationship? Should we think of them as variables that the authors are controlling for in order to improve the precision of their outcomes?

In addition, there is some danger of some of these covariates potentially being on the pathway from, say, objective SES to cognition. For example, if we view objective SES as caste, this is generally determined at birth, which could then influence your life course in multiple ways, including how often you partake in physical activity or social activity (both of which are covariates, as per the manuscript). If some of these covariates are on the pathway from the exposure to the outcome, then the authors may be getting attenuated estimates or worse, their results may be subject to collider stratification bias – the direction of the latter bias is difficult to determine a priori.

This is a difficult issue, but potentially an important one given the fact that it can really affect the estimates from the regression models. I would urge the authors to provide a justification for their choice of covariates. If this is not possible to do in the text (due to word count limitations, say), then it may be useful for the authors to include an appendix table where they mention why they are including each of the covariate they use in their analysis.

Response: Dear reviewer, many thanks for the insightful comments and suggestions. The limitations in the discussion section are substantially revised by mentioning some of the above possibilities including the pathways which are not studied in the current analysis. The conceptual framework has been modified. The literature is cited with details on which the selection of each group of covariates in the study is based.

4. Some of these results make me skeptical about whether the model has been correctly specified (see point #3 above as well). For example, the authors find that “Older adults who were not educated/with minimum education had significantly higher odds of cognitive impairment in reference to older adults with higher education [aOR: 22.4; $p < 0.05$]” (p 14, lines 6-10). An approximately 2140% increase in odds of cognitive impairment seems rather improbable, right? And, while the authors note that this result is statistically significant, the confidence interval on this estimate goes from 10.58 to 47.41, which is very wide.

It's unclear to me what exactly is going on here to lead to a result which seems rather hard to believe. It could be that the sample of higher education older adults is very different from those without education, even after controlling for the several covariates. It could also be that there is some sort of collider stratification bias affecting these estimates as a result of controlling for covariates which could potentially be on the pathway from SES to education (see point #3 above). It could also be that the correct specification for the model should not include subjective SES and objective SES in the same model at the same time (for example, perhaps the regression should just be of the outcome on one of the objective SES measures at one time?). It could be that the functional form is mis-specified in some way. In any case, I think this is worth investigating further from the authors' side.

Perhaps the authors could begin by comparing their point estimates against the literature? I do not believe I saw this being done explicitly, even if the authors do argue that their results on the education exposure are broadly in line with published studies. I would also ask the authors to fit models where they have separate models for each aspect of objective SES and subjective SES. That is, they have four models in total:

M1: cognitive functioning ~ subjective SES + control variables

M2: cognitive functioning ~ MPCE quintiles + control variables

M3: cognitive functioning ~ education + control variables

M4: cognitive functioning ~ caste + control variables

Perhaps the authors could also try to estimate a latent variable model, where they use the three measures of objective SES to construct one latent variable, and then use this latent variable as their primary exposure variable in their regressions.

Response: We thank you for the detailed comment and suggestions. The possible collider stratification bias is explained in the limitations of the study under the discussion section as per earlier suggestion. The supplementary file now includes results of regression analyses (table S1) of the four separate multivariable models that are suggested above. Additionally, supplementary table S2 and S3 provide the multivariable and moderated multivariable regression estimates of cognitive impairment that is adjusted for education by different cut-off scores for each educational category. The results section is revised accordingly mentioning all these additions and their details.

Minor comments:

1. Some suggestions for making the abstract read more crisply:

a. In general, use past tense when describing actions that have already taken place. For example, in the Setting and participant section, "...used data from the Longitudinal Aging Study in India" as opposed to "...uses data from..."

Response: Comment is incorporated.

b. It may be better to use active voice in the Abstract rather than passive voice. For example, in the Primary and secondary outcome measures section, instead of "Descriptive statistics and cross-tabulations were presented", the authors could say, "We estimated descriptive statistics and presented cross-tabulations of the outcome". Similarly, in the Conclusion section, instead of "The findings suggest...", the authors could write, "Our findings suggest..."

Response: Comment is incorporated.

c. Replace "The present study" from the Objective section with "This study". Similarly, replace "The present study" from the Setting and participant section with "This study".

Response: Comment is incorporated.

d. Add the word "nationally" when talking about the representative nature of the survey under the "Design" section of the Abstract

Response: Response: Comment is incorporated.

2. It may be preferable to use the active voice in the main text of the manuscript as well.

Response: Response: Comment is incorporated.

3. Although understandable, the acronym UT has not been previously defined in page 7 of the manuscript under the "Data Source" section

Response: Response: Comment is incorporated.

Reviewer: 3

Dr. Noémie Letellier, Unité 1061 Inserm

Comments to the Author:

The authors did a good job for addressing the reviewers' comments. I just have one more comment, it seems that some research ethics information (e.g., participant consent, ethics approval) is missing from the Data Source paragraph.

Response: Dear reviewer, thank you for this comment. The authors have incorporated the comment.

VERSION 3 – REVIEW

REVIEWER	Heather Whitson Duke Univ
REVIEW RETURNED	10-Jul-2022

GENERAL COMMENTS	<p>I am reviewing a revised version of the manuscript which I have not previously reviewed.</p> <p>Overall, it seems the other reviewers recommended improvements which have been well-incorporated by the authors. The paper addresses a question that is sufficiently novel in a large Indian epidemiological study. The correlational analyses using cross-sectional data are straightforward and appropriate; limitations have been appropriately acknowledged in this revised version. The results go in the expected direction. I have only minor suggestions at this point:</p> <p>1) I understand that in this set of analyses, the authors found a greater strength of correlation between SES and cognitive impairment, when using subjective SES measure, compared to objective SES measure. However, I don't believe that a statistical test was performed to conclude that the difference in these point estimates obtained through different models was statistically significant. If such a test was not performed (which assesses whether the difference in magnitude of the association detected by separate models is non-zero), then I think it would be prudent to note that as a limitation. Additionally, they could soften this as a conclusion but still say something like "Subjective measures of SES were linked to cognitive outcomes, potentially even more strongly than were the objective measures of SES; considering the relative ease of obtaining such measures, subject SES measures are a promising target for future study on socioeconomic indicators of cognitive impairment."</p> <p>2) In the Strengths and Limitations section and in the Discussion, it is noted: "Some individuals may become cognitively impaired because they are illiterate and could not respond with accuracy to several measures." It may be closer to the intended meaning to say "Individuals who are illiterate may be mis-categorized as cognitively impaired because they cannot respond with accuracy to several measures."</p> <p>3) I found this sentence in the Discussion to be a bit jumbled and I think it would benefit from breaking it up and clarifying the main point(s) to be conveyed: "The current study provides crucial clues about what measure of SES highly reflect on the mental health in old age by underlining the importance of the cumulative dimension of subjective SES and different traditional measures including wealth status, education and caste, and showing the underperformance of traditional measure of wealth status compared to subjective SES."</p>
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VERSION 3 – AUTHOR RESPONSE

Reviewer: 4

Dr. Heather Whitson, Duke Univ

Comments to the Author:

I am reviewing a revised version of the manuscript which I have not previously reviewed.

Overall, it seems the other reviewers recommended improvements which have been well-incorporated by the authors. The paper addresses a question that is sufficiently novel in a large Indian epidemiological study. The correlational analyses using cross-sectional data are straightforward and appropriate; limitations have been appropriately acknowledged in this revised version. The results go in the expected direction. I have only minor suggestions at this point:

Response: Dear reviewer, the authors are grateful to you for the remaining comments which we believe helped improve the paper further.

1) I understand that in this set of analyses, the authors found a greater strength of correlation between SES and cognitive impairment, when using subjective SES measure, compared to objective SES measure. However, I don't believe that a statistical test was performed to conclude that the difference in these point estimates obtained through different models was statistically significant. If such a test was not performed (which assesses whether the difference in magnitude of the association detected by separate models is non-zero), then I think it would be prudent to note that as a limitation. Additionally, they could soften this as a conclusion but still say something like "Subjective measures of SES were linked to cognitive outcomes, potentially even more strongly than were the objective measures of SES; considering the relative ease of obtaining such measures, subject SES measures are a promising target for future study on socioeconomic indicators of cognitive impairment."

Response: Dear reviewer, we thank you for these crucial points. Yes, the authors agree with you. The discussion is revised to add the limitation of not conducting any statistical test that assesses whether the differences in the magnitude of observed associations in separate models are non-zero and statistically significant. The conclusion section is revised and added the suggested lines. Thank you for this insightful comment.

2) In the Strengths and Limitations section and in the Discussion, it is noted: "Some individuals may become cognitively impaired because they are illiterate and could not respond with accuracy to several measures." It may be closer to the intended meaning to say "Individuals who are illiterate may be mis-categorized as cognitively impaired because they cannot respond with accuracy to several measures."

Response: Dear reviewer, many thanks for the detailed suggestion. The statement is modified as per your suggestion.

3) I found this sentence in the Discussion to be a bit jumbled and I think it would benefit from breaking it up and clarifying the main point(s) to be conveyed: "The current study provides crucial clues about what measure of SES highly reflect on the mental health in old age by underlining the importance of the cumulative dimension of subjective SES and different traditional measures including wealth status, education and caste, and showing the underperformance of traditional measure of wealth status compared to subjective SES."

Response: The above mentioned statement is broken to two and made clear in revised manuscript.

VERSION 4 – REVIEW

REVIEWER	Heather Whitson Duke Univ
REVIEW RETURNED	01-Aug-2022
GENERAL COMMENTS	The authors have satisfactorily responded to my previous comments.