

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Frailty and driving status associated with disability: A 24-month follow-up longitudinal study
<b>AUTHORS</b>	Doi, Takehiko; Tsutsumimoto, Kota; Ishii, Hideaki; Nakakubo, Sho; Kurita, Satoshi; Shimada, Hiroyuki

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Marco Proietti Department of Clinical Sciences and Community Health, University of Milan, Milan, Italy
<b>REVIEW RETURNED</b>	25-Aug-2020

<b>GENERAL COMMENTS</b>	<p>The authors presented a paper investigating the relationship between driving status, frailty and disability in a large population-based cohort in Japan.</p> <p>The study is of interest, notwithstanding I believe that there issues that should be managed by the authors in an accurate revision of the current manuscript.</p> <ul style="list-style-type: none"><li>- The authors should clearly define how during follow-up the patients were kept into observation (I assume that they used an intention-to-treat approach...if this is true, please state);</li><li>- In the first paragraph of the methods, I suggest the authors give details about the Kihon checklist. Being BMJ Open a worldwide journal, not all the readers could be familiar with this tool and since it is cited several times, I believe it deserves more details;</li><li>- I think that there is a problem with the Frailty definition. In the methods, they stated that: "Based on the values of these five components (weight loss, weakness, exhaustion, slowness, and low activity), our study assigned "frail" to values of 1 and over, including pre-frailty and frailty.[8, 9]". Then, in Table 1 we found both pre-frailty and frailty reported. So it is not clear how they defined the two conditions and, more importantly, what would have been the exposure variable, it is only frailty or prefrailty+frailty?</li><li>- The assessment of disability is, in my opinion, a little bit too wide. Indeed, using any level of LTCl as an outcome could include also patients with a limited request for assistance, which could not be considered a complete disability. If the authors want to keep the current approach, they should at least mention this as a limitation and also justify it clearly. Additionally, I would suggest performing a sensitivity analysis, using only "Care Level 1 or higher" as the disability outcome;</li><li>- In the multivariate analysis, when they presented the data of the association between frailty and driving status separately, it's not clear which were the covariates used for adjustment, in particular, if they put together frailty and driving status in the same</li></ul>
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	<p>multivariate analysis or if the performed two distinct analyses. If they haven't, I would suggest doing so, in order to understand if the two phenomena have an independent role;</p> <ul style="list-style-type: none"> <li>- I think that the authors should work a bit more on the discussion section. It is scarce, not clearly reporting and discussing the current evidence but, more importantly, they don't discuss in any way the clinical and caring implications of their findings. Also, I think that they could add some bits about possible strategies to improve and maintain driving abilities and the possible impact on disability;</li> <li>- The authors should clearly state how their finding cannot be easily generalizable, given a specific ethnicity. Should also discuss if their results can be generalized to the Japanese population;</li> <li>- I recommend the authors to perform a detailed revision for the use of English as some paragraphs are not easily understandable and need to be improved for clarity.</li> </ul>
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<b>REVIEWER</b>	Bruce Newbold McMaster University, Canada
<b>REVIEW RETURNED</b>	02-Oct-2020

<b>GENERAL COMMENTS</b>	<p>This is a well-written paper that explores linkages between frailty, disability and driving status amongst older adults. The analysis is based on data gathered in Japan, with respondents followed over an approximately 2 year period. The followup provides longitudinal detail needed to ascertain the relationship, and the research uses well established measures of ability, ADLS, frailty, etc..</p> <p>Before acceptance, the authors need to consider the following points:</p> <p>While the majority of my questions focus on methods, on the first page of the manuscript, the authors correctly note that "In fact, driving cessation increases the risk of disability." But, the reverse is also true (i.e., disability can lead to driving cessation), and this should be acknowledged. More generally, since the reverse direction is also possible, have the authors accounted for this in their analysis? That is, have they looked at the problem from the opposite direction?</p> <p>In terms of methods and analysis:</p> <ul style="list-style-type: none"> <li>- Please clearly state when frailty was assessed (baseline or followup)? If it was measured at both instances, did the authors see any reversals in frailty (frail at time 0 and not-frail at time 1)? If so, how were these cases coded?</li> <li>- The paper uses the Japanese CHS to define frailty. How does this compare to international measures? Comparability will aid comparison to other international cases.</li> <li>- Does the status 'without a license' (p 6) mean they never had a license? Were participants asked when a license was surrendered / not renewed or how long a person had not been driving?</li> <li>- I assume that the care levels 1-5 cited on page 7 still allow individuals to perform ADLs on their own (?) Perhaps a discussion of the differences between these levels would be beneficial.</li> </ul> <p>There are also other factors that influence driving status or allow an older adult to continue to drive longer. For example, having a partner that can navigate or be a second set of eyes on the road could extend their ability to drive. Perhaps more important in the context of this paper is that how often a person drives is also</p>
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	<p>important. For example, a person with a license may indicate that they drive, but drive infrequently and/or under specific conditions (i.e., daylight, good weather, etc.). Was frequency of driving accounted for? Could the analysis consider frequency of driving? If this is not available, it should be noted as a limitation. More broadly, the authors should acknowledge that there are examples that enable driving ability to be extended.</p> <p>Finally, I would like to know a bit about how licenses can be removed in Japa</p>
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## VERSION 1 – AUTHOR RESPONSE

### Comments to Reviewer: 1

We wish to thank the reviewer for the useful comments. As described in the responses below, we have incorporated all of these suggestions into the revised manuscript. In response to the reviewer, we have thoroughly revised the manuscript. We believe that the paper has been substantially improved by addressing these comments. Below, the reviewer's comments are shown in bold font and our point-by-point responses are shown in Times New Roman font.

#### **Reviewer: 1**

#### **Comments to the Author**

**The authors presented a paper investigating the relationship between driving status, frailty and disability in a large population-based cohort in Japan.**

**The study is of interest, notwithstanding I believe that there issues that should be managed by the authors in an accurate revision of the current manuscript.**

**- The authors should clearly define how during follow-up the patients were kept into observation (I assume that they used an intention-to-treat approach...if this is true, please state);**

Response: After the baseline assessment, all participants were followed using LTCI data (mean follow-up duration: 23.5 months). We received the LTCI data from local governments with monthly updates. Information regarding moving out of the city and death was also collected. If we were unable to follow up with participants due to them moving out or dying, data from these participants were treated as censored data. Using LTCI data, survival analysis was conducted using these exclusion criteria, and an intention-to-treat approach was not used. (p. 5 L. 74-76)

**- In the first paragraph of the methods, I suggest the authors give details about the Kihon checklist. Being BMJ Open a worldwide journal, not all the readers could be familiar with this tool and since it is cited several times, I believe it deserves more details;**

Response: Based on your suggestion, detailed information about the Kihon Checklist was added to the revised manuscript. (p. 5 L. 89- p. 6 L. 93)

**- I think that there is a problem with the Frailty definition. In the methods, they stated that: "Based on the values of these five components (weight loss, weakness, exhaustion, slowness, and low activity), our study assigned "frail" to values of 1 and over, including pre-frailty and frailty.[8, 9]". Then, in Table 1 we found both pre-frailty and frailty reported. So it is not clear how they defined the two conditions and, more importantly, what would have been the exposure variable, it is only frailty or prefrailty+frailty?**

Response: We fully agree with your suggestion. We added the definition of pre-frailty and frailty to the methods section. In addition, the definition of the frail group has been provided in a footnote of Table 2 and in the legend of Figure 1. (p. 6 L. 107-108, Table 2, Fig. 1.)

**- The assessment of disability is, in my opinion, a little bit too wide. Indeed, using any level of LTCI as an outcome could include also patients with a limited request for assistance, which could not be considered a complete disability. If the authors want to keep the current approach, they should at least mention this as a limitation and also justify it clearly. Additionally, I would suggest performing a sensitivity analysis, using only "Care Level 1 or higher" as the disability outcome;**

Response: As you noted, using Japanese LTCI data had several limitations. To regard the certification of LTCI as "disability" has a limitation in that there is a wide range of degrees of disability. We have added a sensitivity analysis wherein a different definition of disability (to be certified as "Care Level 1 or higher") was used. We have added a related description in the methods and results sections regarding this analysis. (p. 8 L. 156-159, p. 12 L. 204-208)

**- In the multivariate analysis, when they presented the data of the association between frailty and driving status separately, it's not clear which were the covariates used for adjustment, in particular, if they put together frailty and driving status in the same multivariate analysis or if they performed two distinct analyses. If they haven't, I would suggest doing so, in order to understand if the two phenomena have an independent role;**

Response: We fully agree with your suggestion and have added an analysis that set frailty and driving status in the same model. We revised the methods and results sections regarding this point. (p. 8 L. 153, p. 12 L. 199-201)

**- I think that the authors should work a bit more on the discussion section. It is scarce, not clearly reporting and discussing the current evidence but, more importantly, they don't discuss in any way the clinical and caring implications of their findings. Also, I think that they could add some bits about possible strategies to improve and maintain driving abilities and the possible impact on disability;**

Response: As you pointed out, clinical implications from the results of this study were insufficient. Our study implies that the assessment of driving status may be useful for evaluating the risk of disability. In addition, continuing to drive may contribute to an active lifestyle among older adults. Thus, these points have been added to the revised manuscript. (p. 15 L. 238-249)

**- The authors should clearly state how their finding cannot be easily generalizable, given a specific ethnicity. Should also discuss if their results can be generalized to the Japanese population;**

Response: We fully agree with your suggestion. A description regarding the generalization of our results has been added to the revised manuscript. (p. 15 L. 259-p. 16 L. 262)

**- I recommend the authors to perform a detailed revision for the use of English as some paragraphs are not easily understandable and need to be improved for clarity.**

Response: The original manuscript was checked by a professional editing service. To ensure that the language used in this draft is professional and clear, we had the revised manuscript checked thoroughly again by a professional editing service. (see attached certification by a professional editing service)

Comments to Reviewer: 2

#### **Reviewer: 2**

We wish to thank the reviewer for the useful comments. As described in the responses below, we have incorporated all of these suggestions into the revised manuscript. In response to the reviewer, we have thoroughly revised the manuscript, particularly the methods. We believe that the manuscript has been substantially improved by addressing these comments. Below, the reviewer's comments are shown in bold font and our point-by-point responses are shown in Times New Roman font.

#### **Comments to the Author**

**This is a well-written paper that explores linkages between frailty, disability and driving status amongst older adults. The analysis is based on data gathered in Japan, with respondents**

followed over an approximately 2 year period. The followup provides longitudinal detail needed to ascertain the relationship, and the research uses well established measures of ability, ADLS, frailty, etc..

**Before acceptance, the authors need to consider the following points:**

**While the majority of my questions focus on methods, on the first page of the manuscript, the authors correctly note that "In fact, driving cessation increases the risk of disability." But, the reverse is also true (i.e., disability can lead to driving cessation), and this should be acknowledged. More generally, since the reverse direction is also possible, have the authors accounted for this in their analysis? That is, have they looked at the problem from the opposite direction?**

Response: We fully agree with your suggestion. Reverse causation (that functional decline related to disability affects driving cessation) is also possible. However, data used in this study could not clarify a casual association between driving and disability. To examine reverse causation, data regarding changes of function, incident disability, and future driving cessation would be required. However, our database does not include such data. This point is a limitation of our study. Thus, a description regarding reverse causation was added to the limitations in the revised manuscript. (p. 16 L. 262-266)

**In terms of methods and analysis:**

**- Please clearly state when frailty was assessed (baseline or followup)? If it was measured at both instances, did the authors see any reversals in frailty (frail at time 0 and not-frail at time 1)? If so, how were these cases coded?**

Response: As you noted, the timing for assessing the data (baseline and follow-up) was not clear. Incident disability was assessed monthly during the follow-up period using LTCI data. Other variables including frailty were assessed at baseline and the status of frailty was not tracked. Detailed information about the timing for assessing the data has been added to the revised manuscript. (p. 5 L. 74-76)

**- The paper uses the Japanese CHS to define frailty. How does this compare to international measures? Comparability will aid comparison to other international cases.**

Response: As you pointed out, our study used the Japanese CHS index to define frailty. Comparison with research using other criteria to define frailty would be useful for understanding frailty. However, our cohort data (NCGG-SGS) did not gather information about other criteria that could define frailty. Thus, this point is also a limitation in this study and has been added to the revised manuscript. (p. 16 L. 266-267)

**- Does the status 'without a license' (p 6) mean they never had a license? Were participants asked when a license was surrendered / not renewed or how long a person had not been driving?**

Response: We asked participants for the following information regarding their driving license: without license, surrendered license, not renewed, has a license but does not drive, and currently driving with a license. "Without a license" means the participant never had a license. Detailed duration information (how long a person had not been driving) was not collected. We revised the description about the participants' driving licenses for clarification. (p. 6-7 L. 112-113)

**- I assume that the care levels 1-5 cited on page 7 still allow individuals to perform ADLs on their own (?) Perhaps a discussion of the differences between these levels would be beneficial.**

Response: The LTCI certifies a person as "Support Level 1 or 2" if he or she needs support for daily activities or "Care Level 1, 2, 3, 4, or 5" if he or she needs continuous care. A computer calculates the applicant's standardized scores for seven dimensions of physical and mental status, estimates the time needed for nine categories of care (grooming/bathing, eating, toileting, transferring, eating, assistance with instrumental activities of daily living, behavioral problems, rehabilitation, and medical services), and assigns a care-needs level based on the total estimated care minutes. The Nursing Care Needs Certification Board, consisting of experts in health and social services appointed by a mayor, determines whether the initial assessment is appropriate considering a statement by the applicant's primary care physician and notes written by an assessor during a home visit. The board's final decision about certification is entered into the LTCI. Characteristics of participants with disabilities may be varied and depend on certified levels. We have added a sensitivity analysis using a different definition of disability (certified as "Care Level 1 or higher"). We have added a description in the methods and results sections regarding this analysis. (p. 8 L. 156-159, p. 12 L. 204-208)

However, data about disability in our study were relatively limited and the follow-up duration was short. Thus, comparison of participants with disabilities between each level of certification could not be conducted. Further studies are required. This point has been added to the discussion in the revised manuscript. (p. 15 L. 254-259)

**There are also other factors that influence driving status or allow an older adult to continue to drive longer. For example, having a partner that can navigate or be a second set of eyes on the road could extend their ability to drive. Perhaps more important in the context of this paper is that how often a person drives is also important. For example, a person with a license may indicate that they drive, but drive infrequently and/or under specific conditions (i.e., daylight, good weather, etc.). Was frequency of driving accounted for? Could the analysis consider frequency of driving? If this is not available, it should be noted as a limitation. More broadly, the authors should acknowledge that there are examples that enable driving ability to be extended. Finally, I would like to know a bit about how licenses can be removed in Japan**

Response: As you noted, several conditions are related to driving. In particular, driving frequency could potentially affect our results. We collected driving frequency data among drivers. Since our groups were set up with two driving variables (driving or not driving), and we only had driving frequency information for those who drive, our study did not consider frequency in our analysis. This point is a limitation in our study and has been added to the revised manuscript. In addition, in accordance with your suggestion, we added a discussion about driving continuity in older adults, particularly in Japan. (p. 15 L. 238-249, p. 16 L. 268-271)

#### VERSION 2 – REVIEW

<b>REVIEWER</b>	Newbold, KB McMaster University
<b>REVIEW RETURNED</b>	21-Jan-2021
<b>GENERAL COMMENTS</b>	I am satisfied that the authors have made sufficient revisions to this paper and that it is now ready for publication.