

## PEER REVIEW HISTORY

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

## ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Predicting the need for assistance in basic and instrumental activities of daily life in elderly Norwegians. The HUNT Study, Norway
<b>AUTHORS</b>	Storeng, Siri Sund, Erik Krokstad, Steinar

## VERSION 1 – REVIEW

<b>REVIEWER</b>	Ranmalee Eramudugolla ANU, Australian National University
<b>REVIEW RETURNED</b>	07-Aug-2017

<b>GENERAL COMMENTS</b>	<p>The study data have the potential to make a useful contribution to our understanding of how functional dependence evolves in late life. However, the manuscript in its current form is very difficult to follow in terms of 1) the rationale for conducting the study and the chosen variables; 2) analysis methods and how and why the sensitivity analyses were included, 3) the final findings.</p> <p>At present, the study seems to present one viewpoint from introduction to half-way through the discussion, then, while discussing the sensitivity analyses, it becomes apparent that the statements made at the start of the discussion are not completely correct, making it difficult for the reader to understand the final message.</p> <p>I suggest that the important methodological considerations such as survivor bias, attrition, and generalisability, that are currently mentioned only at the end of the discussion, be brought in as part of the introduction and integrated into the methods section. The findings presented at the start of the discussion should then incorporate what was found in the sensitivity analyses as well as the main analyses.</p> <p>Following are some specific comments:</p> <p>1) It is unclear why sleep and sitting time should be investigated as risk factors for ADL and IADL dependence. The authors cite past literature that have found physical inactivity and depression to be risk factors for functional dependence. How would examining sleep and sitting time (which are correlated with physical inactivity and depression anyway) as predictors provide additional insights?</p> <p>2) There are numerous measures of ADL and IADLs, some more comprehensive than others. The authors should examine the issue of measurement sensitivity in previous studies and in the present studies, particularly if there are recent population level changes in level of dependence in older adults. Is there any data on the relative</p>
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	<p>sensitivity of the HUNT3 ADL and IADL measures and other measures such as Bayer IADLs, Lawton, Katz, Bristol etc...</p> <p>3) What is the source of the self-rated health variable? Is it custom designed for HUNT3? there any data on how it compares to more standard measures such as the SF-36?</p> <p>4) Were participants given examples of what constitutes low, and vigorous physical activity? Also, partial italicising of the sentences that define physical inactivity and activity makes it hard for the reader to separate it from the questions presented earlier. I recommend keeping them as regular text. Sample applies for page 7, line 45.</p> <p>5) It is not clear whether individuals aged 70+ at baseline had ADL/IADL data, and whether these individuals were followed-up for HUNT3? There are several places where it is stated that no baseline data on ADL/IADL data was available (e.g., page 9, line 5; page 17, line 12), while in other places there is a suggestion that this statement is true only for a portion of the sample – please make this clear in the methods and keep it consistent all the way throughout. A flow chart of which group had what follow-up data would be useful.</p> <p>6) Why are the 70+ group data presented in Supplementary section and the 60-69 group in the main text?</p> <p>7) There should be a statement about how missing data was treated in the analyses and the rationale for this.</p> <p>8) How many separate multinomial logistic regressions were conducted (one for each IV adjusting for gender education, civil status and chronic disease?). Why was the latter variables chosen as covariates?</p> <p>9) The authors present findings that depressive symptoms and self-rated health are 'risk factors' for ADL dependence, and that sleep disturbance, smoking, physical inactivity were 'lifestyle risk factors' for ADL/IADL dependence. I don't think one could present these as independent risk factors given that it is quite plausible that depression and sleep disturbance share variance in their relationship with ADL/IADL disturbance, and the same goes for self-rated health, physical inactivity and sitting time.</p> <p>10) The authors conclude on page 16, line 5 "...we found a decrease in ADL dependency from decade to decade". It appears that this is really only relevant to the 70+ group in the sample. Please make this clear. The authors should also discuss the possible role of healthy survivor bias in the observed decrease in ADL/IADL dependence in the 70+ group, and the lack of baseline data for 60-69 year olds to make such a conclusion about the younger age group.</p> <p>11) The utility of the findings to practice and understanding population changes in ADL/IADL is not clear.</p>
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<b>REVIEWER</b>	Agnieszka Młynarska Electrocardiology Department, Upper Silesian Heart Center Poland
<b>REVIEW RETURNED</b>	13-Aug-2017
<b>GENERAL COMMENTS</b>	<p>The presented to the review research addresses the problem of the need for assistance in the basic and advanced activities of the daily life in elderly Norwegians.</p> <p>The aim of the study is to demonstrate the factors that influence the increase in the need to assistance for the elderly patient.</p>

	<p>1. The results section was written in an unfavorable way.</p> <p>2. Problems with links to tables in the text of manuscript. Unreadable results in tables.</p> <p>3. Over 14% of the presented data is incomplete. With such a large group of people, incomplete records should be withdrawn from analysis.</p> <p>4. Table 1 - it is worth showing in these groups whether the differences were statistically significant.</p> <p>5. Table 2 - Not all abbreviations have been explained.</p> <p>6. The HADS-A and HADS-D scale – additional analysis should be performed - 16 points and above indicates a significantly elevated level of anxiety and depression (this subpopulation should be analyzed).</p> <p>7. Assessing sleep through a question about how many hours you spend in bed can be misleading. The limitation of work is the lack of a standardized questionnaire for assessing quality of life and sleep disorders.</p> <p>8. No results on average values ADL and IADL – the average values would be helpful.</p> <p>9. Statistics - The RRR used</p>
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<b>REVIEWER</b>	Hanna Falk Institute of Neuroscience and Physiology, Sahlgrenska Academy at the University of Gothenburg.
<b>REVIEW RETURNED</b>	18-Aug-2017

<b>GENERAL COMMENTS</b>	<p><b>SIGNIFICANCE</b></p> <ul style="list-style-type: none"> <li>• Norway can expect over 1.3 million people over 70 years old in 2060.</li> <li>• Evidence is equivocal, but increasing prevalence of chronic diseases is likely to cause functional disability and increased public expenditures and strain on health care systems.</li> <li>• Functional ability and health is the most essential dimensions of quality of life and wellbeing of older adults.</li> <li>• It is therefore of interest to maintain good health and high functioning into old age, both for older adults themselves and for the society as a whole. Good!</li> </ul> <p><b>METHOD</b></p> <ul style="list-style-type: none"> <li>• The aim of this study was to investigate factors predicting the need for assistance for activities in daily life (ADL) and instrumental ADL (IADL) among 5050 60-69-year-olds and to explore gender differences and changes in ADL/IADL function from 1995-97 (response rate 69.5%) to 2006-08 (response rate 54.1%).</li> <li>• Dependent variable = needing assistance (yes/no) in one or more ADL (indoor mobility, toileting, bathing, dressing, bed-rise, and feeding) or IADL (food preparation, light housework, heavier</li> </ul>
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	<p>housework, laundry, shopping, economy, medication, go outdoors, and public transportation.</p> <ul style="list-style-type: none"> <li>• The authors need to clarify what they mean by “needing assistance”. Do they refer to help from another person? Did they consider level of difficulty in carrying out the ADL/IADL?</li> <li>• Independent variables = <ul style="list-style-type: none"> <li>o Self-rated health (SRH) dichotomized as “good” and “poor”. o Life-satisfaction dichotomized as “satisfied” and “dissatisfied”.</li> <li>o Anxiety and depression using HADS-A and HADSD. The authors need to describe how they used the scale (i.e. continues or dichotomized into cases/non-cases of anxiety and depression). This becomes obvious once the reader gets to Table 1 but I would prefer if the authors described it in the methods section.</li> <li>o Physical inactivity dichotomized into physically inactive (less than three hours of light physical activity and no hard physical activity per week) and physically activity (more than three hours of light physical activity or any hard physical activity per week)</li> <li>o Problematic alcohol behavior defined as answering yes to two or more of the CAGE-questions.</li> <li>o Sleep dichotomized into “sleeping less than six hours or more than ten hours” and “sleeping seven to nine hours”.</li> <li>o Sitting time dichotomized into “sitting 0-7 hours” and “sitting 8 hours or more”.</li> <li>o Social participation was dichotomized into never, or only a few times a year and participate.</li> <li>o Smoking was dichotomized into daily smoker and not daily smoker.</li> </ul> </li> </ul> <p>STATISTICS</p> <ul style="list-style-type: none"> <li>o Multinomial logistic regression models investigating the effect of; 1) lifestyle risk factors (smoking, alcohol, physical inactivity, sitting time, sleep and social participation), 2) depression, anxiety, self-rated health and life satisfaction on functional disability in ADL and IADL 11 years later.</li> </ul> <p>RESULTS</p> <ul style="list-style-type: none"> <li>• Baseline information on ADL and IADL dependence was not available in 1995-97. To correct this weakness of the study, the authors performed a sensitivity analysis including those answering having good self-rated health and good life satisfaction at baseline. Does this mean that the authors assume that those having good SRH and good life satisfaction were NOT functionally disabled in 1995-97? I find the lack of baseline ADL/IADL data problematic. Especially in relation the aim of this study, i.e. to investigate factors predicting the future need for assistance in ADL/IADL and to explore gender differences and changes in ADL/IADL function from 1995-97 to 2006-08. This need extensive clarification and justification. In</li> </ul>
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	<p>addition 14-17% did not have ADL/IADL data in the follow-up phased in 2006-08. Did the authors exclude these individuals?</p> <ul style="list-style-type: none"> <li>• The analyses were adjusted for age, sex, education, marital status and chronic disease</li> </ul> <p>– factors predicting the future need for assistance in ADL (and death and non-participation at follow-up) was poor self-rated health, depression, short or prolonged sleeping time (“sleeping less than six hours or more than ten hours”), smoking and physical inactivity. Factors predicting the future need for assistance in IADL (and death and non-participation at follow-up) were poor self-rated health, poor life satisfaction, depression, physical inactivity, short or prolonged sleeping time and excessive sitting time.</p> <p>DISCUSSION</p> <ul style="list-style-type: none"> <li>• Did the authors control for cognitive impairment/exclude cases of dementia? Especially important since the ADL and IADL data is self-assessed.</li> <li>• Although the authors raise the missing ADL/IADL data at baseline as a limitation of the study, the sensitivity analysis still needs clarification. How did this analysis minimize this bias? While SRH is a subjective indicator of health status, it has also been found to be a strong predictor of mortality, as it integrates biological, mental, social and functional aspects of a person, including individual and cultural beliefs and health behaviors. It is an all-inclusive, sensitive yet non-specific measure that assesses health and predicts health outcomes in ways that are still unclear, and not necessarily identical with objective health status. Lay definitions of health take a wide range of factors into account, and most individuals rate their health as moderate to good. The rating is based on a cognitive process that is both contextual and subjective in that it incorporates physiological and emotional experiences and expectations, influenced by the social environment of the individual. Constituent parts of health known to influence SRH, and subsequent mortality, include chronic illness, depression, cognitive function, socioeconomic status, functional impairment, and physical activity. Please clarify.</li> <li>• We know that ADL disability is found to increase with age, that it decrease over time and is highly related to physical inactivity, and that women have higher risk of developing functional disability than men. Women also self-rate their health as poorer than do men which might deserve mentioning in the discussion section of this paper. In addition the life-prevalence of depression is greater amongst women which also deserve elaboration in the discussion section, especially in</li> </ul> <p>relation to the aim (i.e. to explore gender differences and changes in ADL/IADL function from 1995-97 to 2006-08).</p> <ul style="list-style-type: none"> <li>• The authors state that the results from this study provide new insights. I would like to see a more mature discussion section where the authors really make use of previous knowledge and relate it to their findings. For example, there are known relationships between sleep disturbance and depression, between inactivity and depression, between inactivity and ADL/IADL disability, between poor SRH and ADL/IADL disability and so forth. Please develop.</li> </ul>
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REVIEWER	Haiqun Lin Yale University, USA
REVIEW RETURNED	22-Sep-2017

GENERAL COMMENTS	<p><b>A review of “Predicting the Need for Assistance in Basic and Instrumental Activities of Daily Life in Elderly Norwegians. The HUNT Study, Norway” (#bmjopen-2017-018942) submitted to <i>BMJ Open</i></b></p> <p>This manuscript investigated the risk factors for ADL/IADL dependence in Norwegian population. The findings on the significant association of self-rated health, depression, sleep time, smoking, and physical inactivity with ADL/IADL dependence are in line with previous studies regarding these risk factors in various northern European populations.</p> <p>(1) Many abbreviations were used in the abstract without introducing them first, please either avoid abbreviations in abstract or write them out.</p> <p>(2) HUNT2 and HUNT3 samples need to be described in terms how they were selected from HUNT. The description of the dependent and the independent variables lack enough detail regarding their timing.</p> <p>(3) The non-participation (mortality or non-response) were dealt as separate outcomes. How a multivariate analysis treating death and ADL/IADL dependence as joint outcomes would affect the findings?</p> <p>(4) Timing of ADL/IADL dependence and death were not taken into account. Since there was no information on ADL/IADL status at baseline, the sample could be re-analyzed as a one with left-censoring and truncation.</p>
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	(5) Timing of independent predictors was not clear either. The time elapsed between a measurement of independent predictor and the detection of ADL/IADL dependence should be taken into account.
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<b>REVIEWER</b>	Philip Schluter University of Canterbury, New Zealand
<b>REVIEW RETURNED</b>	03-Oct-2017

<b>GENERAL COMMENTS</b>	<p>An interesting paper that reports research findings that are solidly undertaken. Well done. Some comments follow:</p> <p>[1] Page 5, line 10. What does 'fairly representative' actually mean? And why is it important here? Should more focus be on internal validity and then external validity? (Maybe see, for example: Kukull WA, Ganguli M. Generalizability: the trees, the forest, and the low-hanging fruit. <i>Neurology</i>, 2012;78(23):1886-91).</p> <p>[2] I would have preferred more detail on the analysis and results than all the detail on the independent variables (which perhaps could be relegated to the supplementary materials).</p> <p>[3] I would have enjoyed a participant flow diagram in the RESULTS - as per the STROBE guidelines - as I struggled to track the participants at each stage. Retention seemed low at 54.1% - which surely must introduce considerable potential for bias. I would also have enjoyed seeing the numbers who died and the numbers not participating (withdrawing and not found) at each stage? (By-the way, how were 'deaths' determined?)</p> <p>[4] Page 8, line 50. Multinomial logistic regression is probably fine. I was interested as to whether competing risk models were considered? And whether any interactions or stratifications (i.e. men vs. women) were thought about? I assume that death and nonparticipation was related to some of the variables in Table 1 (on page 9) - and that these dependencies may affect the reported findings. Perhaps consideration of a table of this in the supplementary materials might be useful?</p> <p>[5] The criteria is not defined for what constitutes ranking of risk factors. On page 15, line 38 it is stated that poor self-rated health and depression were the strongest. But I was not clear how that conclusion was arrived at.</p> <p>[6] Why was the gender breakdown not presented in Table 1 for HUNT 3? Also in the pdf version I had access too - some columns were blank. I assume this was an issue with the conversion process. Also some of the labels used inequalities that excluded a value (i.e. HADS-A had outcomes '&lt;7' and '&gt;8'. Which group would a person with HADS-A =8 be placed? Maybe another conversion problem?)</p> <p>[7] No diagnostic or goodness-of-fit details were presented - nor was what defined statistical significance. These seemed to be assumed.</p> <p>[8] Page 28. The STROBE statement check sheet did not seem to</p>
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	be completed. I think it should, and would benefit the revised paper. [7]
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<b>REVIEWER</b>	Jacob Dreier, MD MPH, PhD Ben-Gurion University of the Negev, Beer Sheva, Israel
<b>REVIEW RETURNED</b>	12-Oct-2017

<b>GENERAL COMMENTS</b>	<p>This is a well-written manuscript describing factors associated with ADL and IADL in elderly participants of a population-based survey in Norway. I was asked to perform a statistical review of the manuscript.</p> <p>1. The main limitations of the study are the possibility of survival bias in the later study, the potential for volunteer bias due to the obvious nature of the study, and the age of the data (the most up-to-date survey was in 2006, over than a decade ago). The first bias is addressed by the statistical analysis addressing the issue of competing risks. The second bias is briefly discussed. I would recommend a comparison of the study population to the general Norwegian population, just to get an idea of the differences between them, in factors available from national data, e.g. the percentage living at home, smoking or physically active. The third point should be addressed.</p> <p>2. I am not familiar with the term "relative risk ratio". The generic term "relative risk" is imprecise; It could be either a risk ratio (based on cumulative incidence) or a rate ratio (based on incidence density). In the present case, risk ratio is the correct term.</p> <p>Additional minor comments:</p> <p>3. Sleep was grouped into "&lt;6 or &gt;10 hours" vs. "7-9 hours". How would a study participant reporting 6 or 10 hours be treated? It should be defined as "≤6 or ≥10 hours" vs. "7-9 hours".</p> <p>4. Regarding Table 1</p> <p>a. Rather than pointing out missing data as a separate line, I would indicate the number of patients with data and then calculate the percentage out of valid answers.</p> <p>b. Abbreviations such as SRH or CAGE should be spelled out at the footnote, as in Table 3.</p> <p>c. I would add a column indicating the statistical significance of differences between the two surveys.</p> <p>d. In the second part of Table 1, the Table suddenly changes from comparing HUNT-2 and HUNT-3 to listing the percentage of participant with ADL/IADL dependence. This is problematic. I suggest splitting the Table into two or diving it some other way.</p> <p>5. In Table 2, there is no need to list the percentage without difficulty in addition to the percentage of participants with difficulties in performing the various ADLs / IADLs, as these columns always add up to 100%. Decreasing the number of columns will increase the readability of the Table. In addition, I would add a columns showing the p for trend over the years for men and women.</p> <p>6. In Table 3, I suggest writing "Non-participation in Hunt-3" as to separate it from social non-participation.</p> <p>7. Regarding the interpretation of findings in Table 3, depression was not statistically significant as a risk factor for dependence in ADL. Similarly, the three lifestyle factors listed were not statistically significant (page 13).</p> <p>8. The sensitivity analysis described on pages 17-18 should be moved to the Results section.</p>
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	9. In references 1 and 3, the year is missing. Reference 2 is written in Norwegian. A translation to English of the publication's name should be written, usually in square brackets, with the words "paper in Norwegian" or "(Norwegian)" next to it.
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## VERSION 1 – AUTHOR RESPONSE

Dear Assistant Editor, Hemali Bedi

We are pleased to resubmit for publication the revised version of “Factors associated with basic and instrumental activities of daily living in elderly participants of a population-based survey; the Nord-Trøndelag Health Study, Norway” to the BMJ Open. We appreciate the time and efforts by the editor and reviewers in reviewing this manuscript. The thorough work and good remarks on the manuscript were greatly appreciated, and we think it has improved the manuscript. The response to the reviewers’ comments are attached below and the revised version of the manuscript and supplementary files are attached as separate documents. We look forward to hear from you.

Yours sincerely,

Siri Høivik Storeng  
Medical Student Research Programme  
Norwegian University of Science and Technology, Trondheim, Norway

Reviewers comments

Editorial Requirements:

- Please revise your title to state the research question, study design, and setting (location), ensuring that you do not include acronyms or abbreviations. Please also note that these should be described fully the first time that they appear in the main text.

Answer:

This has been corrected. The new title is: Factors associated with basic and instrumental activities of daily life in elderly participants of a population-based survey in Norway. The Nord-Trøndelag Health Study, Norway”

- Please remove the titles "what is already known on this subject" and "what this study adds" as they are not part of journal format.

Answer:

This has been removed.

Reviewer(s)' Comments to Author:

Reviewer: 1

Reviewer Name: Ranmalee Eramudugolla  
Institution and Country: ANU, Australian National University  
Please state any competing interests: none declared

Please leave your comments for the authors below

The study data have the potential to make a useful contribution to our understanding of how functional dependence evolves in late life. However, the manuscript in its current form is very difficult to follow in

terms of 1) the rationale for conducting the study and the chosen variables; 2) analysis methods and how and why the sensitivity analyses were included, 3) the final findings.

At present, the study seems to present one viewpoint from introduction to half-way through the discussion, then, while discussing the sensitivity analyses, it becomes apparent that the statements made at the start of the discussion are not completely correct, making it difficult for the reader to understand the final message.

I suggest that the important methodological considerations such as survivor bias, attrition, and generalisability, that are currently mentioned only at the end of the discussion, be brought in as part of the introduction and integrated into the methods section. The findings presented at the start of the discussion should then incorporate what was found in the sensitivity analyses as well as the main analyses.

Answer:

That is a good suggestion. The methodological considerations are now introduced and described in greater detail in the methods section and the findings from extra analyses are presented in the results. The discussion incorporates the results and findings from sensitivity analysis.

Following are some specific comments:

1) It is unclear why sleep and sitting time should be investigated as risk factors for ADL and IADL dependence. The authors cite past literature that have found physical inactivity and depression to be risk factors for functional dependence. How would examining sleep and sitting time (which are correlated with physical inactivity and depression anyway) as predictors provide additional insights?

Answer:

As society and health behaviours change, sleep disturbances and sitting time are increasingly prevalent risk factors. These might obviously be predictors of ADL and IADL function in later life.

2) There are numerous measures of ADL and IADLs, some more comprehensive than others. The authors should examine the issue of measurement sensitivity in previous studies and in the present studies, particularly if there are recent population level changes in level of dependence in older adults. Is there any data on the relative sensitivity of the HUNT3 ADL and IADL measures and other measures such as Bayer IADLs, Lawton, Katz, Bristol etc...

Answer:

There are no studies measuring the relative sensitivity of the ADL and IADL measures used in HUNT compared to other measures. However, Grov (2010) found it useful to make a distinction between personal ADL and instrumental ADL in the HUNT Study, since these are associated with different variables.[1] The same authors state that the ADL and IADL measures used in HUNT correspond to Lawton and Barthel.[2] We are aware that there is found a large variation in the ADL/IADL questions used to measure functional capacity in hospitalized elderly.[3] Measurement sensitivity and validation of the ADL/IADL questions used in HUNT is beyond the scope of this article, but detailed information on the question texts and answer categories used in the HUNT Study are provided in the supplementary material to enable evaluation of the ADL/IADL measure used in this study.

3) What is the source of the self-rated health variable? Is it custom designed for HUNT3? there any data on how it compares to more standard measures such as the SF-36?

Answer:

The self-rated health variable used in the HUNT Study is used in a huge number of epidemiological studies internationally, with slight variations. Five or four answer categories are used. The variable is

also used in the COhort of NORway, CONOR.[4, 5] This is a collaboration that merges 10 different epidemiological studies in Norway through a common standardization. The HUNT Study and other Norwegian epidemiological studies have used 4 answer choices in order to get answers either in positive or negative direction,[6] whereas SF-36 has 5 answer categories[7] and SF-8 has 6 answer categories. The question text in SF-36 is "In general, would you say your health is:" whereas in HUNT it was "How is your health at the moment?" Variability in the question text, answer categories and combinations of the self-rated health question that needs to be taken into account when comparing results from different studies. The information needed to make such comparisons is provided in the supplementary material where a detailed description of the question text and answer categories in the HUNT Study can be found.

4) Were participants given examples of what constitutes low, and vigorous physical activity? Also, partial italicising of the sentences that define physical inactivity and activity makes it hard for the reader to separate it from the questions presented earlier. I recommend keeping them as regular text. Sample applies for page 7, line 45.

Answer:

The participants were given examples of what constituted low (not panting for breath or sweating) and vigorous physical activity (panting for breath or sweating) when they were asked to estimate a weekly average physical activity throughout the year. The exact wording of the questions and answer categories is now included in the supplementary material.

Keeping the definitions as regular text is a good point. Relocating the question texts to the supplementary material should also clarify this issue.

5) It is not clear whether individuals aged 70+ at baseline had ADL/IADL data, and whether these individuals were followed-up for HUNT3? There are several places where it is stated that no baseline data on ADL/IADL data was available (e.g., page 9, line 5; page 17, line 12), while in other places there is a suggestion that this statement is true only for a portion of the sample – please make this clear in the methods and keep it consistent all the way throughout. A flow chart of which group had what follow-up data would be useful.

Answer:

There was no baseline data on ADL/IADL in the 60-69 year age group, which was the study population in the main analysis (multinomial logistic regression). However we compared the ADL and IADL prevalence between 70+ years in HUNT2 to 70+ in HUNT3, since we did not have baseline ADL/IADL information in the study group 60-69 years (HUNT2). However, using two study groups was confusing, as pointed out by several of the reviewers. Therefore the ADL/IADL prevalences for the 70 year+ age group in HUNT2 have been removed from the manuscript in favour of a more detailed description of the ADL/IADL prevalences in HUNT3 (table 1). Further, a flow chart has now been included in the manuscript to clarify the selection of the study sample.

6) Why are the 70+ group data presented in Supplementary section and the 60-69 group in the main text?

Answer:

Table 2 was presented in the supplementary material due to its length. The general guidelines for preparing manuscripts to BMJ journals stated that tables larger than two pages would be published as supplementary material. Also see 5).

7) There should be a statement about how missing data was treated in the analyses and the rationale for this.

Answer:

Missing data due to mortality (survivor bias) and non-response in HUNT3 was included and evaluated by using a multinomial logistic regression model. Those missing information on ADL/IADL in HUNT3 were excluded in the analyses. This has been clarified in the manuscript and illustrated in a flow chart in the methods section.

8) How many separate multinomial logistic regressions were conducted (one for each IV adjusting for gender education, civil status and chronic disease?). Why was the latter variables chosen as covariates?

Answer:

That is correct. Separate multinomial logistic regression models were run for each independent variable, controlled for potential confounders, both with ADL and IADL as outcomes in separate models. In total 20 regressions.

Given the scope of this analysis we chose only to adjust for basic sociodemographic confounders that are time-invariant (age, sex, education). Marital status may change due to the death of a spouse, but for this age group marital status is to a large degree a static phenomenon. Chronic illness (lasting at least one year) also tends to be a static phenomenon that is not easily modified.

9) The authors present findings that depressive symptoms and self-rated health are 'risk factors' for ADL dependence, and that sleep disturbance, smoking, physical inactivity were 'lifestyle risk factors' for ADL/IADL dependence. I don't think one could present these as independent risk factors given that it is quite plausible that depression and sleep disturbance share variance in their relationship with ADL/IADL disturbance, and the same goes for self-rated health, physical inactivity and sitting time.

Answer:

The reviewer is of course correct in pointing out that there is shared variance between the various risk factors and lifestyle risk factors. They may nevertheless have a unique impact on ADL and IADL. For example, a previous study from HUNT found that depression and insomnia were independent risk factors for disability. <https://www.ncbi.nlm.nih.gov/pubmed/?term=18548833>

10) The authors conclude on page 16, line 5 "...we found a decrease in ADL dependency from decade to decade". It appears that this is really only relevant to the 70+ group in the sample. Please make this clear. The authors should also discuss the possible role of healthy survivor bias in the observed decrease in ADL/IADL dependence in the 70+ group, and the lack of baseline data for 60-69 year olds to make such a conclusion about the younger age group.

Answer:

That is correct, we observed a decrease in ADL-dependency for the 70+ age group and the healthy survivor bias is highly relevant. See answer to 5).

11) The utility of the findings to practice and understanding population changes in ADL/IADL is not clear.

Answer:

See 5) about population changes in ADL/IADL. The paragraph about implications for policy makers and clinicians has been elaborated and clarified (page 22).

Reviewer: 2

Reviewer Name: Agnieszka Młynarska

Institution and Country: Electrocardiology Department, Upper Silesian Heart Center, Poland

Please state any competing interests: None declared

Please leave your comments for the authors below

The presented to the review research addresses the problem of the need for assistance in the basic and advanced activities of the daily life in elderly Norwegians.

The aim of the study is to demonstrate the factors that influence the increase in the need to assistance for the elderly patient.

1. The results section was written in an unfavorable way.

Answer:

The section has been re-written under the consideration of the comments from all reviewers.

2. Problems with links to tables in the text of manuscript. Unreadable results in tables.

Answer:

The same link should appear in the text and in the table (ex: Table 1 in both the table and the text). The numbers in the results tables have been formatted to a larger size for better readability.

3. Over 14% of the presented data is incomplete. With such a large group of people, incomplete records should be withdraw from analysis.

Answer:

Several reveiwers have commented on this. The analyses included only complete cases. The encoding of the ADL/IADL variables was based on those being ADL/IADL independent and unfortunately a few who did not report ADL/IADL independence in any of the activities had been mistakenly recorded as missing. This has been corrected and the percentage of missing is 13% (table 1).

4. Table 1 - it is worth showing in these groups whether the differences were statistically significant.

Answer:

Since we did not have ADL/IADL information on the baseline study group (60-69 years), we compared the ADL and IADL prevalence between 70+ years in HUNT2 to 70+ in HUNT3. However using two study groups was confusing, as pointed out by several of the reviewers. Therefore, we chose to omit the ADL/IADL values for HUNT2 as this was not a part of the main anlysis.

5. Table 2 - Not all abbreviations have been explained.

Answer:

All abbreviations should now be explained below each table.

6. The HADS-A and HADS-D scale – additional analysis should be performed - 16 points and above indicates a significantly elevated level of anxiety and depression (this subpopulation should be analyzed.

Answer:

We have only seen litterature supporting cut-offs of HADS-A and HADS-D  $\geq 8$  and HADS-total  $\geq 19$ . [8] It would be of great interest to see studies supporting  $\geq 16$  as a cut-off.

7. Assessing sleep through a question about how many hours you spend in bed can be misleading. The limitation of work is the lack of a standardized questionnaire for assessing quality of life and sleep disorders.

Answer:

This has been included as a limitation of the study. The sleep variables in HUNT have previously been used in a wide range of studies on insomnia.

8. No results on average values ADL and IADL – the average values would be helpful.

Answer:

The average number of ADL / IADL disabilities is now included in the descriptive statistics in table 1.

9. Statistics - The RRR used in the statistics section is not properly used. This statistic defines relative risk reduction.

Answer:

The software producer Stata reports output in multinomial logistic regression models as relative risk ratios (RRR). The multinomial analyses are based on logistic regression models, so the correct term would be odds ratio (OR). RRR has been replaced with OR in the manuscript.

Reviewer: 3

Reviewer Name: Hanna Falk

Institution and Country: Institute of Neuroscience and Physiology, Sahlgrenska Academy at the University of Gothenburg.

Please state any competing interests: No competing interests to disclose.

Please leave your comments for the authors below

This is an interesting manuscript! However, I have some questions/concerns. Please see the attached document. Questions/concerns are underlined and in bold.

## SIGNIFICANCE

- ☐ Norway can expect over 1.3 million people over 70 years old in 2060.
- ☐ Evidence is equivocal, but increasing prevalence of chronic diseases is likely to cause functional disability and increased public expenditures and strain on health care systems.
- ☐ Functional ability and health is the most essential dimensions of quality of life and wellbeing of older adults.
- ☐ It is therefore of interest to maintain good health and high functioning into old age, both for older adults themselves and for the society as a whole. Good!

## METHOD

- ☐ The aim of this study was to investigate factors predicting the need for assistance for activities in daily life (ADL) and instrumental ADL (IADL) among 5050 60-69-year olds and to explore gender differences and changes in ADL/IADL function from 1995-97 (response rate 69.5%) to 2006-08 (response rate 54.1%).
- ☐ Dependent variable = needing assistance (yes/no) in one or more ADL (indoor mobility, toileting, bathing, dressing, bed-rise, and feeding) or IADL (food preparation, light housework, heavier housework, laundry, shopping, economy, medication, go outdoors, and public transportation).
- ☐ The authors need to clarify what they mean by "needing assistance". Do they refer to help from another person? Did they consider level of difficulty in

carrying out the ADL/IADL?

Answer:

“Needing assistance” is defined as needing assistance from another person in daily life routines. This was a dichotomous question in HUNT3 with answer categories “yes” and “no” and thus we did not have information to consider the level of difficulty. This has been clarified in the manuscript, and the exact wording of the questions is included in the supplementary file.

□ Independent variables =

- o Self-rated health (SRH) dichotomized as “good” and “poor”.
- o Life-satisfaction dichotomized as “satisfied” and “dissatisfied”.
- o Anxiety and depression using HADS-A and HADS-D. The authors need to describe how they used the scale (i.e. continuous or dichotomized into cases/non-cases of anxiety and depression). This becomes obvious once the reader gets to Table 1 but I would prefer if the authors described it in the methods section.

Answer: this has now been clarified in the manuscript.

- o Physical inactivity dichotomized into physically inactive (less than three hours of light physical activity and no hard physical activity per week) and physically active (more than three hours of light physical activity or any hard physical activity per week)
- o Problematic alcohol behavior defined as answering yes to two or more of the CAGE-questions.
- o Sleep dichotomized into “sleeping less than six hours or more than ten hours” and “sleeping seven to nine hours”.
- o Sitting time dichotomized into “sitting 0-7 hours” and “sitting 8 hours or more”.
- o Social participation was dichotomized into never, or only a few times a year and participate.
- o Smoking was dichotomized into daily smoker and not daily smoker.

## STATISTICS

- o Multinomial logistic regression models investigating the effect of; 1) lifestyle risk factors (smoking, alcohol, physical inactivity, sitting time, sleep and social participation), 2) depression, anxiety, self-rated health and life satisfaction on functional disability in ADL and IADL 11 years later.

## RESULTS

- Baseline information on ADL and IADL dependence was not available in 1995-97. To correct this weakness of the study, the authors performed a sensitivity analysis including those answering having good self-rated health and good life satisfaction at baseline. Does this mean that the authors assume that those having good SRH and good life satisfaction were NOT functionally disabled in 1995-97? I find the lack of baseline ADL/IADL data problematic. Especially in relation the aim of this study, i.e. to investigate factors predicting the future need for assistance in ADL/IADL and to explore gender differences and changes in ADL/IADL function from 1995-97 to 2006-08. This needs extensive clarification and justification. In addition 14-17% did not have ADL/IADL data in the followup phase in 2006-08. Did the authors exclude these individuals?

Answer:



Those lacking outcome ADL/IADL were excluded from the analyses. This is clarified in the manuscript and illustrated by the flow chart included in the methods section.

The encoding of the ADL/IADL variables was based on those being ADL/IADL independent and unfortunately a few who did not report ADL/IADL independence in any of the activities had been mistakenly recorded as missing. This has been corrected and the percentage of missing is 13% (table 1).

Regarding the sensitivity analysis, see our answer below (in the discussion section).

□ The analyses were adjusted for age, sex, education, marital status and chronic disease – factors predicting the future need for assistance in ADL (and death and nonparticipation at follow-up) was poor self-rated health, depression, short or prolonged sleeping time (“sleeping less than six hours or more than ten hours”), smoking and physical inactivity. Factors predicting the future need for assistance in IADL (and death and non-participation at follow-up) were poor self-rated health, poor life satisfaction, depression, physical inactivity, short or prolonged sleeping time and excessive sitting time.

## DISCUSSION

□ Did the authors control for cognitive impairment/exclude cases of dementia?  
Especially important since the ADL and IADL data is self-assessed.

Answer:

A very good point! Unfortunately we did not have enough information to control for this. Participants had to fill out a questionnaire at home and meet in person for the second part of the survey. This would have been difficult for persons with advanced dementia / cognitive impairment.

□ Although the authors raise the missing ADL/IADL data at baseline as a limitation of the study, the sensitivity analysis still needs clarification. How did this analysis minimize this bias? While SRH is a subjective indicator of health status, it has also been found to be a strong predictor of mortality, as it integrates biological, mental, social and functional aspects of a person, including individual and cultural beliefs and health behaviors. It is an all-inclusive, sensitive yet nonspecific measure that assesses health and predicts health outcomes in ways that are still unclear, and not necessarily identical with objective health status. Lay definitions of health take a wide range of factors into account, and most individuals rate their health as moderate to good. The rating is based on a cognitive process that is both contextual and subjective in that it incorporates physiological and emotional experiences and expectations, influenced by the social environment of the individual. Constituent parts of health known to influence SRH, and subsequent mortality, include chronic illness, depression, cognitive function, socioeconomic status, functional impairment, and physical activity. Please clarify.

Answer:

We performed this sensitivity analysis to determine the robustness of our findings given the lack of ADL/IADL at baseline. This check is by no means perfect, it does not eliminate any bias, but it was the best alternative given available data.

□ We know that ADL disability is found to increase with age, that it decrease over time and is highly related to physical inactivity, and that women have higher risk

of developing functional disability than men. Women also self-rate their health as poorer than do men which might deserve mentioning in the discussion section of this paper. In addition the life-prevalence of depression is greater amongst women which also deserve elaboration in the discussion section, especially in relation to the aim (i.e. to explore gender differences and changes in ADL/IADL function from 1995-97 to 2006-08).

Answer:

The aim of exploring gender differences referred to Table 2 (comparing gender specific ADL and IADL prevalences in HUNT2 and HUNT3). This table has been removed since introducing ADL and IADL prevalences on a separate study group in HUNT2 caused confusion, as pointed out by several reviewers. Gender was adjusted for in the multinomial logistic regression.

□ The authors state that the results from this study provide new insights. I would like to see a more mature discussion section where the authors really make use of previous knowledge and relate it to their findings. For example, there are known relationships between sleep disturbance and depression, between inactivity and depression, between inactivity and ADL/IADL disability, between poor SRH and ADL/IADL disability and so forth. Please develop.

Answer:

The discussion has been altered and developed.

Reviewer: 4

Reviewer Name: Haiqun Lin

Institution and Country: Yale University, USA

Please state any competing interests: None

Please leave your comments for the authors below

The study design in dealing with left truncation of the sample may have flaw. The definition of the outcomes were not clearly described in terms of assessment time and elapsed time.

This manuscript investigated the risk factors for ADL/IADL dependence in Norwegian population. The findings on the significant association of self-rated health, depression, sleep time, smoking, and physical inactivity with ADL/IADL dependence are in line with previous studies regarding these risk factors in various northern European populations.

(1) Many abbreviations were used in the abstract without introducing them first, please either avoid abbreviations in abstract or write them out.

Answer:

This has been corrected, all abbreviations are now written out.

(2) HUNT2 and HUNT3 samples need to be described in terms how they were selected from HUNT. The description of the dependent and the independent variables lack enough detail regarding their timing.

Answer:

A flow chart has been included to illustrate the selection of the study sample. Table 1 has been edited to clarify the timing of the variables. A more detailed explanation follows (which has also been included in the manuscript):

The samples were selected based on age in HUNT2 (60-69 years) and participation in HUNT2 AND HUNT3. The independent variables (lifestyle risk factors, self-rated health, quality of life, anxiety and depression) as well as covariates (marital status, age, gender, education and chronic illness) were measured in 1995-1997, in the HUNT2-survey. The dependent variables (ADL and IADL) were measured in 2006-2008 (in the HUNT3-survey).

(3) The non-participation (mortality or non-response) were dealt as separate outcomes. How a multivariate analysis treating death and ADL/IADL dependence as joint outcomes would affect the findings?

Answer:

When performing the multinomial analysis with a joint outcome all effect estimates lie in between the effect estimates for the separate outcomes ADL / IADL disability and death before HUNT3. We notice that some authors present a joint outcome of ADL / IADL disability and death as a general measure of functional decline. However, since we already present numbers for death as a separate outcome, this extra analysis would not provide additional information as we see it.

(4) Timing of ADL/IADL dependence and death were not taken into account. Since there was no information on ADL/IADL status at baseline, the sample could be reanalyzed as a one with left-censoring and truncation.

Answer:

Given the design of our study we were unable to incorporate a time dimension in the analysis. We have two cross sections 11 years apart in which a number of individuals participated in both. The timing of both dependent and independent variables are fixed to these time points.

(5) Timing of independent predictors was not clear either. The time elapsed between a measurement of independent predictor and the detection of ADL/IADL dependence should be taken into account.

Answer:

See (4).

Reviewer: 5

Reviewer Name: Philip Schluter

Institution and Country: University of Canterbury, New Zealand

Please state any competing interests: None declared

Please leave your comments for the authors below

An interesting paper that reports research findings that are solidly undertaken. Well done. Some comments follow:

[1] Page 5, line 10. What does 'fairly representative' actually mean? And why is it important here? Should more focus be on internal validity and then external validity? (Maybe see, for example: Kukull WA, Ganguli M. Generalizability: the trees, the forest, and the low-hanging fruit. *Neurology*, 2012;78(23):1886-91).

Answer:

The reference shows that the study population on average is comparable to the total Norwegian population in many aspects. The health trends in the county population follow the national trends closely. A comparison between the HUNT participants and the Norwegian population has been addressed in the paper (page 6).

[2] I would have preferred more detail on the analysis and results than all the detail on the independent variables (which perhaps could be relegated to the supplementary materials).

Answer:

Good comment. The details of the independent and dependent variables are now described in the attached supplementary material. The methods and results section has been extended to describe the sensitivity analysis in more detail.

[3] I would have enjoyed a participant flow diagram in the RESULTS - as per the STROBE guidelines - as I struggled to track the participants at each stage. Retention seemed low at 54.1% - which surely must introduce considerable potential for bias. I would also have enjoyed seeing the numbers who died and the numbers not participating (withdrawing and not found) at each stage? (By-the way, how were 'deaths' determined?)

Answer:

A flow diagram of the selection of participants is now included in the manuscript. The HUNT study is regularly updated with administrative national records containing information on current status (died, emigrated, alive) from which we obtained information on death.

The participation rate in HUNT3 has been evaluated in a non-participation study[9] and is discussed in discussion section (page 17).

[4] Page 8, line 50. Multinomial logistic regression is probably fine. I was interested as to whether competing risk models were considered? And whether any interactions or stratifications (i.e. men vs. women) were thought about? I assume that death and nonparticipation was related to some of the variables in Table 1 (on page 9) - and that these dependencies may affect the reported findings. Perhaps consideration of a table of this in the supplementary materials might be useful?

Answer:

Multinomial regression models were specified and in some sense they may be viewed as competing risk models because they allow different outcomes to be modelled simultaneously. Key to interpretation is to look at the effects of predictors simultaneously on the outcomes. A competing risk model in the traditional sense, with information on time-to-event was impossible given available data.

We considered stratification but the samples would become so small that we choose rather to control for gender in the analyses. Information on death and non-participation is included in tables 1-4. We did not specify models with interaction terms primarily because we had no a-priori reasoning for doing it.

[5] The criteria is not defined for what constitutes ranking of risk factors. On page 15, line 38 it is stated that poor self-rated health and depression were the strongest. But I was not clear how that conclusion was arrived at.

Answer:

The effect estimates defined the rank of risk factors. Poor self-rated health and depression had the highest RRR for both ADL and IADL.

[6] Why was the gender breakdown not presented in Table 1 for HUNT 3? Also in the pdf version I had access too - some columns were blank. I assume this was an issue with the conversion process. Also some of the labels used inequalities that excluded a value (i.e. HADS-A had outcomes '<7' and '>8'. Which group would a person with HADS-A =8 be placed? Maybe another conversion problem?)

Answer:

The gender breakdown was not presented for HUNT3 because it was the same as in HUNT2. However, since we presented education which was also a constant variable, we should have completed gender the variable for HUNT3. Anyhow, Table 1 has now been altered to clarify the timing of the variables and omit descriptive statistics not relevant to the main analysis. HADS-A and HADS-D should be  $\leq 7$  and  $\geq 8$ . A person with HADS-A = 8 would be placed in the 8-16 group, defined as having anxiety.

[7] No diagnostic or goodness-of-fit details were presented - nor was what defined statistical significance. These seemed to be assumed.

Answer:

The critical level of statistical significance was set at 0.05 (5%), this has been explicitly stated in the methods section. We have considered outliers and influential points in the models. The HUNT Databank runs continuous quality assurance of the data, reducing the risk for erroneous and extreme observations. Age was the only continuous variable included in the models and the Box-Tidwell Test was used to test the linearity assumption between the continuous variable (age) and log odds. The aim of this study was to investigate the effects of separate lifestyle and health variables on activities of daily living, and with such a large number of analyses we do not see it as purposeful to report goodness-of-fit for each individual model.

[8] Page 28. The STROBE statement check sheet did not seem to be completed. I think it should, and would benefit the revised paper.

Answer:

Thank you for notifying us about this. The checklist was completed when the paper was submitted, but we will double check it again before submitting the revised paper.

Reviewer: 6

Reviewer Name: Jacob Dreier, MD MPH, PhD

Institution and Country: Ben-Gurion University of the Negev, Beer Sheva, Israel

Please state any competing interests: None declared

Please leave your comments for the authors below

This is a well-written manuscript describing factors associated with ADL and IADL in elderly participants of a population-based survey in Norway. I was asked to perform a statistical review of the manuscript.

1. The main limitations of the study are the possibility of survival bias in the later study, the potential for volunteer bias due to the obvious nature of the study, and the age of the data (the most up-to-date survey was in 2006, over than a decade ago). The first bias is addressed by the statistical analysis addressing the issue of competing risks. The second bias is briefly discussed. I would recommend a comparison of the study population to the general Norwegian population, just to get an idea of the differences between them, in factors available from national data, e.g. the percentage living at home, smoking or physically active. The third point should be addressed.

Answer:

A comparison between the HUNT participants and the Norwegian population has been addressed in the paper (page 6). The most up-to-date survey was in 2006, over than a decade ago. However, we believe the findings in this study are general and still valid.

2. I am not familiar with the term "relative risk ratio". The generic term "relative risk" is imprecise; It could be either a risk ratio (based on cumulative incidence) or a rate ratio (based on incidence density). In the present case, risk ratio is the correct term.

Answer:

The software producer Stata reports output in multinomial logistic regression models as relative risk ratios (RRR). The multinomial analyses are based on logistic regression models, so the correct term would be odds ratio (OR). RRR has been replaced with OR in the manuscript.

Additional minor comments:

3. Sleep was grouped into "<6 or >10 hours" vs. "7-9 hours". How would a study participant reporting 6 or 10 hours be treated? It should be defined as "≤6 or ≥10 hours" vs. "7-9 hours".

Answer:

Good comment. A study participant reporting 6 or 10 hours would be placed in too short or too long sleeping time. ≤6 or ≥10 hours is correct. This has been corrected in the manuscript.

4. Regarding Table 1

a. Rather than pointing out missing data as a separate line, I would indicate the number of patients with data and then calculate the percentage out of valid answers.

Answer:

The valid percentage has been added to the numbers and the total number, including the percentage responded (out of the total n = 5050).

b. Abbreviations such as SRH or CAGE should be spelled out at the footnote, as in Table 3.

Answer:

An explanation of the abbreviations used was missing in table 1, this has now been added.

c. I would add a column indicating the statistical significance of differences between the two surveys.

Answer:

Since we did not have ADL/IADL information on the baseline study group (60-69 years), we compared the ADL and IADL prevalence between 70+ years in HUNT2 to 70+ in HUNT3. However using two study groups was confusing, as pointed out by several of the reviewers. Therefore, we chose to omit the ADL/IADL values for HUNT2 as this was not a part of the main analysis.

d. In the second part of Table 1, the Table suddenly changes from comparing HUNT-2 and HUNT-3 to listing the percentage of participant with ADL/IADL dependence. This is problematic. I suggest splitting the Table into two or diving it some other way.

Answer:

Also see answer to d. We chose to omit the ADL/IADL values for HUNT2 as this was not a part of the main analysis. The table now includes the independent variables (HUNT2), adjustment variables (HUNT2) and outcome variables (HUNT3). Hopefully it is more intuitive this way.

5. In Table 2, there is no need to list the percentage without difficulty in addition to the percentage of participants with difficulties in performing the various ADLs / IADLs, as these columns always add up to 100%. Decreasing the number of columns will increase the readability of the Table. In addition, I would add a columns showing the p for trend over the years for men and women.

Answer:

This table has been removed (see 4 c and d).

6. In Table 3, I suggest writing "Non-participation in Hunt-3" as to separate it from social non-participation.

Answer:

Good point, this has been corrected in the manuscript.

7. Regarding the interpretation of findings in Table 3, depression was not statistically significant as a risk factor for dependence in ADL. Similarly, the three lifestyle factors listed were not statistically significant (page 13).

Answer:

The reviewer is of course correct when pointing this out. There is a great deal of controversy over the so-called "Null Hypothesis Significance Testing" (NHST). Statistical significance does not automatically equate to scientific or human significance and given the effect sizes for these risk factors, we chose to interpret them as important regardless of our failure to reject the null hypothesis of no effect.

8. The sensitivity analysis described on pages 17-18 should be moved to the Results section.

Answer:

The sensitivity analysis is now described in the methods section and the results presented in the results section.

9. In references 1 and 3, the year is missing. Reference 2 is written in Norwegian. A translation to English of the publication's name should be written, usually in square brackets, with the words "paper in Norwegian" or "(Norwegian)" next to it.

Answer:

Well spotted. This was an unfortunate mistake that has been corrected.

1. Grov, E.K., S.D. Fossa, and A.A. Dahl, Activity of daily living problems in older cancer survivors: a population-based controlled study. *Health Soc Care Community*, 2010. 18(4): p. 396-406.
2. Grov, E.K., S.D. Fossa, and A.A. Dahl, A controlled study of the influence of comorbidity on activities of daily living in elderly cancer survivors (the HUNT-3 survey). *J Geriatr Oncol*, 2017. 8(5): p. 328-335.
3. Buurman, B.M., et al., Variability in measuring (instrumental) activities of daily living functioning and functional decline in hospitalized older medical patients: a systematic review. *J Clin Epidemiol*, 2011. 64(6): p. 619-27.
4. Magnus, P., et al., CONOR - COhort NORway: historie, formål og potensiale. *Norsk Epidemiologi*, 2003. 13(1): p. 79-82.
5. About CONOR - data from several regional health studies. 2016 [cited 2017 20.10]; Available from: <https://www.fhi.no/en/studies/conor/about-conor---data-from-several-regional-health-studies/>.
6. HUNT Web Databank; Health variable. [cited 2017 20.10]; Available from: <https://hunt-db.medisin.ntnu.no/hunt-db/-/variable/2933>.



7. 36-Item Short Form Survey Instrument (SF-36). [cited 2017 20.10]; Available from: [https://www.rand.org/health/surveys\\_tools/mos/36-item-short-form/survey-instrument.html](https://www.rand.org/health/surveys_tools/mos/36-item-short-form/survey-instrument.html).
8. Bjelland, I., et al., The validity of the Hospital Anxiety and Depression Scale. An updated literature review. J Psychosom Res, 2002. 52(2): p. 69-77.
9. Langhammer, A., et al., The HUNT study: participation is associated with survival and depends on socioeconomic status, diseases and symptoms. BMC Med Res Methodol, 2012. 12: p. 143.

## VERSION 2 – REVIEW

<b>REVIEWER</b>	philip schluter University of Canterbury, New Zealand
<b>REVIEW RETURNED</b>	22-Nov-2017

<b>GENERAL COMMENTS</b>	A major improvement. Thank you for the opportunity to review the paper.
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<b>REVIEWER</b>	Agnieszka Młynarska Department of Internal Nursing, Chair of Internal Medicine, School of Health Sciences , Medical University of Silesia , Katowice , Poland.
<b>REVIEW RETURNED</b>	30-Nov-2017

<b>GENERAL COMMENTS</b>	<p>The presented to the review research addresses the problem of the need for assistance in the basic and advanced activities of the daily life in elderly Norwegians.</p> <p>The aim of the study is to demonstrate the factors that influence the increase in the need to assistance for the elderly patient.</p> <ol style="list-style-type: none"> <li>1. The results section was written in an unfavorable way.</li> <li>2. No data available ADL and IADL in patients aged 60-69. With such a large group of people, incomplete records should be withdraw from analysis.</li> <li>3. The HADS-A and HADS-D scale – additional analysis should be performed - 16 points and above indicates a significantly elevated level of anxiety and depression (this subpopulation should be analyzed).</li> <li>4. The link in the analysis of data for patients with short sleep and those assessing sleep over 10 hours is not valid. Assessing sleep through a question about how many hours you spend in bed can be misleading. The limitation of work is the lack of a standardized questionnaire for assessing quality of life and sleep disorders.</li> <li>5. No results on average values ADL and IADL – the average values would be helpful.</li> <li>6. Style of language in methods should be simple without emotional components of the authors.</li> </ol>
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<b>REVIEWER</b>	Ranmalee Eramudugolla ANU, Australia
<b>REVIEW RETURNED</b>	04-Dec-2017

<b>GENERAL COMMENTS</b>	<p>The manuscript is much improved.</p> <ol style="list-style-type: none"> <li>1. The abstract does not mention that the study also aimed to</li> </ol>
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	<p>examine the association between sleep disturbance and functioning, as well as sitting time and functioning.</p> <p>2. The Introduction section reads really well now.</p> <p>3. The methods section is much clearer. I suggest introducing the Hunt study (paragraph 2 under Materials) first, before describing how the sample for this particular study was selected from the Hunt cohorts.</p> <p>4. Page 6 line 46 "The score was dichotomised where and a score of 8 or more (out of 21) was defined as a case of anxiety or depreesion" - delete "and". Also, depression is spelled incorrectly.</p> <p>5. Throughout the manuscript I would avoid using the term "effect" when discussing the relationship between predictors and outcome, as the study did not manipulate the independent variables to effect change in the outcome - rather observed their association with the outcome. Effect implies causality.</p> <p>6. Each paragraph in the Results section looks like the caption for the Table above it. It confused me at first. Perhaps begin each paragraph with an opening sentence before referring to the Table. The sentence could orient the reader to the specific research question the analyses examined.</p> <p>7. Page 17, Line 44 - the term "insignificant" is confusing when used in a statistical context - does this mean that it was non-significant (i.e., no association), or a statistically significant association that was small in magnitude / not significant in practical terms?</p> <p>8. Page 19, paragraph 2 - If there is a U-shaped relationship between alcohol use and functional disability (with greater disability affecting abstainers and heavy users), then the focus on problem drinkers would bias results toward finding an association with functional disability. However a very small number of participants in this category would produce large confidence intervals. A sentence is needed to clearly explain why alcohol was not found to be associated with functional disability in the Hunt study. Currently the reason is implied but not stated.</p> <p>Also, a new paragraph should be started at line 42 where the discussion abruptly moves to social participation.</p> <p>9. Page 19, line 48 "methods and measurements [of] activities of daily living..."  line 51 "...found [to] vary..."  line 52 "for the for" - delete second "for"</p> <p>10. Variable methods of measurement of ADLs and IADLs does not in itself hamper comparison of findings if all of them measure the same underlying construct and have similar levels of sensitivity in doing so...</p> <p>11. Although the authors provided a response to my earlier query about the rationale for looking at sleep and sitting time as potential predictors of functional disability - I cannot see such a rationale in the introduction or discussion. The reader would still be left wondering why these factors were included and what the findings mean. At least a couple of sentences are required on why reduced or excessive sleep would be associated with functional disability.</p>
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<b>REVIEWER</b>	Jacob Dreier Ben-Gurion University of the Negev Beer Sheva, Israel
<b>REVIEW RETURNED</b>	10-Dec-2017

<b>GENERAL COMMENTS</b>	<p>The authors have taken care of replying to all my previous comments. I have some very minor corrections to the English that could be taken care of at the proofreading stage without need for review.</p> <ol style="list-style-type: none"> <li>1. In the Abstract, replace "676/693 were excluded" with "676 of 693 individuals were excluded..."</li> <li>2. In the Abstract, replace "with ORs of 2.30 (1.93-2.74), 2.29 (1.86-2.81)," with "with ORs of 2.30 (1.93-2.74), and 2.29 (1.86-2.81), respectively," .</li> <li>3. In the Methods section, I would change "material" to "study participants".</li> <li>4. In the Methods section, Variables paragraph - replace "containing information on current status" with "containing information on current vital status"</li> <li>5. In Table 2, change "non participation HUNT3" to "non participation in HUNT3"</li> </ol>
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<b>REVIEWER</b>	Hanna Falk Institute of Neuroscience and Physiology, Sahlgrenska Academy at University of Gothenburg, Gothenburg, Sweden
<b>REVIEW RETURNED</b>	12-Dec-2017

<b>GENERAL COMMENTS</b>	<p>Dear authors, I have read your revised manuscript and I am satisfied with your answers to my comments. Congratulations to a job well done and to an important contribution to the knowledge base on functional ability in older adults.</p>
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## VERSION 2 – AUTHOR RESPONSE

Dear Assistant Editor, Hemali Bedi

We are pleased to submit the second revision of the article entitled “Factors associated with basic and instrumental activities of daily living in elderly participants of a population-based survey; the Nord-Trøndelag Health Study, Norway” for consideration for publication in the BMJ Open. We appreciate the time and thorough work by the reviewers and their helpful comments have further improved the manuscript. The response to the reviewers’ comments is attached below. The revised version of the manuscript is attached as a separate document. Thank you for your consideration and we look forward to hear from you again.

Yours sincerely,

Siri Høivik Storeng  
Medical Student Research Programme  
Norwegian University of Science and Technology, Trondheim, Norway

Authors' responses to the reviewers' comments:

Reviewer: 1

Reviewer Name: Ranmalee Eramudugolla

Institution and Country: ANU, Australia

Please state any competing interests: none declared

Please leave your comments for the authors below

The manuscript is much improved.

1. The abstract does not mention that the study also aimed to examine the association between sleep disturbance and functioning, as well as sitting time and functioning.

Answer:

This should of course be stated and it has now been included in the abstract.

2. The Introduction section reads really well now.

3. The methods section is much clearer. I suggest introducing the Hunt study (paragraph 2 under Materials) first, before describing how the sample for this particular study was selected from the Hunt cohorts.

Answer:

We agree that this would improve the logical structure of the methods section. The paragraph about the HUNT Study has been moved to the start of the section.

4. Page 6 line 46 "The score was dichotomised where and a score of 8 or more (out of 21) was defined as a case of anxiety or depreesion" - delete "and". Also, depression is spelled incorrectly.

Answer:

Well spotted, "and" has been removed and the spelling of depression is corrected.

5. Throughout the manuscript I would avoid using the term "effect" when discussing the relationship between predictors and outcome, as the study did not manipulate the independent variables to effect change in the outcome - rather observed their association with the outcome. Effect implies causality.

Answer:

The reviewer is of course correct in that the correct term is association and not effect in this observational study. "Effect" has been replaced with "association" throughout the manuscript.

6. Each paragraph in the Results section looks like the caption for the Table above it. It confused me at first. Perhaps begin each paragraph with an opening sentence before referring to the Table. The sentence could orient the reader to the specific research question the analyses examined.

Answer:

The caption for the table in the start of each sentence has been replaced with a more detailed description of the analyses. In addition we have improved the table texts.

7. Page 17, Line 44 - the term "insignificant" is confusing when used in a statistical context - does this mean that it was non-significant (i.e., no association), or a statistically significant association that was small in magnitude / not significant in practical terms?

Answer:

That is a good comment. It was not significant in practical terms, but this was not explicitly stated and it would easily be confused with statistical significance. The sentence has been revised under consideration of the comments from all reviewers. It is now written as:

"Problematic alcohol behaviour and low social participation were not associated with ADL and IADL disability."

8. Page 19, paragraph 2 - If there is a U-shaped relationship between alcohol use and functional disability (with greater disability affecting abstainers and heavy users), then the focus on problem drinkers would bias results toward finding an association with functional disability. However a very small number of participants in this category would produce large confidence intervals. A sentence is needed to clearly explain why alcohol was not found to be associated with functional disability in the Hunt study. Currently the reason is implied but not stated.

Also, a new paragraph should be started at line 42 where the discussion abruptly moves to social participation.

Answer:

The explanation should of course be clearly stated and not only implied in the manuscript. A sentence explaining why alcohol was not found to be association with functional disability in the HUNT Study has now been added on page 20.

Alcohol and social participation, which neither were found to be associated with functional disability in our results, were discussed in the same paragraph. But we agree with the reviewer that it improves readability if social participation is moved to a separate paragraph. This alteration has been done in the manuscript on page 20.

9. Page 19, line 48 "methods and measurements [of] activities of daily living..."

line 51 "...found [to] vary..."

line 52 "for the for" - delete second "for"

Answer:

We appreciate the through work from the reviewer in improving the manuscript. The language in these sentences has been corrected.

10. Variable methods of measurement of ADLs and IADLs does not in itself hamper comparison of findings if all of them measure the same underlying construct and have similar levels of sensitivity in doing so...

Answer:

The reviewer is of course correct in that variable methods of ADLs and IADLs do not itself hamper comparison if they measure the same construct and have similar sensitivity. The article also referred to measures of functional decline other than ADL and IADL. Therefore we have removed "activities of daily living" from the sentence and only included "heterogeneity in methods of measurement of functional decline."

11. Although the authors provided a response to my earlier query about the rationale for looking at sleep and sitting time as potential predictors of functional disability - I cannot see such a rationale in the introduction or discussion. The reader would still be left wondering why these factors were included and what the findings mean. At least a couple of sentences are required on why reduced or excessive sleep would be associated with functional disability.

Answer:

The reviewer is of course correct in that this should also be included in the manuscript. The rationale is now explained as follows the introduction (see page 5):

"These are known risk factors for mortality and morbidity[1-7] and are also likely to be associated with disability since disability is a part of the continuum of population health change from risk factors, diseases, loss of function, disability and mortality.[8]"

Reviewer: 2

Reviewer Name: Agnieszka Młynarska

Institution and Country: Department of Internal Nursing, Chair of Internal Medicine, School of Health Sciences , Medical University of Silesia , Katowice , Poland.

Please state any competing interests: no

Please leave your comments for the authors below

The presented to the review research addresses the problem of the need for assistance in the basic and advanced activities of the daily life in elderly Norwegians.

The aim of the study is to demonstrate the factors that influence the increase in the need to assistance for the elderly patient.

1. The results section was written in an unfavourable way.

Answer:

The results section has now been edited under consideration of the response from all the reviewers.

2. No data available ADL and IADI in patients aged 60-69. With such a large group of people, incomplete records should be withdraw from analysis.

Answer:

Those missing on all ADL/IADL questions in HUNT3 were defined as missing (676 and 693 individuals on ADL and IADL). These have been excluded from the analysis. We did not have baseline information of ADL and IADL for any of the 60-69 year olds in HUNT2. Hopefully this was clarifying.

3. The HADS-A and HADS-D scale – additional analysis should be performed - 16 points and above indicates a significantly elevated level of anxiety and depression (this subpopulation should be analyzed).

Answer:

We have only seen literature supporting cut-offs of HADS-A and HADS-D  $\geq 8$ , for the optimal balance between sensitivity and specificity. See for example Bjelland I, Dahl AA, Haug TT, Neckelmann D. The validity of the Hospital Anxiety and Depression Scale. An updated literature review. J Psychosom Res. 2002;52(2):69-77.

It would be of great interest to see references recommending a cut-off of 16 points.

4. The link in the analysis of data for patients with short sleep and those assessing sleep over 10 hours is not valid. Assessing sleep through a question about how many hours you spend in bed can be misleading. The limitation of work is the lack of a standardized questionnaire for assessing quality of life and sleep disorders.

Answer:

Hours of sleep have been found to be associated with mortality and health outcomes. See for example:

Gallicchio L, Kalesan B. Sleep duration and mortality: a systematic review and meta-analysis. *J Sleep Res.* 2009;18(2):148-58

Cappuccio FP, D'Elia L, Strazzullo P, Miller MA. Sleep duration and all-cause mortality: a systematic review and meta-analysis of prospective studies. *Sleep.* 2010;33(5):585-92.

Cappuccio FP, Cooper D, D'Elia L, Strazzullo P, Miller MA. Sleep duration predicts cardiovascular outcomes: a systematic review and meta-analysis of prospective studies. *Eur Heart J.* 2011;32(12):1484-92.

The lack of a standardized questionnaire for assessing quality of life and sleep disorders has been addressed as a limitation in the manuscript, see page 19.

5. No results on average values ADL and IADL – the average values would be helpful.

Answer:

The average values of ADL and IADL were included in table 1 (showing descriptive statistics of the study population) during the previous revision of the manuscript. See page 9 and 10 in the manuscript.

6. Style of language in methods should be simple without emotional components of the authors.

Answer:

We have revised the methods section taking into consideration comments from all the reviewers. We hope that the language in the methods section is satisfactory.

Reviewer: 3

Reviewer Name: Hanna Falk

Institution and Country: Institute of Neuroscience and Physiology, Sahlgrenska Academy at University of Gothenburg, Gothenburg, Sweden

Please state any competing interests: No competing interests to disclose.

Please leave your comments for the authors below

Dear authors,

I have read your revised manuscript and I am satisfied with your answers to my comments. Congratulations to a job well done and to an important contribution to the knowledge base on functional ability in older adults.

Reviewer: 5

Reviewer Name: Philip schluter

Institution and Country: University of Canterbury, New Zealand

Please state any competing interests: None

Please leave your comments for the authors below

A major improvement. Thank you for the opportunity to review the paper.

Answer:

We greatly appreciate the comments from the reviewer and think that they improved the manuscript.



Reviewer: 6

Reviewer Name: Jacob Dreier

Institution and Country: Ben-Gurion University of the Negev, Beer Sheva, Israel

Please state any competing interests: None declared

Please leave your comments for the authors below

The authors have taken care of replying to all my previous comments. I have some very minor corrections to the English that could be taken care of at the proofreading stage without need for review.

1. In the Abstract, replace "676/693 were excluded" with "676 of 693 individuals were excluded..."

Answer:

676 individuals in the ADL analysis and 693 individuals in the IADL analysis were excluded. This was a good suggestion and we made the following amendment: "676/693 individuals were excluded in the analyses due to missing outcomes."

2. In the Abstract, replace "with ORs of 2.30 (1.93-2.74), 2.29 (1.86-2.81)," with "with ORs of 2.30 (1.93-2.74), and 2.29 (1.86-2.81), respectively,".

Answer:

Thank you for a through job in reviewing the article. This has been corrected.

3. In the Methods section, I would change "material" to "study participants".

Answer:

That is a good suggestion. Instead of "study participants" we changed it to "study population."

4. In the Methods section, Variables paragraph - replace "containing information on current status" with "containing information on current vital status"

Answer:

The reviewer is correct in that this term is imprecise and should be changed. However, this variable does not only contain information on vital status but it also includes whether the participant has moved out of the county. Therefore "current status" has been replaced with "participant status" instead of "vital status."

5. In Table 2, change "non participation HUNT3" to "non participation in HUNT3"

Answer:

This has been changed in both table 2 and 3. In addition to this we have improved the table texts.

References:

1. Gallicchio, L. and B. Kalesan, Sleep duration and mortality: a systematic review and meta-analysis. *J Sleep Res*, 2009. 18(2): p. 148-58.
2. Cappuccio, F.P., et al., Sleep duration and all-cause mortality: a systematic review and meta-analysis of prospective studies. *Sleep*, 2010. 33(5): p. 585-92.
3. Cappuccio, F.P., et al., Sleep duration predicts cardiovascular outcomes: a systematic review and meta-analysis of prospective studies. *Eur Heart J*, 2011. 32(12): p. 1484-92.
4. Alvarez, G.G. and N.T. Ayas, The impact of daily sleep duration on health: a review of the literature. *Prog Cardiovasc Nurs*, 2004. 19(2): p. 56-9.

5. Chau, J.Y., et al., Daily sitting time and all-cause mortality: a meta-analysis. PLoS One, 2013. 8(11): p. e80000.
6. Biswas, A., et al., Sedentary time and its association with risk for disease incidence, mortality, and hospitalization in adults: a systematic review and meta-analysis. Ann Intern Med, 2015. 162(2): p. 123-32.
7. Dunstan, D.W., et al., Too much sitting--a health hazard. Diabetes Res Clin Pract, 2012. 97(3): p. 368-76.
8. Crimmins, E.M., Trends in the health of the elderly, in Annual Review of Public Health. 2004. p. 79-98.