

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Sex differences in sickness absence and the morbidity-mortality paradox: A longitudinal study using Swedish administrative registers
AUTHORS	Avdic, Daniel; Hägglund, Pathric; Lindahl, Bertil; Johansson, Per

VERSION 1 – REVIEW

REVIEWER	Dr. Hermann Blessberger Kepler University Hospital Linz, Austria
REVIEW RETURNED	10-Aug-2018

GENERAL COMMENTS	<p>1.) The primary aim of the paper was to elucidate the morbidity/mortality gap between men and women in the Swedish population.</p> <p>2.) The methods used to assess this difference were valid, as far as I can say. The difference-in-difference design needs to be further explained in detail in order to allow other researchers to repeat such an analysis. A reference should be provided, if possible.</p> <p>3.) The methods, results and conclusions were presented clearly.</p> <p>4.) Strongholds of this manuscript:</p> <ul style="list-style-type: none">A. Epidemiologically and economically relevant topic.B. Large sample size.C. Sophisticated statistical analysis with sensitivity analyses (different disease groups, different time intervals for acute myocardial infarction) – results proved to be robust.D. Data were adjusted for potential confounders (economic status, level of education, disease category).E. Imputation techniques are always risky. The authors elegantly provided a sensitivity analysis with results of two models to cover this (sickness absence in the years after death: either zero days or the same days of absence as in the last year prior to death). <p>5.) Suggestions to improve the paper:</p> <ul style="list-style-type: none">A. Past tense should be used if methods and results are reported (the authors often switched between present tense and past tense) – please amend.B. Please, go thoroughly through the manuscript as there are many typos (e.g. p.2 line 38: ... empirical analysis is based on ..., line 40: ... the longitudinal characteristics of our data allow us ..., line 48: please, rephrase, this is unclear to me and the sentence seems not to be finished, p.3 line 15: ... one much-quoted fact is
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	<p>that women outlive men ..., line 56: The large cross-country variation in (?) life expectancy ..., p.4 line 35: ... differences in outcomes reflect ..., p.8, line 31: ... is higher for men one to four years ..., p.10, line 11: close bracket, p.12, line 9: ... zero days of absence ...p. 12, line 53: There exist (?) previous studies (previous studies have reported ...)..., p. 13, line 19: ... that men in this population had a higher risk of dying within five years and that men in the oldest stratum were primarily the driving effect., p. 14, line 18: ... focuses ..., p. 14, line 21: ... gender differences ... reflect ..., line 50: ... statistically significant ..., line 54: ... measures of morbidity ..., ... stronger association with ..., p.15, line 3: "In" should be deleted, line 10: ... one needs methods ...line 17: please, rephrase, p.16, line 40: rephrase like:...Compared with Swedish women, Swedish men could potentially improve their pre- and post-admission health if they make more use of medical services pro-actively ...) etc.</p> <p>C. Methods: Please, explain the difference-in-difference design in more detail (see above).</p> <p>D. Citation p.4, line 53 is unclear (16-65) – please correct.</p> <p>E. Statistics: Please, report the nature of numbers provided in the tables (e.g. result of regression analysis for one/five year(s), regression slope coefficients, regression constants, mean differences)</p> <p>F. Statistics: Please, add 95% confidence intervals to all numbers in the tables provided.</p> <p>G. Limitations: please add that the results reflect the findings in a representative sample of the Swedish population. It is not clear if these results also apply to other countries.</p> <p>H. Limitations: The study and its results heavily depend on the quality and accuracy of ICD classification. Therefore, the authors should briefly comment on this topic. Who is responsible for ICD classification in the Swedish hospital system (doctors, nurses, other personnel)? Are the staff properly trained?</p> <p>I. Figure 1: scaling of the y-axis is different for all 4 smaller panels with patient subgroups. I would recommend using the same scale on the y-axis for all four panels to make the differences more visible to the reader, if possible.</p> <p>J. Please, fill in STROBE statement completely.</p> <p>In this study, the authors could show that Swedish men were on average sicker than Swedish women prior to their first hospital admission and to a lesser extent took advantage of healthcare medical services after their hospital stay (as reflected by days of absence from work). Mortality in men was higher than in women. The data were robust in all sensitivity analyses (except for the neoplasms subgroup as outlined by the authors).</p>
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REVIEWER	Richard G Watt Department of Epidemiology and Public Health, UCL.
REVIEW RETURNED	22-Oct-2018

GENERAL COMMENTS	<p>This is a potentially interesting and important paper on sex differences and the morbidity-mortality paradox.</p> <p>Suggestions to improve the paper include:</p> <ol style="list-style-type: none"> 1. The abstract fails to describe the data analysis performed. 2. In the article summary it is unclear what is meant by 'proactive
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	<p>behaviour' (page 2, line 32) and the text in the final bullet point (page 2, lines 48-54) is incomplete in places and needs rephrasing.</p> <p>3. In the introduction and indeed throughout the paper the authors need to clarify more precisely what they mean by health behaviour. Is sickness absence the only behaviour of interest? What about other health related behaviours such as tobacco use, alcohol consumption etc The title is therefore rather confusing and misleading.</p> <p>4. I am also concerned how hospital admissions are conceptualised and the data analysed. Surely the nature of hospital admission varies considerably from an emergency to a planned elective visit. All hospital admissions cannot therefore be considered as an 'adverse health shock' (page 4, line 12).</p> <p>5. In the methods it is unclear what is meant by 'right censored after death' (page 6, line 12).</p> <p>6. Sickness absence is influenced by a wide and diverse range of factors - the results and discussion do not appear to acknowledge this and instead focus solely on hospital admission as the key influence. Perhaps I have misunderstood the basis of this paper.</p> <p>7. In the discussion, the limitations section should include the fact that no health behaviours were included in the analysis. Also the final implications section needs to be rewritten as it very hard to follow.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Dr. Hermann Blessberger

Institution and Country: Kepler University Hospital Linz, Austria Please state any competing interests or state 'None declared': None declared.

5.) Suggestions to improve the paper:

A. Past tense should be used if methods and results are reported (the authors often switched between present tense and past tense) – please amend.

Done.

B. Please, go thoroughly through the manuscript as there are many typos (e.g. p.2 line 38: ... empirical analysis is based on ..., line 40: ... the longitudinal characteristics of our data allow us ..., line 48: please, rephrase, this is unclear to me and the sentence seems not to be finished, p.3 line 15: ... one much-quoted fact is that women outlive men ..., line 56: The large cross-country variation in (?) life expectancy ..., p.4 line 35: ... differences in outcomes reflect ..., p.8, line 31: ... is higher for men one to four years ..., p.10, line 11: close bracket, p.12, line 9: ... zero days of absence ...p. 12,

line 53: There exist (?) previous studies (previous studies have reported ...)..., p. 13, line 19: ... that men in this population had a higher risk of dying within five years and that men in the oldest stratum were primarily the driving effect., p. 14, line 18: ... focuses ..., p. 14, line 21: ... gender differences ... reflect ..., line 50: ... statistically significant ..., line 54: ... measures of morbidity ..., ... stronger association with ..., p.15, line 3: "In" should be deleted, line 10: ... one needs methods ...line 17: please, rephrase, p.16, line 40: rephrase like:...Compared with Swedish women, Swedish men could potentially improve their pre- and post-admission health if they make more use of medical services pro-actively ...) etc.

Done

C. Methods: Please, explain the difference-in-difference design in more detail (see above).

We added a section.

D. Citation p.4, line 53 is unclear (16-65) – please correct.

Done

E. Statistics: Please, report the nature of numbers provided in the tables (e.g. result of regression analysis for one/five year(s), regression slope coefficients, regression constants, mean differences)
Done

F. Statistics: Please, add 95% confidence intervals to all numbers in the tables provided.

We added the confidence intervals in Table 1 and 4. The reason for not adding the confidence intervals in Table 2 and 3 is that the Tables needs to be extended over two pages. We however add these tables as a supplementary file if the editor rather would include these tables.

G. Limitations: please add that the results reflect the findings in a representative sample of the Swedish population. It is not clear if these results also apply to other countries.

Done

H. Limitations: The study and its results heavily depend on the quality and accuracy of ICD classification. Therefore, the authors should briefly comment on this topic. Who is responsible for ICD classification in the Swedish hospital system (doctors, nurses, other personnel)? Are the staff properly trained?

We clarified that the diagnoses, made by the physicians, are classified by a senior consultant.

I. Figure 1: scaling of the y-axis is different for all 4 smaller panels with patient subgroups. I would recommend using the same scale on the y-axis for all four panels to make the differences more visible to the reader, if possible.

Done.

J. Please, fill in STROBE statement completely.

Done

Reviewer: 2

Reviewer Name: Richard G Watt

Institution and Country: Department of Epidemiology and Public Health, UCL.

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

Suggestions to improve the paper include:

1. The abstract fails to describe the data analysis performed.

Done.

2. In the article summary it is unclear what is meant by 'proactive behaviour' (page 2, line 32) and the text in the final bullet point (page 2, lines 48-54) is incomplete in places and needs rephrasing.

We deleted the second part of the sentence on proactive behavior. The reason is that it is too complicated to discuss this in the abstract given the restriction on number of words. We however extend on the previous introduction where we tried to more clearly define what we mean with proactive behavior with regard to sickness absence.

The first sentence of bullet point 4 was incomplete.

We completed the sentence. We however also changed the wording in bullet point 3 as the wording in this bullet point is important for the understanding of bullet point 4.

3. In the introduction and indeed throughout the paper the authors need to clarify more precisely what they mean by health behaviour. Is sickness absence the only behaviour of interest? What about other health related behaviours such as tobacco use, alcohol consumption etc The title is therefore rather confusing and misleading.

We agree with you that also the use of tobacco, alcohol, etc. are health behavior. If men e.g. are drinking and smoking more than the women this would most likely, all else equal, mean that men's health would be worse than that of the women. We are not primarily interested in sickness absence but it is a good measure of behavior. The reason is that sickness absence can be measure over time with high validity. The difference in difference design removes potential other differences in health behavior (e.g. alcohol, smoking etc.) between the sexes. This means that we can focus on the sex difference in the morbidity outcome sickness absence after a change in health (the hospital admission).

In order to clarify what we mean with health behavior and proactive behavior we rewrote the second paragraph in the introduction.

4. I am also concerned how hospital admissions are conceptualised and the data analysed. Surely the nature of hospital admission varies considerably from an emergency to a planned elective visit. All hospital admissions cannot therefore be considered as an 'adverse health shock' (page 4, line 12).

We agree. Several of these admissions cannot be seen as genuine shocks. The strength with the paper is that we get very coherent results over all diagnosis of which some can be seen as genuine shocks, e.g. Circulatory diseases. These hospital admission are also analyzed in more detail in the paper. As a consequence of your critique we removed all the wordings using "shocks" in the document.

5. In the methods it is unclear what is meant by 'right censored after death' (page 6, line 12).

We removed the word right censored by rewriting the sentence.

6. Sickness absence is influenced by a wide and diverse range of factors - the results and discussion do not appear to acknowledge this and instead focus solely on hospital admission as the key influence. Perhaps I have misunderstood the basis of this paper.

We agree that there may be a lot of factors influencing sickness absence other than a hospital admission. A hospital admission is an indication of a change in health of both men and women. As we observed sickness absence before and after the hospital admission this allows us to hold other than health factors constant (e.g. work conditions, socioeconomic status) by the difference in difference design.

7. In the discussion, the limitations section should include the fact that no health behaviours were included in the analysis. Also the final implications section needs to be rewritten as it very hard to follow.

We have clarified in the paper that sickness absence as a measure is just one out of many other health behaviors. The primary reason is the sickness absence can be measure over time with high validity. Making this measurement powerful for studying differences in behavior between the sexes. The difference in difference design removes these differences in other health behavior (e.g. alcohol, smoking etc.) between the sexes.

The final implications section is revised.

VERSION 2 – REVIEW

REVIEWER	Dr. Hermann Blessberger Kepler University Hospital, Department of Cardiology, Linz, Austria
REVIEW RETURNED	31-Dec-2018
GENERAL COMMENTS	The manuscript has substantially improved. All the points I raised with regards to content were addressed sufficiently. As there are still numerous typos, the paper needs proofreading by a native speaker or language specialist prior to publication.