

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Disability acquisition and mental health: effect modification by demographic and socioeconomic characteristics using data from an Australian longitudinal study
AUTHORS	Aitken, Zoe; Simpson, Julie; Bentley, Rebecca; Kavanagh, Anne

VERSION 1 - REVIEW

REVIEWER	Somnath Chatterji World Health Organization Geneva Switzerland
REVIEW RETURNED	25-Apr-2017

GENERAL COMMENTS	<p>This is an interesting paper describing the mental health impact of incident disability in a panel survey in Australia. The paper uses data from the HILDA study and addresses an area of public health importance. While the present study is described well and the analyses are appropriate a few clarifications are necessary.</p> <ol style="list-style-type: none"> 1. The exact question wording of the item that was used to assess disability would help readers understand the definition used in the study. 2. One assumes that the height and weight information used for calculating BMI was self-reported and not measured - this should be stated explicitly. 3. Since this is a complex design survey, the authors need to clarify if the analyses that are presented excluding the propensity score weighted analyses) are all weighted analyses and if the survey design has been taken into account in the analyses. 4. On page 16 line 9 the authors state that the assumption that data were missing at random is a plausible assumption and yet when they describe their approach to missing data on page 9 the justify the use of chained equations for multiple imputation by stating that the data were not missing completely at random. The authors will need to clarify the statement in the discussion. 5. There seems to be a word missing in line 20 on page 2 "...to better the heterogeneity" - perhaps should have read "...to better understand the heterogeneity..". 6. In supplementary file 2, table S2.3 the authors have the acronym SEIFA - this needs to be expanded for non-Australian readers - SocioEconomic Indexes For Australia.
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REVIEWER	Lauri Kokkinen University of Tampere, Finland
REVIEW RETURNED	25-Apr-2017

GENERAL COMMENTS	<p>1) The title of the paper is very long and obscure</p> <p>2) According to the introduction previous studies do not comprehensively examine a wide enough range of factors that influence the effect of disability acquisition on mental health to fully understand demographic and socioeconomic determinants and a better understanding of these issues would provide useful information. I agree. But after taking this standpoint, why dont the authors provide any theoretical reasoning/discussion on the issue? This recent paper, for example, might be useful: "Øversveen, Rydland, Bambra, Eikemo. Rethinking the relationship between socio-economic status and health: Making the case for sociological theory in health inequality research. Scandinavian Journal of Public Health, Vol 45, Issue 2, 2017.</p> <p>3) At the end of the introduction the authors state that a better understanding of the issue will provide information that can be used to inform the development of targeted social and health policies and that such interventions are likely to improve the mental health of people with disabilities and may even have implications for costs associated with disability. At the end of the discussion section the authors then give an undetailed statement that targeted interventions for people with disabilities have implications for the mental health and wellbeing of these people and their families, as well as for long term health and welfare costs. But what interventions are the authors talking about? And how do these interventions specifically gain from examining a wide range of factors influencing the effect of disability acquisition on mental Health in this study? Is it really realistic to aspire to target interventions to very specific groups defined by using a very wide range of socio-demographic factors? And as people with most severe disabilities were less likely to participate in HILDA in the first place, does this have an effect for informing the interventions?</p> <p>4) Participants were included in the study if they were disability-free for the first two waves of the analysis followed by either two consecutive waves of disability or two consecutive waves of no disability. Why were those with disability-free for the first two waves followed by one wave of disability (within the two latter waves) not analysed at all?</p> <p>5) Can the changes in the covariates be followed?</p> <p>6) In the last phase of the sensitivity analysis the authors excluded people who acquired psychological impairments. But why shouldnt these people be excluded from the beginning as they by definition have poorer mental Health?</p> <p>7) My point 6 would also affect the results and part of the results would no longer be statistically significant.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Somnath Chatterji

Institution and Country: World Health Organization, Geneva, Switzerland

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

This is an interesting paper describing the mental health impact of incident disability in a panel survey in Australia. The paper uses data from the HILDA study and addresses an area of public health importance. While the present study is described well and the analyses are appropriate a few clarifications are necessary.

1. The exact question wording of the item that was used to assess disability would help readers understand the definition used in the study.

The exact wording of the disability question as stated in the HILDA questionnaire has been included in the Methods section (lines 138-140).

2. One assumes that the height and weight information used for calculating BMI was self-reported and not measured - this should be stated explicitly.

Thank you, this has now been specified in the Methods section (line 171).

3. Since this is a complex design survey, the authors need to clarify if the analyses that are presented (excluding the propensity score weighted analyses) are all weighted analyses and if the survey design has been taken into account in the analyses.

The analysis does not take into account the HILDA survey weights. The primary analysis consists of an inverse probability weighted analysis, which cannot be used in conjunction with Stata's commands to account for survey weights. One approach that has been suggested is to multiply the inverse probability weights by the survey weights to account for design effects. When we used this approach, we found that the effect estimates and confidence intervals did not change substantially and the conclusions were unchanged, therefore we did not include survey weights in the analysis. Additionally, the inclusion of survey weights in conventional linear regression models did not substantially change the results. This has been discussed as a limitation in the Discussion (lines 337-339).

4. On page 16 line 9 the authors state that the assumption that data were missing at random is a plausible assumption and yet when they describe their approach to missing data on page 9 they justify the use of chained equations for multiple imputation by stating that the data were not missing completely at random. The authors will need to clarify the statement in the discussion.

We have highlighted the difference between the missing data assumptions 'missing completely at random' (where missing values represent a random sample of those that were intended to be observed) and 'missing at random' (where the missingness is associated with the value of measured variables) in both the Methods (lines 208-212) and Discussion (line 333-337) sections to better explain the justification for use of multiple imputation.

5. There seems to be a word missing in line 20 on page 2 "...to better the heterogeneity" - perhaps should have read "...to better understand the heterogeneity..".

Thank you, this has been amended (line 70).

6. In supplementary file 2, table S2.3 the authors have the acronym SEIFA - this needs to be expanded for non-Australian readers - SocioEconomic Indexes For Australia.

Thank you, this has been amended (Supplementary file 2, Table S2.3).

Reviewer: 2

Reviewer Name: Lauri Kokkinen

Institution and Country: University of Tampere, Finland

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

1) The title of the paper is very long and obscure

We have shortened the title and included information about the study design.

2) According to the introduction previous studies do not comprehensively examine a wide enough range of factors that influence the effect of disability acquisition on mental health to fully understand demographic and socioeconomic determinants and a better understanding of these issues would provide useful information. I agree. But after taking this standpoint, why don't the authors provide any theoretical reasoning/discussion on the issue? This recent paper, for example, might be useful: "Øversveen, Rydland, Bambra, Eikemo. Rethinking the relationship between socio-economic status and health: Making the case for sociological theory in health inequality research. *Scandinavian Journal of Public Health*, Vol 45, Issue 2, 2017.

We have included a paragraph about intersectionality theory to describe the theoretical reasoning underpinning this paper (lines 94-101) and also underlined its importance in the Discussion (lines 301-303).

3) At the end of the introduction the authors state that a better understanding of the issue will provide information that can be used to inform the development of targeted social and health policies and that such interventions are likely to improve the mental health of people with disabilities and may even have implications for costs associated with disability. At the end of the discussion section the authors then give an undetailed statement that targeted interventions for people with disabilities have implications for the mental health and wellbeing of these people and their families, as well as for long term health and welfare costs. But what interventions are the authors talking about? And how do these interventions specifically gain from examining a wide range of factors influencing the effect of disability acquisition on mental Health in this study? Is it really realistic to aspire to target interventions to very specific groups defined by using a very wide range of socio-demographic factors?

The aim of the analysis was to examine whether specific socioeconomic characteristics explained the heterogeneity in the effect of disability acquisition on mental health. What we found was that all socioeconomic characteristics influenced the effect. While we acknowledge that these results may not identify a single subgroup of people with disabilities who would benefit the most from interventions, the results highlight the importance of socioeconomic characteristics in buffering the effect of disability on mental health. Therefore addressing the social determinants of health of people with disabilities, such as interventions that improve people's financial situation, for example investing in employment and education services for people with disabilities, may have substantial beneficial impacts on mental health. Furthermore, it is important that high quality mental health services are accessible and affordable for people who acquire a disability, particularly if they are socioeconomically disadvantaged or not in a relationship. We have discussed this further in the final paragraph of the Discussion (lines 355-366).

And as people with most severe disabilities were less likely to participate in HILDA in the first place, does this have an effect for informing the interventions?

We calculated the average causal effect of disability acquisition on mental health, which is likely to

differ according to type and severity of impairments, as discussed in the Limitations (lines 247-352). Though we acknowledge that the underrepresentation of people with the most severe disabilities in HILDA may affect the generalisability of the results, it is likely to underestimate the effect of disability acquisition on mental health. Therefore improving the social determinants of health of people with severe disabilities may have even greater impacts than for those with less severe disabilities (lines 352-354).

4) Participants were included in the study if they were disability-free for the first two waves of the analysis followed by either two consecutive waves of disability or two consecutive waves of no disability. Why were those with disability-free for the first two waves followed by one wave of disability (within the two latter waves) not analysed at all?

We used two consecutive waves of disability so as to exclude people with transient disability and to reduce the potential for measurement error. We have included this justification in the Methods section (lines 149-151).

5) Can the changes in the covariates be followed?

Thank you for this comment. We agree that investigating changes in socioeconomic characteristics when people acquire a disability would be an important addition to the literature. Such analyses would enable us to better understand the mechanisms explaining the relationship between disability and poor mental health by quantifying how much of the effect is mediated by change in these covariates. Mediation analyses require complex statistical methods with many assumptions, thus it was deemed to be beyond the scope of this paper but is an area for future research.

6) In the last phase of the sensitivity analysis the authors excluded people who acquired psychological impairments. But why shouldn't these people be excluded from the beginning as they by definition have poorer mental Health?

We decided to include all people with disabilities because we believe this is the most relevant policy question as disability policies do not address different types of impairments specifically. Though people with psychological disabilities have poorer mental health by definition, the influence of socioeconomic characteristics is likely to be similar to those with other types of impairments, therefore we chose to include them in the primary analysis. When we look at the results excluding people with psychological impairments, the estimated effect of disability acquisition on mental health is slightly attenuated but the patterns of association with socioeconomic characteristics were generally similar. We have further expanded on why we chose to use this approach in the Methods (lines 140-145).

7) My point 6 would also affect the results and part of the results would no longer be statistically significant.

When we excluded people with psychological impairments, the sample size decreased, reducing the number of people who acquired a disability from 387 to 316, and the effect sizes were slightly attenuated. Though some of the results were no longer statistically significant ($p < 0.05$; of note simply basing findings on this cut-off value is not recommended, see Sterne & Davey Smith 2001) due to the combination of lower power and slightly smaller effect sizes, the mental health declines are still of clinical importance.

Sterne JA & Davey Smith G (2001) Shifting the evidence – what's wrong with significance tests? *BMJ*; 322: 226+231.

VERSION 2 – REVIEW

REVIEWER	Somnath Chatterji World Health Organization Switzerland
REVIEW RETURNED	04-Jul-2017

GENERAL COMMENTS	The authors have adequately responded to the previous comments.
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REVIEWER	Lauri Kokkinen University of Tampere, Finland
REVIEW RETURNED	19-Jun-2017

GENERAL COMMENTS	The authors have revised the manuscript according to my previous comments.
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