

## 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>	Are housing tenure and car access still associated with health? A repeat cross-sectional study of UK adults over a 13 year period
<b>AUTHORS</b>	Ellaway, Anne; Macdonald, Laura; Kearns, Ade

## VERSION 1 - REVIEW

<b>REVIEWER</b>	David Bann Centre for Longitudinal Studies, UCL Institute of Education
<b>REVIEW RETURNED</b>	13-May-2016

<b>GENERAL COMMENTS</b>	<p>The article presents data from the West of Scotland on how housing tenure and car access relate to selected health outcomes. The below points should be addressed to aid the interpretation of the results and the contribution of the paper.</p> <p>Introduction</p> <ul style="list-style-type: none"><li>• The introduction would benefit from the statement (and justification of) the authors' hypothesis. Why would have relationships changed across time? Have house prices increased since the 1990s leading to a different socioeconomic distribution in ownership?</li><li>• It's unclear if previous associations with tenure or car ownership were large in magnitude</li><li>• the abstract is focused on statistical significance—it would be more helpful to give an indication of the magnitude of differences found</li><li>• The abstract introduction suggested that “household assets confer health benefits” — this suggests a causal relationship which the paper does not appear to give evidence for. This should therefore be toned down (eg, ‘associated with’).</li></ul> <p>Methods/Results</p> <ul style="list-style-type: none"><li>• Descriptive statistics should be included, eg, showing the cross-tabulation of tenure / car ownership with other demographic measures. It is very hard to interpret the other results without these.</li><li>• The abstract should state the years of data collection given its relevance to the conclusion. It appears to be 2010, so a later follow up than the 1997 study, but still not the “current economic climate” given changes post 2010.</li><li>• The rationale for outcome selection should be clarified – were all available health-related outcomes chosen? Or were a select number chosen (if so, what was the rationale?)</li><li>• The results do not appear to adjust for both class and income. This seems to be very important to do given the research question.</li><li>• Table 1 is hard to follow (eg, social class appears to be both the exposure and adjustment). These tables could be simplified by presenting one set of models adjusted for income and social class. If missing data are a concern, they could be accounted for by imputation / FIML etc.</li></ul>
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	<ul style="list-style-type: none"> <li>• The p-values in the tables should be greater than 0</li> <li>• The results section should spell out the size of the effects rather than solely referring to significance levels. How should the small <math>\eta^2</math> values be interpreted?</li> </ul> <p>Discussion</p> <ul style="list-style-type: none"> <li>• Some of the language throughout the manuscript appears to be unhelpfully informal and vague. These should be made more specific: eg, “ways of thinking” p 6 (what is meant by this and what evidence is there of it?); “environmental quality is often worse”... “Neighbourhood quality” “relative quality of the home” (p7—what does ‘quality’ refer to?)</li> <li>• The discussion should also consider what other factors car ownership reflects (eg, living in rural / more wealthy area? Individual wealth?), and the extent to which residual confounding could explain findings</li> <li>• The authors discussion potential sources of bias (p7 para 2) but should also consider how this might affect the magnitude of results obtained. The response rates appear to have been very low (38%)</li> <li>• The final paragraph seems to be highly skewed towards psychosocial benefits of tenure/car ownership. The authors should mention that this is one of many potential explanations—their paper does not provide empirical evidence of the psychosocial pathway.</li> <li>• The discussion should refer more broadly to evidence from other datasets when comparing their findings and in interpreting the results. A quick look reveals evidence from Whitehall, suggesting associations between housing and mental health differ by age (<a href="http://www.ncbi.nlm.nih.gov/pubmed/21884619">http://www.ncbi.nlm.nih.gov/pubmed/21884619</a>). This also includes fixed effects analyses by Baker et al in 2012 (<a href="http://usj.sagepub.com/content/50/2/426.full">http://usj.sagepub.com/content/50/2/426.full</a>), the findings of which suggest no causal relationship between housing tenure and mental health</li> </ul>
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<b>REVIEWER</b>	Dermot O'Reilly QUB, Belfast
<b>REVIEW RETURNED</b>	17-Jun-2016

<b>GENERAL COMMENTS</b>	<p>re review Manuscript ID bmjopen-2016- 012268, "Are housing tenure and car access still associated with health? A repeat cross-sectional study of UK adults over a 13 year period.</p> <p>Thank you for asking me to review this paper which is a very timely reminder how important it is to look behind these indicators of SES and ask difficult questions such as what are they actually measuring. I liked the paper and think it is eminently suitable for publication. I have one or two comments and thoughts which the authors may wish to consider, that relate to the methods and discussion.</p> <p>Methods:</p> <p>This section was a little too tightly written for me (perhaps word count was an issue), and some of the detail is missing; for example ...</p> <ul style="list-style-type: none"> <li>• Sampling frame...why was it important to select within the same geography as previously if the authors are trying to make a general statement that extends beyond this group?</li> </ul>
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	<ul style="list-style-type: none"> <li>• how was income determined and was this individual or household ; and if household was it equivalised?</li> <li>• which measure of social class was used and how many categories?</li> <li>• people were excluded based on permanent sickness but was economic activity included in the models?</li> </ul> <p>Discussion:</p> <ol style="list-style-type: none"> <li>1. While I agree with the overall thrust of the discussion: I think the first line needs changed as the phrase 'conferred some health benefits' presumes causality which is difficult in a cross-sectional study.</li> <li>2. limitations:             <ol style="list-style-type: none"> <li>a. I think the authors still need to consider the potential for residual confounding and reverse causation as this is a cross-sectional study; for example poor health – reduced employment-poor housing</li> <li>b. the other possibility is that housing is a measure of wealth which may operate independently of income.</li> </ol> </li> </ol>
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## VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: David Bann

Institution and Country: Centre for Longitudinal Studies, UCL Institute of Education, UK

Competing Interests: None declared

The article presents data from the West of Scotland on how housing tenure and car access relate to selected health outcomes. The below points should be addressed to aid the interpretation of the results and the contribution of the paper.

### Introduction

- The introduction would benefit from the statement (and justification of) the authors' hypothesis. Why would have relationships changed across time? Have house prices increased since the 1990s leading to a different socioeconomic distribution in ownership?

We have revised the introduction, expanding as suggested.

- It's unclear if previous associations with tenure or car ownership were large in magnitude

To aid readers on effect sizes, we have included a note of the potential magnitude of  $\eta^2$  in the Methods section, using Cohen's 1988 guidelines.

- the abstract is focused on statistical significance—it would be more helpful to give an indication of the magnitude of differences found

We have revised the Results section to provide an indication of the magnitude of the differences found.

- The abstract introduction suggested that "household assets confer health benefits" — this suggests a causal relationship which the paper does not appear to give evidence for. This should therefore be toned down (eg, 'associated with').

We have amended the text to that effect.

#### Methods/Results

- Descriptive statistics should be included, eg, showing the cross-tabulation of tenure / car ownership with other demographic measures. It is very hard to interpret the other results without these. We have now included a table with this data (table 1)

- The abstract should state the years of data collection given its relevance to the conclusion. It appears to be 2010, so a later follow up than the 1997 study, but still not the “current economic climate” given changes post 2010. We have amended the text to that effect.

- The rationale for outcome selection should be clarified – were all available health-related outcomes chosen? Or were a select number chosen (if so, what was the rationale?) We chose to use the same outcomes as our previous paper, with the addition of the HADS anxiety measure. We have added some text to support our choice of health measures.

- The results do not appear to adjust for both class and income. This seems to be very important to do given the research question.

In all our papers from our 1997 and 2010 studies we have chosen to run separate models for social class and income as they are often associated with different consumption patterns. We have noted this point in our introduction and provided a reference to support this point.

- Table 1 is hard to follow (eg, social class appears to be both the exposure and adjustment). These tables could be simplified by presenting one set of models adjusted for income and social class. If missing data are a concern, they could be accounted for by imputation / FIML etc.

Missing data was not a concern for the majority of responses in our survey- over 95% of items had less than 5% of missing data (we note this point in the discussion). Income was one of the items which had the most missing data (20%) this is a common feature of UK postal studies, and in a cross sectional study like ours, it would be impossible to impute income. As stated above, we prefer to run separate models for income and social class.

- The p-values in the tables should be greater than 0

We have amended the tables

- The results section should spell out the size of the effects rather than solely referring to significance levels. How should the small  $\eta^2$  values be interpreted? We have amended the results section to reflect this point.

#### Discussion

- Some of the language throughout the manuscript appears to be unhelpfully informal and vague. These should be made more specific: eg, “ways of thinking” p 6 (what is meant by this and what evidence is there of it?); “environmental quality is often worse”... “Neighbourhood quality” “relative quality of the home” (p7—what does ‘quality’ refer to?) We have amended the text to reflect his point

- The discussion should also consider what other factors car ownership reflects (eg, living in rural / more wealthy area? Individual wealth?), and the extent to which residual confounding could explain findings We have added additional text and references to address these points

- The authors discussion potential sources of bias (p7 para 2) but should also consider how this might affect the magnitude of results obtained. We have amended the text to reflect this point

The response rates appear to have been very low (38%)

We have commented in the text on this point and provided supporting references.

- The final paragraph seems to be highly skewed towards psychosocial benefits of tenure/car ownership. The authors should mention that this is one of many potential explanations—their paper does not provide empirical evidence of the psychosocial pathway.

We have amended the text to that effect.

- The discussion should refer more broadly to evidence from other datasets when comparing their findings and in interpreting the results. A quick look reveals evidence from Whitehall, suggesting associations between housing and mental health differ by age (<http://www.ncbi.nlm.nih.gov/pubmed/21884619>). This also includes fixed effects analyses by Baker et al in 2012 (<http://usj.sagepub.com/content/50/2/426.full>), the findings of which suggest no causal relationship between housing tenure and mental health.

We have addressed this point by adding additional text and references in the discussion.

Reviewer: 2

Reviewer Name: Dermot O'Reilly

Institution and Country: QUB, Belfast, Northern Ireland

Competing Interests: None

Thank you for asking me to review this paper which is a very timely reminder how important it is to look behind these indicators of SES and ask difficult questions such as what are they actually measuring. I liked the paper and think it is eminently suitable for publication. I have one or two comments and thoughts which the authors may wish to consider, that relate to the methods and discussion.

Methods:

This section was a little too tightly written for me (perhaps word count was an issue), and some of the detail is missing; for example ...

- Sampling frame...why was it important to select within the same geography as previously if the authors are trying to make a general statement that extends beyond this group?

We chose to sample from the same areas as before as the Glasgow and Clyde Valley Structure Plan area encompasses a range of neighbourhood types with marked variation in health and social status. We have added this information to the Methods section.

- how was income determined and was this individual or household ; and if household was it equivalised?

The income was household and was equivalised—we have amended the text to address this point

- which measure of social class was used and how many categories?

We have amended the text to provide that information

- people were excluded based on permanent sickness but was economic activity included in the models?

We did not include economic activity in the models

Discussion:

1. While I agree with the overall thrust of the discussion: I think the first line needs changed as the

phrase 'conferred some health benefits' presumes causality which is difficult in a cross-sectional study.

We have amended the text in accordance with this comment.

2. limitations:

a. I think the authors still need to consider the potential for residual confounding and reverse causation as this is a cross-sectional study; for example poor health – reduced employment- poor housing

We have amended the text to address these points.

b. the other possibility is that housing is a measure of wealth which may operate independently of income.

Unfortunately we don't have a measure of housing wealth in our study but have included additional text on that point.

#### VERSION 2 – REVIEW

<b>REVIEWER</b>	Dermot O'Reilly Queens University Belfast Belfast
<b>REVIEW RETURNED</b>	06-Sep-2016

<b>GENERAL COMMENTS</b>	I have read the revised manuscript and see that the authors have adequately addresses all my previous concerns.  I readily recommend publication.
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