Reviewer's report

Title: Is Council Tax Valuation Band A Predictor of Mortality? - Cross-Sectional Study

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Reviewer: Mr Michael Pearce

Level of interest: A paper whose findings are important to those with closely related research interests

Advice on publication: Unable to decide on acceptance or rejection until the authors have responded to the compulsory revisions

General

The declared objective of the paper is to examine council tax band of final residence as a marker of mortality and, indirectly, health.
Although not experienced in the evaluation of indirect markers of human health, I agree there is a need for simple and easily measured indicators of health. If novel, then I feel the authors' work will be of interest to those with closely related research interests.
In general, the paper was easy to read, though there are some minor grammatical errors.
The data and result tables are repetitive and don't show all sufficient summary data to allow the reader to make independent calculations.
There is inadequate statistical support for some of the authors assertions and some statistics appear to have been calculated incorrectly.
There is inadequate consideration of bias and confounding issues in the Discussion.
I recommend the paper be reconsidered for publication following substantial revision.

Title

Appropriate, but suggest leave out the rider Cross-Sectional Study although the population denominator data is cross-sectional, in effect it is being used as a denominator data over a three year period, making this a cohort study.

Abstract

Reflects the subject and content of the paper, although comments under Results and Conclusions will have to be reviewed following recalculation of statistics.

Background

Although ultimately an editorial decision, I don't feel sentences should begin with conjunctions e.g.
Line 8. But our understanding...
Line 15. And as often...
aEc Line 10. I am not familiar with the word definitude and the final clause in which this word occurs should be replaced by a sentence stating explicitly what the authors appear to be trying to say: that the definition of death, unlike the definition of social standing, is more stable and transferable; and it is therefore easier to compare findings related to death than findings related to social standing in different studies.
aEc Para 2, line2. I suspect local authorities levy taxation based on the value of a property rather than size of a property? This should be checked and the text amended if necessary.

Methods

aEc The context of the practice and the sources of data are adequately described. However, I am not sure from the text whether only deaths among the 25% of practice patients recruited into the study are being reported or all deaths over the three year period of each cohort were reported. The crude mortality rates in Table 4 do seem very high in the order of 20%50% over a three year period.
aEc We should be told why practice patient lists only contained 25% of patients. Was this also to minimise unscheduled work for Health Authority staff?
aEc It appears from a statement in Results, Numerator data [para 3, line2] that Wiltshire Health Authority could not provide information about deaths of practice patients who died outside this Authority's jurisdiction? If not, this should be noted here.
aEc The explanation of how standardised mortality ratios were calculated is unclear. I suggest the authors describe the method using mathematical formulae or simply refer to methods in a nominated reference.
aEc Given that this is a cohort study, the authors should calculate the relative risk/risk ratio rather than the odds ratio.

Results

Numerator data

aEc Para 1, line 3. Range 13091 should go from lowest to highest i.e. 91130.
aEc Para 2, line 2. The authors should say how many of the 58 patients whose residence they could not determine were older than 50 years. Without this information it is impossible for the reader to determine if the figures in Table 3 are correct.
aEc Para 4, line 4. The authors claim that there are no significant differences in age at death between the groups in any of the cohorts. This assertion should be supported by a statistical test result using a i2 test or, preferably, an exact test.

Standardised mortality ratios

aEc Para 1, line 2. The authors claim that risk of dying during the study period is consistently higher for residents of homes allotted to CTVB A and B. This is apparent visually in most cases, but should be supported with statistical analysis. In the 1993 cohort, there is overlap of the confidence limit range of CTVB B and CTVBs Ca-F+.

Figures and Tables

aEc Table 1. There is a mistake in total figure for age group 8089. It should be 344, not 343.
aEc Table 4. The re-presentation of numerator and denominator data in these tables is unnecessary.
Table 4. The crude mortality rates implied by the data in these tables suggest that all deaths among practice patients were reported compared with 25% of patients at risk. If this is so then it is very misleading. All patients at risk should be reported with all deaths, or 25% of deaths with 25% of patients.

Table 5. The data in this table does not allow the reader to determine what data was used to calculate odds ratios. In addition, the presentation of data is unconventional. It appears that the authors have calculated the odds ratios incorrectly, as follows e.g. cohort 1993:

\[ \frac{156}{330} = 0.48 \]

In fact, they should subtract the deaths from the population at risk to give the following value for the odds ratio:

\[ \frac{156}{330 - 156} = 2.65 \]

The preferred statistic in this case would be the relative risk or risk ratio calculated as follows:

\[ \frac{156(156 + 152)}{154(154 + 398)} = 1.82 \]

I suggest the authors present their 2 x 2 tables as follows and quote the SMD and RR in the text, with confidence limits.

<table>
<thead>
<tr>
<th>CTVB</th>
<th>Deaths</th>
<th>Survivors</th>
</tr>
</thead>
<tbody>
<tr>
<td>A,B</td>
<td>a</td>
<td>b</td>
</tr>
<tr>
<td>C+</td>
<td>c</td>
<td>d</td>
</tr>
<tr>
<td>a+c</td>
<td>b+d</td>
<td>a+b+c+d</td>
</tr>
</tbody>
</table>

Discussion

The authors conclusions are, rightly, measured and they have not overinterpreted the results. However, they need to make some recalculations to statistics and rewrite this section if necessary. I should like to see some consideration given to potential biases and confounding in the discussion. Things to consider are the stability of the age structure of the denominator figures, the impact of patients who have moved from the practice and died elsewhere but are not included in the study, and duration of residence at current address or address before death.

What proportion of patients changed their address during the three year periods of each cohort? The authors have not addressed this issue and the misclassification of outcome resulting from such changes.

References

I am not familiar with the literature in this area and have only checked spelling and grammar of the quoted references.

Ref 2, line 2. Is this truly Penquin Books, or should this be Penguin Books?

Ref 34. Should there be more information given for this reference?

Declaration
aEc In the past five years I have not received reimbursements, fees, funding, or salary from an organisation that I am aware may in any way gain or lose financially from the publication of this paper. 

aEc I do not hold stocks or shares in an organisation that I am aware may in any way gain or lose financially from the publication of this paper.

aEc I do not have any financial or other interests that may compromise my assessment of this paper.

**Competing interests:**

None declared.