Reviewer’s report

Title: Impact of socioeconomic deprivation on cause of death in Severe Mental Illness

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Reviewer: Lydia Gisle

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Impact of socioeconomic deprivation on cause of death in Severe Mental Illness

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On the whole, this manuscript is of interest in the field of psychiatric epidemiology and public health, and globally warrants the importance of linking databases (i.e. PsyCIS and SMR) for future research perspectives. The key finding, that is, premature death is more frequent in SMI patients and even more so in those socioeconomically deprived, is evidenced, though does not come as a surprise. The differential distribution of specific cause of mortality highlighted in the results is also of interest per se, though the analyses need to be refined. However, I would recommend some major and minor revisions (listed below), in order to make the best out of the study.

I. Is the question posed by the authors relevant and well defined?

I would tend to answer “more or less”.

Minor Essential Revisions

I.1. My first recommendation to the authors would be to match more adequately the title of the paper and the content of the study. More specifically, one would expect to have more indication on the impact of socioeconomic status on the cause of death in SMI patients (the main research question) but this does not stem out as the major focus in the results section of the abstract, nor later in the manuscript (except in the discussion). I feel the focus of the study is somewhat deluded in the manuscript. A clear focus on the research questions would help guide structuring the study report and abstract.

I.2. Background:

I find that the background paragraphs §1 and §2 are poorly fitted to the research question posed at the end of §3. It lacks a theoretical framework and/or a logical guideline that builds the knowledge and brings the research question forward. As I understand from the present structure:

Paragraph 1 documents the links [SES-health] & [SES-access to specialist care] and stresses the fact that mortality rates are high in Scotland and especially Glasgow in comparison to other Western countries (Reviewer point of view: 1.
Report some figures as examples? - 2. Question that comes to mind: is the SES in Glasgow and Scotland lower than in other Western countries, explaining these differences? How are these differences interpreted?...

Paragraph 2 states that SMI is an important determinant of premature death, but also of physical (ill) health, access to health care and quality of treatment (Reviewer point of view: must one understand here that poor physical health, access and treatment quality would explain why there is an excess mortality in SMI individuals? – Intuitively I would suspect suicide, intoxication and accidents to be the major causes of death in SMI patients...)

Paragraph 3 stresses that SMI has a negative impact on individuals’ SES over time (so would this be another explanation for the excess mortality in the SMI group with regard to general population?) What reasoning is followed here? Is SES the mediator between SMI and mortality? Is physical health (here, estimated through cause of death) suspected to vary in function of SES and SMI status? What then is the hypothesis in this study and how to reach elements of reply? ...

# I would recommend the authors to define the different research questions more clearly (maybe numbering them) and report the corresponding literature for each question mentioning the rationale for each and link between them and also briefly describe how these questions will be addressed in this study. The manuscript would also gain clarity if the authors would systematically structure the methods, results and discussion sections according to the cited research questions.

II. The methods are relatively poorly described and analyses could be stronger

The recommendations made here are merely to improve methodological clarity for the reader and statistical transparency.

Discretionary Revision

II.1. It is habitual to have a global section “methods” under which the subtitle “Construction and contents” could appear preferably specifying “of the database” or just having the subtitle “Database”. The second subtitle would be “sample” (maybe rule out “participants”!). Moreover, it may be opportune to add a third subtitle “Measures” just above the paragraph starting with “We used the Scottish Index...”. Then “Analyses” would be the fourth subtitle.

II.2. The content of the database is very detailed though only few variables are used in the present study: indeed, all variables included in the PsyCIS dataset are mentioned - some of which unknown to non-Scottish readers and thus should be clarified if kept. However, this part could be shortened without a loss of information regarding the study.

Minor Essential Revisions

II.3. In first § of the section “construction”, ICD-10 appears for the first time in the manuscript, so could be written out in full letters (whereas later, the sole abbreviation can be reported (point 7).

II.4. Under “sample”: Please report at the end of the first sentence the number of individuals sampled (N=230).
II.5. Please provide information about the linkage key (ID number?) used to match the individuals from the two datasets.

II.6. It is not clear whether the identified individuals from the PsyCIS dataset (SMI patients) are included or excluded from the “local Glasgow” and “wider Scottish population” mortality statistics, or even if the authors use mortality data in the analyses or only refer to tables of results…

II.7. Here, the ‘ICD’ abbreviation could be used, because described earlier (see point 2. above)

II.8. Please explain the SIMD score, which is not clear for readers from outside Scotland. Based on the second sentence of that paragraph, one could guess that it is an index determined at community level rather than at individual level (“Each individual was allocated to a SIMD category based on their postcode”) but this should be made obvious if the case. Besides, how is the following information gathered: current income (average?), employment (employment rate in the community??), education? The weightings method is understandable, but not the rationale behind these weights.

II.9. Ethical approval and privacy considerations should be stated at the end of the “measures” (or “sample”) sub-heading instead of under the “analyses” (sub-)heading.

Major compulsory revision

II.10. Authors start by stating that cause of death (…) was calculated and compared with (…). Nevertheless, it is not mentioned what calculation took place (Rates? Time?) and which tests were used when comparing these figures with those of Glasgow and Scotland. The second sentence of this paragraph simply repeats that comparative analyses were carried out. The third sentence is about statistical tests, but it would be useful to report exactly which tests were performed in which analyses and with which variables (including confounders?).

II.11. When analyzing the cause of mortality distribution by SES (also referred to as “social deprivation” in text) among SMI patients, tests such as the Gini coefficient, RII, SII or OR might be better adapted to answer the research question. This would also facilitate comparing the inequality slopes between populations (SMI-Glasgow-Scotland). Please check with statisticians.

III. Results

In text, first section on patterns of mortality in different populations:

Major compulsory revision

III.1. In the section regarding cause of mortality distribution in SMI patients in comparison to Glasgow and Scotland population, age should be analysed in first instance, followed by sex (or else, please explain why sex is not considered in this study?). Given that age at death differs between the 3 populations under study (SMI patients die at earlier ages), and that the cause of death distribution varies in function of age groups (cf proportional mortality ratios), it seems essential to take age into consideration in reporting the cause of death
distribution among the 3 populations, as age (and probably sex) are potential confounders. Age as confounder seems to be at stake in the results regarding the crude mortality ratios by population: cancer and alcohol-related deaths tend to be linked to older age groups (because they develop over time and hit the older populations), whereas suicide and drug-related deaths are more frequent causes of mortality in the younger age-groups. Again, I would recommend the advice of a statistician to carry out age-adjusted analyses.

In relation to Table 2:
Minor Essential Revisions

III.2. Title should better describe the content of table. Also give the time period covered (2006-2010?)

3. Subtitle “Deaths / age group” is preferred to “rate of death / age group” because % and N are both reported.

III.4. First age group defined as <25 is misleading. It is a group aged 18-24 years at death.

III.5. Subtitle “Death rate / Quintile” could be named “Deaths / SES quintiles”

III.6. Under this section (deaths/SES quintiles), it doesn’t appear useful to report the denominators, as the % are indicated.

In text, second section on patterns of mortality by SES:
Minor Essential Revisions

III.7. First sentence: It would maybe be more accurate to say “The number of deaths per 10,000 population” (indicate time unit) because the formula represents the rate, not the numerator.

III.8. It is interesting to note, indeed, that the death rate in SMI compared to other populations is higher even when suicide is excluded. The authors should briefly explain the reason for excluding suicide from the analysis, so it is brought to the attention of the reader.

Major compulsory revision

III.9. The results in Figure 1 are mainly descriptive. No tests are applied for estimating the difference in the SES trend between SMI patients and the larger populations. This would enforce the interpretation of the results.

III.10. The second paragraph of this section is dedicated to the main research question of this study, though the results are not reported extensively (only three causes of mortality reported in text, no table or graph). P-values are reported but what test was applied and were they age-adjusted?

In relation to Figure 1:
Minor Essential Revisions

III.11. Title could be: “Deaths per 10,000 population per 5 years (2006-2010)

III.12. Consistency: first quintile is referred to as “most affluent” in text and table 2, while reported as “least deprived” here, which does not really have the same
weight or significance.

IV. Discussion

Major compulsory revision
IV.1. Overall: the discussion lacks a theoretical framework and explanatory attempts are not straightforward. This part states results and refers to similar findings in the literature, but generally does not give an (evidence-based) interpretation of the findings.

IV.2. Overall: the structure that the authors have given to the background, the results and the discussion sections of the manuscript are different. The study would gain intelligibility if there was a systematic structure throughout the sections, related to precise research questions as mentioned at the beginning of this report (point I.2.).

Minor Essential Revisions
IV.3. First sentence under discussion: “consistent with other studies”: this is too vague, it does not say whether it was on the same population, what SMI were investigated versus what population, what years etc.

IV.4. Second sentence: in SMI, the proportional death ratio is greater among the aged 25-54 years, than in the 55-64. Saying “younger age” is somewhat misleading.

IV.5. First sentence under “Differences in the rate…”: would it be more clear if stated “We found that although individuals with SMI died younger than the Scottish population, the age at death…”

IV.6. Do the authors have a hypothesis on the reason why the age at death in the SMI and in Glasgow is similar, while both differ from the age at death in Scotland?

IV.7. Why should it be counter-intuitive that SMI patients die less from cancer and alcohol-related causes? I would not think so. Are there any scientific grounds to support one or the other observation?

IV.8. Beware, this observation (SMI die less often from cancer…) is (mistakenly?) repeated in the next paragraph.

IV.9. Why are the findings on cardiovascular deaths commented here, as they were not highlighted before?

IV.10. Please give some figures (age-standardised) when reporting the alcohol-related deaths in Scotland and in other countries (or an average for Europe).

IV.11. Role of deprivation.

I am sorry not to grasp the meaning or interpretation of this first paragraph under the mentioned heading. What would be the explanation here?

IV.12. The authors report the existence of a study of SES on mortality in bipolar disorder. What were the results in the light of this study?
IV.13. The “Scottish (...) effect”.
This section is quite long though not directly linked to the main scope of this research. The last paragraph of this section is difficult to grasp.

First sentence: Are we sure that Chi-squared tests are sound enough for testing trends? I would be tempted to check the data with more sophisticated statistics, including the comparisons between the 3 populations.

IV.15. Concerning the strength related to the PsyCIS database: the database is certainly large and comprehensive, but for this study, only 230 patients were part of the sample and only the information on the diagnosis and the postcode were used … Moreover data seemed to be collected prospectively starting in 2005, but the psychiatric condition of individuals in this study was reported in 2002-2005, when the data was entered based on retrospective information.

IV.16. Concerning the limitations. The small sample for the SMI is an important limitation, also for the generalisation of results. Another limitation is the fact that the mortality could only be assessed in a relatively short time span. Indeed, patients with a diagnosis recorded in the PsyCIS database (2002-2005) would have to have died in the 2006-2010 time period to be part of the study. Results may turn out to be different when considering a longer “mortality” time period.

IV.17. Another limitation of the study in my view is that the SES index used (based on area of residence if I understood correctly?) is a proxy for the SES status of the individuals. There may thus be a misclassification of certain individuals in the determined quintiles. As data exist in the PsyCIS dataset on the employment status, education and income of individuals, a “validation check” of the used index could take place?

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
I declare that I have no competing interests