Author's response to reviews

Title: Psychiatric Diagnoses in 3275 suicide: A meta-analysis

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Author's response to reviews: see over
Replies to Reviewers

Reviewer 1

Major Compulsory Revisions
There is one important point – the authors should make a reference to the largely similar systematic review by Cavanagh JT et al., published in Psychological Medicine in 2003, and articulate similarities and differences in methodology. Specifically, please explicate what new the present paper adds, compare the findings, and discuss possible discrepancies.

This is an important point. Over the course of our study, Cavanagh et al. published another meta-analysis of psychological autopsies, obtaining similar results. However, our studies are not identical, as there are differences both in methodology and major aims. While Cavanagh et al identified studies through a larger number of library databases, they included only studies up to June 2000. Moreover, they did not investigate risks attributed to specific diagnostic categories, but rather risks attributed to mental health disorders, presence of an affective disorder and comorbidity. They also investigated the role of a few social variables and did not carry out analyses exploring a possible gender and geographic difference in relative rates of psychopathology. We have revised our manuscript to include a discussion on similarities and differences between our study and that by Cavanagh et al. (discussion section, last paragraph).

Minor Essential Revisions
The paper by Henriksson et al., (in AJP, 1993) from Finland focuses on comorbidity and reports both multiple and principal diagnoses. Please correct the erroneous word “principal” in the table.

The reviewer is correct. We have revised the table.

Discretionary Revisions
The authors could elaborate the methodology of psychological autopsy a little more, perhaps make reference to some methodological reviews, and also explicate what they consider the minimum requirements for a study to be called a “psychological autopsy”. There is quite a lot of variation in the quality of studies, the more modest ones reporting lower prevalence of disorders.

Indeed, the reviewer is correct. We have better explained in the methodology section criteria we used to consider a study a psychological autopsy in the context of this review and have included references to resourceful papers such as one by Isometsa and colleagues published in 2001 and another one by Hawton and colleagues published in 1998.

There are some important sociodemographic and other factors that are likely related to the prevalences of mental disorders found in the studies. The most obvious is age, as the review includes quite many adolescent suicide studies,
and studies of elderly. Could the geographic comparison be adjusted for age? Furthermore, are studies focusing on one specific professional group or suicides by a particular method really “unselected”? I find it likely that both are selected subgroups (see e.g. Pirkola S et al., JNMD 2003). Are your findings the same if such studies are excluded?

This is a valid point. There are a number of interesting variables that could have been better controlled for. However, we were unable to carry out such analyses because there was important between-study variation in how the data was reported, and frequently, common demographic factors such as age and gender were not reported in such a way that allowed us to extract information for particular subgroups. Moreover, controlling for methodological differences would have considerably limited the number of studies included in the review. Therefore, we opted to be more inclusive and consider the results of this review as preliminary and providing information to be further investigated. Similar considerations apply with regard to studies that did not select subjects based on psychopathology but rather based on demographic variables or suicide method. Nevertheless, we reviewed the studies included in our study to include information on age by gender and geographical location (reported on table 5). We have expanded the discussion of our manuscript on the limitations, particularly with regard to gender and geographic analyses.

One important source of variation is often neglected, the fact that many studies focus on urban populations, and rural populations are underrepresented. This may inflate the role of substance use disorders, and result in an underestimate of the role of affective disorders which may be more central among rural populations (see e.g. Isometsa et al., APS 1997). Is the higher prevalence of affective disorders due to higher proportion of rural cases included that in North American or European studies?

This is also a very good point but difficult to assess given that individual studies did not systematically provide information on the proportion of subjects from rural and urban settings and when they did so, it was not always possible to obtain diagnostic information in each subgroup.

Finally, it would be interesting if the authors could compare countries with high and low suicide mortality, in order to generate hypotheses for explaining the variation. Could it e.g. be that in countries with high suicide mortality the relative proportion of substance use disorders is higher among suicide?

The analyses by geographic location, in spite of its methodological limitations, addresses to a certain extent this comment. We prefer this approach to classifying countries according to high and low mortality rates by suicide. This is because most of the psychological autopsy studies were carried out in countries with moderate to high suicide rates.
Reviewer 2

Major Compulsory Revisions

Change title to: “Psychiatric disorders in 3500 cases of suicide: A meta-analysis”
We have changed the title as suggested.

Add a column to Table 1 to include data relating to the proportion of participants with/without a diagnosis in each study.
As suggested, we have included this information. However, for clarity, we have done so to table 2 rather than to table 1.

Minor Essential Revisions

p. 8-9. Use of SD. Do the authors mean standard deviation, if so, this should be noted and these should be presented in brackets.
We have made as requested in the revised manuscript.

p. 5. 2nd line – needs grammatical revision (change “were” to “was”.)
The revision was made as requested.

Discretionary Revisions

A more considered discussion of the geographical findings would be desirable, at the moment it is somewhat descriptive. For example, the authors may wish to note that the geographical differences may be, to some extent, explained by cultural differences. The authors may also wish to “set-up” their findings by expanding on existing knowledge in this area in the introduction (e.g. in the last paragraph on page 3).

This is a valid point and as requested, we have expanded discussion in the revised manuscript concerning possible limitations and explanations of the geographic analysis (discussion section, revised manuscript)

Consider doing a sensitivity analysis to see if studies that differ methodologically influence the overall effect sizes (some variation in studies is recognised as a limitation of the study on page 14).
Although this would indeed be an interesting option, given the large amount of data and diagnoses, we opted not to carry out a sensitivity analysis. As mentioned earlier, we have expanded discussion on the potential limitations of this review.