Reviewer's report
Title: 3500 Cases of Suicide: A Systematic Review
Version: Date: 24 June 2004
Reviewer: Erkki Isometsä

Reviewer's report:

General

The present paper is a systematic review of prevalences of mental disorders among completed suicides as reported in psychological autopsy studies. The overall quality of the paper is good and it certainly merits publication.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

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.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

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.

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Discretionary Revisions (which the author can choose to ignore)

I think the paper could be improved by considering the following points:

1. 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 probably reporting lower prevalences of disorders.

2. 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?

3. 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. Isometsä et al., APS 1997). Is the higher prevalence of affective disorders due to higher proportion of rural cases included than in North American or European studies?

4. 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 suicides?

What next?: Accept after minor essential revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

None