Author's response to reviews

Title: Suicidal intention, psychosocial factors and referral to further treatment - a one-year cross-sectional study of self-poisonings

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Author's response to reviews: see over
Dear Editor,

Thank you for your review of our manuscript entitled Suicidal intention, psychosocial factors and referral to further treatment: A one-year cross-sectional study of self-poisoning. (MS: 4489331493024770).

The paper has been copyedited by a professional copyediting service and I therefore hope the quality of written English is now acceptable. Please let me know if further improvements are needed. The manuscript has been formatted according to the BMC Psychiatry journal style.

We have tried to address all the comments from the reviewers, and a point-by-point description of the changes is included below. Please let me know if further explanations or revisions are needed. We thank the reviewers for insightful and encouraging comments, and believe the manuscript have been substantially improved with their help. We hope our manuscript now will be acceptable for publication in BMC Psychiatry.

Yours sincerely,

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Reviewer 1: Jess Fiedorowicz

Reviewer's report:
The authors present a large, representative, cross-sectional study of 908 hospital admissions for self-poisoning in Oslo. Strengths of the study include a large sample of all such admissions during a one year period from multiple sites. One weakness of the manuscript is the presentation of large volumes of descriptive data, which may obscure focus and the key findings. The two most notable findings of the manuscript appear to be:

1) The striking similarity between those self-poisonings identified as suicide attempts and those described as “appeals.” This is a very important finding as it suggests, at least based on the cross-sectional clinical and sociodemographic factors assessed, identified intent does not well delineate distinct groups.

2) Patients with self-poisonings to substance abuse are less likely to be provided follow-up plans for discharge. This highlights an important apparent health care disparity.

The authors appropriately highlight these two findings in their abstract and conclusions though these findings do not appear adequately highlighted in the body of the work. The conclusions very well capture these highly relevant findings.

Major compulsory revisions:
A1) The terminology applied by the authors is somewhat unique and even varies through the manuscript. To avoid confusion, consider use of consistent terms for the three groups throughout and avoiding terms such as “suicidal.” The term “appeal” may be as problematic as the once popular term “gesture.” Care should be taken in choice of terms and these should be clearly operationally defined in the methods. For instance, does substance-abuse related poisoning reflect the substance used for self-poisoning or the primary diagnosis of the individual involved? The authors ultimately lump “definite suicide attempts” with “possible suicide attempts.” Perhaps it would be simplest to just initially delineate the group as “suicide attempts” which are defined as possible, probable, or definite self-poisonings. It is also not clear precisely how an “appeal” is delineated from a “possible suicide attempt.” This may involve clarifying the operational definition of the term and even renaming it accordingly.

MB: Thank you for this useful comment. The terminology has now been made more consistent throughout the manuscript, and the term “suicidal” is avoided. The paragraph entitled “Criteria for classifications” in the methods section have been modified, and the categories for intention are now better defined.

The field of suicidology suffers under the lack of consistency in the terms used (1). Clinically, there are a spectrum of self-poisonings varying from the clearly planned, medically serious suicide attempt with an outspoken intention to die, to impulsive actions which are never life-threatening, and where the intention is not to die, but perhaps make an appeal to others (2). The term “appeal” is problematic, as some fear the term will devalue the intention behind these self-poisonings, or that doctors will take these actions, and therefore these patients, less seriously. However, we lack an appropriate term for this group of patients. Both “gesture” and “cry of pain” patients have been used in the past, but are probably even less appropriate. The reviewer proposes that we use the terms high to moderate suicide intent vs. low
suicide intent (see B3). We have chosen to define suicide attempts as patients with a high to moderate intent, and appeals as patients with a low or no suicide intent. We have included a definition of substance use-related poisonings as well.

We chose to keep the term “appeal” as we lack a better term, and because this was the term used in the original study form presented to the participating physicians who evaluated the patients. We have commented on the problematic term “appeal” in Strengths and limitations, last paragraph. We tried to use the term “patients with a high to moderate suicide intent” vs. “patients with a low to no suicide intent”, but this made the sentences long and less reader-friendly. We will however change the terms if the editor insists.

A2) The results and discussion sections in particular could be better focused and organized. Refitting the discussion into a more traditional format may prove helpful.
MB: The result section has been re-organized, and we believe it is now better focused (see also B2). The discussion section has been re-organized as well. The first paragraph now highlights the key findings, as suggested by the reviewer. The following paragraphs have been modified according to the suggestions made in B4-8, but the structure is altered as well. Please let me now if the Discussion still warrants re-organizing.

Minor essential revisions:
B1) It is really quite impressive that the authors were able to capture “all patients contacting health care services because of self-poisonings in the capital of Norway.” The authors present data on “all hospitalized.” Is there any data on what proportion of those presenting with self-poisonings are admitted? Also, to clarify, did only one patient refuse to participate? How could the participation rate be so high? Were participants paid? Consented?
MB: The patients gave an informed consent to participation before the interview, and only one patient refused to participate. We have included this in the Ethics section. The patients were not paid. The high participation rate is interesting, as this reflects that these patients, who are often believed to be hard to engage in studies, were willing to participate when asked by their physician in a study they presumably found relevant.

Of all self-poisonings in Oslo during the study period (2997 poisoning episodes treated by the health care services), 69% were treated outside hospital, i.e. by ambulance services, or in the Oslo Emergency Ward (an outpatient clinic) (3). Only 31% were transferred to hospital. Those who were not transferred to higher levels were more often substance use-related poisonings than those who were hospitalized. In Oslo, the majority of opiate or opioid poisonings are treated on scene by the ambulance services, unlike many other countries (4). We have commented on this in the Discussion, third paragraph.

The completeness of the inclusion of patients in these types of studies can always be questioned. However, we included patients at three levels of healthcare (ambulance services, the outpatient clinic and hospitals), and transfers between these levels were common. Because of each patient’s unique social security number, we were able to trace all patients through different levels of health care. This helped to make the study more complete because each patient could have been included in up to three treatment facilities during each episode, and a study of repetition patterns among the patients revealed that very few patients were lost to follow-up when transferred to a higher level (3). In each hospital, there was a study coordinator.
supervising the inclusion of patients, and the study group supervised these coordinators on a weekly basis in order to ensure a high participation rate, and we believe that our figures are as close to all poisoning episodes as we could get. We have included this in the Strength and Limitations section, first paragraph.

B2) The results section could be better focused, particularly with the volume of tabular data presented. This may facilitate the authors focusing on the main findings. Without presenting general population data from Oslo, the internal comparisons (i.e. between the three primary study groups) seem much more relevant to external comparisons to the study base (particularly without the reporting of any general population data).

**MB:** We have omitted some of the tabular data from the results section, in order to focus more on the main findings. The comparison to general population data from Oslo is included in the Discussion (see B4).

B3) Discussion, paragraph 2. Please consider changing “partly on the patient’s intention” to “partly on the patient’s reported intention.” Given the statement regarding “ambivalence” being characteristic of suicidal behavior, does the term “appeal” seem appropriate? Should we perhaps simply be characterizing this as low intent versus moderate to high intent? The last three sentences of this paragraph are somewhat inconsistent and unclear.

**MB:** The paragraph has been modified and made more consistent and clear. For comments on the term “appeal”, see A1.

B4) Discussion, paragraph 3. The discussion about social integration is interesting. While presumably unemployment in Oslo is not 2/3, these external comparisons could perhaps be bolstered by some data which compares to the study base (population of Oslo).

**MB:** Data from the general population of Oslo have been added, in order to compare the study sample to the general population, see Discussion paragraph eight.

B5) Discussion, paragraph 4. Much of this paragraph can be eliminated as it seems to distract from the key finding (less frequent prior attempts in substance use-related poisonings). The next several paragraphs seem to focus on differences between substance use-related poisonings and suicide attempts or “appeals” for a given variable. The key message here seems to be that substance use related poisonings appear cross-sectionally different from suicide attempts while “appeals” do not. However, the individual paragraphs frequently digress. This can perhaps be condensed into a paragraph highlighting similarities and another with differences with comparison to the relevant literature.

**MB:** We have eliminated much of this paragraph. Paragraph 2 and 3 now focus on the differences and similarities for appeals and substance use-related poisonings compared to suicide attempt patients.

B6) Discussion, paragraph 5. ”Determined presence of a death wish over a significant time” does not clearly arise from the methods presented. It is not clear what data this conclusion in resting upon.

**MB:** The sentence is omitted and the paragraph modified.

B7) It is not clear what “those on sick leave” is referring to from the data presented.
B8) Discussion, paragraph 8. The sentences beginning “The gradient of suicidal intent” appears to contradict rather than support the following sentence beginning “Appeals and suicide attempters.” If a gradient of suicidal intent influences risk, than why would the very low intent “appeals” be in the same group as suicide attempters. 

MB: The sentences have been modified in order to clarify the intended meaning.

B9) Table 3. Please specify what the models are adjusted for in the table legend. How is the crude OR for age 30-49 significant when the 95% C.I. crosses 1.

MB: The table legends have been extended, as suggested. The OR for age 30-49 in table 3 had been shifted to the wrong rows, but this has now been corrected.

B10) Table 6. This table seems to be a distraction from the point of the manuscript and could be eliminated.

MB: The table has been eliminated.

Discretionary revisions:

C1) Background, paragraph 3. Consider replacing “fundamental principle” with “challenge” since the preceding data suggests this is difficult to do. Later consider replacing “repetition” with “repeat attempt” for clarity. Reference 12 appears to be a key reference for a main finding and perhaps should be expanded. 

MB: The terms have been changed as suggested. Reference 12 is commented in the discussion.

C2) The correlation between current and past psychiatric treatment does not seem useful. Consider removing.

MB: The correlation has been removed.

C3) Discussion paragraph 9. 67% not referred does not seem to similar. It represents a 3-fold higher referral rate. Consider re- wording.

MB: The sentence has been re-written.

C4) S&L, paragraph 1. Consider replacing “in order to generalize from the study population” to “to facilitate comparison of study sample.” In paragraph 2, consider replacing “bias” with “limitation” and “possible” with “easier.”

MB: The terms have been changed as suggested.

C5) Table 1. Consider combining “well not tell” and “unknown” columns.

MB: The columns have been combined as suggested.

C6) Table 2. The unknown rows could also be listed as a percent for consistency.

MB: We chose not to include the unknown rows in the calculations of percentages, as we assumed that the unknown answers were distributed evenly among the patients. Therefore, the proportion of cases in each category would have been the same if these answers were known. If we listed the unknown rows as a percentage, this would reduce the percentage of employees etc., possibly giving a false low percentage. Furthermore, we would introduce “unknown” as an occupational status, which is not meaningful. The “unknown” groups were not included in the multinomial analyses. However, we chose to report them (table 3, 5), as we believe they are useful when
considering the reliability of the percentages reported. The reliability is commented in the Strengths and Limitations section, third paragraph.

C7) Table 7. Please include OR in column header. A very impressive finding regarding the no referral frequency with the substance abuse group. Again, I think this warrants more emphasis and discussion.

MB: OR are now included in the column header. This finding is now emphasized in the first paragraph of the discussion, and discussed in the fourth and fifth paragraph.

English: Needs some language corrections before being published

MB: The paper has been copyedited by a professional copyediting service in order to improve the quality of written English.

References:

Reviewer 2: Ahmad Ghanizadaeh

Method
What does assess the standardized registration form? Is its validity and reliability enough? What are the criteria for classifications? Considering that only one category was applied to each patient, what about its intra- and inter-raters reliabilities? Of course, it is a concern of the authors as they mentioned in the limitation section.

MB: As discussed in the Strengths and Limitations section, there standardized registration form are not tested for intra- or inter-rater reliabilities. Testing the forms validity and reliability was beyond the scope of this study. The form is based on clinical terms commonly used in clinical interviews and the patient’s charts. We therefore believe it to be as reliable and valid as any clinical evaluation, with its strengths and weaknesses, and we have included this comment in the S&Ls paragraph 2. As for the criteria for classification, particularly of intention, reviewer 1 also commented this and we have modified the Criteria for classification section. Please see A1 in the comments to reviewer 1.

Some numbers in tables and text needs to be rechecked or clarified. For example: 33% of them had been hospitalized in psychiatry ward before. 42% among suicidal, 35% among appeals, and 19% among substance use related poisoning (42%+35%+19%= 96%).

MB: The percentages in table 3 and 5 are calculated for each column, not row. We have included this in the table legends to clarify this matter. The figures used for calculating percentages (N=number of patients) are already included in the column headings.

Some of the total percents in table 1 are more than 100 percent.
MB: We have corrected this.