Author's response to reviews

Title: The Association Between Social Relationships and Self-Harm: A Case-control Study

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Title: Social Support and Social Network of People with Self-Harm: A Case-control Study

Response to the reviewers

We are very grateful for the reviewers’ helpful and constructive comments. After re-examining our data and adding relevant confounding factors in the analysis, we have made major revisions to the entire manuscript. We are pleased to outline our responses as follows.

Reviewer #1

The authors present a case control study examining social isolation and self harm, employing an interesting measure of social isolation. Although the study could make a valuable contribution to our understanding of social risk factors for self harm, I think that more consideration of confounding factors is necessary, as is recognition of the limitations of cross sectional data.

◆ We accept this point and have added some additional potential confounders including:
  1. Major life events
  2. Physical illness
  3. Mental health conditions: depression (assessed by the PHQ-9).

We have inserted text describing the measurements of these factors in the Method section (please see p.8-10, the subheading of “Measurements”) and have revised the Results section (please see p.11) accordingly in red font. We believe that these updates towards the manuscript have provided better support for the hypothesis and our findings.

The authors could also elaborate on the cross cultural aspect of the study and perhaps relate the findings more explicitly to theoretical models. Suggestions below are to improve the manuscript.

The authors raise in the introduction a very interesting cultural comparison – whether social support is more important regarding suicide in non-western cultures. However no comparison data between cultural groups is offered and the authors do not return to
this issue. Could the authors elaborate on this question, which has interesting implications for the important of social support and also for the need for culturally specific risk prevention?

◆ We appreciate this comment. However, we did not aim in this study to generate data for explicit comparison with those from Western countries to which we do not have access. The aim of this study was to present an evidence-based study within Taiwan (to where findings from Western research cannot automatically be assumed to apply) and to address a specific question around the influence of social relationships on self-harm within this population. Nonetheless, we have added material in p.5 Line 7-11 of Paragraph 1 to strengthen our point regarding the potential role of culture together with our aim of addressing the need for more evidence in this field (from p.5 second paragraph to p.6 in red font). We have also discussed the social structural influence to self-harm in Taiwan and the Chinese cultural in general (please see from p.12 second paragraph to p.13 first paragraph in red font). We believe that our findings provide an important basis for future studies but believe that an explicit between-culture comparison would be beyond its scope.

In terms of interpretation from the data, the authors conclude “It is likely that active and regular social contact makes a person free from self-harm behavior.” I think this is over reaching from the data, in particular I don’t think that sufficient consideration is given to the directionality of these effects (does self harming lead to people isolating themselves) or the possibility that they are correlates but not causally linked.

◆ Since we have considered additional confounding factors in the revised analyses, we feel that the findings do provide a stronger basis for readers to judge the influences of social network on self-harm. We have revised the Conclusions according to our results, please see p.17.

However, we acknowledge the limitation of inferring causality from a case control study. We have considered in more detail the directionality of the social networking association with health and have discussed its application to our results through the Berkman model [Reference 12] (see both p.5 Line 3-7 in Paragraph 1 and p.12 Line 2-5 from the bottom of Paragraph 2). Limitations around inferring causality are highlighted on p.15 Line 2-6.
from bottom of Paragraph 2.

I also think some consideration of the possibility that mental health status would account for the differences is necessary – there is no evidence provided that social isolation is specific to self harm – this at least needs to be considered in the limitations section. For example, in choosing the controls, the authors note they excluded patients with a psychiatric history, but this could risk the findings relating only to psychiatric populations (as we would assume the participants who self harm would qualify for psychiatric treatment) rather than relating to self harm specifically – was a psychiatric, non self harming control group considered? (Further to this, could the authors justify their choice of family medicine outpatients – would a comparison of A&E attendees who attended for different reasons have been better?)

◆ The potential influences of mental health have been controlled for in the revised analysis and discussed in p.11 second paragraph contents in red font. In terms of the selection of controls, because the family medicine outpatients may consist of a range of potential mental health problems which are prevalent in the cases as well in the medical context in Taiwan, we felt that it is appropriate to use such a control group (note: visits to the hospitals in Taiwan are mainly self-referral and people access specialized services based on their own judgment without any primary care ‘gate keeping’ or process of referral, for more information about common mental disorders in general medical patients in Taiwan, please see Liu et al, Psychol Med 43(4):629-637, 2002).

The study is atheoretical. How might these findings on social support relate to recent theories of self harm?

◆ We have inserted contents stating our conceptual basis and theoretical framework in p. 5 Line 3-7 of Paragraph 1 and p.5 Line 9-14 of Paragraph 2. We have focused on the influences of social relationship on self-harm, studies of which have been very limited resulting in a lack of a theoretical basis in this respect. We believe that our revised manuscript will provide helpful evidence for future research and further theoretical development.

In the discussion, the authors refer to several ‘reasons for social isolation’ such as unemployment and major life events – were these factors examined in the study? Could social isolation be a mediating factor leading to their impact on self harm? I
think defining more specifically how the authors think social factors impact on self harm would help to identify what further data is necessary and draw out whether the conclusions are support by the correlational data.

In our earlier response to this comment, we have mentioned that potential confounders have been adjusted in the revised manuscript. So the factors you referred to has been examined (see p.11 second paragraph and p.12 for main results and discussion). We have also added text more specifically about the impact of social relationships on health and their correlations in the Background (please see p.4 Paragraph 2 to p.5 Paragraph 1). Please also see our justification of how our data would be important reference to the literature in p. P12 paragraph of “Key results”.

Finally, the introduction mixes references to suicide and DSH. The authors need to be clear which they are investigating, and if they are arguing that investigating DSH has implications for reduction of suicide, this point needs to be made explicitly and with references (the majority of people who engage in DSH do NOT attempt suicide, though the risk is higher, and it has been debated in the literature whether they can be viewed as the same population or not.) These points would be helped by providing a clear definition of the terms as used in the manuscript. A focus on attempted suicide would more clearly link to suicide prevention but the authors state ‘cases’ were defined regardless of suicidal intention. I think these definitions could therefore be clarified to make the objectives and population clearer. For example, the first sentence says that deliberate self harm has a risk of ‘fatal repetition’ but this confuses the terms (deliberate self harm is by definition without fatal intent). Should this say ‘is linked to risk of completed suicide’? A second example is the sentence “The degree and aspect of social risk factors related to completed suicide may not be the same as those of DSH. Studying social attributes among people who have harmed themselves is important for early prevention of suicidal behaviour.” If the risk factors for suicide are different from those of self harm, then studying factors prevalent among people who self harm does not necessarily lead to prevention of suicide. Clarifying this will make it clearer to the reader where the study sits in terms of suicide reduction.

We have made a clear distinction of self-harm and suicide and also made major amendments to the Background section where the definitions of self-harm and the differences between self-harm and suicide have been described in more detail. Please refer to p.4 Paragraph 1 for our revisions.
Reviewer #2

- Major Compulsory Revisions

1. Title uses awkward English and lacks specificity. Does a person “have self-harm” or “commit self-harm”? Is self-harm fatal (i.e. suicide included), non-fatal, both? Does not state what was found.

   ◆ We have revised the Title of the study as, “The Associations Between Social Relationships and Self-Harm: A Case-control Study”. Consistent use of the term, “self-harm” has been employed in the revised manuscript with its definition described in more details in the first paragraph of the Background section (p.4).

2. Abstract, Results second sentence is incorrect: age±SD given is for cases only. Abstract, Conclusions first sentence is an over statement: (1) based on single case control study, and (2) unclear how these results relate to suicide prevention - especially since intent is not assessed.

   ◆ We have made major revision towards the sections of Abstract (p.2), Results second sentence (see the revised description about age±SD in p.11), and Conclusions (p.17).

3. The major concepts used in this research are not well described/presented. First sentence of Introduction uses terms “deliberate self-harm (DSH)”, “attempted suicide”, and “completed suicide” but never describes them and/or how/if they relate to one another and/or differ. One could assume “attempted suicide” and “completed suicide” are both subtypes of DSH. Importantly, these descriptions are central to the research and never operationalized. Further confusing the central concept of the manuscript the authors do not use terms consistently throughout (e.g. “self-harm”, “suicide”, “suicide attempters”, “non-fatal self-harm”, etc.)

   ◆ We have unified the term into “self-harm” throughout the manuscript and inserted more details about our conceptual basis and theoretical framework in p. 5 Line 3-7 of Paragraph 1 and p.5 Line 9-14 of Paragraph 2.

4. In general, the manuscript is sub-optimally written (i.e. use of English language,
structure, content). Authors should rewrite manuscript based on STROBE Statement for case control studies (see http://www.strobestatement.org/index.php?id=available-checklists). Some examples follow. The manuscript includes non-standard (e.g. quality assurance) subheadings that do not organize content in logical flow and important information is left out (e.g. timing of data collection, data sources, analytic variables are never described). As a result it is very difficult to get a clear understanding of the scientific quality of the study or the importance of the findings. Methods is broken into Cases and Controls, but there are no sections on study design, setting, population, variables that succinctly describe where the study is conducted, how many persons are included (total and by type, why 85 cases unmatched), time frame of the study, why timeframe for cases and controls is different, justification of selected timeframes, data sources, operationalization of variables, etc. Instead, this critical information is incompletely (sometimes inappropriately) distributed throughout various sections making it difficult to arrive at a proper overview of the study or interpretation of validity of findings.

◆ We have revised our article specifically following the rules stated in the STROBE statement. Please refer to the inserted changes in red font for more details on pages 7-10. We have clarified that the number in each group was 124, and there were no unmatched subjects.

5. First paragraph Background refers to official statistics but authors do not say from where, or why they are underestimates. Are there no statistics anywhere in the world on self-harm? Are all underestimates? A simple Google search keyword “self-harm” gives over 2 millions hits, first hit statistics from USA. Moreover, the context for the study needs to be presented. What is the specific research gap being addressed, no information on self-harm social support worldwide, in East Asia, specifically Taiwan? Literature on social support and suicide is presented but how does this relate to self-harm (are they different, related concepts – see point 3 above) as investigated by this study? The Discussion mentions several self-harm social support studies but state of current knowledge and knowledge gap to be addressed by this study are not presented in Background and should be.

◆ We have made major revision towards the Background section and provided more information on social relationships and the context of the study. Please refer to the changes in red font in pages 4-6. Regarding the knowledge gap issue, we have added some further consideration in the
6. Methods, Measurements is three paragraphs of detailed explanation of Close Persons Questionnaire (CPQ). No other variables were described. DSH (inappropriately placed in Methods, Cases) should be clearly described in variable section – what is the source of the data, how was it measured/used for analytic purposes? The CPQ is not new and therefore should only require a brief explanation with references to the seminal article and all other psychometric evaluations. It also seems the references for the CPQ are perhaps inappropriately used. Methods, Measurements first paragraph: first CPQ sentence should give the seminal article as reference, reference nine does not seem by title a psychometric evaluation of CPQ, third sentence reference 26 does not seem by title an article about CPQ structure, etc.

◆ We have revised the introduction of the CPQ in the Measurement section, please see p.8 the subheading of “Social relationships” for more details. We also acknowledged the need to add the confounding factors and therefore have added descriptions of their measures in p.9 and improved our analysis by including these factors in the statistical results (see “Statistical analysis” in p.10 and p.11 Paragraph 2 for more details). We believe that these revisions have improved the quality of this study and manuscript.

7. Methods, Statistical Analysis is insufficiently described. The authors mention use of z-scores but provide no rational or reference justifying their method. There is no information on statistical methods used for descriptive statistics or model building. Methods, Statistical Analysis: why were stratified analyses “exploratory”? Later in Results: “Secondary subgroup analyses…” inconsistent/confusing use of terms. Nowhere do the authors state if they had a priori hypotheses so there is no context for understanding primary, secondary or exploratory analyses. Table 1 lists paired t-tests – why paired? Etc.

◆ We have provided more detailed information on statistical analyses and have rewritten the whole sections of the Methods and Results. The rationale of transforming the scores of each subscale was simply to generate a unified ‘unit’ (which was the standard deviation of control group representative to the general population without self-harm behaviour) and common platform of comparisons among them. The rationale has been emphasized in p.10 under the subheading “Statistical analysis”. All the terms about subgroup analyses were unified as “subgroup analyses…” in sections of the Methods
and Results and the term “exploratory” has been removed. Finally, because of the study design involving matched case-control pairs, we believe that paired t-tests represent the standard statistical method to compare a continuous variable between the two groups.

8. Results first paragraph should describe the study population (cases and controls). Results second paragraph “…CPQ was not a significant exposure …” CPQ was never defined as “exposure” and assume authors mean “statistically significant”. Every variable mentioned in Results or listed in tables should be described in Methods section and consistently used throughout. There is no need to present p values and 95% confidence intervals.

◆ We have revised our Results first paragraph and stated the exposure measure in P.8 in the first paragraph of Measurements. Every variable mentioned in the Results or listed in the tables has also been described in the Methods section and consistently used throughout. Please refer to the major changes in red font throughout the whole manuscript in the pages of 8-11 for the consistency.

9. Discussion section is not well structured. Information is mixed throughout (e.g. limitations exist in many paragraphs) and content does not logically flow (i.e. expect to have summary of key findings, findings compared to other studies, strengths and weaknesses, etc.) Methodological issues are the biggest part of the Discussion content, needs revision and restructure (some examples follow): the study design does (not may) limit generalizability, “… negative findings should be interpreted with caution…”(i.e. what does this mean, findings that were not statistically significant?), case control design makes blind assessment impossible not timing of assessment, “…reverse causality…” unclear should be explained. Discussion fourth paragraph “ unclear what “Researchers made efforts to lower influences of emotion on data accuracy…” means, how/when it was implemented, or if technique is valid/reliable/measurable. Discussion, Conclusions is an over statement: (1) based on single case control study, and (2) unclear how these results relate to suicide risk prevention - especially since intent is not assessed.

◆ The Discussion section has been re-written throughout p.12-13 with major revisions. We have sought to improve the flow and logic of the contents based on our revised literature review and results. Given the limitation of the case-control approach in this study, we have considered confounding to the best of our ability
in our analysis and have re-addressed the impact of social relationships on self-harm and its implication in an oriental society in the Discussion (p.13 Line 1-5).

10. The manuscript uses abbreviations but does not include a section on abbreviations as required by author instructions. The authors do not consistently use or introduce properly abbreviations throughout the manuscript. For example, (Methods, Cases paragraph 1) should be “…attending the Accident & Emergency (A&E) had been…”

◆ We have unified the use of abbreviations and used the terms consistently throughout the revised manuscript.

- Minor Essential Revisions
1. There are many grammatical issues (inconsistent use of hyphens, improper use of abbreviations, awkward use of English language e.g. “caseness”, “strongest odds ratio”) throughout that should be addressed by a compete rewrite of the manuscript.
◆ Thanks for your comments. The manuscript had been revised by a native English speaker for these issues.