Author's response to reviews

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

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Version: 3 Date: 25 February 2013

Author's response to reviews: see over
Dear Professor Emmelkamp

Thanks very much for giving us the opportunity to revise our manuscript again. We are also appreciative about the two reviewers’ opinions and find them useful in revising our article. We present our point-to-point responses as shown below. Please also refer to the revisions in the manuscript in red font for more details.

Reviewer: Kerri Clough-Gorr

Reviewer's report:
The authors have made considerable changes since the original draft. In my opinion, however, it still requires major revisions. Some (not all) of which are summarized below. I would strongly recommend the authors seek a senior investigator with lots of scientific peer-reviewed publications to help restructure/edit the manuscript.

[Authors’ response]
We are thankful for Dr. Clough-Gorr’s suggestion. The article has been proof-read by an English-speaking co-author, and we have improved the quality of our manuscript.

Major Compulsory Revisions
It seems the aim of the study is to consider social factors in relation to self-harm (SH) specifically in an Asian setting. Perhaps the title should indicate this.

[Authors’ response]
We have added a few short and concise sentences to make our aims more clear (See p.2 the last sentence of Background in the Abstract and p.5 the last paragraph). We have also revised our title as, “The Association between Social Relationships and Self-Harm: A Case-control Study in Taiwan” to reflect the Asian setting of this study.

The abstract neglects to mention the primary outcome.

[Authors’ response]
We have stated our primary outcome in the first sentence of the Methods in the
Abstract. Please see p.2 where we inserted that, “The primary outcome was self-harm with hospital presentation”.

There is still confusion about the definition of SH & suicide. Introduction sentences 2 and 3 are inconsistent. First sentence says SH includes failed suicides and second implies otherwise.

[Authors’ response]
We have provided definitions of suicide and self-harm as well as explained further about the relationship between self-harm and suicide (see p.4 Line 2-4 & 8-10). The detailed definition of self-harm can also be found in p.7 Line 4-10. We believe that we have used a commonly and internationally acknowledged definition of the outcome, i.e. self-harm, which was consistent with our previous publications.

The methods section is still poorly structured, lacks information and information is mixed up between sections (see previous review regarding Strobe criteria). For example, the section on study population (participants) should include all information about inclusion/exclusion criteria. There is an entire paragraph describing life events which are not used in the analyses. The analytic methods are never explained (see previous review). Variables lack proper explanation/operationalization (see Strobe criteria). For example what diseases were on the list? Did it include severity? Why were those diseases chosen? Aim of the study shows up under methods-measurements instead of introduction. What was criteria for including variables for adjustment? Etc.

[Authors’ response]
We have conformed to the STROBE criteria more strictly. Subheadings were revised according to each item of the Methods criteria in the STROBE checklist in order to avoid information mix-ups. Major changes were as follow:

1. Information regarding the participants was entitled with the subheading of “Participants” and had been independent from other contents in the Methods section in p.6. The inclusion/exclusion criteria of the sample was described in details from the last lines of 1-5 in p.6 to the first 1-4 lines in p.7.

2. The life events paragraph describes its definition. We included this variable because it was suggested as a potential confounder in the literature, as shown in the second paragraph in p.5. We have pointed out more clearly that this variable was one of the controlling factors in our statistical analysis (See p.7 under Subheading of “Measurements and variables”). Therefore we kept the contents of this paragraph.

3. Each variable listed in the “Measurements and variables” section remained in our
revised draft. We have double-checked explanation/operational definition of each variable and further added necessary information about how each variable was analysed or recorded, as shown in the red font of each paragraph in p.7-9. Specifically, the variable of “Physical health” had been described with its definition, the reason why we performed review of system disorders, the meaning of its severity, as well as where we chose these diseases (See p.8 the second paragraph).

4. In response to the reviewer’s previous review opinions, we have improved the quality of contents in the Methods by dividing proper subheadings based on the STROBE guideline and re-organize contents in a more logical flow. We have also answered each question including timing of data collection (p.6 Line 2-8 under the subheading of “Study design and setting”), data sources (p.7 Line 6-9), analytic variables (p.7-9), and persons included (p.6 “Participants”).

5. The descriptions about the aim of the study that shown-up right under the Measurement section were deleted.

6. Criteria for including these variables for adjustment were based on suggestions of the literature. Please see p.5 the second paragraph.

The results are very limited and text pithy compared to other sections. Authors redundantly state that logistic regression was used (2 times in methods, once misplaced in results). Text only provides descriptive info for cases. Perhaps inverse association would be more accurate than negative. The lack of results gives the impression that the study is too small an advance to publish.

[Authors’ response]

1. We have made our best to provide sufficient information including primary and subgroup analysis in the Results section in order to examine the underlying hypothesis of this study.

2. We have deleted the redundant description of logistic regression, leaving only one mentioning of its rationale in p.9 Line5-7 of Statistical Analysis section.

3. Descriptions regarding the case and control group were more balanced with contents re-organised in a consistent and comprehensive manner.

4. As our results showed, significant association remained for self-harm and social networks after adjustment in the logistic regression. Although an inverse association would be possible in this case-control study, which would require a different study design (e.g. cohort study) to examine its case-effect relationship, a salient relationship of the underlying investigation could not be neglected. Therefore our results were based on the evidence and provided important implications for clinical and research. Taking into considerations of the
confident intervals and other statistics, they also showed that our study was not severely under-powered. These all showed considerable values for publishing this study.

The discussion remains too long and unfocused. Completely out of proportion with stated results. The discussion does not succinctly/logically explain how the research constitutes a useful contribution to the field.

[Authors’ response]
We have made our discussion section more concise and readable with powerful points of discussion. Please see the revised contents in the manuscript.

Reviewer: Sarah E Knowles

Reviewer’s report:
Major Compulsory Revisions
Use of English in the some of the revised sections is poor in terms of grammatical errors and sentence construction. I would recommend a thorough proof reading.

[Authors’ response]
Thank you for Dr. Knowles suggestion. The revised manuscript has been proof-read by an English-speaking co-author once again. We are sure that the quality of writing has been much improved.

I appreciate the inclusion of a theoretical model (Berkman) although I think linking the results to a model of self harm specifically may be more appropriate. For the example, the Cry of Pain model (Williams 2001) includes social rejection as a risk factor and social support as a protective intervening factor and would be relevent here. The decision to use Berkman is also odd given that in their comments to reviewers, the authors state an international comparison is beyond the scope of the study, but Berkman’s model is specifically concerned with wider culture and society. It may help to specify whether the authors view their study as relating to the micro, psycho-social mechanisms of the model.

[Authors’ response]
We appreciate Dr Knowles’s suggestion of adopting the CoP model in our study. We feel that the concept in the model indeed reflects part of our aims. We have cited the study in our Introduction and discussed how our results fit with the theory in the Discussion. However, the measure and scope of social support in the model were different from ours. We meant to examine the relationship between a comprehensive
measure of social support/network and self-harm. Therefore we cited some concepts of the Berkman’s model in an attempt to explore the psycho-social mechanisms in an Asian context. We have added the following contents in the manuscript specifying how the CoP model relates to our study.

1. Conceptual framework with citation of the CoP model in p.4 Line 10-12;
2. Implication of our results in relation to the CoP model in p.12 Line 6-7.

p10 - Please state exactly which measures were controlled for as covariates and also whether adding this number of covariates has any implications for power.

[Authors’ response]
We have added a sentence at the bottom of p.9 to specify the covariates controlled as confounders for adjustment, which were also highlighted in Tables 2 and 3 as footnotes. Despite the dilemma that adding confounders would increase the precisions of estimation on the effect of major exposure of interest (in our case, components of CPQ) while losing statistical power, confounders can be entered in the final model given they are well-characterized independent risk factors. Since we have had a reasonable sample size and a fairly efficient study design (i.e. individually matched case-control study), the risk of adding covariates and losing power should be less of a concern. It also shows that the minimum detectable relative risk (i.e. odds ratios as shown as 1.18 and 0.85) at the power level of 85% is one of the best achievable approaches in the literature. Thus we have controlled for the covariates listed in p.7-9 under the subheading of “Measurements and variables” in the manuscript.

Minor Essential Revisions

p4 The authors state "Given that risk factors for suicide have not been found to generalize necessarily to self-harm [5], further research is needed" - however there is no reference provided that social networks are a known risk factor for suicide either?

[Authors’ response]
We have deleted the contents mentioned by the reviewer and revised the overall descriptions of this paragraph. Please see the second paragraph in p.4 to p.5.

p9 "We recorded the participants’ diagnosed medical illness from a list." This is too vague - does ‘a list’ refer to hospital records, a self report form?

[Authors’ response]
We have revised the original descriptions mentioned by the reviewer. Basically all reported disorders related to every organ system were evaluated. We have also provided how we calculated the severity of physical illness. Please see p.8 the second
The authors state "we controlled for a range of potential confounders including measures of physical and mental health" but there appears to have been only one measure each of physical and mental health included and it is not clear in the paper why these are particularly important as confounds. Can the authors reference studies showing the importance of physical health conditions and depression to self harm, to justify why controlling for these strengthens the analysis?

**[Authors’ response]**
We have provided the reference of a systematic review to justify our analysis with adjustment. Please see p.5 the last paragraph where we cite Reference 11.

Discussion - I do think that the authors are over-reaching from the data, which is a cross sectional, descriptive study, to suggesting interventions. I would prefer to see more discussion of how the findings relate back to theoretical models (though some discussion of this is now included) and what this leads to in terms of further research (for example, longitudinal studies to overcome the limitations of case control designs).
On other issues such as measurement I think there is appropriate discussion of the limitations.

**[Authors’ response]**
We have re-addressed the Discussion and Conclusion sections based on the evidence of our results and the reviewer’s suggestion. Our findings have been related back to theoretical models, and we have also suggested longitudinal studies for future research. Please see the last paragraph in p.11 to the first paragraph to p.12 where we inserted our discussions relate to the CoP model (Reference [12]) and the Berkman’s model (Reference [16]). As for what our findings lead to study, policy and clinical implications, please see the second paragraph in p.13.