Author's response to reviews

Title: The situation of former adolescent self-injurers as young adults: A follow-up study

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Version: 3 Date: 30 April 2015

Author's response to reviews: see over
Dear Dr. van Ballegooijen

We would like to thank for the opportunity to re-submit a revised version of our paper “The situation of former adolescent self-injurers as young adults: a follow-up study”. The reviewers raised some concerns on which we would like to answer point-to-point (see below). We added information about Axis II disorders based on the reviewers’ comments, which additionally strengthened our paper. We feel that we were able to further improve our manuscript by working on the reviewers’ comments and adding new data and analyses. To our knowledge, this is the first study which ever followed up adolescent psychiatric patients who injured themselves to young adulthood. Although our sample size is small, we still feel that our findings present a unique view on adolescent NSSI that will be of interest to both a clinical and research-oriented readership.

We hope that you find our study suitable for publication in BMC Psychiatry and look forward to your answer.

Yours, sincerely

PD Dr. Paul L. Plener, MD

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EDITOR'S COMMENTS:

Dear authors,

The reviewers have returned their reports. They are raise a few concerns, but I think you are able to address their points. I have a few comments myself as well.

The rationale for conducting this study can be more pronounced. Your research group has a lot of experience on the subject, so I’d expect a rather critical review of the literature and a clear case why the current study is important. The sample is small and likely to have specific characteristics. For example, for a sample drawn from 18 to 30-year-olds, the average age is lower than expected. It would add to the robustness of this study to add characteristics of the 545 individuals who were eligible.

We added some thoughts about the rationale for this study at the end of the introduction section. To date – to the best of our knowledge – no study has ever followed up on adolescent psychiatric inpatients with NSSI, looking for their outcome in young adulthood. Furthermore, our study does not rely on self-report for assessment of NSSI in adolescence but is based on a thorough medical assessment. Therefore we think that although our sample size is rather small, this study will be of
value to researchers with an interest in NSSI as well as to clinicians treating adolescents with NSSI.

Among a sample with an average age of 21 years of which half still engages in NSSI, it is a rather obvious finding that age of onset of NSSI is correlated with the duration of NSSI. Equally obvious is the finding that participants who still engaged in NSSI reported a longer duration. It is like correlating age and number of publications among scholars. Such analyses and conclusions would make more sense among a sample with an average age of 30 or above and when most of the participants have ceased NSSI.

Although we agree with the editor that an even older sample would be attractive, we would like to point out, that our findings on duration and age of onset have been adjusted for age, meaning that we controlled for the effect the editor mentioned. Therefore we still believe that these findings are important as they allow the conclusion that “early starters” have a higher risk for maintaining the behavior.

Line 344-345: However, all results presented in this study survived Bonferroni-correction and showed high effect-sizes.
- When a sample size is small, significant effect sizes are bound to be large, otherwise they wouldn’t reach statistical significance.

This is true, however Bonferroni-correction should be applied nevertheless and we feel that this makes the results even stronger.

In general the Discussion section compares the findings of the present study with those of other studies. I’d like to see an emphasis on what this study adds to the literature and what the implications could be for mental health care.

We added further points to the discussion section.

Kind regards,

Wouter van Ballegooijen, PhD
Associate editor

Reviewer's report
Title:
The situation of former adolescent self-injurers as young adults: A follow-up study
Version:
2
Date:
23 March 2015
Reviewer:
Naomi Sadeh  
Reviewer's report: 
**Major Compulsory Revisions**

1. My biggest concern is the very low participation rate in this study (only 10% of invited individuals participated). A greater discussion of the limitations of the findings due to this methodological design flaw needs to be included to help readers understand how the findings may be biased by self-selection into the study.

   *We agree with the reviewer that the sample size presents a problem and added further comments on it in the limitation section. As the change from adolescence into adulthood often goes along with leaving the parent’s house, we were not able to reach all of the possible participants although we used the official state registry to search for participants who did not reply to our study.*

2. On a similar note, it is important for the authors to describe how individuals who participated in the study differed from those who chose not to participate. At a minimum, differences between the study group and those that declined to participate should be described in terms of the information available in the chart reviews and/or demographic information. This would help contextualize the study findings and would help bring to light differences between these groups that may be biasing the findings.

   *Although we fully agree with the reviewer, we have problems in addressing this point as the IRB did only allow for the inclusion of data from participants who gave their written consent.*

**Minor Essential Revisions**

1. Line 52, “has investigated” should be changed to “have investigated”

   *We changed the wording accordingly*

2. Line 65, “trough-out” should be changed to “throughout”

   *We changed the wording accordingly*

3. The authors should note that the association between age of NSSI onset and duration of NSSI in years is potentially an artifact of the fact that participants who started at a younger age have had more of an opportunity to engage in NSSI. This makes the finding less interesting than the authors claim (unless they somehow equate participants on the number of years they have had to engage in NSSI).

   *The findings on early age and duration already were adjusted for age for this particular reason. We still think that the finding of “early starters” as a particular vulnerable group is of clinical interest.*

**Discretionary Revisions**

1. It would be helpful for the authors to include more specific information when reviewing the literature in the introduction about the quality of the research they are reviewing. In particular, how big were the longitudinal studies they reviewed
We added the numbers of participants.

2. It is not clear why the authors did not measure Axis II Disorders, like Borderline Personality Disorder, given the well-established connection they have to NSSI. Please provide a rationale for not measuring Axis II Disorders.

We did assess Axis II Disorders using the SCID-II interview, but thought that including them would create a problem with the focus of this paper. However, based on the reviewer’s comment, we have now included these findings and added according comments in introduction, results and discussion.

Level of interest:
An article whose findings are important to those with closely related research interests

Quality of written English:
Needs some language corrections before being Published

The paper was checked by a native speaker with a clinical background before re-submission.

Statistical review:
No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests

Reviewer's report
Title:
The situation of former adolescent self-injurers as young adults: A follow-up study
Version:
2
Date:
20 March 2015
Reviewer:
Giles Newton-howes
Reviewer's report:
The situation of former adolescent self-injurers as young adults: A follow-up study
Review
Thank you for the opportunity to review this paper. It looks at the outcome at ~21 years of a small sample of patients with a history of NSSI.
Generally
1. Although I cannot identify any specific problem with the introduction I find the English a little difficult to read and somewhat ponderous. It’s off putting the way you have structured your argument (for example in para 2 you justify the known steep decline in NSSI throughout adolescence by mentioning Paul’s excellent population based study and follow this with your systematic review and then a
somewhat throw away comment about two studies for rates of NSSI comparing adults and adolescents. I get the notion of what you are trying to say but it could be done significantly more neatly.

We thank the reviewer for this comment. We re-structured the introduction and added some thoughts about BPD and NSSI (as both reviewers remarked that they wanted to see results of Axis II disorders). Furthermore the manuscript was checked by an native-speaker with a clinical background.

2. The sample is small and opportunistic. You present data on ~10% of respondents (although Im not sure how you chose your patients to ask, see below point 3) and this is so small I am led to significantly distrust your findings and struggle to see to whom they are generalizable. This is clearly not a proof of hypothesis paper. IF you cannot make the applicability of the paper very clear at best is suggests a line of further investigation only, at worst it adds little to the literature.

We agree that our sample size is small; however, adolescent psychiatric inpatients are a hard to follow-up population especially in the age in which most young adults leave their parents’ home. We tried to track their addresses using the official state registry; however we were not able to secure more than around 10%. We agree however that our paper is not suitable for a proof of hypothesis, therefore we did not present one. However, we feel that our exploratory data are of value both from a clinical as well as from a research point of view as to the best of our knowledge so far no study ever followed up adolescent psychiatric patients with NSSI in their young adulthood.

Specifically
Intro

1. It’s a small point but NSSI isn’t a symptom of BPD, deliberate self-harm is.

We thank the reviewer for this remark sparking further discussion, and we would like to comment on this issue to substantiate the rationale, why we chose to keep NSSI as a symptom of BPD in our manuscript: Using the current DSM-5 BPD criteria, the wording of criterion 5 is: “recurrent suicidal behavior, gestures, or threats, or self-mutilating behavior” (APA 2013, p. 663.). This does neither include NSSI nor DSH. Furthermore, the DSM-5 states: “Completed suicide occurs in 8%-10% of such individuals, and self-mutilative acts (e.g., cutting or burning) and suicide threats and attempts are very common” (APA 2013, p. 664). Again, neither NSSI nor DSH is mentioned, but self-mutilation is specified (e.g., cutting or burning), both methods that are commonly used for NSSI. In addition, the APA writes about the co-occurrence of NSSI and BPD: “Historically, nonsuicidal self-injury was regarded as pathognomonic of borderline personality disorder. Both conditions are associated with several other diagnoses. Although frequently associated, borderline personality disorder is not invariably found in individuals with nonsuicidal self-injury” (APA 2013, p. 805). In summary, the DSM-5 does in fact comment on NSSI and BPD and there is a differentiated view used in criterion 5, delineating suicidal acts and self-mutilative acts (using “or”). In addition, NSSI may be considered a subtype of DSH. We therefore think it is fair to state, that indeed NSSI is a symptom of BPD, why we decided to keep it in the manuscript.
2. I’m not sure reference 3 supports your statement (although again it’s a minor point and its interesting to see how these terms are used. For example in the most recent draft guidelines for management of DSH published by the RANZCP they state its not easy to clearly define this and don’t!)

*We wrote that: “…especially in adolescence, it most often exists comorbid to further psychopathology like depressive or anxiety symptoms”. Reference 3 (Jacobson & Gould (2007) say: “Correlations of NSSI include a history of sexual abuse, depression, anxiety,…” (p. 129). Furthermore the authors quote: “Several studies […] have reported on the diagnostic profile of those who have engaged in NSSI […] In each of these studies, the most common diagnosis among the adolescents who have engaged in NSSI was Major Depressive Disorder […] in each study a substantial percentage had an anxiety disorder and/or PTSD.” (p.139). The reviewer also might want to check table 1 (p. 134). We think it is therefore fair to say, that reference 3 supports our statement.*

Methods

3. I have no idea how you recruited patients. As I can’t tell who you recruited I can’t work out what your ‘denominator’ (or population of relevance) is. Whose archived notes did you scan (every single child and adolescent through these two services ever?)? Over what time period? What are these areas like (urban, rural, social deprived, ethnicity, etc.)? Why did you choose these patients and not others? Did they have to now have no psychiatric contact (you state they are former patients)?

*This is a major concern and we agree that we supplied too little detail about our study procedure. We therefore added more information about the process.*

4. How did you identify NSSI from the notes? Did you also review ED notes? Did you consider self poisoning? Did you consider spontaneous drug intoxication? I’m unclear and this seems important if you are going to narrow your description to the DSM outline.

*We added further details on how the files were scanned electronically. We used files from two departments of child and adolescent psychiatry as we were focusing an the population of adolescent psychiatric patients. We did not include ED files. Furthermore we did not include self poisoning, but stuck to the APA DSM-5 NSSI definition that includes direct damage of one’s body surface only.*

5. Recruitment results are results and should be described in the results section.

*We changed the order and present these in the result section.*

6. I’m worried that you misquote yourself when discussing the utility of the SITBI. You looked at its utility in 12-19 year old inpatients, a group that seems quite different from your current group. You identify construct validity (and should state it is this) that is modest at best. Your excellent test retest reliability data from ref 21 is of little relevance here as you use this as a one off. This seems a little
sloppy to me.

We tried to describe the SITBI-G as instrument. Test retest reliability is a standard measure of an instrument’s quality, which we provided. However, as the reviewer does not feel comfortable with it, we omitted details about test retest reliability. The reviewer is right that the SITBI-G has only been used in adolescent populations worldwide so far. However, given that it is the only structured assessment interview tool for NSSI in Germany and our population is still in its young adulthood we decided that the SITBI-G is the best available option for in-depth research.

7. When discussing % in the results section please add absolute numbers also. 41% isn’t so helpful without us seeing you are referring to 21 patients by this.

Results

We added absolute numbers accordingly.

7. Am I right in thinking ~16% of your sample is 19 or younger?? (ave age 21 w a SD of 2). Not sure they would count as ‘former adolescents’ and your numbers are very small.

The age range was (as stated) between 18 and 28. Given that legal age is 18 in Germany, we think that calling 18 year olds “young adults” is suitable. To be precise, we never used the term “former adolescents” throughout the manuscript; we used the term “former adolescent patients”, which describes exactly what they were with regards to our study.

8. I’m confused by the ‘psychological impairment section’. I am struggling to see how this relates to your discussion. It would be useful to know this information for the two groups you plan on analyzing to help us see what covariates and confounders there might be.

We are sorry as somehow our wording seems to have confused the reviewer. Nevertheless after giving details for the whole sample, we presented our data in exactly the way as it was suggested: a two group comparison (e.g. see table 2)

9. Its interesting to note anxiety disorder are most prevalent but you specifically measure for depression and not this. This seems to be a limiting factor to discuss.

Using the quite elaborate Mini-DIPS interview, it is fair to say that we assessed for BOTH depression and anxiety as the interview measures a wide range of psychopathology. We assessed for depression additionally using the BDI-II since MDD was the most prevalent disorder in the archival files (we added this information to clarify the rational for using an additional instrument). Given the results of the follow-up, the reviewer is right that an additional measure on anxiety would have been valuable as well. However, we did not know about the results when deciding on the follow-up assessment.

10. I can’t see any mention of personality disorder or measurement of personality traits. This seems to be a major problem. As you note in your intro there is a strong link here. I don’t understand why you don’t present personality data.
We assessed for Axis II disorder using the SCID-II and added further discussion on the relationship between BPD and NSSI.

11. Am I right that each participant on average reported 334 episodes of NSSI, with a median of 50. How is this data collected? If its self report by recollection I would be very concerns about its accuracy. Do you look at the geometric means of the self report that may be more accurate? This needs to be clarified.

The data is collected in the SITBI interview. The geometric mean is 334.46 which was stated in the original version (“On average, participants stated a lifetime number of NSSI of M=334.46 episodes (SD=576.80; min=1, max=2000).”). We also have addressed the reviewer’s concerns about recollection in the limitation section of the original paper (“In addition, data on lifetime NSSI and lifetime suicidal behavior was assessed retrospectively which may have distorted the results.”). Given that the reviewer asks information that was already available in the original version, we are unsure how to best meet the reviewer’s suggestions. We therefore decided to keep this information.

12. Section 6 of your results needs to be tampered by the lack of personality diagnosis.

We now have additional information about BPD diagnosis.

I’m not comfortable commenting on the discussion in light of the above considerations.

Level of interest:
An article of insufficient interest to warrant publication in a scientific/medical journal
Quality of written English:
Needs some language corrections before being Published

The paper has now been revised by an native speaker with a clinical background.

Statistical review:
No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I declare I have no competing interests