Reviewer's report

Title: Combining the Suicide Intent Scale and the Karolinska Interpersonal Violence Scale in Suicide Risk Assessment

Version: 2
Date: 24 March 2015
Reviewer: Martino Belvederi Murri

Reviewer's report:

This is a longitudinal study evaluating the diagnostic accuracy of a combined assessment for suicide (suicide intent and interpersonal violence) as a reanalysis of a previous study from 2012 [Stefansson et al., Journal of Affective Disorders 136 (2012) 167–171].

The research question is relevant to the clinical field and well defined. Indeed, several studies attempted at identifying predictive factors for suicide, with relatively low success. The analyses are appropriately conducted and the writing style is concise and clear.

Despite this, there are few issues that need to be clarified before considering the manuscript for publication.

Major Compulsory Revisions

- The author mention that the SIS alone reached a PPV of 16.7% and adding the KIVS it raises to 18.8%. I wonder whether a 2.1% raise in the positive predictive value really corresponds to a meaningful improvement in clinical practice. Also in table 3, we see that only 77 patients had both SIS and KIVS ratings. With only 7 suicides in the whole sample, I fear that the change of predictivity might also be influenced by the exclusion of some subjects. Second, in the previous study the authors found that “Four items were used to test a shorter version of the SIS in the suicide prediction. The positive predictive value was 19% and the AUC was 0.82” then the authors should give better argumentations of how this is an improvement. By clinical experience, one can deem that exposure to violence increases the likelihood of self-harm, but, put this way, these data seem to show overall little change compared to the use of only the SIS. Third: instead, the use of SIS+KIVS “adult subscale” seemed to reach higher values (26.3%), while maintaining acceptable levels of specificity and sensitivity. Possibly, this should be evident in the abstract and in the discussion, rather than the other finding? However I would consider that, As in every study attempting at predicting suicide over the long-term, prediction is made difficult by “clinical interference” (i.e. the case management after the assessment). One cannot ignore that patients assessed as displaying more severe suicidal intent, will likely to be treated more intensively. Possibly (and luckily) this limits the maximum PPV that any study can reach.

- I think the authors should clarify the relationship of this study with the other recent study on 181 suicide attempters recruited between 1993 and 2005,
seemingly from the same setting [M. Rajalin et al./Journal of Affective Disorders 148 (2013) 92–97]. Thus, a hundred patients (cohort 2) are excluded from the present study, possibly because “In the first part of the inclusion period the information was collected from patient records”, instead of the KIVS. Or maybe because of a longer follow up available for cohort 1. In any case, it would have been interesting to compare if the same information, drawn through another instrument has similar predictive value. I think these explanations should be given and the study should be cited in the paper.

- the sample size is relatively small: only 7 completed suicides. The authors point this out, but without sufficient emphasis in my opinion. Conclusions should be taken with caution before replication in larger samples.

Minor Essential Revisions

- How many patients received a borderline PD diagnosis? Were patients displaying Non-Suicidal Self-Injury included? NSSI might represent an important factor to take in account (as it does in clinical practice)

- Were all eligible patients approached to take part in the study? Is data available on how many patients were not included in the study because of refusal? Representativity of the sample is an important issue that might be further highlighted.

- What were the causes of death for the seven non-suicides? Was there any margin for ambiguity in the classification?

- there might be some misuse of English terms (e.g. line 101 “evinced”)

- please report AUC p values and confidence intervals from ROC curve analyses

- the authors might want to consider reporting a very brief description of the types of suicide attempts made in the cohort at the time of the evaluation

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I don’t have competing interest in relation to this paper.