Reviewer's report

Title: The Adolescent Depression Rating Scale (ADRS): A validation study

Version: Date: 29 September 2006

Reviewer: Glenn Alexander Melvin

Reviewer's report:

General
Substantial revision was made to the manuscript. While these changes constitute hard work on the author’s behalf and should be acknowledged, serious problems remain with the manuscript, which cast doubt over the findings of the paper. Some of these problems the authors were not able to address.

Further, the authors indicate that the manuscript was not proofread by anyone who has English as their first language. This was obvious in places. Hard work aside, it raises the question of how serious the authors were about trying to improve the manuscript for an English journal.

-------------------------------------------------------------------------------

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

The key difficulties in the manuscript are listed below:
1. The sample is made up of psychiatric and medical outpatient or inpatients at multiple sites. It seems too ambitious to use participants from two different settings, particularly when we have such limited information each of the sub-samples, e.g., why were the youth attending the adolescent medical centre? Typically when a new measure is being developed a very controlled, narrow and well described sample is used, so as to convincingly demonstrate that the measure can measure what it is said to measure. This said, I would recommend validating the measures on a sample of adolescents for whom the strongest evidence exists regarding their depression, then a normal population and finally broadening the type of sample in which the instrument is validated in.
2. No mention is made of the young people who were found to meet DSM-IV criteria for depression but were not considered to be depressed by the clinician. Was this possibility not measured, if not, it casts further doubt onto the integrity of the sample or was it not observed?
3. The use of the intermediate group remains quite unconvincing. Also I wonder whether it is indeed necessary to even group the individuals in the analysis, as a range would be evident without this division. Given the limitations of the diagnostic process, I would recommend that in further revision of this manuscript that the widely described and utilized HDRS or even the BDI-13 be used to assess depression in the sample.
4. The manuscript has been prepared in English, however not all measures are presented in English and the authors acknowledge that there are problems with the English translation of the measure. This makes it extremely difficult for the English reader to make sense of the measure, as is acknowledged by the author when discussing the ‘ideas of death’ item. Therefore, would it not make more sense to publish the study findings in a French journal? Then when the problems with the English version have been corrected, publish a validation paper on the use of the instrument in an English sample in an English journal.
5. Only five adolescents were used in the qualitative phase. While this was one component of the development of items, is it really possible to gain a comprehensive understanding of such a diagnosis that has a variety of presentations from five adolescents?
6. A table of factor loadings per item for the entire sample for the exploratory factor analysis is required. The justification for conducting 2,3 and 4 –factor solutions is missing. The proportion of variance explained by the two factor model is not presented. How then is the reader to have confidence in how much variance is being accounted for by the measure?
7. Completion of exploratory and confirmatory factor analyses on the same data-set are ill advised as the outcomes become circular.
8. No detail is provided on how many of each of the sub-samples (medical and psychiatric) were depressed or otherwise.
9. Details of why items were excluded needs to be better justified. The reader is left wondering exactly how the 32 items were dismissed.
10. Concurrent validity. It does not appear that the CGI’s correlations with ADRS were higher than the HDRS or BDI-13.
11. The first “limitation” does not read as if it is a limitation.

In sum, I would recommend that the author have somewhat more modest goals when writing up the data and to publish in a French journal given the problems with interpretation. Perhaps a cleaner, tighter sample can be extracted from the total sample and a focus be made on this group first. I agree with the author’s assertion that depression measures need to be able to measure the entire range of the depressive experience. However I believe the first step in creating such an instrument would be to see that it can convincingly measure what it purports to measure in a tightly defined population and I don’t think this has been achieved yet. Once this is done, the next step would be to validate the measure in a normal population then specialist populations e.g., minority group, hospitalized populations.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Discretionary Revisions (which the author can choose to ignore)

What next?: Reject because scientifically unsound

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

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests’