Reviewer's report

**Title:** Validation of the Turkish version of the Centre for Epidemiologic Studies Depression Scale (CES-D) in patients with Type 2 diabetes mellitus.

**Version:** 1  **Date:** 11 April 2011

**Reviewer:** Mirjana Pibernik-Okanovic

**Reviewer's report:**

This study aims to determine a factorial structure and psychometric properties of the Turkish version of the Centre for Epidemiologic Studies Depression Scale (CES-D) in patients with Type 2 diabetes mellitus. Validating psychological scales for use in different clinical settings is considered important and clinically relevant. However, there are some parts of the manuscript which require additional explanation to make the paper clearer to the readers. My remarks and suggestions will be given in the order of the points recommended to be considered in a reviewing process.

1. The question raised by the authors is well defined in relation to the psychometric properties of the Turkish version of the CES-D. However, a rationale for determining a latent structure of the questionnaire is not sufficiently clear. Why did the authors decide to perform a factor analysis in a relatively small and homogeneous sample of diabetic patients? Using an oblique rotation „based on the assumption that a test measuring depression will have correlated factors“ does not sufficiently clarify the underlying theoretical prerequisites.

My suggestion would be to focus the paper on the instrument's basic psychometric characteristics, i.e. its validity and reliability.

2. The methods are generally well described. However, I would suggest adding some details about the translation process. Were the Turkish versions of the applied instruments translated and back-translated in accordance with the WHO methodology? Did the piloting process of the translated instruments indicate any specificities in understanding the items by Turkish patients with diabetes?

The Statistical analysis section should specify which correlations were used to test convergent validity (the last two lines in this section). The correlational method used should also be mentioned in Table 3.

3. I would suggest some changes in data presentation, particularly in Table 1 which could give readers a better insight into the data trend across gender and centres.

The percentages of patients with the CES-D scores above 16, and the WHO scores below 50 are expressed as part of the entire sample, rendering the whole picture confusing. In order to enable comparison of depressive symptoms between female and male patients, and in particular centres, the percentages should be referred to the number of examinees in the subsamples.

Could the authors check the standard deviations of the PAID scores? Are they as...
high as visible from the table? How many patients were above the score of 40 which indicates serious diabetes-related problems?

The sum of percentages indicating male and female subjects across centres is not 100% but 99%. Please correct this.

Data on the instrument's reliability should, in my opinion, be presented within a table. A description given on page 8 is somewhat confusing, evaluating the item-total correlation to be high, but mentioning the range from 0.25-0.73. Could the authors give a more comprehensive information about the instrument's internal consistency?

4. There are no substantial remarks related to relevant standards for reporting data and data deposition. However, the Results section (page 7, line 5) comprises one detail which is not correct. According to the criteria, the WHO-5 scores between 50 and 28 indicate mood below average, and scores below 28 as being indicative for depression. Please check the criteria which were used in classifying data in such a way.

5. Do the authors think that the obtained 2-factor solution, and particularly the combination of depressed mood and somatic complaints, may be due to the specificities of the examined patients? Literature data indicate that somatic items inconsistently indicate depressive symptoms while hypothetically reflecting complaints related to hyperglycemia. Would you please comment a hypothetical relation between the sample’s characteristics (well regulated patients, homogenous with respect to diabetes therapy) and the obtained results?

6. The authors do not seem to be clear enough when elaborating hypothetical limitations of the study (page 11, lines 9-12. Stating that „Determining the percentage of diabetes patients with clinically significant levels of depression....“ is not quite correct and should be changed into „.....elevated level of depressive symptoms“. According to the literature, only about one third of patients with elevated symptoms is classified as clinically relevant depression by using a diagnostic clinical interview.

As mentioned before, the WHO-5 cut-point of 50 is not in accordance with the standard interpretation of the results.

7. Acknowledging previous work is considered correct.

8. I would suggest to replace the part of the abstract stating „The original five-factor structure proposed by Radloff was rejected“ into „.....was not confirmed“. Due to the limitations of the study a more moderate interpretation level is considered more appropriate. This would be applicable in case that factor analysis remains a part of the manuscript.

9. The writing style is considered acceptable.

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

**Quality of written English:** Acceptable
Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests.