Reviewer's report

Title: Social adaptability and substance abuse: Predictors of depression among hemodialysis patients?

Version: 2 Date: 26 June 2012

Reviewer: Shervin Assari

Reviewer's report:

Authors have revised their papers based on some of my comments. They have provided reliability measures for depression (not SAI), they have cited the literature on SAI, and they have revised their discussion on social support. I am satisfied with these revisions. But I still have a few concerns.

In my previous round of review, I had raised 4 major comments which required compulsory revisions (based on the journal policy). These include:

1- SAI is calculated based on five variables: 1) employment status, 2) education level, 3) marital status, 4) income, and 5) substance abuse. As the results are negative, we need to know more about the descriptive (distribution of these components). For instance, we need how SAI vary. Is it a highly skewed variable?

2- Substance abuse has been entered to the model twice. Once separately, and then as a part of ASI. This make cause statistical problem. Is there any coliniarity problem?

3- No reports on Cronbach's alpha of CESD and SAI are available in methods of the paper. Low reliability of SAI may be a cause for the negative results. Again, to realize why the association does not exist, we need more statistical information in the results of this paper.

4- What will be the results if instead of SAI, we enter its components to the model. This is very important. Thus, the associations between the five variables which make the SAI and the depressive symptoms should be included in the results.

Regarding my first comment, I still have not received descriptive statistics for SAI itself. My guess is that this is a highly skewed variable, and if so, the variable may need a transformation before regression analysis. In table 2, now we have frequency of components of SAI, but it is not enough. We need to know mean / sd / skewness indices for the SAI itself, as well.

Regarding my comment 2, I am afraid to say entering a variable twice to a model causes statistical problem. I asked for coliniarity measures, and I did not receive it. I am afraid that SAI and drug abuse may covary, and I do not encourage authors to keep them in a model.

Regarding my comment 3, we need Cronbach measure for SAI (not based on the
literature, but based on their data). They can easily get this measure in SPSS or other statistical packages.

Regarding the 3rd comment, I had requested reanalysis including individual components of SAI to the model because of the negative results that were reported. If there was a significant association between SAI and depression, I would not ask for this new analysis. This was essential because I had concern about the skewness of the SAI (as a reason for the negative results). Unfortunately, authors have not provided evidence to show their SAI is not highly skewed.

Based on these comments, I still think there is a need for another revision.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

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