Reviewer’s report

Title: Magnitude and concurrence of anxiety, depression among attendees with multiple sclerosis at a tertiary care Hospital in Oman

Version: 3 Date: 23 February 2015

Reviewer: Kathleen Bogart

Reviewer’s report:

This study reports anxiety and depression levels among people with MS in the Arab world for the first time. The authors communicate clearly the need to document this issue in the Arab culture. Strengths of this study include the use of appropriate, validated measures, and the analysis of associations between demographic variables and anxiety and depression among this little-studied culture. I do have some concerns about the samples, which I detail below.

Major compulsory revisions:

1. The authors should describe their sampling rationale and process in more detail. For example, the authors mention that they excluded patients with MS who were recently diagnosed. Why? How was “recently diagnosed” defined?

2. Additionally, the authors should indicate in the participant characteristics the average length of time participants were diagnosed and/or experienced symptoms. It would also be appropriate to examine whether anxiety and depression are related to duration.

3. Of most concern is the control sample. Much more specific information about the recruitment should be given. Where were they obtained? What was the incentive for control participants? Were they paid? Did they have illness or disability?

4. Ideally, control participants should be selected to match cases based on age and gender, at least, and marital status and education at best. Given the gender differences between the two samples, it is impossible to know if the differences between the groups on depression and anxiety are due to MS or to demographics. For example, as is typical in the MS population, there were more women than men in the sample, but in the control sample, men dominated. Women may be more likely to report psychological distress than men, regardless of their disability status. To address this issue, the authors could remove analyses comparing people with MS to controls from the study. However, making such a comparison with individuals of the same culture without MS clearly strengthens the study, thus, I recommend that the authors select a more appropriately matched control group.

Minor Essential Revisions

1. The abstract does not report any of the analyses comparing the MS and control groups. Please revise the abstract to describe all major analyses.
2. There are English language issues throughout. A thorough editing for this issue is required. Following are some examples:
3. Line 51: “hospital” should be “hospitals.”
4. Line 53: “were subjected to” should be “completed.”
5. Line 59: change “are” to “were.”
6. Sentence starting on line 93 seems to be missing some words.
7. Sentence starting on line 104 is grammatically incorrect and unclear.
8. Line 119: “a quick glimpse” is not an appropriate phrase here. The authors should indicate that they have examined the literature thoroughly.
9. Line 233: “educational” should be “education.”
10. Line 270: “somatic odium of distress, a feat” is unclear phrasing.
11. Line 338 is unclear. By “relegate,” do you mean “neglect?”

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

**Declaration of competing interests:**

I declare that I have no competing interests.