Title: Comparison of the CES-D and PHQ-9 depression scales in people with type 2 diabetes in Tehran, Iran

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Author's response to reviews: see over
Dear Editor-in-Chief

I am writing to thank you for having our manuscript peer reviewed, and to provide you with the updated manuscript.

As requested I have detailed the changes that have been made in response to the peer review comments. I have also altered these on the manuscript as track changes so they can be viewed easily. I have listed the changes next to each point the peer reviewer has made:

Reviewer number: 1
Reviewer's report:

1.1 Whether or not it is ‘recommended’ that vulnerable groups be screened for depression, this does not negate the fact that depression treatment does not seem to improve the management of diabetes. (Pouwer, 2009). I am not saying that this patient population should not be screened for depression – as depression would logically further complicate pre-existing conditions like diabetes. I recommend the authors revise their statement to say that accurate assessment of depression in diabetic populations is important to the treatment of depression in this sample and may improve diabetes management.

We added the valuable suggested sentences by respectful reviewer to the text, (first paragraph- the last line in blue ink)

1.2 I appreciate the authors response regarding their comprehensive search of Iranian publications. I think it would greatly strengthen the manuscript if the authors emphasized, in the manuscript, the fact that a very good screening measurement tool (CES-D) has only be explored once in an Iranian population. This may coincide with a slight shift in the overall tone of the paper, emphasizing accurate depression assessment in Iranian and diabetic populations as the main focus.

These points are now included in the last paragraph of introduction.

1.4 The authors make a strong case for their findings regarding the high
prevalence rate of depression in the Iranian population. It would strengthen the manuscript if the authors pointed out in the manuscript that due to the high rate of depression in this population and its relation to increased complications in diabetes, accurate screening is critical. I recommend the authors provide some of the citations in the manuscript to provide evidence for readers that these rates are in fact typical.

These points are now included in the last line of first paragraph of discussion.

1.5 Again, my concern is that the authors are refuting my argument with a statement that is based on authority (the recommendation of unnamed studies) and the suggestion that this should be part of common primary care. I would agree that this screening is important. However, I think it would greatly strengthen the manuscript to state that generally, assessment of depression in primary care is problematic (Mitchell et al., 2009). This argument has broader appeal and carries over into specialized areas such as general practitioners working with a diabetic population.

According to reviewer’s suggestion we changed our recommendation and tone of article (remove some debatable sentences from introduction and also from discussion)

2.1 There are still errors in the reference format. Introduction, paragraph 2, 2nd sentence. The period appears prior to the reference numbers.

These points are now amended and included in the text.

2.3 This sentence, Intro, 3rd paragraph is still difficult to follow. “Despite its relevance to the course of diabetes and its chronic character, depression is recognized and treated appropriately in fewer than 25% of depressed diabetic patients…” Is depression not recognized in those who are already KNOWN to have depression and who are diabetic? Or is this referring to an estimated proportion of depressed diabetics that aren’t accurately diagnosed? Or is this referring to diabetics who might have depression but it cannot be determined because of poor diagnosis?

That sentence was omitted from the text.

2.4 I was not recommending that the reviewers provide the exact reference for the SCID. I do not think this is necessary. I was suggesting that the reviewers might acknowledge in the manuscript that some may think there are other gold-standards of assessment.

We changed the verb “IS” to “COULD”, in the fourth paragraph of introduction, indicating the existing other methods.
Reviewer number: 2
Reviewer's report:

I would like that it is made explicit in the text that the expert psychiatrist based her interviews on the SCID (and not only on the DSM-IV criteria).

Amended and it could be viewed in method section.

I would like too that it is made explicit in the discussion that the prevalence of depression in Iran is higher than in most of other countries (with the references).

References 23 and 24 added and more explanation was added to the text as well.

If you have any further questions or queries please do not hesitate to contact me.

Yours Sincerely,

Dr Mohammad E Khamseh