Author's response to reviews

Title: Screening of Depression In Primary Care Setting - Prevalence and Instrument

Authors:

Waleed Al Qadhi (dwaqs@hotmail.com)
Saeed Ur Rahman (rahmans@ngha.med.sa)
Mazen Saleh Ferwana (ferwanam@ngha.med.sa)
Abdulmajeed Imad Addin (abdulmajeedim@ngha.med.sa)

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Author's response to reviews: see over
COVER LETTER

Dear Editorial Board,

Please find below point by point responses to reviewers’ concerns. Hopefully all the raised concerns have been addressed in the third revision. Some reviewer suggestions are still not feasible with our current resources in our primary care clinics. We hope that these responses would suffice. Thanking you with best regards.

Drs. Waleed Qazi, Saeed ur Rahman, Mazen Ferwana and Abdul Majeed Imad Yaseen

Responses & corrections:

Reviewer: Anna Sasdelli

1. Editorial review of the whole text was done again for brackets, full stops and commas.
2. Text content was reviewed again for better grammar and simplifying text.
3. All numbers in tables reviewed, and corrections made.

Reviewer: Klaas Huijbergts

1. The part regarding 20% differential in costs between treated and untreated was rephrased and referenced.
2. The authors appreciate the suggestion of the reviewer regarding the cost of depression related references to be more and in line with other studies, however we already have ‘review and systematic-review’ articles in our references and there is varied methodologies of cost analysis and variables within those reviews as well. There is no doubt that any cost-analysis can be revisited and improved, but our limitation are not due to lack of references but rather lack of reliable healthcare cost data available in the Kingdom of Saudi Arabia. This lack of healthcare cost data, has pressured us into making logical assumptions and limits our methodology.
3. Regarding the point prevalence comment, what was meant that we excluded those with pre-existing depression and therefore our point-prevalence is expected to be lower than the actual, so it is conservative. This shows that we proceeded with prudence.

Reviewer: Mark Haddad

The authors take exception to the unsubstantiated comments of the reviewer regarding weaknesses in study design, and insufficient highlighting of study limitations. Most of the critique of the reviewer is about ‘Background’ (Introduction) section with concerns about the strength of references used. The replies are below:
1.1 The correction made regarding use of term ‘incidence’ instead of prevalence, as the reference of the reference used the term prevalence.

1.2 Opinion of reviewer regarding strength of references, that authors disagree with.

1.3 We appreciate the reviewers concern but this has already been addressed in previous critique: This study is about screening for depression in primary care settings in Saudi Arabia, not in general population. Screening visitors for depression has pragmatic applicability in day to day care rather than conducting population-based periodic surveys or surveillance for depression. Selection bias is expected as prevalence of depression in primary health care, sick-visitors might have come out higher than general population. Limitations have already been given in the discussion, and expanded. Exclusion of those with pre-existing diagnosed depression has made the point-prevalence estimates in our study, conservative rather than exaggerative.

2 We appreciate the concern of the reviewer regarding the lifetime risk of suicide of depressed of 15% and this has been addressed answered, however we would like to mention again that this number was taken from a WHO review article. Regardless of strength of the reference in the background, we used a suicide risk estimate of two per thousand (0.2%) per year in our cost-estimates among those diagnosed with depression.

3 We appreciate the concern of the reviewer regarding limitation of screening for depression in the absence of available follow-up but this has been answered before:

3.1 Changes have been made in the background text regarding the reference number 41.

3.2 This has been answered before: USPSTF recommendation that screening is not beneficial in the absence of follow-up services, is the main idea behind carrying out this study, to highlight the burden of disease, value of screening, and cost-benefit of providing treatment facilities locally, which are lacking locally. How can one make a case of more follow-up services unless a presentation is made about the burden of disease highlighted by screening which comes before requesting for manpower and other resources needed down the care-path?

4 The authors believe that the usefulness of PHQ 9 and /or PHQ 2 has been validated in the region already, and it beyond the scope of this study. Please note this has been addressed before as well: It can be argued that using the PHQ9 cutoff of ≥ 10 score for classifying those with ‘depression’ in the study instead of a gold standard such as DSM IV classification for MDD, would overestimate point-prevalence of depression and costs, but we have used in our analysis, 45% of the 20% ‘depressed’, as receiving treatment which in literature is rated as depression being under diagnosed and undertreated. In addition, this percentage of 9% primary care patients is quite close to MDD point-prevalence based on DSM IV diagnostic criteria, in general population. Using this percentage is conservative and reasonable, knowing that a lot of depression is missed. In addition, diagnostic validity of PHQ9 with clinical judgment based on DSM IV criteria, was beyond the scope of this study, since it is about usefulness of screening instrument (which is more sensitive) in primary care settings, and not diagnostic confirmation by psychiatrists (which is more specific).

Regarding the change in writing the cut-off as 3 & above rather than >2 for PHQ2 scores have been made, as suggested by the reviewer and is appreciated.