

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Diagnostic test study: Combination Use of Beck Depression Inventory and Two-Question Case-Finding Instrument as a Screening Tool for Depression in the Workplace
<b>AUTHORS</b>	Branko Aleksic, Ryoko Nobata, Tatsuyo Suzuki, Keizo Yoshida, Yuichiro Ono and Norio Ozaki

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Dr Anne Kouvonen, Lecturer, Warsaw School of Social Sciences and Humanities, Wroclaw Faculty, Poland  Competing interests: None declared.
<b>REVIEW RETURNED</b>	01/12/2011

<b>THE STUDY</b>	<p>More information about the study samples is needed to assess the validity of the results. I would like to see gender, age and SES distributions. Only after that it will be possible to assess representativeness issues.</p> <p>The statistics in this paper need to be reviewed by a specialist. I am not familiar with the analysis methods used and could thus not review the statistical analysis.</p>
<b>GENERAL COMMENTS</b>	<p>1 General comments</p> <p>The present study aimed to validate economic screening tools to identify depression in employee populations. The study relates to an important public and occupational health concern and has clear practical implications. The tool combining the 2 existing scales seems useful and valid. The study design fits the objective well and the analysis is carefully conducted. There are a few things, however, that might be done to improve the present ms. Please accept my comments/suggestions below:</p> <ol style="list-style-type: none"><li>1. More information about study samples is needed to assess the validity of the results. I would like to see gender, age and SES distributions. These need to be presented both in the Abstract and (particularly) in the Methods section.</li><li>2. Were all the employees in the 2 companies invited to participate in the study?</li><li>3. Would it be possible to include socio-demographics in the analyses as covariates?</li><li>4. The reference is needed for the M.I.N.I. tool on page 8.</li><li>5. Table 1 is rather lengthy and could be presented in a more condensed format. Perhaps scores 19+ could be combined into one category.</li></ol>

<b>REVIEWER</b>	<p>Anastasios Konstantinidis, M.D.; Psychiatrist Department of Psychiatry and Psychotherapy Medical University Vienna; Austria</p> <p>Anastasios Konstantinidis has received honoraria from Affiris AG, AstraZeneca, Pfizer and Servier, served as consultant for AstraZeneca, and as a speaker for AstraZeneca and Bristol Myers Squib.</p>
<b>REVIEW RETURNED</b>	12/02/2012

<b>GENERAL COMMENTS</b>	<p>The paper by Adachi and colleagues addresses the relevant question if with using two self-reporting questionnaires you could sufficiently detect depression in workplace. The colleagues used Beck Depression Inventory combined with a Two-Question Instrument.</p> <p>This topic has not been addressed in this manner in other papers. The findings although limited provide clinical advise in diagnosing depression in workplace.</p> <p>The paper is good written and the results very interesting. Still the paper could be strengthened through a minor revision.</p> <p><b>Major Problems:</b> The main problem of the study is the fact that the authors simply translated (and re-translated) the two-question depression-screening tool by Whooley et al. Although also to my knowledge there is no validated Japanese version, still a validation of the screening-tool in Japanese should have been considered as a first step in their research work.</p> <p>Furthermore the authors declare that they did not perform M.I.N.I. diagnostic interviews in the larger group of the study. This fact makes it difficult to answer the question set through their research paper. Authors declare that this was not feasible through the large number of group 2; but still they should have considered using shorter versions of M.I.N.I., which especially screen for depression.</p> <p><b>Minor Problems:</b> 1. Introduction: The authors concentrate too much on the economic burden of depression, although they did not address an economic question in their paper. Furthermore they acknowledge as a limitation of their study that they did not address the combination of socio-economical and clinical factors. Therefore I would advise them to shorten the introduction section. They could discuss possible socio-economical factors in the discussion part. 2. The authors should consider to present some tables as a graphic (e.g. Table 1). Furthermore the omission of table 3 should be considered. 3. Discussion: The authors should state in the discussion section all limitations of their study including the above mentioned major problems.</p>
-------------------------	--

### VERSION 1 – AUTHOR RESPONSE

Reviewer: Dr Anne Kouvonen, Lecturer, Warsaw School of Social Sciences and Humanities, Wroclaw Faculty, Poland

1. More information about study samples is needed to assess the validity of the results. I would like to

see gender, age and SES distributions. These need to be presented both in the Abstract and (particularly) in the Methods section.

(Reply): Although we have already presented the information of age and sex in supplementary table 1, we added this information into the Abstract and Methods sections. Unfortunately, no data of the SES distribution was collected in this study; therefore, it could not be presented in the manuscript.

(Pg3;Ln11-12) "(81 males and 8 females with a mean age of  $38.4 \pm 6.6$  yrs)"

(Pg3;Ln13-14) "(1408 males and 92 females with a mean age of  $40.9 \pm 7.2$  yrs)"

(Pg7;Ln11-12) "The mean age of them was 38.4 (SD, 6.6) and 81 (91.0%) were male."

(Pg7;Ln19-20) "The mean age of them was 40.9 (SD, 7.2) and 1408 (93.9%) were male."

2. Were all the employees in the 2 companies invited to participate in the study?

(Reply): We added a sentence as follow.

(Pg7;Ln17-18) "All the employees in the companies were invited to participate in the study."

3. Would it be possible to include socio-demographics in the analyses as covariates?

(Reply): To the best of our knowledge, it is impossible to include other factors as covariates in the analytic method which we employed.

4. The reference is needed for the M.I.N.I. tool on page 8.

(Reply): We added the reference for the M.I.N.I. (Pg7;Ln7)

5. Table 1 is rather lengthy and could be presented in a more condensed format. Perhaps scores 19+ could be combined into one category.

(Reply): The data included in Table 1 is now displayed graphically (Figure 1).

Reviewer: Anastasios Konstantinidis, M.D.; Psychiatrist  
Department of Psychiatry and Psychotherapy  
Medical University Vienna; Austria

Major Problems:

The main problem of the study is the fact that the authors simply translated (and re-translated) the two-question depression-screening tool by Whooley et al. Although also to my knowledge there is no validated Japanese version, still a validation of the screening-tool in Japanese should have been considered as a first step in their research work.

(Reply): Thank you for your thoughtful review of our manuscript. We take your concerns seriously and have addressed them to the best of our abilities. We address your comments point by point in italic font as follow:

As you indicated, the Japanese version of TQI used in this study was not validated. This was added as a limitation into the Discussion section (Pg18;Ln5-8).

Furthermore the authors declare that they did not perform M.I.N.I. diagnostic interviews in the larger group of the study. This fact makes it difficult to answer the question set through their research paper. Authors declare that this was not feasible through the large number of group 2; but still they should have considered using shorter versions of M.I.N.I., which especially screen for depression.

(Reply): As you indicated, we should have considered using shorter versions of M.I.N.I. in group 2 in order to increase the number of subjects for analysis. This was added as a limitation into the Discussion section (Pg18;Ln5-10).

Minor Problems:

1. Introduction: The authors concentrate too much on the economic burden of depression, although they did not address an economic question in their paper. Furthermore they acknowledge as a limitation of their study that they did not address the combination of socio-economical and clinical factors. Therefore I would advise them to shorten the introduction section. They could discuss possible socio-economical factors in the discussion part.

(Reply): Following the advice, the too long description of the economic burden of depression was deleted from the Introduction section (Pg5). Some references were deleted accordingly.

2. The authors should consider to present some tables as a graphic (e.g. Table 1). Furthermore the omission of table 3 should be considered.

(Reply): The data included in Table 1 is now displayed graphically (Figure 1). We contemplated whether to leave or remove Table 3, and arrived at the conclusion that it would be meaningful to leave it.

3. Discussion: The authors should state in the discussion section all limitations of their study including the above mentioned major problems.

(Reply): We stated all the above mentioned problems as limitations in the Discussion section (Pg18;Ln5-10).

### VERSION 2 – REVIEW

<b>REVIEWER</b>	Dr Anne Kouvonen Lecturer School of Sociology, Social Policy & Social Work Queen's University Belfast, UK  Competing interests: None declared.
<b>REVIEW RETURNED</b>	02/03/2012

<b>THE STUDY</b>	The Authors have carefully addressed a number of concerns I raised. However, I would like to see some further clarification in terms of the main point (point 1) of my original review:  Since data on SES was not available, the Authors should discuss this as an additional limitation of the study.  The vast majority of the participants were men. I'm concerned that this might have biased the tests. The Authors need to discuss this issue in the paper and make it clearer that they used a male-
------------------	--

	dominated sample. Moreover, they might want to re-run all their analyses in the male only sample, excluding female participants.
<b>RESULTS &amp; CONCLUSIONS</b>	The vast majority of the participants were men. I'm concerned that this might have biased the tests. The Authors need to discuss this issue in the paper and make it clearer that they used a male-dominated sample.

<b>REVIEWER</b>	Anastasios Konstantinidis has received honoraria from Affiris AG, AstraZeneca, Pfizer and Servier, served as consultant for AstraZeneca, and as a speaker for AstraZeneca and Bristol Myers Squibb
<b>REVIEW RETURNED</b>	05/03/2012

<b>GENERAL COMMENTS</b>	The authors revised the paper and the comments of the reviewers have been taken into consideration. Still the paper shows some methodological problems, but the authors undertook all efforts to strengthen their paper through a critical review of their data. Congratulations to the authors on their work.
-------------------------	--

## VERSION 2 – AUTHOR RESPONSE

Reviewers Reports:

Reviewer: Dr Anne Kouvonen, Lecturer, Warsaw School of Social Sciences and Humanities, Wroclaw Faculty, Poland

1. Since data on SES was not available, the authors should discuss this as an additional limitation of the study.

(Reply): As you indicated, no data of the SES distribution was collected in this study. This was added as a limitation into the Discussion section (Pg18;Ln20-21).

2. The vast majority of the participants were men. I'm concerned that this might have biased the tests. The Authors need to discuss this issue in the paper and make it clearer that they used a male-dominated sample. Moreover, they might want to re-run all their analyses in the male only sample, excluding female participants.

(Reply): As you mentioned, the possibility of the bias due to the male-dominant sample should be considered. This was added as a limitation into the Discussion section (Pg18;Ln4-8). Moreover we conducted additional analyses in the male only sample and presented the results in the supplementary materials.

It is our hope that our manuscript will now be found to be acceptable for publication in the BMJ Open.