



PEER-REVIEW REPORT

Name of journal: World Journal of Psychiatry

Manuscript NO: 52649

Title: Visual impairment and depression: Age-specific prevalence, associations with vision loss, and relation to life satisfaction

Reviewer's code: 00123524

Position: Editorial Board

Academic degree: MD, PhD

Professional title: Associate Professor

Reviewer's Country/Territory: Brazil

Author's Country/Territory: Norway

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Reviewer chosen by: AI Technique

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Scientific quality	<input type="checkbox"/> Grade A: Excellent <input type="checkbox"/> Grade B: Very good <input checked="" type="checkbox"/> Grade C: Good <input type="checkbox"/> Grade D: Fair <input type="checkbox"/> Grade E: Do not publish
Language quality	<input type="checkbox"/> Grade A: Priority publishing <input checked="" type="checkbox"/> Grade B: Minor language polishing <input type="checkbox"/> Grade C: A great deal of language polishing <input type="checkbox"/> Grade D: Rejection
Conclusion	<input type="checkbox"/> Accept (High priority) <input type="checkbox"/> Accept (General priority) <input type="checkbox"/> Minor revision <input checked="" type="checkbox"/> Major revision <input type="checkbox"/> Rejection
Re-review	<input type="checkbox"/> Yes <input type="checkbox"/> No
Peer-reviewer statements	Peer-Review: <input checked="" type="checkbox"/> Anonymous <input type="checkbox"/> Onymous Conflicts-of-Interest: <input type="checkbox"/> Yes <input checked="" type="checkbox"/> No



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SPECIFIC COMMENTS TO AUTHORS

This is an interesting and well-written manuscript about an important and often overlooked public health problem. I am grateful for the opportunity to review this work and I hope that my humble comments below may help the authors to strengthen it further.

1. My most important concern involves the fact that the analyses that were reported do not seem to have taken into account the stratified nature of the sample, with oversampling of young and middle-aged adults. Stratified samples differ from simple random samples and demand methods that take the sampling strategy into account. 2.

I miss an overall prevalence estimate of depression and major depression across all age groups. 3. Page 9, first paragraph. Visual impairment “acquired < 20 years” means VI acquired before one reached 20 years of age or less than 20 years ago at the time of the interview? That variable is sometimes described as “nature of VI” and sometimes it is described as “losing vision late in life”. I think that in this case the inconsistent nomenclature generated some confusion. Moreover, I would like to ask why the 20-years cutoff was used and if those data were collected as a continuous time variable that was later dichotomised. 4. I understand that the regression analyses that were presented concern all depressive disorders, however I would like to ask the authors to repeat those analyses using major depression as the outcome. Isn't major depression more important clinically than “any depression”? At a minimum, I think the authors should present the results for both major depression and overall depression separately. 5. Results that were presented in the abstract and in the main text describe young age as a risk factor for depression using the following prevalence ratio 0.85, 95% CI: 0.76, 0.94. I think it would be more accurate to state that older age was protective, because the interpretation of those results is that each year of life decreases by 15% the prevalence of depression when keeping the remaining variables constant. Moreover, that estimate is really



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remarkable and probably too good to be true because it would imply that people with VI aged 10 years above the mean age of the study population (if that variable was centred) would have 150% decreased risk of depression. I believe that once the sampling method is taken into account, that estimate will change. 6. What criteria were adopted to decide which patients to refer to psychological counseling? 7. I think it would be important to present the data on suicidal ideation among people with visual impairment. I would recommend excluding the analyses involving life satisfaction and depression because that comparison sounds really obvious, and I would recommend performing analyses using suicidal ideation as the outcome variable. 8. The conclusion of the abstract is not completely aligned with the conclusion of the main text. In the abstract the authors “suggest a need for mental health professionals with specific expertise in the challenges faced by those with VI”, which is somewhat outside of the scope of the results that were presented because the study did not address the appropriateness of the level of expertise of mental health professionals treating people with VI and depression. On the other hand, the statement available in the text that depression in people with VI should be addressed by healthcare authorities and user organisations seem more reasonable. 9. Page 15, last paragraph. It would be great if you could provide a reference supporting the claim that there is a lack of knowledge among health personal about the mental health adversities associated with VI. 10. Table 2. I would like to suggest using just one p-value for the overall comparisons across age groups and men and women as was done in table 1. 11. I was not able to understand the content of table S1. Do the numbers represent the overall PHQ-9 score? If they do, please, remove the % symbols and the # and modify the title of the table to make it clearer. Please, add a footnote describing the maximum range possible for that scale. 12. Please provide a copy of the document whereby the Regional Committee for Medical and Health Research Ethics confirmed that the study required no formal ethical approval as it was carried out in accordance with



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principles of anonymized data (Reference number: 2016/1615A). Minor Issues Page 12, second paragraph, please substitute “having additional impairments” for “having addition impairments”.