Review of: "Prevalence of Common Mental Illness and Its Associated Factors among Hawassa City High School Students in Hawassa, Sidama Region, Ethiopia"

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Potential competing interests: No potential competing interests to declare.

The article proposes to study the prevalence and risks associated with CMD in the population of young Ethiopian students. This study is well carried out, the methodology used is relevant and makes it possible to draw up a useful portrait for public health in this country. I am in favor of publishing this article but I nevertheless suggest a number of modifications in order to make certain aspects less ambiguous for readers.

My main recommendation would be that authors can better highlight the interest of their work. To the extent that similar studies have been carried out in other countries, it would be interesting to highlight the specificities of the country and the population concerned. First, the authors indicate in the abstract, but not in the main text, that such a CMD prevalence study has never been conducted on this specific population (Ethiopian students). This must be emphasized. Secondly, it would be even more interesting if the authors developed the interest of focusing on this specific population in relation to the existing studies in the international literature concerning the CMD of students from comparable countries: how can the specifities of these students in this country improve our understanding of CMD in other contexts? Third, it seems to me that the authors could also further present the interest of their study for public health policy strategies. This point should be developed at the end of the introduction and also in the discussion.

My second main remark concerns the way in which the authors present and interpret their results. From my point of view, they should be more nuanced regarding CMD. The CMD does indeed cover some of the psychological disorders (anxiety, depression, etc.), but it is not a diagnosis of clinical pathology either. On the one hand, international psychometric studies show, to my knowledge, that the tool they used here (SRQ-20) measure symptoms and problems that are only potentially associated with CMD. On the other hand, the tool cannot replace a psychopathology diagnosis. I therefore suggest that the authors modify the beginning of their introduction in this sense and above all that thye add these limits in the discussion of the study.

My last main remark concerns the interpretation of some results. The authors thus cite the adaptation of the SRQ-20 by Netsereab et al. (2018) but do not follow their recommendation to apply different thresholds for women and men: why?

Finally, it seems to me that the confusion between CMD and severe CMD really needs to be clearer in the introduction because the results still show two results that are not really discussed: 1/a low prevalence of severe CMD; 2/a large part of the population which is below the threshold and whose protective factors are not mentioned. This reading of the results could be useful for thinking about public health strategies.

Minor notes:

A legend is required for Table 5 (AOR/COR).

I suggest presenting the results in Figure 1 based on the threshold applied (less than 7 symptoms; 7 or more symptoms; 15 or more symptoms for severe CMD)