Author’s response to reviews

Title: A school-based epidemiological field survey: difficulties in collecting psychiatric outcome data in a middle-income country

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Author’s response to reviews:

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Dear Editor, Anna Clark,

BMC Psychiatry,

Thank you for the detailed and careful corrections of the manuscript BPSY-D-17-00272 entitled "A school-based epidemiological field survey: difficulties in collecting psychiatric outcome data in a middle-income country". Reviewers’ suggestions were extremely useful and thoughtful, and they have helped us to further refine our manuscript.

We are sending you the new version of the manuscript, which has been revised according to the reviewers’ suggestions.
Our position and thoughts regarding the comments of the two reviewers are described below.

Sincerely Yours,

Thiago M Fidalgo, MD, PhD

Matty A.S. de Wit, PhD (Reviewer 1)

We thank the reviewer for the time dedicated to analyzing our work and for the valuable contributions presented.

An additional analyses about loss-to-follow-up would improve the article. Was follow-up selective (based on scores on the K-SADS in the first wave)?

We appreciate this comment. The loss-to-follow-up did not appear to be selective, based on K-SADS scores; the table below shows that there were no differences on the K-SADS diagnostic scores between the adolescents who completed the follow-up and those who did not.

(table as a supplementary file)

We clarified this issue with the sentence added to the manuscript: “Loss-to-follow-up did not appear to be influenced by diagnostic status, as there were no differences on the K-SADS diagnostic score between the adolescents who completed the follow-up and those who did not.”, which is on page 6, line 116.

It is a bit unclear how the fact that counseling was offered at the schools, but no to those in the study, has affected the response rate. The authors describe it as an effort to engage the community, but on an individual level it seems to be an incentive not to participate.
We thank the reviewer for this comment. In order to clarify this issue, the following text has been added on page 12, line 262: “It is important to state that this reward was offered only after data collection was completed and that only school staff was aware of this reward. This was important to prevent selection bias, as on an individual level it could be an incentive not to participate (people with active mental disorders or drug and alcohol problems might be less interested in participating, because of a desire to avoid diagnosis or referral to appointments).”

The authors state that an outdated list of enrolled students is a bigger problem in small schools than in large schools. However, is it not more likely that the number of errors on the list is a percentage of the list and not absolute? A bit more explanation would be helpful.

We thank the reviewer for this comment. The main problem here was not due to the quality of the information provided on the lists, but due to the difficulty of finding replacements, as small schools had fewer spare students with which to replace unreachable students. We tried to make this clearer, as follows: “However, in small schools (three out of the nine included), which had less than 100 students, these discrepancies were difficult to solve and sampling probabilities could have been affected, as there were fewer possibilities for replacement due to the small number of students.” This is on page 9, line 192.

There is no mention of why the choice was made to contact respondents by phone. The lack of accurate phone numbers seems to suggest other ways might be more appropriate or could be used in addition. Are letters given to the students for their care-givers or school-meetings for care-givers not an option? A description of why other options were not feasible might be useful, since these are probably also specific to LMIC.

We appreciate this comment. In order to further explain this issue, we have added the following, on page 4, line 85: “All students born in 2002 and enrolled in the 7th grade during 2014 were eligible for recruitment and received a letter, explaining the study’s research goals and procedures and providing the research team’s contact information, to bring home to their caregiver. In addition, school-meetings for caregivers were scheduled in four schools. Attendance at these school meetings by parents/caregivers was quite low, however. In the other five schools, the principals did not agree to schedule such meetings between the research team and parents.”
In addition to that, we have added the following, on page 5, line 100: “Although the lack of accurate phone numbers seems to suggest that alternative forms of contact would be more appropriate in this study, other modes of contact were even less feasible. Electronic communication (i.e. email) did not seem to be prudent as only 50% of Brazilian households have a computer or an equivalent device (notebook or tablet) and only 50% have access to an internet connection (TIC Domicílios, [23]). In addition, only 60% of Brazilian people have ever used a computer or have ever used an internet connection. These rates are lower for those from low SES (only 28% of Brazilians from low SES have ever used an internet connection (TIC Domicílios, [23]). Finally, only 64% of Brazilians use the internet to access email accounts (38% of those from low SES - TIC Domicílios, [23]).”

Michael Dunne (Reviewer 2)

We thank the reviewer for the time dedicated to analyzing our work and for the valuable contributions presented.

First, the authors focused mostly on attrition between T1 and T2. However, more should be said about the low initial participation rate. Only half of the target sample could be reached. The main reason discussed was that phone numbers were not available or invalid. While that may explain many or most cases, they should also discuss the extent to which their failure to get accurate phone numbers from children was due to passive refusal. The amount of time the children were asked to volunteer for the interviews, and the expectation that children and parents should come to the school on the weekend to be interviewed, may have made participation unattractive. Rather than refuse outright, the children may not have responded positively to requests from the researchers or their teachers to access their caregiver's current phone numbers. The authors could expand the discussion on page 8 regarding other reasons for refusal at time 1.

We thank the reviewer for this comment. We have improved the text on the presented issues, as can be seen on page 10, line 202: “In thinking about our initial low participation in the recruitment phase, it is important to consider that at least part of our failure in getting accurate phone numbers might be due to passive refusal. This might have happened due to the need of children and parents to come to the school on the weekend to be interviewed, which may have made participation unattractive. This would be difficult to remedy, however, because during weekdays parents would largely be unable to attend appointments due to their jobs. Rather than
refuse outright, the children may not have responded positively to requests from the researchers or their teachers to access their caregiver's current phone numbers.”

Page 5, line 91: There is inappropriate use of the term "several numbers" as there appear to have been many phone numbers that were out of date.

We appreciate this comment. We have replaced the word several for the word “many”, in order to be more accurate. That is on page 5, line 94.

Page 8, line 177: how many schools actually have fewer than 100 students enrolled?

We thank the reviewer for this comment. This information was added on page 9, line 192, as follows: “However, in small schools (three out of the nine included), with less than 100 students…”

Page 11, lines 232: When discussing the ethics of using incentives, the authors pointed out the national regulation in Brazil that precludes cash or kind inducements or compensation for participation in survey research. This obviously raises significant barriers. The authors argued that an acceptable approach may be to offer free psychological assessment and counseling (after the study) for participants and carers/parents who agree to participate. However, they should consider that this offer may result in participation bias (on the dependent variable), where people with active mental disorders, drug and alcohol problems and so on may be more interested in taking part in an epidemiological study of related health problems.

We thank the reviewer for this comment. In order to clarify this issue, the following text has been added on page 12, line 262: “It is important to state that this reward was offered only after data collection was completed and that only school staff was aware of this reward. This was important to prevent selection bias, as on an individual level it could be an incentive not to participate (people with active mental disorders or drug and alcohol problems might be less interested in participating, because of a desire to avoid diagnosis or referral to appointments).”
Here is the crux of the matter: The authors refer to the advantages of using structured psychiatric diagnostic interviews in preference to more superficial scales of mental distress. The benefit should be better internal validity. That's true, but given the considerable problems involved in doing comprehensive psychiatric assessment in these social contexts, the participation is low and attrition high, thus damaging external validity. The authors appear in this case study to be verifying why many researchers in youth and family mental health do not use the more comprehensive approach in psychiatric assessment. It may be preferable to do smaller, more controlled studies that validate brief screening tools against psychiatric interviews, and then proceed with the more efficient screening tools in larger school-based surveys. The data are less nuanced and clinically precise, but at least the findings from surveys with high participation are indicative of mental distress and more generalizable.

We really appreciate this comment. We have addressed the issues raised in the discussion of the paper. This can be seen on page 19, line 393, as follows: “Many researchers in youth and family mental health do not use the more comprehensive approach in psychiatric assessment. It may be preferable to do smaller, more controlled studies that validate brief screening tools against psychiatric interviews, and then proceed with the more efficient screening tools in larger school-based surveys. The data are less nuanced and clinically precise, but at least the findings from surveys with high participation are indicative of mental distress and more generalizable.”