Reviewer's report

Title: Student-teacher relationship trajectories and mental health problems in young children.

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Reviewer: Gabriela Roman

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General comments:

This manuscript represents contributions to the field by focusing on a non-US sample, a slightly younger age span (yet one largely overlapping with a previous study) and including more 'control' variables than previous studies. The study is noteworthy in the choice and execution of analyses (especially the Latent Class Growth Analysis) and is a great example of rigorous developmental research. Overall, the manuscript was well written and clearly presented. I particularly appreciated the full and thorough acknowledgement of limitations, opening a clear path to how future research can take this topic forward.

• Discretionary Revisions

1. One of the main contributions of this study is that it involves the transition from preschool to formal schooling. A major concern however is that children’s scores on teacher-child relationship quality may be mostly a function of teacher perceptions, especially given that 5 weeks were considered sufficient time for the new teachers to know the children. It is great that the authors acknowledge this in the discussion (page 20), but it does mean that results need to be interpreted and used with caution. Relatedly, the manuscript gave a feeling that teacher-child relationship quality reflects mainly characteristics of the child, who should be the target of future interventions, with sentences such as “[…] we hypothesised that there would be distinct subgroups of children who would follow different trajectories of student-teacher relationship quality […]” (page 7) or “The results of the present study suggest that helping children develop stable high quality relationships with their teachers may improve their mental health, particularly in school” (page 21). I would welcome the introduction of a few more sentences on the fact children might also be helped through interventions targeted at teachers.

• Minor Essential Revisions

2. On page 10, paragraph 1, line 4: “teacher rated” should read “teacher-rated”.

• Major Compulsory Revisions

3. In my understanding, the final analyses were conducted on a sub-sample of participants will data at all time-points, however, listwise deletion is known to introduce bias. Why not use the full sample and a full information maximum
likelihood function to estimate trajectories of teacher-child relationship quality in the presence of missing data? Would doing that lead to different findings?

4. How was the issue of data skeweness dealt with in the latent class growth model?

5. The authors tackle the issue of multi-level modelling at the preschool level. However, I would say that the problem is not only that each teacher provided information about multiple children, but that the way in which children were clustered changed over time. It is known that teachers’ reports will include biases related to anchoring (if a pupil is in a class with low-ranked peers, this pupil might get a high ranking due to the comparative assessment of the teacher). Perhaps a quick analysis on whether the children’s membership to one trajectory or the other was conditional upon them moving into a class with children who had had a high or low teacher-child relationship quality in preschool (i.e. whether the clustering with children of different initial levels mattered)? If such an analysis is not possible this general drawback should be noted in the manuscript.

6. In general, in order to conduct trajectory modelling, one needs to first ensure that the measure functioned equivalently at the different time-points. This is particularly important here because the teacher-child relationship quality is reported by different teachers at different time-points. If teachers had a slightly different understanding of the measure, then any trajectory modelling (based on the comparison of means/scores across time) is meaningless. It would greatly help if the authors conducted measurement invariance tests on the measure of the teacher-child relationship quality to show that this measure functioned similarly at child ages 4, 5 and 6.

7. It would be very helpful if, in Table 4, model 3, the authors included a few rows on the relationship between children’s mental health and the various variables included as ‘confounds’: child age at preschool, child gender, parent distress, parental warmth, maternal and paternal education and employment, receiving welfare benefit, and single parent family status, previous SDQ scores. Without this information, the battery of additional variables are of limited use, merely ruling out confounding effects (as the authors intended). However, the inclusion of this information gives additional insights into how family and background variables might interact with teacher-child relationship quality to predict child mental health, which could lay the grounds for future studies.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.