Author’s response to reviews

Title: Change in physical activity is not associated with change in mental distress among adolescents: The Tromsø Study: Fit Futures

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Author’s response to reviews:

Dear Editor in Chief

May 16th we received an invitation to re-submit our manuscript to your journal after addressing the concerns your reviewers have raised. We have finished revising the manuscript, and were able to address all the concerns the reviewers had, and where we did not accommodate the requests, we have justified our choices. Enclosed is the revised manuscript “Change in physical activity is not associated with change in mental distress among adolescents: The Tromsø Study: Fit Futures”, in addition to the point-per-point response letter.

Reviewer reports:

Serge Brand (Reviewer 1): In their longitudinal study the authors investigated the associations between changes of physical activity levels and changes in psychological functioning. Against
expectations, increased levels of physical activity were not associated with favorable changes in psychological functioning.

The sample is large, the Introduction provides an excellent overview of both physiological and psychological explanations as to why (regular) physical activity might favorably impact on body and mind. References are timely; I suggest to introducing the following publications: (Brand et al., 2014; Brand et al., 2017; Gerber et al., 2015); from these studies it turned out that a minimum of physical activity intensity (moderate to high intensity) appeared to be necessary to be substantively related to favorably improved psychological functioning and mental well-being.

• Page 5, lines 8-14: We have added several of the suggested references.

Further, the authors might also turn their results the other way round: perhaps the physical activity intensity was such to counter-balance an unfavorable change in psychological functioning and mental well-being; in this view, as the authors correctly stated, for instance (Bor et al., 2014) showed that (at least among female adolescents) internalizing problems increased over the last three decades and that accordingly, in the present study, physical activity intensity might have been sufficient to counteract such kind of unfavorable development.

• Page 5, lines 5-9: We have now addressed the possibility of reversed causality.

While I do agree with the authors' statement that compared to subjective ratings objective physical activity measurements should be more reliable, also 'objective' measurements might be biased, as often missing data must be adjusted post-hoc, and as conventional devices such as Actigraph®, Fitbit® and similar do not fully explain the underlying algorithm to assess the 'true' level of physical activity.

• Page 21, lines 3-5: One additional sentence regarding the validity of accelerometers is added to the methods section.

Next, the authors should consider that 'more' is not always 'better', or simply put: there is mounting evidence that adolescents highly involved in (competitive) strenuous physical activity are at increased risk to suffer from symptoms of burnout and depression (Gerber et al., 2018a; Gerber et al., 2018b; Gerber et al., 2019).

• Page 18, lines 4-9: This excellent point has been added in the discussion.

Abstract: Generally well written. Conclusions: "Conclusion: The results of our study indicate that for adolescents,..."; I suggest not to overstate their results from their study sample to 'all adolescents'.

• Page 3, line 22: We agree, and have changed this in line with the remark.

Methods: well-performed.
Results: well reported.

Discussion: well performed. The authors might take in consideration the findings mentioned in the papers above and below.

- We have taken some considerations from above and below.

References


Peter Kremer (Reviewer 2): This study examines the longitudinal relationship between PA and symptoms of psychological distress among adolescents. Objectively measured PA and anxiety/depression symptom data was captured from a sample of secondary school students at baseline and follow-up. Data for a range of covariates was also captured at one or both time-points. Change score variables were constructed for two activity measures (steps, MVPA) and psychological distress symptoms and associations modelled using separate linear regression analyses. Results indicated that changes in steps/day and MVPA were both not associated with changes in psychological distress symptoms.

I have two principal concerns with the manuscript. First, a considerable proportion of the PA and parental education data was missing and hence values imputed for analysis - for some variables almost half of the scores were imputed.

• Regardless if one imputes missing values or applies methods that use all available data, the uncertainties regarding missing values will still exist. The crucial question regarding the precision of imputed values pertains to the degree to which the missing values are missing at random or not. The imputation becomes imprecise if missingness depends upon the missing values. The problem is that most often one does not have information as to why the participants have missing data. Comparing the respondents with missing data to the respondents with valid data may shed some light upon the randomness of missing data. In our data, the respondents with missing data at T2 were compared to the respondents with valid data on T1 physical activity and mental distress, and no systematic differences were found. However, this does not guarantee that the non-response pattern is missing at random. We have discussed this under «methodological considerations» in the discussion. Provided that the data is missing at random, multiple imputation provides unbiased estimates, even at high amounts of missing data (Jin Hyuk Lee & John Huber Jr., 2011. "Multiple imputation with large proportions of missing data: How much is too much?," United Kingdom Stata Users' Group Meetings 2011 23, Stata Users Group.).

Second, given the purpose of the study, I am not convinced about the use of change scores and the analytic approach - other longitudinal analytic methods could be (better) utilised to test for associations between the study variables. In my view, further attention to each of these issues (eg. also analyse on available data; use of linear mixed modelling) would strengthen / verify reported conclusions.

• The choice to use change scores in the analyses was based upon the research question we intend to investigate. In case of no missing data, analyzing change scores in a wide format data file would give about the same results as using a mixed models approach in a long format data file in which a time variable was included. Had we decided to not impute data, using a mixed models approach would have been better than using change scores analyses, but given the robustness of multiple imputation methods in estimating missing values, we believe that the chosen method of analysis is appropriate. Using a linear mixed model with data from two time points would also in effect answer a different research question, that is, is the relationship between physical activity and mental distress the same at T1 as at T2, and we believe this research question to be of less interest than analyzing how change in physical activity relates to change in mental
distress. We did consider using a linear mixed model, but decided against it due to the considerations mentioned here. We hope that the reviewer finds this reasonable.

- Page 15, lines 3-4, show that we have conducted the analysis on only complete data, and due to this resulting in the same results, we reported the data from the imputed dataset.

Other matters include:

- Page 6, Line 6: Spelling ‘resent’
- Page 6, line 8: “Recent” is now correctly spelled.

- Page 7, Line 25: Is it possible to specify the nature of the hypothesised relationship (eg. positive/negative)?
- Page 8, line 3: The nature of the hypothesized relationship is now specified in the revised manuscript.

- Page 8, Par 1-2: Clarify why change scores are important for understanding the relationship between PA and psychological symptoms - ie. what does this approach add when compared with other longitudinal approaches? What unique information does it provide?
- Page 14, lines 4-8: More information on the use of change score is added. We assessed that it is more appropriate to clarify this in the section where the statistical analyses are described.

- Page 8, Para 2: Why was a new cohort of students who had not participated at T1 invited into the study? How many of these participated as T2? Non-participation at T1 inevitably means data is missing on key variables at T1 (see issue of imputation indicated previously).
- Page 8, lines 18-20: Thank you for pointing out that this was confusing. We have added some necessary details.
- Please also see page 13, lines 11–14.

- Page 9, Line 19: Spelling ‘Analyiss’
- Page 9, lines 18: “Analysis” is now correctly spelled.
Why are average scores (rather than change scores) computed and modelled for several covariates? It seem more 'elegant' to use same variable construction approach within the same analysis.

- We assumed that, in general, it was more important to adjust for levels of T1 variables, under the assumption that, for instance, having a poor social network at T1 may to a larger degree impact mental distress at T1, rather than if there has been a change in social network. However, we do agree that change in possibly influential factors may also be of importance, and consequently, we have rerun the analyses with both T1 levels of variables and change score variables in cases where we have information from both time points. The mean T1 and T2 variables have been removed from the analyses.

- Page 11, lines 21-22
- Page 12, lines 5-9
- Page 13, lines 5-7
- Table 3 and table 4

Given the age range of participants and the fact that they are still growing, the BMI measure should be standardised (i.e. BMI-z).

- Page 12, lines 4-5: We have categorized the BMI variable based on cut off values identified by Cole et al. (2000) to take age and gender into consideration.

As previously noted, I think other longitudinal modelling approaches should be utilised to test for relationships between the variables of interest. Alternatively, the rationale and justification for using a change score approach needs to be made clear early in the manuscript.

- Page 14, lines 4-8: We understand your concern, and we hope we have clarified in previous comments the reason why we chose to use change score variables as predictor and outcome variables. We have also added a justification in the manuscript regarding this.