Author's response to reviews

Title: Delayed Sleep Phase Syndrome in Adolescents. Prevalence and Correlates in a Large Population Based study.

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Author's response to reviews: see over
Thank you for the positive comments and invitation to revise and resubmit our paper. We have done our best to follow all suggestions, and believe the manuscript has improved significantly. Below you will find our response to each of your comments:

**Reviewer 1**

This is a well-written paper that defines the research question clearly. The design and data analytic techniques are appropriate as are the measures of DSPS and insomnia. The authors mostly stay within the limits of their design when discussing their results.

Overall, a scholarly paper.

There are several minor issues. The authors had only a 53% response rate. This is quite low, and raises the issue of potential bias. Do the authors have any data demonstrating that there were no significant differences between participants and non-participants on DSPS, insomnia or depression? If not, there is a potential fatal flaw.

- **Response:** We agree that the potential problem with a low response rate should be mentioned as study limitation. Although we had no opportunity of examining non-responders in the current study, we do have data on this from a previous wave of the Bergen Child Study. We have now added the following paragraph on the Discussion addressing this:
  
  “Finally, the attrition from the study could affect generalizability, with a response rate of about 53% and with adolescents in schools overrepresented. Based on previous research from the former waves of the Bergen Child Study, non-participants often have more psychological problems than participants [32], and it is therefore likely that prevalences of both DSPS, insomnia and depression may be underestimated in the current study.

The sample also is from one area in Norway. Is this representative of Norway as a whole? One assumes so, but can this be verified by authors?

- **Response:** Although we have no data available to verify the representativeness of the ung@hordaland study in terms of the Norwegian population as a whole, a previous large population based study from the same county, but in an older cohort (the Hordaland Health Study), demonstrated that the county is indeed representative of the Norwegian population. The following sentence was added to the Methods:
  
  “A previous population-based study from the Hordaland county, the Hordaland Health Study (HUSK), found the county to be representative for the Norwegian population [27].”

As I understand the MFQ, it does not measure just depression. The short form also does not focus just on depression. What the authors have is a measure of psychological distress or demoralization. They should treat it accordingly in their discussions. It taps depression but also other symptoms.

- **Response:** We agree that SMFQ does not provide a definite proxy of clinical depression; rather it taps a broader spectrum of depressive symptomatology. Therefore, the following sentence was added to the Discussion:
  
  “Third, depression was assessed by a self-report instrument, the SMFQ. As no validated cut-off exists for Norwegian adolescents, the 90th percentile on the total SFMQ score was chosen as an operationalization of depression. Clearly, this does not imply existence of a clinical diagnosis, such as MDD, and the lack of clinical interview in confirming a clinical diagnosis of depression is a limitation of the present study. In relation to this it should be noted that the SMFQ neither contains any sleep items nor items that assess any other vegetative symptoms. In contrast to conventional depression rating scales which normally contain such items, this prevents circularity...
and make the interpretation of associations between symptoms of sleep and affective problems unambiguous in the present study. Tiredness was included in the SMFQ, but the association to several sleep parameters was not higher for this item than for other depressive symptoms.”

The authors also play loose with their descriptions of associations. This is a cross sectional study and no measure increases or decreases the risk of any other. The have odds ratios and the most they can say is a higher or lower odds is found between two factors. They should so phrase their results and their discussion.

- **Response:** We fully agree with this comment, and we have replaced the word “risk” with “odds”, at all relevant places in the manuscript, thus the wording is now more appropriate given the cross-sectional design. We have also rephrased other occurrences where direction of causality was inferred in the results and discussion.

**Reviewer 2:**
This study by Sivertsen et al. aimed to investigate the prevalence of Delayed Sleep Phase Syndrome in adolescents and to examine its association with insomnia and school non-attendance. While the large sample size is a strength of the study, the cross-sectional design, as acknowledged by the authors is a limitation. The article is well-written; however, I have some comments about the manuscript.

1. **Interpretation of the study findings should be confined to “associations”. Words such as, “risk”, “impact” and “consequently” are inappropriate for these data, as they imply causality for which these data cannot provide information.**

    **Response:** This issue was also raised by Reviewer #1, and we fully agree with this comment. We have now removed/rephrase all instances where causality was implied.

2. **Abstract:** Under “Methods”, a brief mention about the sleep parameters assessed would be helpful. Under “Results”, % of overlap of DSPS with insomnia and effect estimates for association of DSPS with 1) insomnia and 2) non-attendance for boys and girls should be provided.

    **Response:** We have now added the following sentence to the abstract:
    “Other sleep parameters included time in bed, sleep duration, sleep efficiency, oversleeping, sleep onset latency, wake after sleep onset, subjective sleep need, sleep deficiency, sleepiness and tiredness. Sleep data were calculated separately for weekdays and weekends.”

    We also included more detailed information regarding the associations (% overlap and odds ratio) in the results section of the abstract.

3. **Introduction:** Page 4 “due to recent introduction of new technologies (e.g., smart phones, video games) that can .....new and updated surveys are warranted”. It appears as if the authors are going to account for the use of these technologies in the analysis, however, they didn’t.

    **Response:** We agree, and this sentence has now been removed from the text.

**Methods:**

4. **Sample:** Substantial number of students declined to be in the study, are there data on differences between them and study participants? What about students who were excluded, is there any difference between them and those included in the study?
Response: The issue regarding the response rate was also raised by reviewer #1, and we added a new paragraph concerning this issue when discussing study limitations (please see response over). There were no significant age differences between participants being excluded due to invalid/missing response on the sleep items, but there were more boys (61.8%) than girls (38.2%) among the excluded participants. This information has now been added to the Methods section.

5. Demographic information: Is the information on parental marital status, and income collected? What about information on lifestyle such as smoking, alcohol, coffee drinking, BMI etc.?

Response: We thank the reviewer for these inputs, and we have now added the suggested information to Table 1. In short we found participants with DSPS to be more likely to come from separated/divorced families, have poorer family economy, and being more likely to ever have tried alcohol. No differences were found for smoking status and Body mass index (BMI). In addition, we now also control for these variables (only the ones with significant differences between the DSPS and non-DSPS groups from Table 1) in the logistic regression analyses in Table 3. These additional adjustments do not alter the overall findings or conclusions. The methods section has also been updated accordingly with the following paragraph:

“Parental cohabitant situation was assessed using a dichotomized variable (yes/no) asking if the parents currently live together. Family economy was assessed by asking the participants how their family economy is compared to most others. Response alternatives were 1= “approx. like most others”, 2=”better economy”, and 3=“poorer economy”. Smoking was assessed by asking the participants if they smoke. Response options included 1=“yes, daily”, 2=”yes sometimes, but not daily”, 3=“no not now, but previously”, 4=“no, not anymore”, and 5=“no”. Current smoking was operationalized as endorsing the two first response alternatives, whereas choosing response alternatives 3-5 was coded as non-current smoker. Alcohol use was assessed by asking if they had ever tried alcohol (yes/no). Body mass index (BMI) was calculated from self-reported body weight (kilogram) divided by squared height (meter²).”

6. Delayed Sleep Phase Syndrome, page 6: The authors have provided the criteria for assessing DSPS and the reference. However, it would be helpful if the authors provide the actual questionnaire used (only for the non-attendance the actual questionnaire was mentioned). There is no mention about the duration of the symptoms assessed, e.g. sleep symptoms in the past 1 month, 3 months etc.

The description of how DSPS was operationalized has now been substantially expanded. It now reads:

“The following questions were used to operationalize DSPS: “At what time do you usually go to bed”, “How much time does it take before you fall asleep (hours and minutes)”, “When do you usually get out of bed in the morning”, “How many nights per week do you have difficulties falling asleep (0-7)”, “How many nights per week do you have problems with nightly awakenings (0-7)”, “How often do you oversleep (“never”, “seldom”, “sometimes”, “mostly”, “always”). The participants provided sleep data separately for weekdays and weekends. To approximate assessment of the ICSD-R criteria for DSPS, the following criteria were used (based on the aforementioned sleep items), as specified in Johnson et al. published in Pediatrics [8] 1) minimum 1-hour shift in sleep-onset AND wake times from the weekdays to the weekend, 2) complaint of frequent (≥ 3 days per week) difficulty falling asleep, 3) report of little or no (≤ 1 day per week) difficulty maintaining sleep, and 4) frequent difficulty awakening (oversleep “sometimes” or more often).”

We also assessed the duration of many of these sleep symptoms, but this information was
included in the definition of DSPS, in accordance with the Pediatrics-paper. However, as specified in the paper, the insomnia operationalization does include information on duration of the insomnia.

Statistics:

7. The second aim of the present study was to examine the overlap and association between DSPS and insomnia. The authors have provided the % of overlap, however, there were no statistics supporting the association between DSPS and insomnia.

Response: We do not entirely agree with this, as Table 1 also shows the p-level from the Pearson chi-square test comparing the prevalence of insomnia in the DSPS vs. the non-DSPS groups. Also, Figure 1 and 2 include 95% confidence intervals (error bars), enabling the reader to interpret the differences by inspecting overlapping vs. non-overlapping 95% confidence intervals.

8. The authors mentioned that all analyses were conducted separately for boys and girls. However, Table 1 and 2 were not stratified by boys and girls. It’s not clear if there is a difference in sleep characteristics between boys and girls.

Response: Thank you for pointing out this error, indeed – only the logistic regression analyses were conducted separately for boys and girls. We have also tried stratifying Table 1 and 2 by gender, but this resulted in very large tables, so we ultimately decided against this. While we agree that the gender issue is important to examine in DSPS, we were mostly interested in examining gender differences with regards to the overlap between insomnia and DSPS (detailed in the figures) and associations/ORs (detailed in Table 3). We are of course willing to reconsider this and present all results stratified by gender, but we fear this might reduce the readability of the paper.

9. Additional analyses using self-reported school non-attendance: Since this is the only additional analysis included, at the minimum the authors should provide the mean of self-reported non-attendance and the OR (95% CI) of this outcome for boys and girls.

Response: Mean number of self-reported non-attendance (days and hours) has now been added to Table 1. Also, we added the crude and adjusted ORs for the associations between DSPS and self-reported non-attendance to the text at the end of the results section, which now reads:

“Additional analyses using self-reported school non-attendance during the last month as the outcome variable were also conducted. These findings showed the same pattern of associations and with similar gender differences. The crude ORs for self-reported days of school non-attendance were 1.86 (95%CI 1.33-2.60) for girls and 4.46 (95% CI 2.97-6.71) for boys. The corresponding odds for hours of school non-attendance were 2.75 (95%CI 1.91-3.98) for girls and 3.64 (95% CI 2.28-5.81) for boys. The associations were only slightly attenuated and remained significant in the fully adjusted analyses (results not shown).”

10. Is there any difference in the prevalence and association with non-attendance by age?

Response: This is an interesting point, and we have now added the following paragraph to the Results:

“The prevalence of DSPS increased with age: whereas 2.8% of adolescents aged 17 fulfilled the criteria for DSPS, the prevalence rates among 18- and 19-year-olds were 3.1 and 4.2%, respectively ($\chi^2(2)=8.53, p=.014$).” and “No significant age differences were found for the associations between DSPS and school non-attendance.”