Author's response to reviews

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

Authors:

Børge Sivertsen (borge.sivertsen@fhi.no)
Ståle Pallesen (staale.pallesen@psysp.uib.no)
Kjell M Stormark (Kjell.Stormark@uni.no)
Tormod Bøe (Tormod.Boe@uni.no)
Astri J Lundervold (Astri.Lundervold@psybp.uib.no)
Mari Hysing (Mari.Hysing@uni.no)

Version: 4 Date: 20 November 2013

Author's response to reviews: see over
Thank you for these positive comments and constructive feedback. Here are our point-by-point responses to the concerns:

1. **Abstract, Results:** ORs for non-attendance should be provided from the multivariable fully adjusted model rather than from the crude one and also should include 95% CI.

   **Response:** As suggested, we have now replaced the crude ORs with fully the adjusted ORs, and also included their corresponding 95% CI.

2. **Background:** Aims, suggest removing the word “association” from aim 2 as prevalence is not a measure of association. Suggest revising aim 4 as “to investigate if there are gender differences in the association between DSPS and non-attendance at school”.

   **Response:** We agree with these suggestions and have changed the formulation of the aims accordingly.

3. **Methods:** “Delayed Sleep Phase Syndrome”: Regarding my query on the duration of the symptoms assessed, the authors have provided the duration of symptoms for the risk categorization, but not for the assessment duration. What I meant is if the questionnaire had a specified time frame of evaluation, for example, “past month”, “past 6 months” etc.

   **Response:** We are sorry for this misunderstanding. The questionnaire contained no time frame. This information has now been added to the Methods section.

4. **Information in all 3 figures is exactly presented in words in the text. Suggest shortening the text without providing the exact values or leave the text as is but remove the figures.**

   **Response:** As we understand it, the reviewer here refers to the section “Overlap between DSPS and insomnia.” We agree that there is some repetition of the figures in the text. However, we find it difficult to remove the figures entirely, while at the same time communicating the results with sufficient accuracy. Therefore, after carefully consideration, we have chosen to keep this section as it was.

5. **I agree with the authors’ decision of not stratifying Table 1 and 2 by gender. However, since the association is stronger in boys, it would be better if the authors test for interaction by gender statistically in the association between DSPS and non-attendance.**

   **Response:** We agree with this comment, and we have now included the results from these analyses of interaction effects in the Results section. The relevant section now reads:
“Boys had slightly higher crude OR of days of non-attendance compared to girls (OR=4.6 vs. OR=3.2), but there was no significant interaction effect between gender and DSPS on non-attendance (Wald = 1.67, df = 1, P = .196). A somewhat stronger gender difference was found for hours of non-attendance, with a significant interaction effect between gender and DSPS (Wald = 6.07, df = 1, P = .014).”

6. Discussion, paragraph 1: Suggest removing the word “rate” from the last sentence “Girls had a higher rate” and replace it with “prevalence.”

Response: Agreed, and fixed.