Reviewer’s report

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

Version: 2 Date: 30 September 2013

Reviewer: Charumathi Sabanayagam

Reviewer’s report:

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.

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.

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.

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?

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.?

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.

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.

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.

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.

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

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.