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ARTICLE DETAILS

<table>
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<th>TITLE (PROVISIONAL)</th>
<th>Association of screen time with self-perceived attention problems and hyperactivity levels in French students: cross-sectional study</th>
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<tr>
<td>AUTHORS</td>
<td>Montagni, Ilaria; Guichard, Elie; Kurth, Tobias</td>
</tr>
</tbody>
</table>

VERSIO 1 - REVIEW

| REVIEWER             | Teresa B Jensen, MD  
Assistant Professor  
Consultant, Department of Family Medicine  
Mayo Clinic  
200 1st St SW  
Rochester, Minnesota  
USA |
<table>
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<td>REVIEW RETURNED</td>
<td>03-Sep-2015</td>
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GENERAL COMMENTS

1. Page 14 of 29 states “We conceptualized that high screen time exposure leads to increased risk of ADHD…” which cannot be concluded from the study. I believe that you meant to say ‘leads to increased self reporting of inattention and hyperactivity in college students.”

2. There are several errors in English grammar which are easily addressed

3. I would recommend that you qualify the importance of the self reported data. The diagnosis of adult ADHD is based on self reporting along with correlation with clinical interviews and collateral information such as information from childhood. Keep your message clear with the self reported aspects, not the diagnosis of ADHD (Cognitive Impairment in Adult ADHD-Perspective Matters! Fuermaier, et al, Neuropsychology, Vol 29 (1) Jan 2015, 45-58.)

4. Your key message is that this study suggests a correlation between higher exposure to screen time and a higher risk of self-perceived components of ADHD. The opposite is also likely—that college students with inattention and hyperactivity spend more time using electronic devices to reduce their interactions with others, satisfy their ‘need for speed’, facilitate a sense of success due to their problems with the usual cognitive endeavors of academics and enable their use of alcohol and marijuana. Adults with ADHD have a greater likelihood of additional psychiatric diagnoses (depression, anxiety disorder, substance abuse), role impairment, vocational issues and imprisonment). These confounding conditions also lead to isolation and thus potentially more screen time. (Ginsberg et al, Underdiagnosis of attention-deficit/hyperactivity disorder in adult patients: a review of the literature, Prim Care Companion CNS Disord, 2014;16 (3)) (McCann et al, Screening and Diagnostic Utility of Self-Report Attention Deficit Hyperactivity Disorder Scales in Adults, Comprehensive Psychiatry, Vol. 45, No. 3 May/June, 2004 pp. 175-183) (Gray, et al, The Adult ADHD Serif-Report Scale
The article covers a topic of great public interest. In a study of postgraduate student 18 and older screen time use and symptoms of attention and hyperactivity/impulsivity was measured. The study is interesting, but with some methodological limitations including recruitment procedure and ADHD symptom assessment was based on a short screener.

### Introduction

The introduction is well-written and covers the topic well. Some minor issues.

The second paragraph covers association between screen time and some associated problems. I found that the low vision was a little out of place, and if included it could be specified (all the others are behavioral problems)

In general I believe that the cited studies are mainly cross-sectional (including suicide ideation anxiety and depression. If this is true the first sentence in the second paragraph (As for mental health…” may not be fitting. And you should consider rewriting the sentence. And may also lead to suicide ideation, with words that do not indicate causality.

Similarly, in the fourth paragraph you should consider if you have the necessary empirical studies to support the statement "too much television can negatively affect brain development of all ages (if so please refer to specific empirical papers and maybe be more specific in how the studies are referred to.)

In the results both confounding and intermediate factors are presented based on previous studies. A short background for the choice of the measures would help build the case for the analysis.

### Methods

The French version of the ASRS screener is used. Are there French validation studies that you could cite? If not are there other studies using the ASRS with these cut-offs? Adding some information on the psychometrics of the instrument would be helpful. E.g Cronbachs alphas (for the total and the two subscores). The dichotomous variable labeled ADHD is somewhat misleading as this is a screener. Please rephrase into screen positive or other words that indicate that they are high scorers. If this cut-off is not validated in a French sample I would suggest that you only use the dimensional scores in the analysis.
The definition of the included covariates should be included (e.g.: is depression defined as such?)

Results

The labeling of the tables could be improved to increase readability. For table 1, quintiles of global scores may be rephrased into something more meaningful. E.g. ASRS a screener quintile scores or Attentional and hyperactivity/impulsivity.

Table 1 is very long and this makes it hard to get the important numbers. It would be helpful if the percentage adds up to 100%. Further, there are multiple ways to shorten the paper (e.g. only one gender, and other dichotomous variables (not both response alternatives). It could be that sum variables could be considered. Some statistical analysis of the overall differences in the background variables would also be useful.

Discussion:

It would be interesting to include a section on the possible mechanisms leading to the observed association.

Limitations

The ASRS screener is used. However, this is constricted to a couple of items on each of the subscales. This should be mentioned in the limitations.

Generalizability:

The representativeness of the sample and the results could be discussed in the limitations. For instance girls are overrepresented. What are the reasons for the skewed gender participation?

Appendix:

The screener is attached as an appendix. Is this in line with the publishing policy? I thought that one had to refer to the homepage http://www.hcp.med.harvard.edu/ncs/asrs.php.

**VERSION 1 – AUTHOR RESPONSE**

Reviewer: 1

Please leave your comments for the authors below

1. Page 14 of 29 states “We conceptualized that high screen time exposure leads to increased risk of ADHD...” which cannot be concluded from the study. I believe that you meant to say ‘leads to increased self reporting of inattention and hyperactivity in college students.”

Response: You are right, this is an omission, since we have always referred to self-reporting of inattention and hyperactivity. Our study does not refer to the diagnosis of ADHD. We have corrected this sentence accordingly (“self-reporting of inattention and hyperactivity in college students”).
2. There are several errors in English grammar which are easily addressed

Response: We apologize for the editorial errors. We have revised the entire document with a native English speaker and hope that it reads smoothly now.

3. I recommend that you qualify the importance of the self reported data. The diagnosis of adult ADHD is based on self reporting along with correlation with clinical interviews and collateral information such as information from childhood. Keep your message clear with the self reported aspects, not the diagnosis of ADHD (Cognitive Impairment in Adult ADHD-Perspective Matters! Fuermaier, et al, Neuropsychology, Vol 29 (1) Jan 2015, 45-58.)

Response: We have qualified the importance of the self-reported data by adding this sentence to the strengths and limitations list ("However, prior studies have demonstrated good correlations between the ADHD Self-Report Scale and clinical diagnoses of ADHD") and another sentence in the strengths and limitations paragraph in the text ("As for self-reporting of inattention and hyperactivity, Kessler's [21] cutoff has excellent validity for the clinical diagnosis of ADHD"). A further sentence ("The diagnosis of adult ADHD is based on self reporting along with correlation with clinical interviews and collateral information such as information from childhood") has been added in the Introduction and corroborated by the citation of the study by Fuermaier and colleagues.

4. Your key message is that this study suggests a correlation between higher exposure to screen time and a higher risk of self-perceived components of ADHD. The opposite is also likely-that college students with inattention and hyperactivity spend more time using electronic devices to reduce their interactions with others, satisfy their 'need for speed', facilitate a sense of success due to their problems with the usual cognitive endeavors of academics and enable their use of alcohol and marijuana. Adults with ADHD have a greater likelihood of additional psychiatric diagnoses (depression, anxiety disorder, substance abuse), role impairment, vocational issues and imprisonment). These confounding conditions also lead to isolation and thus potentially more screen time. (Ginsberg et al, Underdiagnosis of attention-deficit/hyperactivity disorder in adult patients: a review of the literature, Prim Care Companion CNS Disord, 2014;16 (3)) (McCann et al, Screening and Diagnostic Utility of Self-Report Attention Deficit Hyperactivity Disorder Scales in Adults, Comprehensive Psychiatry, Vol. 45, No. 3 May/June, 2004 pp. 175-183) (Gray, et al, The Adult ADHD Serlf-Report Scale (ASRS): utility in college students with attention-deficit/hyperactivity disorder. Peer J, 2014,Mar 25:2: e324)

Response: We very much agree with you comment and we have extended our discussion. Specifically, we wrote: "We conceptualized that high screen time exposure leads to self-reporting of inattention and hyperactivity in college students, but the inverse may also be true although this seems less likely". Nevertheless, individuals with with ADHD may isolate more easily and utilize electronic devices more likely as a consequence. Thank you very much for sharing us the articles. Your point of view on the "cognitive endeavors of academics and enable use of alcohol and marijuana" looks plausible but further data are needed as we do not have enough people exposed to these drugs in our study.

Reviewer: 2

Please leave your comments for the authors below
The article covers a topic of great public interest. In a study of post graduate student 18 and older screen time use and symptoms of attention and hyperactivity/impulsivity was measured. The study is interesting, but with some methodological limitations including recruitment procedure and ADHD symptom assessment was based on a short screener.

Response: We agree that the ADHD symptom assessment was based on a short instrument, which, nevertheless has been validated for studies in population-based settings like ours (Kessler et al., 2007; Kessler et al., 2005).

We have revised the description of the study sample and recruitment process.

Introduction

The introduction is well-written and covers the topic well. Some minor issues. The second paragraph covers association between screen time and some associated problems. I found that the low vision was a little out of place, and if included it could be specified (all the others are behavioral problems).

Response: We have distinguished, as suggested, the behavioral problems from low vision which is a physical associated problem.

In general I believe that the cited studies are mainly cross-sectional (including suicide ideation anxiety and depression. If this is true the first sentence in the second paragraph (As for mental health…") may not be fitting. And you should consider rewriting the sentence. And may also lead to suicide ideation, with words that do not indicate causality.

Response: We understand the comment of the reviewer. However, it is incorrect that in general cross-sectional studies cannot prove causality. The main issue with this study design is the time sequence which may not be adequately addressed. We have revised the text of the introduction to reflect the underlying design of the studies we cite.

Similarly, in the fourth paragraph you should consider if you have the necessary empirical studies to support the statement "too much television can negatively affect brain development of all ages (if so please refer to specific empirical papers and maybe be more specific in how the studies are referred to.)

Response: The biological impact on the brain is maybe too strong. We have revised this sentence limiting ourselves to the association with attentional problems in children. A misplaced citation has been omitted.

In the results section both confounding and intermediate factors are presented based on previous studies. A short background for the choice of the measures would help build the case for the analysis.

Response: The choice of the measure is explained in the Discussion section where each factor proposed by other studies is taken into account and compared to our results. Studies on each factor are precisely cited in the method section as well (Statistical Analysis). In the section “Statistical Analysis” we wrote that our covariates were “Based on the literature on the magnitude, composition and time-distribution of screen use, as well as literature on social and environmental determinants of
ADHD*. We have inserted the citations here as well.

Methods

The French version of the ASRS screener is used. Are there French validation studies that you could cite? If not are there other studies using the ASRS with these cut-offs? Adding some information on the psychometrics of the instrument would be helpful. E.g Cronbachs alphas (for the total and the two subscores). The dichotomous variable labeled ADHD is somewhat misleading as this is a screener. Please rephrase into screen positive or other words that indicate that they are high scorers. If this cut-off is not validated in a French sample I would suggest that you only use the dimensional scores in the analysis.

Response: The ASRS instrument has been developed for use in population based setting. We have used the proposed cut-offs by Kessler and colleagues, as we now clearly cite. We believe that it is not part of our study to describe this instrument in further detail as it is fully explained in other studies.

The definition of the included covariates should be included (e.g: ow is depression defined a.s.so

Response: We have revised our method section to make clear that all our covariate information is based in self-reports.

Results

The labeling of the tables could be improved to increase readability. For table 1 Quintiles of global scores may be rephrased into something more meaningful. E.g ASRS a screener quintile scores or Attentional and hyperactivity /impulsivity.

Response: We have added in the title that we refer to ASRS quintile score.

Table 1 is very long and this makes it hard to get the important numbers. It would be helpful if the percentage adds up to 100%. Further, there are multiple ways to shorten the paper (e.g only one gender, and other dichotomous variables (not both response alternative). It could be that sum variables could be considered. Some statistical analysis of the overall differences in the background variables would also be useful.

Response: Thank you for this comment. The numbers do not round up to 100% due to rounding, which we now clearly state in the footnote. We have omitted groups that are obvious (i.e., only show one gender, etc.). The table should be more readable now.

Discussion:

It would be interesting to include a section on the possible mechanisms leading to the observed association.

Response: We understand this comment and we have briefly added a few sentences () but as our data cannot directly proof or disproof any of these hypotheses, we believe that it is not the right place for an extensive discussion about this point.
Limitations

The ASRS screener is used. However, this is constricted to a couple of items on each of the subscales. This should be mentioned in the limitations.

Response: We believe it is rather a strength that we use a validated instrument in this population-based study. The major underlying issue is potential misclassification and we have addressed this clearly.

Generalizability:

The representativeness of the sample-and the results could be discussed in the limitations. For instance girls are overrepresented. What are the reasons for the skewed gender participation?

Response: We have clearly addressed that our sample is not representative. With regard to the specific question under study, we have difficulties to find reasons why the results we find are driven by the study sample. If one argues that individuals with ADHD would participate less in studies like this, our result may even be an underestimation of effect.

Appendix:

The screener is attached as an appendix, Is this in line with the publishing policy. I thought that one had to refer to the homepage http://www.hcp.med.harvard.edu/ncs/asrs.php.

Response: We have eliminated the appendix and inserted the link as the publishing policy of the ADHD-ASRS v1.1 requires.

VERSION 2 – REVIEW

<table>
<thead>
<tr>
<th>REVIEWER</th>
<th>Mari Hysing</th>
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<tr>
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</tr>
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<td>15-Nov-2015</td>
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GENERAL COMMENTS

I have read the revised manuscript and I am sorry to say that I cannot recommend it for publication. While I did previously suggest major revision, most of my suggestions have not been sufficiently addressed and new issues are raised.

There are some improvements in the use of references, the English has improved as well as header for tables and other minor issues. The topic is also of interest and the study has many strengths as mentioned previously.

However, some new issues are raised. I think the others reviewers comments on qualifying the importance of self-report data. However, the sentence that they now have included “clinical diagnosis of ADHD”). A further sentence (“The diagnosis of adult ADHD is based on self reporting along with correlation with clinical interviews and collateral information such as information from childhood”) is not clear to me what they mean. I think this would need to be rephrased.
Some minor changes have been done, but mostly they have not done what I believe are necessary changes in the use of references, support for the confounders they have used, improvements of tables and the inclusion of mechanism that could strengthen the observed association. I will not go into details as I have already done this in my last review.

One example is methodological limitations by using a screener that has only been validated in a US sample. It is not certain that the same cut-off applies here. This would usually at least be a limitation. Similarly their reference used is on the total screener (six items) that has previously been validated against a clinical diagnosis. As far as I can see from the references they have not the same support for using this as two subscales (inattention and hyperactivity-impulsivity). I do not find their response satisfactory on important questions that are raised.

**VERSION 2 – AUTHOR RESPONSE**

Reviewer Name

Mari Hysing

Institution and Country

Uni Research Health, Bergen, Norway

Please state any competing interests or state 'None declared':

None declared

Please leave your comments for the authors below

I have read the revised manuscript and I am sorry to say that I cannot recommend it for publication. While I did previously suggest major revision, most of my suggestions have not been sufficiently addressed and new issues are raised.

There are some improvements in the use of references, the English has improved as well as header for tables and other minor issues. The topic is also of interest and the study has many strengths as mentioned previously.

However, some new issues are raised. I think the others reviewers comments on qualifying the importance of self-report data. However, the sentence that they now have included "clinical diagnosis of ADHD"). A further sentence ("The diagnosis of adult ADHD is based on self-reporting along with correlation with clinical interviews and collateral information such as information from childhood") is not clear to me what they mean. I think this would need to be rephrased.

Response: thank you for your comment and we have revised this section of the paper.

Some minor changes have been done, but mostly they have not done what I believe are necessary changes in the use of references, support for the confounders they have used, improvements of tables and the inclusion of mechanism that could strengthen the observed association. I will not go into details as I have already done this in my last review.

Response: We have added in the first revisions references how covariates have been selected.
Specifically we say: Based on the literature on the magnitude, composition, and time-distribution of screen exposure, as well as the literature on social and environmental determinants of ADHD (references 22 to 35 in the paper). We do not think that further detailed discussion on the previously suggested links are helpful for readers in this paper. Most importantly, we have selected the potential confounders and intermediates based on prior knowledge and not based on statistical association patterns in our study.

As our data do not provide direct evidence of potential mechanisms, we still believe that such speculations do not belong in our paper. Nevertheless, we have added a few more references pointing to potential biological links.

One example is methodological limitations by using a screener that has only been validated in a US sample. It is not certain that the same cut-off applies here. This would usually at least be a limitation. Similarly, their reference used is on the total screener (six items) that has previously been validated against a clinical diagnosis. As far as I can see from the references they have not the same support for using this as two subscales (inattention and hyperactivity-impulsivity). I do not find their response satisfactory on important questions that are raised.

Response: The total ADHD score of the ASRS has been found to be meaningful, reliable, and valid in French adults [21]. We have added that the subscales, however, have not been validated in France.
Association of screen time with self-perceived attention problems and hyperactivity levels in French students: a cross-sectional study
Ilaria Montagni, Elie Guichard and Tobias Kurth

BMJ Open 2016 6:
doi: 10.1136/bmjopen-2015-009089

These include:

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