Author’s response to reviews

Title: Prevalence of problematic smartphone usage and associated mental health outcomes amongst children and young people: a systematic review, meta-analysis and GRADE of the evidence

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Author’s response to reviews:

Rebuttal : BMC Psychiatry - BPSY-D-19-00274

Editor Comments:

Thank you for your submission. It has been recommended for major revision.

Please attend to the reviewers' comments below and especially make clear in the methods and analysis section the nature and details of your dual approaches: meta-analysis and narrative synthesis. Be sure to clearly justify and provide rationale for the outcomes identified.

Response to The Editorial comments:

We thank the reviewers and editors for allowing us to clarify and improve the manuscript.

We have revised the introduction and updated the methods including removing the term “narrative” from the review. We thank the editor and reviewers for highlighting the typographical error in the abstract and have clarified that the primary outcome is to estimate the prevalence, and secondary objective were to quantify the association between PSU and outcomes.

We have removed the term narrative synthesis throughout
Reviewer One: Emily Goldmann

Comment 1
Introduction:
There could be a bit more background provided. It is unclear why sleep and educational performance were included as primary outcomes. They are important, but rationale for including them as mental health outcomes is needed. For example, why not also include other outcomes such as social isolation, physical activity, activity involvement, bullying, etc? It might also be helpful to describe the burden of mental health problems in this population, why it is important to focus on this population from a mental health perspective, and perhaps the increase in these conditions noted in recent years. This information is helpfully provided in the Discussion but should be moved to the Introduction to provide rationale for why this is an important public health issue.
Why the focus on problematic smartphone use vs. smartphone use in general? Has that topic already been reviewed?

Response to comment 1:
We thank the reviewer for raising their comment, and we have corrected the typographical error in the abstract, and revised the introduction to reflect their concerns.
However, we note that sleep is an established mental health outcome, and poor education is a surrogate of poor day to day functional impairment demonstrated. We have also clarified how we measured the primary outcome, and this is now included in the Methods section.

“The primary outcome is the proportion of individuals exhibiting PSU, measured using scales such as the Smartphone Addiction Scale (SAS) or Mobile Phone Problematic Use Scale (MPPUS).”

Comment 2
How can this review fully shed light on the causal association between PSU and mental health outcomes, given that the large majority of included studies are cross-sectional and that behavioral addictions (e.g., gambling disorder) are often comorbid with other mental health conditions?
The rationale for conducting a systematic review and meta-analysis of this topic needs further development.

Response to Comment 2:
We agree that only three of the included studies were cohort studies, with the majority cross-sectional. Thus, in the title, results and conclusions we have argued that we report on associations without causality. We acknowledge the concern of the review and review the weaknesses section to highlight that we cannot exclude reverse causality and further highlight the concern of the reviewer.
Comment 3

Methods:
Did study selection eligibility also require that studies include the primary outcomes (e.g., depression, anxiety)? Or were all studies of PSU included for the first aim, and then studies that happened to also include information on the association between PSU and mental health outcomes then focused on for the second aim? This is a bit unclear. There is mention of inclusion of studies in a "qualitative synthesis" and those included in a "narrative synthesis" in Figure 1, but it unclear how the aims of these syntheses differ. It is then stated in the Results section that 20 studies looked at the relation between PSU and mental health outcomes (the narrative synthesis). This could be clarified in Figure 1 and Methods.

Response to Comment 3:
Studies were included based on the inclusion criteria and not whether they reported on the outcomes stated in the protocol. This was carried out so that if later outcomes are included at a later update, we can assume the included studies as a starting point without having to repeat the searches. This is inline with the Cochrane Handbook for Interventional studies (Higgins et al, 2012).

Comment 4
Why only include studies after 1/1/2011? What is significant about 2011 in terms of smartphone technology, particularly given the diversity of geography in the included studies?
Line 150-151: Please clarify what is meant by "A descriptive narrative of PSU as assessed…"
Stress is listed as a primary outcome, but it is unclear what is meant. Perceived stress? Experience of stressors?

Response to Comment 4:
When reporting this work, we are mindful to ensure that readers would appreciate that smartphone technology that is consistent with that of today. By including technology prior to this time would increase the heterogeneity of the forms of technology used. The included reference also used this cut off to over-come the evolving technology bias. As highlighted in the methods section:

“This time restriction was specifically chosen to capture studies of current and modern smartphone technology”

Stress was measured using self-report questionnaires, and the text is updated to perceived or self-reported stress.

Comment 5
Results:
Figure 1 describes two studies identified through other sources (apart from database searches). What were these sources? Were reference sections of selected studies also reviewed? This information could be added to the Methods section. Also, there are three abstracts excluded because they could not be found. Was the full article also unavailable? Why not hold on to these and confirm that they are not eligible during full text review?
In Figure 1, it is unclear what the exposure and outcome are… In Supplementary Table 2, the exposures are listed as phones and outcomes as PSU. I'm not sure I would call cellphones exposures. It's a bit confusing since the second aim of the study looks at the association between PSU (presumably the exposure) and mental health conditions (the outcomes).

Response to Comment 5:
We thank the reviewer and have updated our introduction and methods section to reflect their view. In response to their comment, we describe the exposure as problematic smartphone usage more clearly throughout, and present each of the measures used by the included studies in the supplementary data.

We highlight in the discussion weaknesses section that the definition of exposure has heterogeneity of individuals in each study. In Supplementary Table 1 the outcome is a direct extract of the reported outcome of the study.

Comment 6
When reporting percentages, please keep decimal places consistent.
While interesting, the section on sociodemographic characteristics associated with PSU does not quite tie into the stated aims of the study. Perhaps this can be added as a study aim. It would also helpful to summarize the study populations of the included studies in terms of age, gender distribution, etc.
The mental health outcomes can all be measured in different ways (and are often measured differently across studies). Can the authors please give the reader a sense of how these outcomes were measured (i.e., what screening or diagnostic instruments were used?) in included studies and whether it was fairly consistent across these studies?

Response to Comment 6:
We would like to thank the reviewer for their considered comments, and have amended the number of decimal places, and also included the Sociodemographic characteristics in the objectives

Comment 7
For anxiety (line 228), it is stated that seven articles assessed the relation between PSU and this outcome, but only six studies were further assessed?

Response to Comment 7
We thank the review and have updated the manuscript

Comment 8
Studies that examined suicidal ideation and other psychological factors associated with PSU are discussed, but these outcomes were never mentioned specifically as study outcomes. There is also still an indication to co-authors that a citation is needed (line 274).
Response to Comment 8:
We thank the reviewer for highlighting that suicidal ideation were never mentioned in the review objectives. This was added as an outcome after the protocol was registered. We have added this under the section

“Changes since the protocol was registered section”

Comment 9
Discussion:
Given that most studies were cross-sectional and that temporal ambiguity is a serious threat to making causal inferences from these studies, what would the authors suggest in terms of future research to help move us closer to understanding whether there is a causal relationship between PSU and poor outcomes in this population?

Response to Comment 9:
We thank the reviewer and reassure them that we do not make casual inference from this analysis. Furthermore, for allowing us to highlight the discussion and limitations section where we state:

“However, given the nature of the review question, studies were non-randomised and at a high risk of bias”

“Furthermore, reverse causality cannot be excluded as rational for the associations found”

We also thank them for highlighting the future work to public health, policy and research and have amended the Implications for Policy, practice and research section as below

Comment 10
Other:
Figures should have titles that tell the reader what is being shown, and need to be formatted a bit more. It would be helpful to include each mental health outcome in Figure 3. Why focus just on depression here? In general, there is great information in the supplementary material that perhaps could be the focus of figures included in the manuscript itself. For example, I would prefer to see a table of selected articles and their characteristics as opposed to Figure 2, since Figure 2 provides relatively little information, although this may be dependent on what is allowed by the journal.

Response to Comment 10:
We thank the reviewer for their comment, the Figure titles are presented (as per journal guidance) at the end of the manuscript. In response to the review have included all of the mental health outcomes in Figure 3

Response to Reviewer 2: James C. Anthony
Comment 1
With general faithfulness to PRISMA guidelines and the 'a priori' protocol, the systematic review and meta-analysis seem quite sound, with no more than minor issues I choose to leave to the attention of other reviewers.

My comments reflect a puzzle that involves description of methodological approach and perhaps a misunderstanding that, I believe, will face other reviewers and readers.

Response to Comment 1:
We thank the reviewer for their kind comment, and assure them that we try to exhibit science in a transparent manner.

Comment 2
Namely, I count at least three response variables with no clearly stated relationship of the three: (1) text emphasizes Problematic Smartphone Use (PSU), whereas the lead figure is a meta-analysis summary labeled with an 'Addiction' outcome, and the second figure speaks of Problematic Mobile Phone Use.

Response to Comment 2:
Thank you very much for highlighting the inconsistency in naming Problematic Smartphone Use. As a group we have discussed nomenclature – Problematic Mobile Phone Use, Problematic Smartphone Use and smartphone addiction are used interchangeably in the literature. We settled on Problematic Smartphone Use to reflect the uncertain status of this pattern of behaviour. We have updated all terms to Problematic Smartphone Usage (PSU).

Comment 3
As described in the text, the estimated associations purport to be based on what essentially are cross-classifications of PSU with mental health and behavioral outcomes most readers would characterize as unhappy experiences (anxiety, depression, sleep difficulties, and social problems). But the size of the associations are remarkably small, given an apparent reliance upon a PSU case definition that is not fully explained in the text, but that specifies the presence of anxiety, etc, as a defining feature of PSU. The result is that the y1*y2 cross-classification is of y1 (Smartphone use accompanied by anxiety, etc.) with y2 (anxiety, etc.).

An allowance can be made for the possibility that an attempt to be succinct has gotten in the way of this reader's comprehension of the research approach, but the 'control' in the research design may be inadequate. For example, think of smartphone users as a set (y0==1) and think of anxious people as a set (y2==1), with overlap as in a Venn diagram. Unless set y0 is defined without reference to components of set y2, the result has an uncontrolled upward bias. It seems that PSU is defined to require anxiety or some other manifestation of an unhappy experience (e.g., neglecting responsibilities).

Response to Comment 3:
We thank the reviewer for questioning the correlation of those that present with PSU and those that exhibit poor mental health outcomes. Firstly, there is debate in the literature of any link, and
this work has been undertaken to present the evidence in an unbiased manner, whilst stating the limitations of the included studies and potential inherent bias.

For example, we reassure the reviewer that we do not make casual inference from this analysis. Furthermore, for allowing us to highlight the discussion and limitations section where we state

We argue that individuals reported as ‘unhappy’ e.g. depression, are considered by psychiatrists to constitute ‘common mental disorders’, that is, common clinical conditions which warrant clinical attention and treatment may be associated with considerable morbidity. In response we have included an additional supplementary tables to show how each study mapped the exposure of PSU to each of the behavioural addiction criteria.

Whilst we share some of the concerns of the reviewer, we are arguing that an affective disturbance in the ‘withdrawal’ state, whether it takes the form of anxiety (as in alcohol), or dysphoria and irritability as in cocaine or pathological gambling, is a common feature of addictions. This is state-related. Depression and anxiety disorders are distinct from withdrawal-related affective disturbance and have separate features that are captured in both the diagnostic classification syndromes and self-report screening questionnaires. It is considered a legitimate field of enquiry in the addiction literature as a whole to examine psychiatric comorbidity or risks in populations with addictions: see the large body of work produced by the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC) from the United States examining the relationship between alcohol dependence and mood and anxiety disorders (Hasin and Grant: The National Epidemiological Survey on Alcohol and Related Conditions (NESARC) Waves 1 and 2: review and summary of findings.


Comment 4
What might be of greater interest and public health significance in psychiatry is a cross-classification of y0 with y2 — that is, frequency of smartphone use 'per se' as cross-classified with the y2 vector of outcomes under study here. But apparently that is not what has been accomplished.

Clarity on this matter is needed. In the meantime, the conclusions are uncertain.

Response to Comment 4:
We thank the reviewer for their comment, and confirm that the set of included studies, explicitly excludes samples or populations that were studies of specific diseases, or disorders [Line 113-116]. We agree than if the included studies were of individuals that were under investigation for a specific disorder (namely gaming), the association would be biased towards those that were gamers.
Whilst all sampling frame are subject to bias, offering limitation in the generalisation of the finding. We have taken every opportunity to exclude these studies and have drafted the discussion – limitation section to specify that bias assumed with non-randomised studies should be considered.

The magnitude of the effects reported are consistent with large effects and these concerns should be treated to raise debate of these probable genuine associations.

Comment 5
There are some typographical errors (e.g., Prevalance), which deserve attention.

The presentation and labeling of figures with estimates do not quite meet standards of other systematic reviews and meta-analyses and might deserve some attention.

Response to Comment 5:

We thank the reviewer for allowing us to improve the labelling and quality of the figures, these have been improved.