

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

This paper was submitted to the ADC but declined for publication following peer review. The authors addressed the reviewers' comments and submitted the revised paper to BMJ Open where it was re-reviewed and accepted.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Parental modelling, media equipment and screen-viewing among young children: Cross-sectional study
AUTHORS	Jago, Russ; Sebire, Simon; Lucas, Patricia; Turner, Katrina; Bentley, Georgina; Goodred, Joanna; Stewart-Brown, Sarah; Fox, Ken

VERSION 1 - REVIEW

REVIEWER	Kreuser, Friederike University Medical Center, Department of Rehabilitative and Preventive Sports Medicine
REVIEW RETURNED	29-Nov-2012

GENERAL COMMENTS	<p>Comments and questions</p> <p>Page 2: The abstract has only limited significance. The study design is simple and the methods are completely missing. The conclusions do not represent the intention of the study.</p> <p>Page 3: The background is worked out clearly but it is already mentioned that there are various of studies who investigate the problem of a high screen consumption in childhood and parental modeling.</p> <p>Page 4: The methods describe the recruitment of the study participants. Because it is an online survey only parents who use often the internet and take time to sit in front of screen-media pay attention on the online survey. That distorted the participant samples. Parents who don't use the online services are missing.</p> <p>Page 5: In the "analyses" rest a question. Which kind of instrument did the researches use to evaluate the data?</p> <p>Page 5: In the results the GCSE level is not described. What does it mean?</p> <p>Page 6: In the discussion the results are discussed but ideas to realize intervention strategies to reduce screen-time consumption in families are not mentioned.</p> <p>Page 8: The limitations given by the authors predominate and exclude a high quality of the paper.</p> <p>As above mentioned only those parents took part in the online survey that use screen-media frequently and have enough time to</p>
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	<p>complete a questionnaire. Thereby all parents who do not use or use screen-media irregularly did not take part in the study. Furthermore, the researchers did not know who filled out the online survey. Personal interviews would be meaningful to have in the end an additive quantifying factor next to the online survey.</p> <p>The researchers only requested the multi screen-viewing time during weekdays. We investigated in a previous study the differences in weekday and weekend time and found out large differences. Therefore it should have been interested to investigate as well the weekend days.</p> <p>The online survey was only targeted on parents. Parents should report the screen-viewing time of themselves and their children and this could lead to bias.</p> <p>The fact that further studies should investigate in strategies to reduce the multi screen-viewing days of children is a very important statement. But it would be interesting to have some ideas already in this paper.</p> <p>What this study adds is not new. The presences of media equipment in children´s bedroom is associated with a higher levels of screen-time is already shown in previous studies (Saelens BE, 2002, van Zutphen M, 2007).</p> <p>Tables: The tables have important elements but “SV” and “#SV” should be explained in the legend.</p>
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VERSION 1 – AUTHOR RESPONSE

Page 2: The abstract has only limited significance. The study design is simple and the methods are completely missing. The conclusions do not represent the intention of the study.

Response: The abstract has been re-written to comply with the BMJ Open guidelines.

Page 3: The background is worked out clearly but it is already mentioned that there are various of studies who investigate the problem of a high screen consumption in childhood and parental modelling.

Response: The paper stresses that the key addition to the literature is the focus on different types of screen-viewing among pre-school age children. This are has not been studied before. This is highlighted throughout the introduction on pages 4 and 5.

Page 4: The methods describe the recruitment of the study participants. Because it is an online survey only parents who use often the internet and take time to sit in front of screen-media pay attention on the online survey. That distorted the participant samples. Parents who don´t use the online services are missing.

Response: We accept this limitation which has been highlighted in the limitations section on page 9.

Page 5: In the “analyses” rest a question. Which kind of instrument did the researches use to evaluate the data?

Response: This was a self-reported survey. This is highlighted on page 5.

Page 5: In the results the GCSE level is not described. What does it mean?

Response: GCSE's are the exams that are taken at the end of secondary school at age 16. This has been clarified at the top of page 6.

Page 6: In the discussion the results are discussed but ideas to realize intervention strategies to reduce screen-time consumption in families are not mentioned.

Response: Ideas for interventions have been added and are shown on Page 8.

Page 8: The limitations given by the authors predominate and exclude a high quality of the paper.

Response: We disagree with this reviewer comment. While the paper has limitations (which are acknowledged) the paper makes a major contribution to the literature by examining different forms of screen-viewing among pre-school aged children.

Tables: The tables have important elements but "SV" and "#SV" should be explained in the legend.

Response: We have now highlighted in the tables that SV relates to screen-viewing.

As above mentioned only those parents took part in the online survey that use screen-media frequently and have enough time to complete a questionnaire. Thereby all parents who do not use or use screen-media irregularly did not take part in the study. Furthermore, the researchers did not know who filled out the online survey. Personal interviews would be meaningful to have in the end an additive quantifying factor next to the online survey.

Response: There are limitations with the survey but by conducting interviews it would not be possible to achieve such a large sample. The limitation of this approach has been highlighted on page 9.

The researchers only requested the multi screen-viewing time during weekdays. We investigated in a previous study the differences in weekday and weekend time and found out large differences. Therefore it should have been interested to investigate as well the weekend days.

Response: We recognise that it might have been interesting to collect information on different types of screen-viewing but in order to achieve a very high number of responses we had to limit the number of survey items to a number that was easy to complete. This was the compromise that we made.

The online survey was only targeted on parents. Parents should report the screen-viewing time of themselves and their children and this could lead to bias.

Response: The survey focuses on the SV patterns of pre-school aged children. Children of this age could not report their own SV and as such we are reliant on parental reports.

The fact that further studies should investigate in strategies to reduce the multi screen-viewing days of children is a very important statement. But it would be interesting to have some ideas already in this paper.

Response: Some ideas of how to limit multi-screen viewing have now been added. Please see paragraph 2 on page 8.

VERSION 2 – REVIEW

REVIEWER	Dr Andrew J Atkin Career Development Fellow UKCRC Centre for Diet and Activity Research (CEDAR) UK
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REVIEW RETURNED	29-Jan-2013
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THE STUDY	There are potential issues with the sampling method and sample characteristics. Also, the statistical analysis requires further clarification.
RESULTS & CONCLUSIONS	Issues regarding sampling and analysis (including adjustment for potential confounding variables) require clarification.
GENERAL COMMENTS	<p>Major issues</p> <p>Methods, page 5, line 14-17. The method of sampling is novel. Intuitively, however, it seems that this approach is likely to result in a highly biased sample. Is it possible to draw valid inferences regarding the broader UK population based upon this sample? Insufficient information is reported on the social / demographic characteristics of the parents participating in the study to make any reasonable judgement about representativeness.</p> <p>Methods, page 5, lines 25-45. No information is provided on the validity or reliability of the items used to assess parents or children's sedentary behaviours. Please add this information to the text and comment upon any limitations in the discussion.</p> <p>Methods, page 6, analysis. I have a number of queries regarding the analysis strategy. Firstly, the results are presented separately for boys and girls, but did the authors test formally for effect modification by sex in regression models? Looking at the results, there appear to be relatively few examples of differential associations between boys and girls; therefore why not present for the whole sample? Without formal tests for interaction it is not clear whether observed differences between boys and girls are valid or rather a function of differential sample size. Secondly, were regression models mutually adjusted for all exposure variables? If so, please clarify this in the text. If not, I suggest this should be added to the analysis. The studied exposures are likely to be correlated to a degree; in order to identify independent correlates all significant exposures should be included in a multivariate model. Lastly, a number of potentially important confounding variables are omitted from the analysis; for example, ethnicity, weight status, urban/rural location and family structure (single/dual parent household, presence of siblings). If information on these, and other, potential confounders is not available, the extent to which reported associations may be subject to residual confounding should be acknowledged in the discussion.</p> <p>Minor issues</p> <p>Abstract, main outcome, line 14. The sentence "Parental report of the number of hours per weekday that they and their 6-8 year old..." seems to imply that parent/child co-viewing was the outcome of interest. See also my comment on this issue in the methods. Please clarify.</p> <p>Background, page 4, lines 11/12. A more up to date statement from the American Academy of Pediatrics (2011) is available.</p> <p>Background, page 4, lines 15-19. Given the international scope of the journal, I think it would be preferable to report nationally representative data on the prevalence of screen-viewing behaviours.</p>

	<p>Methods, page 5, lines 28-30. Please clarify in the methods that parents reported separately their own and their children's screen-viewing behaviours. As currently phrased, it may be interpreted that the outcome was joint parent and child screen-time. Also, please clarify what is meant by 'multi-screen' viewing and how this was defined for the participants.</p> <p>Methods, page 5, lines 37-40. The AAP guideline of 2 hours actually refers to 'total non-educational screen-time' not TV viewing. I suggest this sentence is re-phrased as appropriate. Given the range of behaviours assessed, an outcome variable more consistent with the AAP guideline could have been derived – please explain why such a variable was not included in the analysis.</p> <p>Discussion, page 8, lines 31-33. Do the authors believe that removing media equipment from children's bedrooms is a strategy that will be acceptable to parents / children? There is evidence that doing so may create conflict between parents and children (Jordan et al. Pediatrics 2006)</p> <p>Discussion, page 9, lines 43-45. All that is known about the characteristics of the parents in the sample is sex and education – I don't believe this is sufficient information on which to state that "the sample provides a reasonable representation of parents of 6-8 year old children in the UK". What about age, ethnicity, occupation, location, etc?</p>
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REVIEWER	Dr Natalie Pearson School of Sport, Exercise & Health Sciences Loughborough University
REVIEW RETURNED	04-Feb-2013

GENERAL COMMENTS	<p>This manuscript examines the associations between parent's screen viewing, attitudes and access to screens and children's screen viewing behaviours. The manuscript is well written and offers a novel insight into associations between parents and their children.</p> <p>Abstract:</p> <ol style="list-style-type: none"> 1. There is no mention of attitudes / access in the methods or results part of the abstract. It is worth an additional sentence in these sections. <p>Background:</p> <ol style="list-style-type: none"> 2. Last sentence of the first paragraph should begin with 'furthermore' otherwise it seems a bit out of place. 3. In the third paragraph of the background section you begin talking about parental attitudes but this seems to come from nowhere. How do you get from modelling and multi-screen use to parental attitudes. Some further literature is needed here as to why you think attitudes are important as well as why multi-screen use should be examined. <p>Methods:</p> <ol style="list-style-type: none"> 4. Did you provide a definition or an example of what you meant by 'multi-screen'? 5. The AAP guideline is for total media time (as you state in the background), why have you categorised TV alone into 2 hours? Why didn't you keep the variable as continuous? 6. You say that smart phone time was coded as none versus some – what is some? 7. You repeat about parent education level etc. at the beginning of the 2nd paragraph of methods and again before analysis – have it in
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	<p>one place only.</p> <p>8. In the analysis section it reads as though only one set of analyses were run with child screen viewing as the outcome. State that each screen behaviour was treated separately.</p> <p>Results:</p> <p>9. When you say parent and child dyads it assumed that you collected data from both and have matched it, which is not the case. Perhaps soften this language here and throughout the text.</p> <p>10. On page 7 you say that boys were more than five times more likely to exceed the threshold – what threshold?</p> <p>11. Last paragraph of results you say if a parent reported spending some time – what is some time?</p> <p>12. There is no mention of parental attitudes in the results?</p> <p>Discussion:</p> <p>13. The discussion is very well written</p> <p>14. When you are talking about removing media from a child's bedroom on page 8, it might also be worth saying that is we are educating parents perhaps we should suggest not putting media in the bedrooms in the first place.</p> <p>15. I doubt that removing media from bedrooms is simple and there is some evidence that this can cause arguments and tension in families.</p> <p>16. Missing words from the first sentence of the third paragraph on page 8.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Major issues

Methods, page 5, line 14-17. The method of sampling is novel. Intuitively, however, it seems that this approach is likely to result in a highly biased sample. Is it possible to draw valid inferences regarding the broader UK population based upon this sample? Insufficient information is reported on the social / demographic characteristics of the parents participating in the study to make any reasonable judgement about representativeness.

Response: We accept the reviewer's comment that there is potential that the sample is in some ways biased as a result of our data collection process which relied on an on-line survey. It is important to recognise that almost all data collection processes are subject to a potential self-selection bias. To address the reviewer's concern, we have now provided additional information on the demographic profile of the parents who took part in the study including age, education, gender and number of children into a new Table 1 (so all following table numbers have also changed). The limitations of this study design and implications that these limitations have on our ability to draw conclusions on the representativeness of the sample are acknowledged in the limitations section on page 10.

Methods, page 5, lines 25-45. No information is provided on the validity or reliability of the items used to assess parents or children's sedentary behaviours. Please add this information to the text and comment upon any limitations in the discussion.

Response: We have now added to the bottom of page 5 a summary of the available reliability / validity information on these measures. The lack of reliability and validity data in this sample has however been added to the limitations on page 10.

Methods, page 6, analysis. I have a number of queries regarding the analysis strategy. Firstly, the results are presented separately for boys and girls, but did the authors test formally for effect modification by sex in regression models? Looking at the results, there appear to be relatively few examples of differential associations between boys and girls; therefore why not present for the whole sample? Without formal tests for interaction it is not clear whether observed differences between boys and girls are valid or rather a function of differential sample size. Secondly, were regression models mutually adjusted for all exposure variables? If so, please clarify this in the text. If not, I suggest this should be added to the analysis. The studied exposures are likely to be correlated to a degree; in order to identify independent correlates all significant exposures should be included in a multivariate model. Lastly, a number of potentially important confounding variables are omitted from the analysis; for example, ethnicity, weight status, urban/rural location and family structure (single/dual parent household, presence of siblings). If information on these, and other, potential confounders is not available, the extent to which reported associations may be subject to residual confounding should be acknowledged in the discussion.

Response: We thank the reviewer for the careful consideration of the analytical approach. We have now run some preliminary analyses which indicate that there was no evidence ($p > 0.05$) of gender differences in the outcome variables. This has now been highlighted in the description of the preliminary analyses at the bottom of page 6.

In light of this finding we have therefore run all analyses for the overall sample (i.e. boys and girls combined). All models were mutually adjusted for each other. We have also adjusted all analyses for the demographic variables that were measured including parental education, parental age and number of children. This has been highlighted in the analysis section on the top of page 7 and has been clarified in the footnotes for all tables. We have also revised all tables in line with the new analyses.

Abstract, main outcome, line 14. The sentence “Parental report of the number of hours per weekday that they and their 6-8 year old...” seems to imply that parent/child co-viewing was the outcome of interest. See also my comment on this issue in the methods. Please clarify.

Response: Thanks for catching this error. The text has been amended to say “Parental report of the number of hours per weekday that they and separately their 6 to 8 year old child spent watching TV”. Please see the revised abstract.

Background, page 4, lines 11/12. A more up to date statement from the American Academy of Pediatrics (2011) is available.

Response: We have now added a reference to the more up to date guidance. Please see the revised text on the top of page 4.

Background, page 4, lines 15-19. Given the international scope of the journal, I think it would be preferable to report nationally representative data on the prevalence of screen-viewing behaviours.

Response: We have now added information from the 2008 Health Survey for England to paragraph 1 on page 4.

Methods, page 5, lines 28-30. Please clarify in the methods that parents reported separately their own and their children’s screen-viewing behaviours. As currently phrased, it may be interpreted that the outcome was joint parent and child screen-time. Also, please clarify what is meant by ‘multi-screen’ viewing and how this was defined for the participants.

Response: This has now been changed to say “Parents reported the number of hours per weekday that they and their 6 to 8 year old child spent watching TV, using a games console and using a smart-phone”, please see paragraph 4 on page 5.

We have now inserted the question mentioning multi-screen viewing, which defines what we meant by this to the bottom of page 5. The question reads “Adults and children sometimes use more than one screen device at the same time (such as a TV and laptop). We call this “multi-screen viewing”.

Methods, page 5, lines 37-40. The AAP guideline of 2 hours actually refers to ‘total non-educational screen-time’ not TV viewing. I suggest this sentence is re-phrased as appropriate. Given the range of behaviours assessed, an outcome variable more consistent with the AAP guideline could have been derived – please explain why such a variable was not included in the analysis.

Response: We have now amended the text in the first paragraph of page 4 so that it refers to the 2011 guideline. We have also now added the following text to the top of page 6 to highlight the rationale for reporting each variable separately.

“As children are likely to engage in multi-screen viewing, summing time spent in individual screen activities may lead to an over estimation of total screen-time. Moreover, the use of the four different outcomes facilitates the assessment of whether associations are different for the different types of screen-viewing; information that would aid the design of targeted behaviour change interventions. Thus, separate outcomes were created for each different type of screen-viewing.”

We also recognise that our measure did not differentiate between educational and non-educational screen-viewing. As such the following text has been added to page 10: “A further limitation is that parents were not asked to differentiate between their or their child’s educational and non-educational SV. Future research could develop self-report measures of SV which account allow for outcome variables to be aligned with recommendations”

Discussion, page 8, lines 31-33. Do the authors believe that removing media equipment from children’s bedrooms is a strategy that will be acceptable to parents / children? There is evidence that doing so may create conflict between parents and children (Jordan et al. Pediatrics 2006).

Response: We thank the reviewer for highlighting this issue. We have now added the following text to the bottom of paragraph 1 on page 9:

“It is however, important to highlight that previous qualitative work has shown that many children and parents are resistant to removing TV’s from a child’s bedroom and that making this change might be difficult to achieve [1]. As such, parental education efforts to discourage the introduction of TVs and media equipment into the bedroom might be a more effective and less contentious approach [1]”.

Discussion, page 9, lines 43-45. All that is known about the characteristics of the parents in the sample is sex and education – I don’t believe this is sufficient information on which to state that “the sample provides a reasonable representation of parents of 6-8 year old children in the UK”. What about age, ethnicity, occupation, location, etc?

Response: We accept the reviewer’s concern. We have therefore removed this sentence and added the following text to the limitations on page 10:

“Equally, as a sampling framework was not used it is possible that the sample was skewed towards participants who had more available time to use the website and we are therefore unable to draw any conclusions about the representativeness of the sample.”

Reviewer: 2

This manuscript examines the associations between parent's screen viewing, attitudes and access to screens and children's screen viewing behaviours. The manuscript is well written and offers a novel insight into associations between parents and their children.

Response: Thank you for the very supportive comments.

Abstract:

1. There is no mention of attitudes / access in the methods or results part of the abstract. It is worth an additional sentence in these sections.

Response: Thank you for catching this oversight. The exposures are now mentioned in the Methods and Results section of the abstract.

Background:

2. Last sentence of the first paragraph should begin with 'furthermore' otherwise it seems a bit out of place.

Response: Change made.

3. In the third paragraph of the background section you begin talking about parental attitudes but this seems to come from nowhere. How do you get from modelling and multi-screen use to parental attitudes. Some further literature is needed here as to why you think attitudes are important as well as why multi-screen use should be examined.

Response: We have now added the following text to the bottom of paragraph 2 on page 4: "It may therefore be the case that parental attitudes to these issues are associated with the child's screen-viewing. Obtaining information on these associations is important because if there is some evidence of an association, strategies to change these variables could form part of intervention approaches."

Methods:

4. Did you provide a definition or an example of what you meant by 'multi-screen'?

Response: We have now inserted the question mentioning multi-screen viewing, which defines what we meant by this to the bottom of page 5. The question reads "Adults and children sometimes use more than one screen device at the same time (such as a TV and laptop). We call this "multi-screen viewing".

5. The AAP guideline is for total media time (as you state in the background), why have you categorised TV alone into 2 hours? Why didn't you keep the variable as continuous?

Response: We have also now added the following text to the top of page 6 to highlight the rationale for reporting each variable separately.

"As children are likely to engage in multi-screen viewing, summing time spent in individual screen activities may lead to an over estimation of total screen-time. Moreover, the use of the four different

outcomes facilitates the assessment of whether associations are different for the different types of screen-viewing; information that would aid the design of targeted behaviour change interventions. Thus, separate outcomes were created for each different type of screen-viewing.”

6. You say that smart phone time was coded as none versus some – what is some?

Response: We have now added the following definition to the bottom of paragraph 1 on page 6:

“Games console and smart-phone time were coded as none versus some (i.e. less than 1 hour per day or greater).”

7. You repeat about parent education level etc. at the beginning of the 2nd paragraph of methods and again before analysis – have it in one place only.

Response: Thanks, we have deleted the second reference. Education assessment is now just mentioned in paragraph 4 on page 5.

8. In the analysis section it reads as though only one set of analyses were run with child screen viewing as the outcome. State that each screen behaviour was treated separately.

Response: We have now amended this section so that it states that: “Five logistic regression models were run with child screen-viewing (TV Viewing or computer time or games console time or smart-phone use or multi-screen viewing) as the outcome.” Please see the bottom of page 6.

Results:

9. When you say parent and child dyads it assumed that you collected data from both and have matched it, which is not the case. Perhaps soften this language here and throughout the text.

Response: Thank you for this comment. We have now removed mention of dyads throughout.

10. On page 7 you say that boys were more than five times more likely to exceed the threshold – what threshold?

Response: In light of the revised analyses that were requested by Reviewer 1 this text has been removed.

11. Last paragraph of results you say if a parent reported spending some time – what is some time?

Response: Please see the response to item 6 above.

12. There is no mention of parental attitudes in the results?

Response: Where there was some evidence of an association, this has now been added. Please see paragraph 4 on page 7.

Discussion:

13. The discussion is very well written

Response: Thank you for the kind words.

14. When you are talking about removing media from a child's bedroom on page 8, it might also be worth saying that is we are educating parents perhaps we should suggest not putting media in the

bedrooms in the first place.

Response: Thank you, we like this suggestion and have added the following text to the bottom of paragraph 1 on page 9:

“It is however, important to highlight that previous qualitative work has shown that many children and parents are resistant to removing TV’s from a child’s bedroom and that making this change might be difficult to achieve [1]. As such parental education efforts to discourage the introduction of TVs and media equipment into the bedroom might be a more effective and less contentious approach [1]”.

15. I doubt that removing media from bedrooms is simple and there is some evidence that this can cause arguments and tension in families.

Response: Please see response to point 14.

16. Missing words from the first sentence of the third paragraph on page 8.

Response: We have now re-written this sentence (paragraph 2 on page 9) so that it says the following:

“In this study, there was little evidence to suggest that parents’ attitudes in relation to screen-viewing as good family time, a source of entertainment or valuable family time were associated with high child screen-viewing.”

Cited references:

1. Jordon AB, Hersey JC, McDivitt JA, et al. Reducing Children’s Television-Viewing Time: A Qualitative Study of Parents and Their Children. *Pediatrics* 2006;118(5):e1303-e1310.