

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Social Determinants and Changes in Energy Drink Consumption among Adolescents in Norway, 2017-2019: A Cross-sectional Study
<b>AUTHORS</b>	Kaldenbach, Siri; Strand, Tor A.; Solvik, Beate; Holten-Andersen, Mads

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Marriott, Bernadette Medical University of South Carolina
<b>REVIEW RETURNED</b>	29-Jan-2021

<b>GENERAL COMMENTS</b>	Thank you for this excellent manuscript! The introduction was particularly good in setting the rationale for the study. The question is one that more countries should address in as clear and direct a fashion as your group has. This reviewer has no corrections with the exception that on page 8 line 3: the name of the mentioned group should read American Academy of Pediatrics with an "s" on the end of the word. Thank You again for an excellent study and manuscript.
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<b>REVIEWER</b>	Lebacqz, T Universite Libre de Bruxelles, Ecole de Santé Publique
<b>REVIEW RETURNED</b>	02-Mar-2021

<b>GENERAL COMMENTS</b>	<p>General comment</p> <p>The article investigates an interesting topic using a large population-based sample. The paper should however be improved by being more concise, particularly in the result section, by using appropriate vocabulary, especially in section and table titles, by connecting the results to the methods used, and connecting the conclusions more closely to the results.</p> <p>Specific comments</p> <p>ED or Eds please check the consistency of these abbreviation.</p> <p>Abstract</p> <p>Line 6: "increased with &gt;50%" should be replaced by "increased by over 50%".</p> <p>Lines 12-14: Please mention explicitly the objectives of the study. The sentence "This study describes the extent and trends in ED consumption among Norwegian adolescents in 2017, 2018 and 2019." Is not clear enough. What do you mean with "the extent"?</p>
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	<p>Line 17: Please specify that the sample size concern all three years together.</p> <p>Lines 19-22: Please indicate which models were used to reach your objectives. You mention here “identifying determinants for ED consumption”, is that also your objective? If it is the case, you should indicate in the introduction what is already known about such determinants (not only age and sex) and discuss it in the discussion.</p> <p>Lines 30-32: “The proportion of female high consumers increased from ... to ... from 2017-2019” should be replaced by “The proportion of female high consumers increased from ... to ... between 2017 and 2019”.</p> <p>In the abstract, indicate what does “high consumer” mean.</p> <p>Lines 45-47: “from 2017-2019” should be replaced by “between 2017 and 2019”.</p> <p>The last sentence of the abstract is disconnected from your results. Please provide a conclusion based on your results.</p> <p>Article summary</p> <p>“demonstrates” should be replaced by “identifies”.</p> <p>The repeated character of the survey should be mentioned in the second bullet point.</p> <p>Introduction</p> <p>The introduction needs to be improved.</p> <p>The first paragraph could be better structured: for instance, by beginning with the definition, and then mentioning the marketing strategies.</p> <p>P6, line 15: “sports”, do you mean “athletes”?</p> <p>Please provide more information about the potential adverse effects, especially in adolescents (you only mention sleep disturbance but there are other effects).</p> <p>“It is mainly the high caffeine content in EDs combined with the sugar content and sweet flavor, and high content of stimulating substances which is giving rise to concern.” Please reformulate this sentence to be clearer and more concise.</p> <p>In p7, line 3, you mention “the potential negative short and long-term effect”, you should describe them better in the previous paragraph when you mention the adverse consequence of consuming Eds.</p> <p>The paragraph beginning with “According to the findings of the VKM review,” should be shortened and merged with the previous one.</p> <p>P7, lines 46-48: what do you mean with “ED consumption rate” and “high school kevel”?</p> <p>Were there already studies on this topic? On the time trends in ED consumption in adolescents.</p> <p>P7, line 59: “less adapted” should be replaced by “more sensitive”.</p> <p>P8, line 10: “parallels this development” should be replaced by “followed this development”.</p> <p>Please mention the hypotheses at the end of the introduction.</p> <p>Methods</p> <p>Please specify whether the Ungdata survey is repeated every year.</p> <p>P9, line 14: “The study was predefined according to the Norwegian Centre for Research data.” could be removed.</p>
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	<p>P9, line 24: "The study was approved by the Norwegian Centre for Research Data" could be removed since it is already mentioned afterwards.</p> <p>P9, line 35: "every third year" should be replaced by "every three years"</p> <p>Could you please explain if all schools of each municipality participate in the survey or only a sample of schools in each municipality.</p> <p>The last paragraph of page 9 (starting with "In Norway, children start school...") could be deleted.</p> <p>"The overall participation from the total number of eligible students to those who answered the ED question was 74.4% over the three-year period. See figure 1 for more details." Please reformulate as you do not speak about participation rate but missing data. In addition, please refer to the figure by an indication into brackets (Figure 1).</p> <p>Regarding the section on measures, the first two paragraphs (P10, lines 35-50) are not necessary and could be removed.</p> <p>Please add the sources and validity information for the different measures. When not obvious, you should also indicate all the answer options in the text.</p> <p>"non-ED consumers" should be replaced by "never consumers".</p> <p>P11, line 24: Please replace "the three lower options" by "the first three options".</p> <p>"SES was assessed according to a five-point scale which again was based on a compound score from three different dimensions (22, 23)." Should be replaced by "SES was assessed according to a five-point scale covering three different dimensions".</p> <p>Concerning the residency, it should be stated more clearly that this variable is not an individual variable but a school-level variable.</p> <p>Regarding statistical analyses: is that correct that you used generalized linear models? Why did you not use multiple logistic regressions if the outcome is binary?</p> <p>P12, line 25: replace "generated" by "estimated".</p> <p>P12, line 56: replace "consumers" by "consumption".</p> <p>"The interactions were also estimated on an additive scale using the relative excess risk due to interaction (RERI) to calculate the risk difference, and in generalized linear models with the binomial distribution family and identity link function (28)." -&gt; about which interactions are you speaking here? It is really difficult to follow.</p> <p><b>Results</b></p> <p>The first section should be strongly shortened. All the details are in the table. It is not needed to describe everything, please focus on differences between years.</p> <p>For the next two sections ("Energy drink consumption among boys and girls" and "Proportion of energy drink consumption"), please reformulate the section titles, they are not appropriate. More generally, you should connect better the results you present to the methods you used. It will be easier for the reader to follow in this way.</p> <p>"The proportion of female high ED consumers increased by an average of 23% per year." Please precise if it is a relative percentage.</p> <p>"this increase was lower at 10%": "lower than 10%".</p> <p>For the last three paragraphs of the results section, please make a reference to the result table (Table 3?). Which year is concerned by these results? All? If you refer to Table 3, you should not speak about proportions but about RR, as presented in the table.</p>
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	<p>Discussion</p> <p>Please remind the objectives before summarizing the main results. “In line with our expectation, we observed that the typical high ED consumer was male, had a lower SES, lived less central, spent more than 6 hours daily watching a screen, and was either not physically active or very physically active.” Please reformulate this sentence “high ED consumption was associated...” or “males were more likely to...”. In addition, I am not sure that “lived less central” will be understood by everyone...  “expectation” should be replaced by “expectations”.  You discuss the results on physical activity and screen time, but it would also be relevant to discuss the results about gender, SES and residency.  P17, line 57: “risk taking behavior” should be changed into “risk-taking behaviors”.  “which might have confounded the observed time trend seen over the three-year period.” I think “confounded” is not an appropriate word in this situation.  “In addition, the participating schools within these municipalities were not selected at random. The lower secondary schools are run by the municipalities while the upper secondary schools are organized according to regional districts. This might have led to biases regarding representability of the study.” How were the schools selected if not by random? It should be mentioned in the methods. This sentence is difficult to understand for non-Norwegian people, why the different organization of schools may lead to biases. By “representability”, do you mean “representativity”?  As you mention it as one of the main strengths, the representativity of your sample should be verified to argue it is representative, for instance by comparing your sample to population data.  “This is an important finding to considerate in future research, which should focus on the possible effects of long-term ED consumption among adolescents.” The second part of this sentence is not connected to your results...</p> <p>Conclusion</p> <p>Globally your conclusion is not connected to your results. You should provide a more appropriate conclusion based on your objectives and your results.</p> <p>Figure</p> <p>There should not be connections between all boxes. The horizontal lines are not needed. For more clarity, would it be possible to color the boxes according to the year?</p> <p>Table 1</p> <p>It would be relevant to compare these variables according to the year and provide p-values of the tests.  Would it be possible to merge some subcategories of the variables presented to make the table shorter? For instance, by using the same subcategories than in the subsequent multivariable models.</p> <p>Tables 2</p>
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	<p>It would be nice to present figures of time trends rather than tables.</p> <p>Please try to provide only one table rather than two. The line “ED consumption is not clear”, it includes the never-consumers, so it is not appropriate to call it like that.</p> <p>Table 3</p> <p>The title is not appropriate.</p> <p>If the interaction year and gender is significant, then it would be relevant to present the results by gender.</p>
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<b>REVIEWER</b>	Russo, Rienna NYU Langone Medical Center
<b>REVIEW RETURNED</b>	16-Mar-2021

<b>GENERAL COMMENTS</b>	<p>The objective of this paper was to investigate the trends and factors associated with energy drink consumption among youth in Norway from 2017 to 2019. The authors address an important topic, as EDs are highly caffeinated and sweetened with artificial sugars. The implications of ED on health range beyond disrupted sleep to issues with overweight and obesity and long-term impacts of high consumption remain unexplored. The paper is well written, though the presentation of the results should be double-checked. I would also urge the authors to revise the discussion to be more in line with the results from the paper.</p> <p>The authors use a Poisson Regression to evaluate the change in the number of high consumers by sex and indicate in the results that there is an average increase of 23% per year for females and 10% per year for males. However, from Table 3, the RR for high ED is 1.24 for 2018 female, and 1.46 for 2019 female, and 1.10 for 2018 male, and 1.12 for 2019 male. For females, this corresponds to a 24% increase in high consumers comparing 2018 to 2017 and a 46% increase comparing 2019 to 2017, and for males, this corresponds to a 10% increase comparing 2018 to 2017 and a 12% increase comparing 2019 to 2017. Based on the information available, the authors don't appear to calculate the average change across years in this analysis (or p-trends) but rather are comparing each year to one another. I don't see where the authors are getting the 23% and 10% numbers from, however, there may have been an additional analysis that wasn't outlined in the text/table from which these numbers are derived.</p> <p>The third paragraph of the discussion, starting with line 27, discusses the sex differences in ED consumption. The authors jump to attributing higher consumption among males to the nature of ED marketing/advertising. Authors claim that as the marketing targets masculinity and risk-taking, males are more likely to consume the products as they are more inclined to risk-taking behavior. I caution the authors to make this leap. Moreover, the reference to studies finding that girls follow and adapt the same behavioral pattern to engage in more risk-taking behavior seems misplaced, are the authors claiming that ED consumption is qualified as risk-taking behavior? The reference does not include dietary behavior or food consumption as a behavior explored as one that girls follow boys in adapting. As it's written, the authors insinuate that the greater increase in the percentage of high ED female consumers vs high ED male consumers may be attributed</p>
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to this adaptation of increased risk-taking behavior. However, this seems like an extreme assumption, especially as the sample was not the same surveyed each year and there is nothing to indicate that 2017-2019 timeframe would have prompted more risk-taking, more so than 2014-2017. Rather than discussing risk-taking behavior as a cause for sex differences, I would urge authors to have a more thoughtful discussion about the marketing components. What are the tactics used by the agencies? What makes them 'masculine'? There is literature discussing the use of famous athletes as spokespersons for EDs, as well as the use of cartoons and 'youth-friendly' tactics. I would suggest a more nuanced investigation into the marketing and link it back to the screen time finding. One would think that increased screen time would mean increased exposure to ED advertising on TV, and thus affect ED consumption.

In turn, I would modify the conclusion so as to avoid the risk-taking behavior aspect and focus on how there is a dearth of research on the long-term impacts of ED consumption. Additional suggestions could include researching the specific types of EDs that are consumed (i.e., do they differ by sex/education/age?) and reasons for ED consumption (i.e., for energy, for taste, because their favorite athlete drinks them, etc.). After these changes, I believe the paper will be suitable for publication.

#### Minor Comments

##### Abstract

Line 6 – Remove 'with' before >50%

Line 29 – Define high consumers.

##### Introduction

###### Page 6

Lines 18-21 – This sentence is a bit awkward would suggest something like 'Viewers of channels with greater ED advertising have increased odds of ED consumption'

Lines 31-35 – Would refrain from starting the sentence with 'it is', instead try "The main causes for concern are the high caffeine..." and taking out "which is giving rise to concern" at the end of the sentence

Line 35 – Again, would refrain from starting with 'it is,' especially here when the subject of the argument is unclear. Did a study find that combinations of ingredients cause faster uptake? Specify who is making that argument

Line 46 – Suggest switching 'eventually result in' to 'contribute to' and 'next to' to 'as well as'

###### Page 7

Line 23 – Change 'affecting' to 'to'

Lines 26-33 – Just wanted to note that I really love how the authors included this! Having an example makes understanding the recommendations a lot easier. Great addition!

Line 41 – General comment but I would suggest using sex rather than gender unless the survey specifically asks gender as the participants identify. If that's the case please specify that in the methods.

###### Page 8

Line 5 – Change 'Given' to 'Despite'

Line 8 – Remove 'it is still unclear' and start with 'the extent to which the consumption of ED...' and end with 'remains unclear'

	<p>Page 10 Line 53 – Does the question specify the amount? I.e., how often do you usually drink energy drinks of at least 128g? Or is it just any amount of energy drink?</p> <p>Page 11 Line 6 – What was the rationale for defining ED consumers as greater than 1x/month? Seems like people who consume energy drinks every other week may not be comparable to people who consume it 3x/week</p> <p>Page 14 Line 9 – Please include the % after ‘Most’ Lines 16-24 – These don’t seem like results, rather better suited for Methods Lines 32-35 – Again, these don’t seem like results but rather implications so possibly switch to Methods/Discussion</p> <p>Page 15 Lines 32-40 – These sentences are somewhat redundant. Consider consolidating Line 46 – Consider including what centrality index 6 means again for readers</p> <p>Page 17 Line 34 – Remove ‘for’ before ‘why’ Line 39 – Remove ‘,’ before and after ‘be due to the fact’</p> <p>Page 18 In discussing the limitations and strengths, I find it odd that the representation component is both a limitation and a strength. Consider presenting it as a limitation, in that the sampling strategy made it so the results were not necessarily generalizable to the national population of youth, but that the breadth of the municipalities sampled made it sufficiently large and wide-reaching that it’s only a minor limitation</p> <p>Page 19 Line 29 – Remove ‘in’ after ‘closing’ Line 32 – Remove ‘It is important’ and start with ‘Finding out...’ and end with ‘...is important.’</p>
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<b>REVIEWER</b>	Parkes, Brandon Imperial College London, SAHSU
<b>REVIEW RETURNED</b>	24-Mar-2021

<b>GENERAL COMMENTS</b>	<p>The title of the manuscript "Recent Time Trends in Energy Drinks Consumption Among Adolescents in Norway" suggests a time series analysis however, whereas time trend analysis is performed, the bulk of the analysis is not done using a time series, consequently the title is misleading, a more appropriate title may be "Recent Time Trends and Social Determinants for Energy Drinks Consumption Among Adolescents in Norway. Similarly the results section of the abstract does not summarise (quantitatively) the associations between the determinants and ED consumption.</p> <p>Overall it is not sufficiently clear what the results in table 3 are showing. Are they showing results of a single multiple regression</p>
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	<p>model using all the explanatory variables or are the explanatory variables being used singly in different models? Both should be performed and the results given (some may be as supplementary data). Overall the regression models should be explained more clearly in the statistical methods section and more results (including any sensitivity analysis results) made available.</p> <p>P4. the total number of municipalities in Norway are not indicated, consider including the % when giving the absolute numbers of municipalities.</p> <p>P7. The manuscript claims "associations were estimated both on an additional and multiplicative" but these are not given in the results.</p> <p>P12. The phrase "the typical high ED consumer..." is vague and undefined. Either remove this sentence or define in terms of the results what this means.</p> <p>P13. "there seems to be a quite even distribution..." - this assertion needs to be backed up, either with a reference or data should be included, perhaps in supplementary material.</p> <p>P20/21 (fig1/table1. The numbers for 2018 are strikingly different from 2017 &amp; 2019 - can some explanation be given?</p> <p>P24/25 (table 3) Gender regression - why is the RR for males by individual year given with respect to females (all years)? This seems inconsistent.</p> <p>P6. In the manuscript (p6) energy drink consumption is defined as ED <math>\geq</math> once a month, but this is not one of the categories in table 1 (p22). Is this an error or is 'never' defined as <math>&lt; 1</math> ED per month. This needs to be clarified.</p> <p>P5/6. Were the definitions of 'ED consumption' and 'High ED consumption' chosen arbitrarily? Further analysis should be provided (possibly as sensitivity analyses) with different definitions of 'High ED consumption'.</p> <p>p13. Is there any literature on the extent of recall bias and social desirability bias?</p>
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<b>REVIEWER</b>	Yabes, Jonathan University of Pittsburgh, Medicine
<b>REVIEW RETURNED</b>	09-Apr-2021

<b>GENERAL COMMENTS</b>	<p>This study used a national annual survey in Norway to estimate the yearly energy drink (ED) consumption of children and adults in Norway. It also examined factors associated with ED consumption. Representativeness and size of the sample as well as high survey response rate are strengths of the study. However, the observational study design and self-report measures, as properly acknowledged by the authors, provide weak evidence of the associations observed. The manuscript was well written in general, and the statistical methods were mostly well described and adequate, but there are several concerns:</p> <p>Major:</p>
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	<ul style="list-style-type: none"> <li>- The premise of the study was that ED sales in Norway were increasing. This logically translates to increased consumption in general, including among kids/adolescents that this study focused on. However, whether the rate of increase among kids/adolescents is different compared to adults was not addressed.</li> <li>- The abstract presented prevalence estimates, but no measures of precision (confidence intervals) were included.</li> <li>- The definition of ED consumer in the methods (i.e., ED <math>\geq</math> once a month) is not consistent with the survey question choices. The representations in Table 2 appear more consistent.</li> <li>- What were the reasons for choosing the cut points used in collapsing/categorizing ED consumption?</li> <li>- It is not entirely clear what the "exposure" variables are (P7 L25).</li> <li>- Why were regression models for ordinal data not considered, considering the outcome is naturally ordinal?</li> <li>- How was clustering by municipality accounted for in the analysis? Was robust variance clustered by municipality?</li> <li>- All models presented in Table 3 were poisson models; does that mean all the log-binomial models failed to converge?</li> <li>- The methods section indicates the use of RERI to quantify interactions on the additive scale; however RERI was never presented in the results section.</li> </ul> <p>Minor:</p> <ul style="list-style-type: none"> <li>- I suggest using "multivariable" instead of "multiple" regression models to avoid confusion between multiple predictors and multiple models being fitted.</li> <li>- P7 L32: "additional and multiplicative..." should be "additive and multiplicative..."</li> </ul>
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<b>REVIEWER</b>	Chan, Grace UConn Health, Psychiatry
<b>REVIEW RETURNED</b>	10-Apr-2021

<b>GENERAL COMMENTS</b>	<p>A very short (3 time points) series of cross-sectional annual data on self-report energy drinks (EDs) consumption from a large number (278,891) of students attending lower (approximate age range = 13 – 16) and/or secondary (approximately age range = 16 – 18) schools in Norway was used to investigate the levels of EDs consumption over time (from 2017 to 2019) and how EDs consumption may relate to other factors. Besides demographics (e.g., year of survey, gender and school grade), these factors included adolescent's average daily leisure screen time and frequency of physical activity; a household/family-level composite socio-economic status (SES) score; and centrality of school location. Two dichotomous outcome variables were considered: any consumption of ED and high level of ED consumption, which was defined as &gt; 4 times a week. Gender-and-year specific prevalence of these outcomes were only presented in table format. Regression-based analyses (either Binomial with log link, or Binomial with identity link, or modified Poisson with log link) were used to examine relating factors.</p> <p>Not surprising and consistent with other studies, males, older, and in later year were associated with any EDs consumption and high level of EDs consumption. Though the gender differences were decreasing. Other examined factors also showed association in the expected directions.</p> <p>Overall, the study was well designed and the manuscript was well written. Please consider the following suggestions and comments to improve the article.</p>
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	<ul style="list-style-type: none"> <li>• Should “recent time trends” be part of the title given there were only 3 cross-sectional annual data? Please provide the rationale for such short time frame.</li> <li>• Representativeness of this sample: Is it possible to compare the reported adolescents’ characteristics in 2017, 2018, and 2019 (Table 1) against national statistics (both in and out of school) for the corresponding years to empirically determine the representativeness of this study sample?</li> <li>• Please report annual response rates either in the Methods and/or the Results section, not only in the Discussion section.</li> <li>• Please graphically display gender-and-year specific prevalence of both “any ED consumption” and “high level of ED consumption” with both point estimates and confidence intervals.</li> <li>• Based on the description in the Methods section, “residency” for each participant was “based on where the adolescent attends school”. Please consider using a more appropriate term to label this factor.</li> <li>• Why only results on adjusted relative risks (RR) from modified Poisson regression were reported?</li> <li>• It is unclear why year of survey was included in regression as categorical instead of as continuous explanatory main and interaction with gender. Please justify.</li> <li>• Relative excess risk due to interaction (RERI) was mentioned in the Methods-Statistical analyses subsection, but neither its point estimate nor confidence intervals was reported.</li> <li>• Please include the limitation that despite some adolescents might have participated multiple times, no adjustment was made in any analysis due to the anonymous web-based data collection approach.</li> <li>• Can the authors provide more information regarding ED advertisement and availability in or near schools in Norway during the study time period? In particular, if there were noticeable increases.</li> </ul>
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**VERSION 1 – AUTHOR RESPONSE**

Reviewer: 1

Dr. Bernadette Marriott, Medical University of South Carolina

Comments to the Author:

Thank you for this excellent manuscript! The introduction was particularly good in setting the rationale for the study. The question is one that more countries should address in as clear and direct a fashion as your group has.

1. This reviewer has no corrections with the exception that on page 8 line 3: the name of the mentioned group should read American Academy of Pediatrics with an "s" on the end of the word. Thank You again for an excellent study and manuscript.

Author’s replay: Thank you for your comment. We have now added an “s” to the end of the word.

Reviewer: 2

Dr. T Lebacq, Universite Libre de Bruxelles

Comments to the Author:  
General comment

The article investigates an interesting topic using a large population-based sample. The paper should however be improved by being more concise, particularly in the result section, by using appropriate vocabulary, especially in section and table titles, by connecting the results to the methods used, and connecting the conclusions more closely to the results.

Specific comments

1. ED or Eds please check the consistency of these abbreviation.

Author's replay: Thank you for your comment. We have checked the abbreviation throughout the document.

Abstract

2. Line 6: "increased with >50%" should be replaced by "increased by over 50%".

Author's replay: Thank you for your comment. This sentence has been removed from the abstract.

3. Lines 12-14: Please mention explicitly the objectives of the study. The sentence "This study describes the extent and trends in ED consumption among Norwegian adolescents in 2017, 2018 and 2019." Is not clear enough. What do you mean with "the extent"?

Author's replay: Thank you for your comment. The objectives have now been altered to: "To describe the social determinant and trends in energy drink consumption among Norwegian adolescents in 2017, 2018 and 2019". The word "extent" has been removed since it was a very broad term.

4. Line 17: Please specify that the sample size concern all three years together.

Author's replay: Thank you for your comment. This has been added, please see abstract under the heading "participants".

5. Lines 19-22: Please indicate which models were used to reach your objectives. You mention here "identifying determinants for ED consumption", is that also your objective? If it is the case, you should indicate in the introduction what is already known about such determinants (not only age and sex) and discuss it in the discussion.

Author's replay: Thank you for your comment. The objective has been clarified and more specific objectives are defined in the introduction. These are based on previous knowledge.

6. Lines 30-32: "The proportion of female high consumers increased from ... to ... from 2017-2019" should be replaced by "The proportion of female high consumers increased from ... to ... between 2017 and 2019".

Author's replay: Thank you for your suggestion. We have changed the sentence to your suggestion.

7. In the abstract, indicate what does "high consumer" mean.

Author's replay: Thank you for your comment. High consumers are now defined in the abstract as well.

8. Lines 45-47: "from 2017-2019" should be replaced by "between 2017 and 2019".

Author's replay: Thank you for your suggestion. We have changed the sentence to your suggestion.

9. The last sentence of the abstract is disconnected from your results. Please provide a conclusion based on your results.

Author's replay: Thank you for your comment. We have changed the conclusion to the following: "We found an increase in high consumers among both boys and girls between 2017 and 2019. The observed increase in energy drink consumption among adolescents can explain some of the increased sales of energy drink in Norway."

#### Article summary

10. "demonstrates" should be replaced by "identifies".

Author's replay: Thank you for your suggestion. We have replaced the word.

11. The repeated character of the survey should be mentioned in the second bullet point.

Author's replay: Thank you for your comment. We have added "annual" to the following sentence: "Data are derived from a large, annual, national survey in Norway with close to 300,000 adolescents participating in the study." We hope this clarifies the surveys character.

#### Introduction

12. The introduction needs to be improved.

Author's replay: Thank you for your comment. Please see the comments regarding introduction from yourself and other reviewers. We hope it is better suited now.

13. The first paragraph could be better structured: for instance, by beginning with the definition, and then mentioning the marketing strategies.

Author's replay: Thank you for your suggestion. However, we feel that the beginning of the introduction is catchier the way it is now.

14. P6, line 15: "sports", do you mean "athletes"?

Author's replay: Thank you for your comment. Athletes is indeed a better word.

15. Please provide more information about the potential adverse effects, especially in adolescents (you only mention sleep disturbance but there are other effects).

Author's replay: Thank you for your comment. We have added some more potential adverse effects to the manuscript. Please see the second paragraph of the introduction.

16. "It is mainly the high caffeine content in EDs combined with the sugar content and sweet flavor, and high content of stimulating substances which is giving rise to concern." Please reformulate this sentence to be clearer and more concise.

Author's replay: Thank you for your comment. This has been changed to: "The main causes for concern is the high caffeine e content in EDs combined with the sugar content and sweet flavor, and high content of stimulating substances"

17. In p7, line 3, you mention "the potential negative short and long-term effect", you should describe them better in the previous paragraph when you mention the adverse consequence of consuming Eds.

Author's replay: Thank you for your comment. The potential negative short and long-term effects have now been described more. Please see the second paragraph of the introduction on page 1.

18. The paragraph beginning with "According to the findings of the VKM review," should be shortened and merged with the previous one.

Author's replay: Thank you for the comment. We discussed this when writing and structuring the introduction. However, to avoid long paragraphs, we feel the two should be kept separate, in addition they are making two different points.

19. P7, lines 46-48: what do you mean with "ED consumption rate" and "high school kevel"?  
Were there already studies on this topic? On the time trends in ED consumption in adolescents.  
Author's replay: Thank you for your comment. In the study we are referring to, the author's compared energy consumption rates between middle school level students and high school levels students at the same point in time. The sentence has been revised accordingly: "Moreover, young (male) adolescents at middle school level have a higher ED consumption compared to older adolescents at high school level".

20. P7, line 59: "less adapted" should be replaced by "more sensitive".  
Author's replay: Thank you for your suggestion. We have changed the sentence according to your suggestion.

21. P8, line 10: "parallels this development" should be replaced by "followed this development".  
Author's replay: Thank you for your suggestion. We have changed the sentence according to your suggestion.

22. Please mention the hypotheses at the end of the introduction.  
Author's replay: Thank you for your suggestion. However, we aimed for this paper to be descriptive and not hypothesis driven. We hope that the aim of the study is clearer in the revised version.

## Methods

23. Please specify whether the Ungdata survey is repeated every year.  
Author's replay: Thank you for your comment. We have added the word annual in the first sentence and paragraph of the methods. Ungdata is not repeated in the sense that each municipality is not obliged to participate each year, but they are encouraged to participate every three years as has been described in more detail in the methods section.

24. P9, line 14: "The study was predefined according to the Norwegian Centre for Research data." could be removed.  
Author's replay: Thank you for your comment. The sentence has been joined with the comment below.

25. P9, line 24: "The study was approved by the Norwegian Centre for Research Data" could be removed since it is already mentioned afterwards.  
Author's replay: Thank you for your comment. Please see the comment above.

26. P9, line 35: "every third year" should be replaced by "every three years"  
Author's replay: Thank you for your suggestion. We have changed the word according to your suggestion.

27. Could you please explain if all schools of each municipality participate in the survey or only a sample of schools in each municipality.  
Author's replay: Thank you for your question. The survey is offered to municipality and they then decide which schools should participate. We have added a sentence to clarify this. Add sentence:

28. The last paragraph of page 9 (starting with "In Norway, children start school...") could be deleted.

Author's replay: We decided to keep this because it illustrates how many that have participated.

29. "The overall participation from the total number of eligible students to those who answered the ED question was 74.4% over the three-year period. See figure 1 for more details." Please reformulate as you do not speak about participation rate but missing data. In addition, please refer to the figure by an indication into brackets (Figure 1).

Author's replay: Thank you for your comment. We have revised the paragraph accordingly.

30. Regarding the section on measures, the first two paragraphs (P10, lines 35-50) are not necessary and could be removed.

Author's replay: Thank you for your comment. We agree that they are not necessary and have removed the sentences.

31. Please add the sources and validity information for the different measures. When not obvious, you should also indicate all the answer options in the text.

Author's replay: Thank you for your comment. For the variables used, we have included all answer-options in table 1. This is referred to in the text.

32. "non-ED consumers" should be replaced by "never consumers".

Author's replay: Thank you for your comment. We have revised the sentence.

33. P11, line 24: Please replace "the three lower options" by "the first three options".

Author's replay: Thank you for your comment. We have revised the sentence to your suggestion.

34. "SES was assessed according to a five-point scale which again was based on a compound score from three different dimensions (22, 23)." Should be replaced by "SES was assessed according to a five-point scale covering three different dimensions".

Author's replay: Thank you for your comment. However, we feel that the some of the meaning of the sentence is lost if we altered to your suggestion. We prefer to leave it as it is.

35. Concerning the residency, it should be stated more clearly that this variable is not an individual variable but a school-level variable.

Author's replay: Thank you for your comment. We have added the following to the final sentence of the paragraph to make it more clearly: "Residency for each individual is based on where the adolescent attends school, and it not perse the place of living."

36. Regarding statistical analyses: is that correct that you used generalized linear models? Why did you not use multiple logistic regressions if the outcome is binary?

Author's replay: Thank you for your question. It is correct that we used generalized linear models (GLM) as this allows for binary outcome variables. We chose to use GLM with the binomial distribution family in order to express the effect measure estimates as relative risks (or relative proportions) rather than odds ratios. This is because ORs are poor approximations for RRs for common outcomes. GLMs also allow calculation of risk differences (differences in proportions) which cannot be done by logistic regression analyses.

37. P12, line 25: replace "generated" by "estimated".

Author's replay: Thank you for your suggestion. The word has been replaced.

38. P12, line 56: replace "consumers" by "consumption".

Author's replay: Thank you for your suggestion. The word has been replaced.

39. "The interactions were also estimated on an additive scale using the relative excess risk due to interaction (RERI) to calculate the risk difference, and in generalized linear models with the binomial distribution family and identity link function (28)." -> about which interactions are you speaking here? It is really difficult to follow.

Author's replay: Thank you for your comment. We wanted to demonstrate the effect modification between year and gender both on an additional and multiplicative scale. The Poisson model can only be used for analyses on a multiplicative scale while the RERI can also be used for interactions on an additive scale. We have modified our analyses and corresponding text in the methods section to make it easier to follow. The results were unaltered by these modifications.

## Results

40. The first section should be strongly shortened. All the details are in the table. It is not needed to describe everything, please focus on differences between years.

Author's replay: Thank you for your comment. We have shortened this section.

41. For the next two sections ("Energy drink consumption among boys and girls" and "Proportion of energy drink consumption"), please reformulate the section titles, they are not appropriate.

Author's replay: Thank you for your comment. We have removed the titles.

42. More generally, you should connect better the results you present to the methods you used. It will be easier for the reader to follow in this way.

Author's replay: Thank you for your comment. We have altered the results section to better suit the methods presented.

43. "The proportion of female high ED consumers increased by an average of 23% per year." Please precise if it is a relative percentage.

Author's replay: Thank you for your comment. We have specified this in the sentence after. Please see page 11.

44. "this increase was lower at 10%": "lower than 10%".

Author's replay: Thank you for your comment. However, we like to keep our sentence as it has a different meaning with your suggestion.

45. For the last three paragraphs of the results section, please make a reference to the result table (Table 3?). Which year is concerned by these results? All? If you refer to Table 3, you should not speak about proportions but about RR, as presented in the table.

Author's replay: Thank you for your comment. We have revised some of the results to make it clearer, in addition, table 3 has become table 2 now. Please see page 11 and forward.

## Discussion

46. Please remind the objectives before summarizing the main results.

Author's replay: Thank you for your comment. We have now added the objectives to the discussion.

47. "In line with our expectation, we observed that the typical high ED consumer was male, had a lower SES, lived less central, spent more than 6 hours daily watching a screen, and was either not physically active or very physically active." Please reformulate this sentence "high ED consumption was associated..." or "males were more likely to...". In addition, I am not sure that "lived less central" will be understood by everyone...

Author's replay: Thank you for your comment. We have altered the sentence to make it clearer.

48. "expectation" should be replaced by "expectations".

Author's replay: Thank you for your comment. The word has been removed from the sentence.

49. You discuss the results on physical activity and screen time, but it would also be relevant to discuss the results about gender, SES and residency.

Author's replay: Thank you for your suggestion. We have made some changes according to other reviewers' suggestions as well as this. We hope it is better suited now.

50. P17, line 57: "risk taking behavior" should be changed into "risk-taking behaviors".

Author's replay: Thank you for your comment. This has now been changed.

51. "which might have confounded the observed time trend seen over the three-year period." I think "confounded" is not an appropriate word in this situation.

Author's replay: Thank you for your comment. We have changed the wording.

52. "In addition, the participating schools within these municipalities were not selected at random. The lower secondary schools are run by the municipalities while the upper secondary schools are organized according to regional districts. This might have led to biases regarding representability of the study." How were the schools selected if not by random? It should be mentioned in the methods. This sentence is difficult to understand for non-Norwegian people, why the different organization of schools may lead to biases.

Author's replay: Thank you for your comment. We have added the word completely in the following sentence: "In addition, the participating schools within these municipalities were not selected completely at random." In addition, we clarified the selection in the methods.

53. By "representability", do you mean "representativity"?

Author's replay: Thank you for your comment. The sentence containing this word has been removed from the paragraph.

54. As you mention it as one of the main strengths, the representativity of your sample should be verified to argue it is representative, for instance by comparing your sample to population data.

Author's replay: Thank you for your comment. The Ungdata survey is regarded as the most complete dataset on Norwegian adolescents. We have information on the annual number of adolescents in the different school levels from Statistics Norway (national registry), but there are no additional details available.

55. "This is an important finding to considerate in future research, which should focus on the possible effects of long-term ED consumption among adolescents." The second part of this sentence is not connected to your results...

Author's replay: Thank you for your comment. We have now revised the last part of the discussion to be more connected to the results.

## Conclusion

56. Globally your conclusion is not connected to your results. You should provide a more appropriate conclusion based on your objectives and your results.

Author's replay: Thank you for your comment. We have revised the conclusion.

## Figure



57. There should not be connections between all boxes. The horizontal lines are not needed. For more clarity, would it be possible to color the boxes according to the year?

Author's replay: Thank you for your comment. We removed the horizontal lines between the boxes, but we have kept the figure in neutral colors.

Table 1

58. It would be relevant to compare these variables according to the year and provide p-values of the tests.

Author's replay: Thank you for your comment. However, we feel that the table presenting numbers and percentages according to year is sufficient to illustrate the data material.

59. Would it be possible to merge some subcategories of the variables presented to make the table shorter? For instance, by using the same subcategories than in the subsequent multivariable models.

Author's replay: Thank you for your comment. We think it is valuable to keep all the answer options to illustrate the different responses next to avoid repeating ourselves in the methods where we mention the variables used.

Tables 2

60. It would be nice to present figures of time trends rather than tables.

Author's replay: Thank you for your suggestion, we have now made two figures to illustrate the time trends.

61. Please try to provide only one table rather than two. The line "ED consumption is not clear", it includes the never-consumers, so it is not appropriate to call it like that.

Author's replay: Thank you for your comment. The tables have now been removed from the manuscript.

Table 3

62. The title is not appropriate.

Author's replay: Thank you for your comment. The title has now been altered from: "Modified Poisson Regression With Interaction Term According to ED Consumers and High ED Consumers." To: Determinant for being energy drink consumers (any ED or high ED) in Norwegian adolescents.

63. If the interaction year and gender is significant, then it would be relevant to present the results by gender.

Author's replay: Thank you for your comment. However, we do present the data by gender (yearly increase). Table 2 has been modified and we hope that it is clearer now.

Reviewer: 3

Dr. Rienna Russo, NYU Langone Medical Center

Comments to the Author:

The objective of this paper was to investigate the trends and factors associated with energy drink consumption among youth in Norway from 2017 to 2019. The authors address an important topic, as EDs are highly caffeinated and sweetened with artificial sugars. The implications of ED on health range beyond disrupted sleep to issues with overweight and obesity and long-term impacts of high

consumption remain unexplored. The paper is well written, though the presentation of the results should be double-checked. I would also urge the authors to revise the discussion to be more in line with the results from the paper.

1. The authors use a Poisson Regression to evaluate the change in the number of high consumers by sex and indicate in the results that there is an average increase of 23% per year for females and 10% per year for males. However, from Table 3, the RR for high ED is 1.24 for 2018 female, and 1.46 for 2019 female, and 1.10 for 2018 male, and 1.12 for 2019 male. For females, this corresponds to a 24% increase in high consumers comparing 2018 to 2017 and a 46% increase comparing 2019 to 2017, and for males, this corresponds to a 10% increase comparing 2018 to 2017 and a 12% increase comparing 2019 to 2017. Based on the information available, the authors don't appear to calculate the average change across years in this analysis (or p-trends) but rather are comparing each year to one another. I don't see where the authors are getting the 23% and 10% numbers from, however, there may have been an additional analysis that wasn't outlined in the text/table from which these numbers are derived.

Author's replay: Thank you for your comment. We have specified the numbers in the text to clarify. 23% and 10% are rough yearly averages made across the three years. The modified text and table 2 should make this easier to understand.

2. The third paragraph of the discussion, starting with line 27, discusses the sex differences in ED consumption. The authors jump to attributing higher consumption among males to the nature of ED marketing/advertising. Authors claim that as the marketing targets masculinity and risk-taking, males are more likely to consume the products as they are more inclined to risk-taking behavior. I caution the authors to make this leap. Moreover, the reference to studies finding that girls follow and adapt the same behavioral pattern to engage in more risk-taking behavior seems misplaced, are the authors claiming that ED consumption is qualified as risk-taking behavior? The reference does not include dietary behavior or food consumption as a behavior explored as one that girls follow boys in adapting. As it's written, the authors insinuate that the greater increase in the percentage of high ED female consumers vs high ED male consumers may be attributed to this adaptation of increased risk-taking behavior. However, this seems like an extreme assumption, especially as the sample was not the same surveyed each year and there is nothing to indicate that 2017-2019 timeframe would have prompted more risk-taking, more so than 2014-2017. Rather than discussing risk-taking behavior as a cause for sex differences, I would urge authors to have a more thoughtful discussion about the marketing components. What are the tactics used by the agencies? What makes them 'masculine'? There is literature discussing the use of famous athletes as spokespersons for EDs, as well as the use of cartoons and 'youth-friendly' tactics. I would suggest a more nuanced investigation into the marketing and link it back to the screen time finding. One would think that increased screen time would mean increased exposure to ED advertising on TV, and thus affect ED consumption.

Author's replay: Thank you for your comment and suggestion. We have taken this into account and have rephrased this section of the discussion accordingly.

3. In turn, I would modify the conclusion so as to avoid the risk-taking behavior aspect and focus on how there is a dearth of research on the long-term impacts of ED consumption. Additional suggestions could include researching the specific types of EDs that are consumed (i.e., do they differ by sex/education/age?) and reasons for ED consumption (i.e., for energy, for taste, because their favorite athlete drinks them, etc.). After these changes, I believe the paper will be suitable for publication.

Author's replay: Thank you for your comment. We have included most of your suggestions to the manuscript.

Minor Comments

## Abstract

4. Line 6 – Remove ‘with’ before >50%

Author’s replay: Thank you for your suggestion. The sentence with these words have been removed from the abstract.

5. Line 29 – Define high consumers.

Author’s replay: Thank you for your comment. High consumers have now been defined in the abstract.

## Introduction

### Page 6

6. Lines 18-21 – This sentence is a bit awkward would suggest something like ‘Viewers of channels with greater ED advertising have increased odds of ED consumption’

Author’s replay: Thank you for your suggestion. We agree that the sentence was strange and have altered it to your suggestion. Please see the first paragraph of the introduction.

7. Lines 31-35 – Would refrain from starting the sentence with ‘it is’, instead try “The main causes for concern are the high caffeine...” and taking out “which is giving rise to concern” at the end of the sentence

Author’s replay: Thank you for your comment and suggestion. We have now changed the sentence to your suggestion. Please see the second paragraph of the introduction.

8. Line 35 – Again, would refrain from starting with ‘it is,’ especially here when the subject of the argument is unclear. Did a study find that combinations of ingredients cause faster uptake? Specify who is making that argument

Author’s replay: Thank you for pointing this out. We have now changed the sentence to start with: “According to Iversen et. al,...”

9. Line 46 – Suggest switching ‘eventually result in’ to ‘contribute to’ and ‘next to’ to ‘as well as’

Author’s replay: Thank you for your suggestion. We have changed the words into the following sentence: “Increased energy intake may contribute to in overweight and obesity, as well as dental caries due to the high sugar and citric acid content of ED and SSBs”

### Page 7

10. Line 23 – Change ‘affecting’ to ‘to’

Author’s replay: Thank you for your suggestion. We have changed the words.

11. Lines 26-33 – Just wanted to note that I really love how the authors included this! Having an example makes understanding the recommendations a lot easier. Great addition!

Author’s replay: Thank you for your comment. We are pleased to hear you like it.

12. Line 41 – General comment but I would suggest using sex rather than gender unless the survey specifically asks gender as the participants identify. If that’s the case please specify that in the methods.

Author’s replay: Thank you for your comment. However, we feel that is the appropriate term to use as the answered which gender they feel they belong to in the survey.

### Page 8

13. Line 5 – Change ‘Given’ to ‘Despite’

Author’s replay: Thank you for your comment. We have changed the word to your suggestion.

14. Line 8 – Remove ‘it is still unclear’ and start with ‘the extent to which the consumption of ED...’ and end with ‘remains unclear’

Author’s replay: Thank you for your suggestion. We have changed the sentence according to your suggestion.

Page 10

15. Line 53 – Does the question specify the amount? I.e., how often do you usually drink energy drinks of at least 128g? Or is it just any amount of energy drink?

Author’s replay: Thank you for your question. The question does not specify the amount and is indeed any amount of energy drink.

Page 11

16. Line 6 – What was the rationale for defining ED consumers as greater than 1x/month? Seems like people who consume energy drinks every other week may not be comparable to people who consume it 3x/week

Author’s replay: Thank you for your comment. This was a typo; it should have been 1x a week instead of month.

Page 14

17. Line 9 – Please include the % after ‘Most’

Author’s replay: Thank you for your comment. We have now added this to the sentence.

18. Lines 16-24 – These don’t seem like results, rather better suited for Methods

Author’s replay: Thank you for your suggestion. We have moved it to the methods as we see that it fits better there.

19. Lines 32-35 – Again, these don’t seem like results but rather implications so possibly switch to Methods/Discussion

Author’s replay: Thank you for your comment. However, we think this is an important point to make in order to understand the distribution and geographic locations in Norway, and we think it is best suited to mention this here.

Page 15

20. Lines 32-40 – These sentences are somewhat redundant. Consider consolidating

Author’s replay: Thank you for your comment. We have revised this section.

21. Line 46 – Consider including what centrality index 6 means again for readers

Author’s replay: Thank you for your comment. We added the words “least central” to help the reader remember.

Page 17

22. Line 34 – Remove ‘for’ before ‘why’

Author’s replay: Thank you for your suggestion. We have now removed “for” from the sentence.

23. Line 39 – Remove ‘;’ before and after ‘be due to the fact’

Author’s replay: Thank you for your comment. The sentence has been altered to not include these words anymore.

Page 18

24. In discussing the limitations and strengths, I find it odd that the representation component is both a limitation and a strength. Consider presenting it as a limitation, in that the sampling strategy made it

so the results were not necessarily generalizable to the national population of youth, but that the breadth of the municipalities sampled made it sufficiently large and wide-reaching that it's only a minor limitation

Author's replay: Thank you for your comment. We have revised the limitations section according to your suggestion.

Page 19

25. Line 29 – Remove 'in' after 'closing'

Author's replay: Thank you for your comment. We have removed the word from the sentence.

26. Line 32 – Remove 'It is important' and start with 'Finding out...' and end with '...is important.'

Author's replay: Thank you for your suggestion. We have altered the sentence to your suggestion.

Reviewer: 4

Dr. Brandon Parkes, Imperial College London

Comments to the Author:

1. The title of the manuscript "Recent Time Trends in Energy Drinks Consumption Among Adolescents in Norway" suggests a time series analysis however, whereas time trend analysis is performed, the bulk of the analysis is not done using a time series, consequently the title is misleading, a more appropriate title may be "Recent Time Trends and Social Determinants for Energy Drinks Consumption Among Adolescents in Norway. Similarly the results section of the abstract does not summarise (quantitatively) the associations between the determinants and ED consumption.

Author's replay: We appreciate your suggestion, we changed the words "time trends" and added the study design to clarify. The new title is: Recent Development in Energy Drink Consumption among Adolescents in Norway: a cross-sectional study.

2. Overall it is not sufficiently clear what the results in table 3 are showing. Are they showing results of a single multiple regression model using all the explanatory variables or are the explanatory variables being used singly in different models? Both should be performed and the results given (some may be as supplementary data). Overall the regression models should be explained more clearly in the statistical methods section and more results (including any sensitivity analysis results) made available.

Author's replay: Thanks for the input. Table 3, which is table 2 in the revised manuscript has been updated to ease the interpretation. The effect estimates are the regression coefficients from multiple Poisson models. We have also updated the table legends and the description of the methods to clarify this.

3. P4. the total number of municipalities in Norway are not indicated, consider including the % when giving the absolute numbers of municipalities.

Author's replay: Thank you for your comment. We have now included the total number of municipalities in Norway in 2019.

4. P7. The manuscript claims "associations were estimated both on an additional and multiplicative" but these are not given in the results.

Author's replay: Thank you for your comment. More details are now given in the results regarding this.

5. P12. The phrase "the typical high ED consumer..." is vague and undefined. Either remove this sentence or define in terms of the results what this means.

Author's reply: Thank you for your comment. The sentence has been altered to no longer include the phrase "typical high ED consumer".

6. P13. "there seems to be a quite even distribution..." - this assertion needs to be backed up, either with a reference or data should be included, perhaps in supplementary material.

Author's reply: Thank you for your comment. We have revised this sentence.

7. P20/21 (fig1/table1. The numbers for 2018 are strikingly different from 2017 & 2019 - can some explanation be given?

Author's reply: Thank you for your question. Unfortunately, we do not have an explanation for this.

8. P24/25 (table 3) Gender regression - why is the RR for males by individual year given with respect to females (all years)? This seems inconsistent.

Author's reply: All RRs are the relative change compared to the reference which is females or 2017. This has been clarified in the methods section and in table 2 (old table 3).

9. P6. In the manuscript (p6) energy drink consumption is defined as ED  $\geq$  once a month, but this is not one of the categories in table 1 (p22). Is this an error or is 'never' defined as  $< 1$  ED per month. This needs to be clarified.

Author's reply: Thank you for your comment. Unfortunately, there was a type error in the methods. It should have been 1x a week. This has now been corrected.

10. P5/6. Were the definitions of 'ED consumption' and 'High ED consumption' chosen arbitrarily?

Further analysis should be provided (possibly as sensitivity analyses) with different definitions of 'High ED consumption'.

Author's reply: Thank you for your question. This definition is based on previous studies, which makes our result more easily comparable to other findings.

11. p13. Is there any literature on the extent of recall bias and social desirability bias?

Author's reply: Thank you for your question. To our knowledge there is no literature on this regarding ED specifically. We feel that it is relevant to comment on this in general as this is typical for the nature of surveys.

Reviewer: 5

Dr. Jonathan Yabes, University of Pittsburgh

Comments to the Author:

This study used a national annual survey in Norway to estimate the yearly energy drink (ED) consumption of children and adults in Norway. It also examined factors associated with ED consumption. Representativeness and size of the sample as well as high survey response rate are strengths of the study. However, the observational study design and self-report measures, as properly acknowledged by the authors, provide weak evidence of the associations observed. The manuscript was well written in general, and the statistical methods were mostly well described and adequate, but there are several concerns:

Major:

1. The premise of the study was that ED sales in Norway were increasing. This logically translates to

increased consumption in general, including among kids/adolescents that this study focused on. However, whether the rate of increase among kids/adolescents is different compared to adults was not addressed.

Author's replay: Thank you for your comment. The was not addressed because there is no information available at the moment for adults with the same coverage and number of respondents as for adolescents nationally.

2. The abstract presented prevalence estimates, but no measures of precision (confidence intervals) were included.

Author's replay: Thank you for your comment. We have now added different measures of precision to the abstract.

3. The definition of ED consumer in the methods (i.e., ED  $\geq$  once a month) is not consistent with the survey question choices. The representations in Table 2 appear more consistent.

Author's replay: Thank you for your comment. There was a type error in the text. It should be:  $>$ once a week. This has now been changed.

4. What were the reasons for choosing the cut points used in collapsing/categorizing ED consumption?

Author's replay: Thank you for your question. Please see our response to question 10 by reviewer 4.

5. It is not entirely clear what the "exposure" variables are (P7 L25).

Author's replay: Thank you for your comment. This has been rephrased. Please see abstract and page 7.

6. Why were regression models for ordinal data not considered, considering the outcome is naturally ordinal?

Author's replay: Thank you for your question. Regression models for ordinal data was considered, but we consider it easier to interpret and communicate a binary outcome.

7. How was clustering by municipality accounted for in the analysis? Was robust variance clustered by municipality?

Author's replay: Thank you for your question. We performed analysis with and without clustering by municipalities. There were only negligible differences in the standard errors following these adjustments. We therefore keep table 3 (now table 2) without cluster adjusted effect estimates. We added a line about this in the method and results section. See page 7 and 11.

8. All models presented in Table 3 were poisson models; does that mean all the log-binomial models failed to converge?

Author's replay: Thank you for your comment. No, but because some did, we chose to present only the Poisson models.

9. The methods section indicates the use of RERI to quantify interactions on the additive scale; however RERI was never presented in the results section.

Author's replay: Thank you for your comment. We have removed the concept of RERI to avoid confusion, however we have added the results from the generalized linear models with the binomial distribution family and identity link function and to the results.

Minor:

10. I suggest using "multivariable" instead of "multiple" regression models to avoid confusion between multiple predictors and multiple models being fitted.

Author's replay: Thank you for your suggestion. We agree that multivariable is better fitted to describe our models.

11. P7 L32: "additional and multiplicative..." should be "additive and multiplicative..."

Author's replay: Thank you for your comment. This has now been altered

Reviewer: 6

Dr. Grace Chan, UConn Health

Comments to the Author:

A very short (3 time points) series of cross-sectional annual data on self-report energy drinks (EDs) consumption from a large number (278,891) of students attending lower (approximate age range = 13 – 16) and/or secondary (approximately age range = 16 – 18) schools in Norway was used to investigate the levels of EDs consumption over time (from 2017 to 2019) and how EDs consumption may relate to other factors. Besides demographics (e.g., year of survey, gender and school grade), these factors included adolescent's average daily leisure screen time and frequency of physical activity; a household/family-level composite socio-economic status (SES) score; and centrality of school location. Two dichotomous outcome variables were considered: any consumption of ED and high level of ED consumption, which was defined as > 4 times a week. Gender-and-year specific prevalence of these outcomes were only presented in table format. Regression-based analyses (either Binomial with log link, or Binomial with identity link, or modified Poisson with log link) were used to examine relating factors.

Not surprising and consistent with other studies, males, older, and in later year were associated with any EDs consumption and high level of EDs consumption. Though the gender differences were decreasing. Other examined factors also showed association in the expected directions.

Overall, the study was well designed, and the manuscript was well written. Please consider the following suggestions and comments to improve the article.

1. Should "recent time trends" be part of the title given there were only 3 cross-sectional annual data? Please provide the rationale for such short time frame.

Author's replay: Thank you for your comment. We only had 3 years available, Norwegian government want us to keep track. The title is representative of the study as it is.

2. Representativeness of this sample: Is it possible to compare the reported adolescents' characteristics in 2017, 2018, and 2019 (Table 1) against national statistics (both in and out of school) for the corresponding years to empirically determine the representativeness of this study sample?

Author's replay: Thank you for your comment. Please see our response to question 54 by reviewer 2.

3. Please report annual response rates either in the Methods and/or the Results section, not only in the Discussion section.

Author's replay: Thank you for your comment. We have added this to the methods.

4. Please graphically display gender-and-year specific prevalence of both "any ED consumption" and "high level of ED consumption" with both point estimates and confidence intervals.

Author's replay: Thank you for your comment. We have added the confidence intervals in the text. Please see the abstract and result section.

5. Based on the description in the Methods section, "residency" for each participant was "based on



where the adolescent attends school". Please consider using a more appropriate term to label this factor.

Author's replay: Thank you for your comment. We have specified this in the methods, but we think that residency remains the most fitting word to describe this variable.

6. Why only results on adjusted relative risks (RR) from modified Poisson regression were reported?

Author's replay: Thank you for your comment. We decided to only include the adjusted Poisson estimates because the log-binomial models resulted in similar results.

7. It is unclear why year of survey was included in regression as categorical instead of as continuous explanatory main and interaction with gender. Please justify.

Author's replay: Thank you for your comment. We have no data on the exact date when the forms were completed, only the year. It is accordingly more appropriate to treat the variable as a categorical than a continuous variable. The interaction between gender and year was decided a priori. We realize that the regression table (now table 2) was somewhat unclear, and we hope that it is more clear and easier to understand now.

8. Relative excess risk due to interaction (RERI) was mentioned in the Methods-Statistical analyses subsection, but neither its point estimate nor confidence intervals was reported.

Author's replay: Thank you for your comment. We have removed the concept of RERI to avoid confusion, however we have added the results from the generalized linear models with the binomial distribution family and identity link function and to the results.

9. Please include the limitation that despite some adolescents might have participated multiple times, no adjustment was made in any analysis due to the anonymous web-based data collection approach.

Author's replay: Thank you for your suggestion. We have now included your suggestion in the manuscript.

10. Can the authors provide more information regarding ED advertisement and availability in or near schools in Norway during the study time period? In particular, if there were noticeable increases.

Author's replay: Thank you for your question. To our knowledge, there is no data available on this topic. It would however have been interesting to look at this as well.

Reviewer: 1

Competing interests of Reviewer: None declared

Reviewer: 2

Competing interests of Reviewer: None declared

Reviewer: 3

Competing interests of Reviewer: None declared

Reviewer: 4

Competing interests of Reviewer: None

Reviewer: 5

Competing interests of Reviewer: None declared

Reviewer: 6

Competing interests of Reviewer: none

### VERSION 2 – REVIEW

<b>REVIEWER</b>	Russo, Rienna NYU Langone Medical Center
<b>REVIEW RETURNED</b>	10-May-2021

<b>GENERAL COMMENTS</b>	<p>Thank you for addressing my earlier comments, I appreciate your attention to detail. The clarity of the manuscript and implications of the findings have been much improved. However, there are some remaining concerns that need to be addressed.</p> <p><b>Introduction</b> Page 1, Line 6 – Remove “Being” and start the sentence with “Marketed” Page 3, Line 14 – Replace semi-colon after “Pediatrics” with a comma Page 3, Lines 21 – 26 – Consider revising the objective statement, “development” is confusing. I understand you may be hesitant to use the term “trends” as this is not a longitudinal study but rather a series of cross-sectional data, however, I think “changes” would be appropriate. Example: “The objective of this study is to describe the social deterrents of health associated with ED consumption, and changes in ED consumption frequency in 2017, 2018, and 2019.”</p> <p><b>Methods</b> Page 6, Lines 52 -59 – Consider including what the term centrality and index mean in the first sentence, Example: “According to Statistics Norway, centrality refers to an index of travel time to workplaces and service functions from all populated basic units. Group 1 contains the most...” And then remove the “The calculation of the index...” sentence.</p> <p><b>Results</b> Page 11, Lines 39-42 – I am still struggling to understand the use of the “average of 23% per year”. I see in your response that you made “rough yearly averages made across the three years” but I would appreciate a greater description of how this number was derived. From 2017 to 2018, a two-year period, there is a 24% increase, from 2017 to 2019, there is a 46% increase. How does this translate to an average of 23% per year? Given that these are three cross-sectional studies, with not necessarily the same participants per year, I’m not sure that it’s appropriate to present the “rough yearly averages” without a detailed description as to how and why they were calculated acknowledging the study design and analysis type.</p> <p>Page 16, Lines 13-15 – What do you mean when you say there is a non-linear relation between high ED consumption and physical activity? From Table 2, I see that compared to people reporting no physical activity, any amount of physical activity, from seldom to &gt;5 times a week, decreases ED consumption. I also see that as physical activity increases, the decrease in ED does not necessarily increase. Is this what you meant? Ordinal variables, like the physical activity one, cannot be approximated as</p>
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	<p>continuous variables, for which you could discuss linearity. The spacing between the categories is not uniform. For example, the jump from “seldom” to “1-2 times a month” is ambiguous and likely not the same as the jump from 3-4 times a week to &gt;5 times per week. As such, I would caution and urge you to refrain from discussing the relationship as “non-linear.”</p> <p>Discussion Page 17, Line 6 – Again, would suggest you use “change” rather than “development” for clarity.</p> <p>Page 17, Lines 33 – Page 18, Line 18 – These paragraphs are a bit disjointed. I appreciate the revision of the language about ED marketing/advertising and masculinity, however, think this section could benefit from some rearranging. I would recommend moving the sentences discussing the Emond paper through the sentences discussing the Hammond et al. paper to the end of the second discussion paragraph (Page 17, Line 49.) Then go into the marketing of ED as boosters of an active lifestyle and the gender differences. I am still a bit confused about the introduction of the “masculinity” argument as a reason for greater ED consumption among boys. Is there evidence to suggest that “masculinity” is used in ED marketing? I would caution you from equating marketing of an active lifestyle and higher levels of performance to a masculine marketing tactic, which as of now is how it reads.</p> <p>Page 19, Line 37 – “development” to “change”</p> <p>Page 19, Lines 37-57 – This seems a bit repetitive, like the first paragraph of the discussion and to the concluding paragraph below. Moreover, you claim that the main finding is the increasing proportion of female high ED consumers, but this is the first time you highlight this finding as most important. I recommend moving that sentence (lines 42 – 49) to the first paragraph of the discussion (page 17). I recommend moving lines 49 -57 to the conclusion removing talk of the increase in ED sales in Norway, and that adolescents are most likely not the only group with increased consumption. The conclusion should discuss the implications and takeaways of your study – which is the importance of understanding gender differences in consumption, types of consumption, and longitudinal effects.</p> <p>Table 1 How did you treat people with missing data? I see that “Missing” is listed under Grade, gender, physical activity, and screen time. In the Methods section, you addressed the missingness of the gender variable, but I don’t see any discussion of the missingness of other data. The final sample in the multivariable models must have been smaller than the 278,891 adolescents mentioned at the beginning of the results if there was data missing across these covariates.</p>
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<b>REVIEWER</b>	Parkes, Brandon Imperial College London, SAHSU
<b>REVIEW RETURNED</b>	13-May-2021

<b>GENERAL COMMENTS</b>	A clearer explanation of how the results presented in table 2 has been provided, but there is no mention of univariable Poisson regression results. I would at least like a sentence or two of
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	<p>explanation as to why univariable models were not run. Same goes for any sensitivity analyses that may have been considered.</p> <p>Regarding my original comment about the definitions of 'ED consumption' and 'High ED consumption', the authors' response is 'This definition is based on previous studies, which makes our result more easily comparable to other findings'. Please cite the previous studies in the manuscript.</p>
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<b>REVIEWER</b>	Yabes, Jonathan University of Pittsburgh, Medicine
<b>REVIEW RETURNED</b>	23-May-2021

<b>GENERAL COMMENTS</b>	This manuscript has substantially improved and have addressed my concerns from the previous version.
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<b>REVIEWER</b>	Chan, Grace UConn Health, Psychiatry
<b>REVIEW RETURNED</b>	17-May-2021

<b>GENERAL COMMENTS</b>	<p>The authors had addressed many of the previous comments. As a result the revised manuscript was substantially better than the original submission. In particular, it has a more appropriate title and better structured abstract. However, there are a few major and minor issues.</p> <p>Major issues:</p> <ul style="list-style-type: none"> <li>• The nested data structure: students nested within schools and schools nested within municipalities or regional districts, was neither clearly stated at the methods section nor taken into consideration in data analyses. Also, only some of these were mentioned in the Discussion section as limitation. If the regression results with and without adjusting for municipality clustering were similar, why not report results from the more appropriated analysis adjusting for dependence between students?</li> <li>• The location explanatory variable/factor was still incorrectly referred to as “residency” yet centrality was defined based on the location of schools NOT where students lived! Moreover, with many students attending the same school, but may live in different locations, the data analyses ignore this form of dependency and treated student-level and school-level as well as higher-level explanatory variables/factors as if there were all measured at student-level. Please reword phrases like “Most of the participants LIVED in municipalities with the centrality index two and three.” as “Most of the participants ATTENDED SCHOOLS in municipalities with the centrality index two and three.” in all similar places.</li> <li>• Please include details of type 3 (a.k.a. partial) tests at the explanatory variable/factor level of both modified Poisson regressions after adjusting for clustering. If all the categorical explanatory variables were included in the regression as nominal factor, it should NOT be interpreted as ordinal factor. In particular, please justify these two sentences: “There was a non-linear relation between high ED consumption and physically active. However, this was not observed for the any ED consumption group.”</li> <li>• In the abstract, please clarify that 278,891 was NOT the number of students responded to the survey, but was the number of students who had answered the energy drink question among the 297,102 partial or full respondents out of the 374,970 eligible participants.</li> </ul>
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	<p>Minor issues include the followings among others:</p> <ul style="list-style-type: none"> <li>• In Table 1, please provide more details description of the three lowest response options for leisure screen time to clearly justify the combining of them in subsequent analyses. Please correct either the Tables or the text regarding the leisure screen time response options: “less than two hours” vs. “≤ 2 hours daily” and “more than six hours” vs. “≥ 6 hours daily”. In addition, please include detailed counts and percentages of the 5-level social-economic status explanatory variable by year as well as total since it is unclear if the “five equally sized groups” were based on the entire sample or within each year.</li> <li>• Error bars (either 95% confidence interval or standard error) should be added to both Figures 2 and 3.</li> <li>• Please clarify how the response option “&lt; once a week” was treated when comparing “ED consumer (ED ≥ once a week)” with “never ED consumers”.</li> </ul>
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## VERSION 2 – AUTHOR RESPONSE

Reviewer: 3

Dr. Rienna Russo, NYU Langone Medical Center

Comments to the Author:

Thank you for addressing my earlier comments, I appreciate your attention to detail. The clarity of the manuscript and implications of the findings have been much improved. However, there are some remaining concerns that need to be addressed.

Introduction

1. Page 1, Line 6 – Remove “Being” and start the sentence with “Marketed”

Author’s replay: Thank you for your suggestion. We have now removed the word “being”.

2. Page 3, Line 14 – Replace semi-colon after “Pediatrics” with a comma

Author’s replay: Thank you for your suggestion. We have replaced the semi-colon with a comma.

3. Page 3, Lines 21 – 26 – Consider revising the objective statement, “development” is confusing. I understand you may be hesitant to use the term “trends” as this is not a longitudinal study but rather a series of cross-sectional data, however, I think “changes” would be appropriate. Example: “The objective of this study is to describe the social deterrents of health associated with ED consumption, and changes in ED consumption frequency in 2017, 2018, and 2019.”

Author’s replay: Thank you for your comment. We have replaced development with changes throughout the manuscript.

Methods

4. Page 6, Lines 52 -59 – Consider including what the term centrality and index mean in the first sentence, Example: “According to Statistics Norway, centrality refers to an index of travel time to

workplaces and service functions from all populated basic units. Group 1 contains the most..." And then remove the "The calculation of the index..." sentence.

Author's replay: Thank you for your suggestion. We have now revised the sentences.

## Results

5. Page 11, Lines 39-42 – I am still struggling to understand the use of the "average of 23% per year". I see in your response that you made "rough yearly averages made across the three years" but I would appreciate a greater description of how this number was derived. From 2017 to 2018, a two-year period, there is a 24% increase, from 2017 to 2019, there is a 46% increase. How does this translate to an average of 23% per year? Given that these are three cross-sectional studies, with not necessarily the same participants per year, I'm not sure that it's appropriate to present the "rough yearly averages" without a detailed description as to how and why they were calculated acknowledging the study design and analysis type.

Author's replay: Thank you for your comment. We have removed the word on average to avoid confusion and are now only referring to the given values in the table.

6. Page 16, Lines 13-15 – What do you mean when you say there is a non-linear relation between high ED consumption and physical activity? From Table 2, I see that compared to people reporting no physical activity, any amount of physical activity, from seldom to >5 times a week, decreases ED consumption. I also see that as physical activity increases, the decrease in ED does not necessarily increase. Is this what you meant? Ordinal variables, like the physical activity one, cannot be approximated as continuous variables, for which you could discuss linearity. The spacing between the categories is not uniform. For example, the jump from "seldom" to "1-2 times a month" is ambiguous and likely not the same as the jump from 3-4 times a week to >5 times per week. As such, I would caution and urge you to refrain from discussing the relationship as "non-linear."

Author's replay: Thank you for highlighting this. We have now removed the word non-linear and are only referring to the relationship between high ED and physical activity.

## Discussion

7. Page 17, Line 6 – Again, would suggest you use "change" rather than "development" for clarity.

Author's replay: This has been revised.

8. Page 17, Lines 33 – Page 18, Line 18 – These paragraphs are a bit disjointed. I appreciate the revision of the language about ED marketing/advertising and masculinity, however, think this section could benefit from some rearranging. I would recommend moving the sentences discussing the Emond paper through the sentences discussing the Hammond et al. paper to the end of the second discussion paragraph (Page 17, Line 49.) Then go into the marketing of ED as boosters of an active lifestyle and the gender differences. I am still a bit confused about the introduction of the "masculinity" argument as a reason for greater ED consumption among boys. Is there evidence to suggest that "masculinity" is used in ED marketing? I would caution you from equating marketing of an active lifestyle and higher levels of performance to a masculine marketing tactic, which as of now is how it reads.

Author's replay: Thank you for your suggestion. We have now revised the paragraph and removed the word "masculinity".

9. Page 19, Line 37 – "development" to "change"

This has been revised.

10. Page 19, Lines 37-57 – This seems a bit repetitive, like the first paragraph of the discussion and to the concluding paragraph below. Moreover, you claim that the main finding is the increasing proportion of female high ED consumers, but this is the first time you highlight this finding as most important. I recommend moving that sentence (lines 42 – 49) to the first paragraph of the discussion (page 17). I recommend moving lines 49 -57 to the conclusion removing talk of the increase in ED sales in Norway, and that adolescents are most likely not the only group with increased consumption. The conclusion should discuss the implications and takeaways of your study – which is the importance of understanding gender differences in consumption, types of consumption, and longitudinal effects.

Author's reply: Thank you for your suggestion. We have now changed the ending of the discussion and the conclusion.

Table 1

11. How did you treat people with missing data? I see that "Missing" is listed under Grade, gender, physical activity, and screen time. In the Methods section, you addressed the missingness of the gender variable, but I don't see any discussion of the missingness of other data. The final sample in the multivariable models must have been smaller than the 278,891 adolescents mentioned at the beginning of the results if there was data missing across these covariates.

Author's reply: There was listwise deletion of missing data. The total sample size in the analysis has been specified in the footnote of table 2.

Reviewer: 4

Dr. Brandon Parkes, Imperial College London

Comments to the Author:

1. A clearer explanation of how the results presented in table 2 has been provided, but there is no mention of univariable Poisson regression results. I would at least like a sentence or two of explanation as to why univariable models were not run. Same goes for any sensitivity analyses that may have been considered.

Author's reply: Thank you for your comment. No substantial difference between crude and adjusted estimates were found, therefore only adjusted estimates are shown. This has now been clarified in the text.

2. Regarding my original comment about the definitions of 'ED consumption' and 'High ED consumption', the authors' response is 'This definition is based on previous studies, which makes our result more easily comparable to other findings'. Please cite the previous studies in the manuscript.

Author's reply: Thank you for your comment. We have added the references to the manuscript.

Reviewer: 5

Dr. Jonathan Yabes, University of Pittsburgh

Comments to the Author:

This manuscript has substantially improved and have addressed my concerns from the previous version.

Reviewer: 6  
Dr. Grace Chan, UConn Health

Comments to the Author:

The authors had addressed many of the previous comments. As a result the revised manuscript was substantially better than the original submission. In particular, it has a more appropriate title and better structured abstract. However, there are a few major and minor issues.

Major issues:

1. The nested data structure: students nested within schools and schools nested within municipalities or regional districts, was neither clearly stated at the methods section nor taken into consideration in data analyses. Also, only some of these were mentioned in the Discussion section as limitation. If the regression results with and without adjusting for municipality clustering were similar, why not report results from the more appropriated analysis adjusting for dependence between students?

Author's replay: Thank you for your question. We only have information about the municipalities, and are not able to identify the schools due to the possibility of backwards identification of the data. This is described in the statistics, however we have altered table 2 which now includes cluster adjusted (municipality) standard errors, confidence intervals, and p-values.

2. The location explanatory variable/factor was still incorrectly referred to as "residency" yet centrality was defined based on the location of schools NOT where students lived! Moreover, with many students attending the same school, but may live in different locations, the data analyses ignore this form of dependency and treated student-level and school-level as well as higher-level explanatory variables/factors as if there were all measured at student-level. Please reword phrases like "Most of the participants LIVED in municipalities with the centrality index two and three." as "Most of the participants ATTENDED SCHOOLS in municipalities with the centrality index two and three." in all similar places.

Author's replay: Thank you for your comment. We have rephrased this accordingly.

3. Please include details of type 3 (a.k.a. partial) tests at the explanatory variable/factor level of both modified Poisson regressions after adjusting for clustering. If all the categorical explanatory variables were included in the regression as nominal factor, it should NOT be interpreted as ordinal factor. In particular, please justify these two sentences: "There was a non-linear relation between high ED consumption and physically active. However, this was not observed for the any ED consumption group."

Author's replay: Thank you for your comment. Please see our response to comment number six by reviewer number three.

4. In the abstract, please clarify that 278,891 was NOT the number of students responded to the



survey, but was the number of students who had answered the energy drink question among the 297,102 partial or full respondents out of the 374,970 eligible participants.

Author's replay: Thank you for your comment. We have changed it to 297,102 which is the total number of students who answered the questionnaire.

Minor issues include the followings among others:

5. In Table 1, please provide more details description of the three lowest response options for leisure screen time to clearly justify the combining of them in subsequent analyses. Please correct either the Tables or the text regarding the leisure screen time response options: "less than two hours" vs. " $\leq 2$  hours daily" and "more than six hours" vs. " $\geq 6$  hours daily". In addition, please include detailed counts and percentages of the 5-level social-economic status explanatory variable by year as well as total since it is unclear if the "five equally sized groups" were based on the entire sample or within each year.

Author's replay: Thank you for your comments. We have changed the text to be the same as the table. In addition, the definition was based on entire sample and the percentages are given in the table 2.

6. Error bars (either 95% confidence interval or standard error) should be added to both Figures 2 and 3.

Author's replay: Thank you for your suggestion. We have written the 95% CI in the text, and we decided not to add these to the figures as they are very narrow and therefore difficult to see.

7. Please clarify how the response option "< once a week" was treated when comparing "ED consumer (ED  $\geq$  once a week)" with "never ED consumers".

Author's replay: Thank you for highlighting this. We have now included < once a week in the definition to clarify.

### VERSION 3 – REVIEW

<b>REVIEWER</b>	Parkes, Brandon Imperial College London, SAHSU
<b>REVIEW RETURNED</b>	02-Jul-2021

<b>GENERAL COMMENTS</b>	The authors have addressed the remaining concerns I had with the previous version.
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<b>REVIEWER</b>	Chan, Grace UConn Health, Psychiatry
<b>REVIEW RETURNED</b>	12-Jul-2021

<b>GENERAL COMMENTS</b>	The authors had addressed almost all of the previous comments. Thank you. There remain a few minor issues. <ul style="list-style-type: none"> <li>Given that all five adjusted odds ratios and their confidence intervals of "being High ED consumer" for the higher level of physical activity were less than 1 comparing to the lowest level, i.e., "Never" in Table 2, the first sentence in the third paragraph in</li> </ul>
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	<p>the Discussion section is INCORRECT. In particular, “being very physically active” was NOT positively associated with high ED consumption. Though the relationships between physical activity and “being ED consumer” was less clear.</p> <ul style="list-style-type: none"> <li>• Figures 2 and 3: Please adjust the vertical axes’ scales to enhance visual display of confidence intervals. For Figure 2, change from a range of 0 – 80 to a range of 40 – 70, and for Figure 3 from 0 – 14 to 3 – 12.</li> <li>• While it is understandable that due to low endorsement rates of the lowest three levels of leisure screen time, they were combined in regression analyses (Table 2). However, for descriptive statistics as presented in Table 1, please separate them out so that readers would be aware of what they were and hence better able to judge the scientific merit of combining them.</li> <li>• “Residency” vs school location: <ul style="list-style-type: none"> <li>o Page 9 just above Table 1: replace “fewer people are living in” to either “fewer students were attending schools in” or “fewer schools in” whichever reflects the reality in 2017 – 2019.</li> <li>o Likewise, Page 17 last paragraph: replace “rural living” to either “attending rural school” or “attending school located in rural areas”.</li> </ul> </li> </ul>
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### VERSION 3 – AUTHOR RESPONSE

Response to reviewers comments:

Reviewer Reports:

Reviewer: 4

Dr. Brandon Parkes, Imperial College London

Comments to the Author:

The authors have addressed the remaining concerns I had with the previous version.

Reviewer: 6

Dr. Grace Chan, UConn Health

Comments to the Author:

The authors had addressed almost all of the previous comments. Thank you. There remain a few minor issues.

1. Given that all five adjusted odds ratios and their confidence intervals of “being High ED consumer” for the higher level of physical activity were less than 1 comparing to the lowest level, i.e., “Never” in Table 2, the first sentence in the third paragraph in the Discussion section is INCORRECT. In particular, “being very physically active” was NOT positively associated with high ED consumption. Though the relationships between physical activity and “being ED consumer” was less clear.  
Author’s replay: Thank you for your comment. We have now rephrased the sentence.

2. Figures 2 and 3: Please adjust the vertical axes’ scales to enhance visual display of confidence intervals. For Figure 2, change from a range of 0 – 80 to a range of 40 – 70, and for Figure 3 from 0 – 14 to 3 – 12.  
Author’s replay: Thank you for your suggestion, we have altered the scale to your suggestion. Except we used 35 instead of 40 to start the range for figure 2 because it was nicer visually. However, this does not help the visual display of the confidence intervals, so we have still not added them to the figure. They confidence intervals are mentioned in the results.

3. While it is understandable that due to low endorsement rates of the lowest three levels of leisure screen time, they were combined in regression analyses (Table 2). However, for descriptive statistics as presented in Table 1, please separate them out so that readers would be aware of what they were and hence better able to judge the scientific merit of combining them.

Author's replay: Thank you for your comment. We have altered Table 1 to your suggestions.

"Residency" vs school location:

4. Page 9 just above Table 1: replace "fewer people are living in" to either "fewer students were attending schools in" or "fewer schools in" whichever reflects the reality in 2017 – 2019.

Author's replay: Thank you for noticing this. We changed the sentence to: ..., as there are fewer schools in these areas.

5. Likewise, Page 17 last paragraph: replace "rural living" to either "attending rural school" or "attending school located in rural areas".

Author's replay: Thank you for highlighting this. We changed the sentence to your second suggestion.

#### VERSION 4 – REVIEW

<b>REVIEWER</b>	Chan, Grace UConn Health, Psychiatry
<b>REVIEW RETURNED</b>	08-Aug-2021
<b>GENERAL COMMENTS</b>	Thank you for addressing all previous comments.