PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Suicidal ideation in relation to disordered eating, body size and weight perception: a cross-sectional study of a Norwegian adolescent population - The HUNT Study
AUTHORS	Saeedzadeh Sardahaee, Farzaneh; Holmen, Turid; Micali, Nadia; Sund, Erik; Bjerkeset, Ottar; Kvaløy, Kirsti

VERSION 1 - REVIEW

REVIEWER	April Smith Miami University
REVIEW RETURNED	26-Mar-2019

GENERAL COMMENTS	The manuscript entitled "Understanding suicidal ideation in relation to disordered eating, body size and weight perception: a retrospective cohort of adolescents" used a large sample of female and male adolescents to investigate how eating behaviors and weight perceptions may relate to suicidal ideation. The manuscript explores an important topic in an understudied group (non-clinical adolescents) and I think the results will make a meaningful contribution to the literature. However, there are several notable limitations, which are reviewed below. Strength and limitations of the study • The second bullet point in this section could be reworded, especially since there are more than two types of gender. For example, the authors could say, “Results are of general relevance as they are observed in a large, population-based adolescent dataset that included males and females.” Introduction • Overall, the Introduction could be significantly strengthened by including more recent work on risk factors for suicide and work discussing the relationship between disordered eating and suicide. Franklin, J. C., Ribeiro, J. D., Fox, K. R., Bentley, K. H., Kleiman, E. M., Huang, X., ... & Nock, M. K. (2017). Risk factors for suicidal thoughts and behaviors: a meta-analysis of 50 years of research. Psychological Bulletin, 143(2), 187. Ribeiro, J. D., Franklin, J. C., Fox, K. R., Bentley, K. H., Kleiman, E. M., Chang, B. P., & Nock, M. K. (2016). Self-injurious thoughts and behaviors as risk factors for future suicide ideation, attempts, and death: a meta-analysis of longitudinal studies. Psychological medicine, 46(2), 225-236. Smith, A. R., Ortiz, S. N., Forrest, L. N., Velkoff, E. A., & Dodd, D. R. (2018). Which Comes First? An Examination of Associations and Shared Risk Factors for Eating Disorders and Suicidality. Current psychiatry reports, 20(9), 77. • Further, although the authors state they have not made any

	casual conclusions, the way the paper is set up and the literature reviewed implicitly assumes DE precedes SI, however, some research has called this into question. • Additionally, the flow of both the Intro and Discussion was disjointed and would benefit from better transitions and organization. • On line 23, page 3, it may be unclear to the reader that 'Disorders of feeding and eating' is pertaining to the categorization of eating disorders in the DSM so this should be clarified. Materials and methods • Was BMI considered in creating the "poor appetite/undereating" and "uncontrolled appetite/overeating" groups? It seems these groups could be more rigorously defined if they also included BMI. For instance, only those with a BMI of less than 18.5 might be considered "poor appetite/undereating". • For Body Size Perception, were these validated against actual BMI? For instance, one could be normal weight but feel very fat whereas another person could be more accurate in their perception (i.e., have an overweight BMI and perceive self as overweight). Might there be meaningful differences between these groups? Results • Given that SI was zero inflated were Poisson or negative binomial models considered? • Line 12, page 11 – please clarify that the comment about higher odds in boys as compared to girls was when all relevant variables were adjusted for (and not necessary the case across all adjustment categories). Discussion • "intricate interplay" appears to be an overstatement given that predictors were not considered together or in an interactive fashion. • This sentence needs clarification as it is not true for all adolescent boys, "Adolescent boys showed a vulnerability for having SI and require special attention in risk assessment and treatment approaches." • One of the main findings, that adolescent boys with DE had elevated SI was glossed over and not discussed. Authors are encouraged to dig into the literature to better situate and explain this finding. Murray, S. B., Nagata, J. M., Griffiths, S., Calzo, J. P., Brown, T. A., Mitchison, D., ... & Mond, J. M. (2017). The enigma of male eating disorders: A critical review and synthesis. Clinical Psychology Review, 57, 1-11. • Overall, as noted in the Intro, the Discussion is underdeveloped and there are several vague statements, e.g., "Future research in clinical populations seems warranted." What kind of research? What kinds of populations? What designs> General comments • Authors should proof read paper as several writing errors were found throughout (e.g., 'takin' on line 11, page 5 should be 'taking'; the period in line 19, page 15 should be changed to a comma) • Instead of stating that participants 'have' disordered eating (e.g., line 15, page 5) a better description would be stating participants engage in disordered eating.

REVIEWER	Enrique Garcia Bengoechea University of Limerick, Ireland
REVIEW RETURNED	02-Apr-2019

GENERAL COMMENTS	General Comments This study clearly addresses an important topic and in my opinion the findings offer valuable insights and implications in terms of public and mental health, for adolescents in general and adolescent boys in particular. I've missed some clarity regarding the definition and use of key terms in the Introduction and the specific aims of this study. I also have some questions for the authors regarding the use of separate logistic regression models for each exposure factor and the implication for interpretation of findings. Specific Comments Strengths and limitations of this study P. 3, L. 5. In line with the limitations stated at the end of the manuscript, consider adding the study's cross-sectional design to the single item measure alluded to here. Introduction P. 3, L. 19. The acronym 'EDs' is used for the first time, but it should have been used before the first time the term 'eating disorder' appears in the text (P. 1, L.17). P. 3, last paragraph and P. 4, first paragraph: Terms such as 'eating disorders (EDs)', 'disorders of feeding and eating', and 'disordered eating (DE)' are used, but it is not fully clear to me what these term refer to. More conceptual/terminological clarity is needed here, particularly in terms of the differences between 'EDs' and 'DE'. P.4, L. 5: Related to my previous point, the acronym 'DEs' is used for the first time. Since the term is not fully spelled out, should I assume it is used to mean 'disordered eating(s)'? Again, my point here is about conceptual and terminological clarity and precision in the Introduction, which I think could and should be improved. Incidentally, through the paper the acronyms 'DE' and 'DEs' seem to be used interchangeably, which I have found somewhat confusing. P.4, last paragraph before Materials and Methods section: I can only see one stated purpose of the study ("first examine the prevalence of DEs and SI in a sample of more than 7,000 Norwegian adolescents…") and then a number of hypotheses about associations between variables that is related, as it appears, to a purpose that has not been stated explicitly. Please provide more clarity about the specific aims of this study before stating any hypotheses. In addition, please provide references about the "evidence available from adults" on which the stated hypotheses are based. Materials and Methods P. 4, L. 19: The setting where YH1 and YH1 were conducted is stated. Why this is not the case for YH2? P. 4, L. 20: "Data was collected through self-reported questionnaires". All data? Just after this, there is reference to "clinical measurements". Please be more specific about what kind of data were collected through self-reported questionnaire. P. 5, L. 2: "participants older than 15 years of age". Is this correct or do you mean "participants 15 years of age and older"? It is not the same, in my view. P. 5, L. 10-11: Given its key role in the study, please provide information on the origin and, if available, validity and reliability of the single-item suicidal ideation item used in the study. P. 5, L. 15-19: I may be missing something, but, based on the information provided, it is not clear to me how individuals with

	disordered eating were identified. P.6, L. 2-3: “about the same as others” and “like others” seems to be used interchangeably here and though the paper. For clarity purposes, please be consistent with the use of terminology. P. 7, L. 18-20. Associations between exposure factors and suicidal ideation were examined in separate models for each exposure factors. So the models are “multivariate” in a way (i.e. adjusted by a number of possible confounders). However, in the absence of mutual adjustment among exposure factors, how can we have an estimation of the “unique” effect of each of these factors on suicidal ideation? Please elaborate on the implications of the analytical strategy used for the interpretation of the results. P. 7, L. 19: For consistency purposes, please use the acronym “DE” instead of “disordered eating” Results P. 10, L. 10: It is reported that “Majority of participants (67.1%) were comfortable with their weight and did not engage in weight reduction measures (Table 1)”. Again, I may be missing something, but was data on whether participants actually engaged in weight reduction actions available? P. 11, L. 1-2: Based on the information about the SCL-5 provided in the Methods section, I believe “high” levels of mental distress would be more appropriate than “higher” levels of mental distress. P. 12, L. 4-5: Please state how did you test for moderator effects of sex of participants. Discussion P. 15, L. 3-5: Coming back to my previous point on the analytical strategy used, considering that the effects of disordered eating, body size and weight concerns on suicidal ideation have been examined in separate models, is it accurate to refer to the “intricate interplay [of these factors] with one another”? P.15, L. 18-19. Check sentence (it seems a comma would work better than a period). In general: In the results section there is mention to a decrease in prevalence of uncontrolled appetite/overeating from YH1 to YH3 and also to changes in the distribution of weight status across survey administrations. Considering that both YH1 and YH3 surveys were conducted a sizeable number of years ago, I have missed in the Discussion some mention of the potential effect of the time elapsed in terms of prevalence of the variables considered in the study and the implications of this for the interpretation and possible application of the findings.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: April Smith

Institution and Country: Miami University

Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

The manuscript entitled "Understanding suicidal ideation in relation to disordered eating, body size and weight perception: a retrospective cohort of adolescents" used a large sample of female and male adolescents to investigate how eating behaviors and weight perceptions may relate to suicidal ideation. The manuscript explores an important topic in an understudied group (non-clinical adolescents) and I think the results will make a meaningful contribution to the literature. However, there are several notable limitations, which are reviewed below.

Authors' response:

Thank you very much for your constructive comments, which authors have used to improve the manuscript. We have addressed each comment separately and if needed, edited the manuscript accordingly. We are very grateful to the reviewer for providing us with insightful knowledge about the subject and wish to thank her especially for taking time to suggest new and useful references.

Strength and limitations of the study

• The second bullet point in this section could be reworded, especially since there are more than two types of gender. For example, the authors could say, "Results are of general relevance as they are observed in a large, population-based adolescent dataset that included males and females."

Authors' response:

Thank you very much for your comment. We have changed the text to: "Our results are of general relevance since our observations were made in a large, population-based adolescent dataset that included both males and females".

Introduction

• Overall, the Introduction could be significantly strengthened by including more recent work on risk factors for suicide and work discussing the relationship between disordered eating and suicide.

Franklin, J. C., Ribeiro, J. D., Fox, K. R., Bentley, K. H., Kleiman, E. M., Huang, X., ... & Nock, M. K. (2017). Risk factors for suicidal thoughts and behaviors: a meta-analysis of 50 years of research. Psychological Bulletin, 143(2), 187.

Ribeiro, J. D., Franklin, J. C., Fox, K. R., Bentley, K. H., Kleiman, E. M., Chang, B. P., & Nock, M. K. (2016). Self-injurious thoughts and behaviors as risk factors for future suicide ideation, attempts, and death: a meta-analysis of longitudinal studies. Psychological medicine, 46(2), 225-236.

Smith, A. R., Ortiz, S. N., Forrest, L. N., Velkoff, E. A., & Dodd, D. R. (2018). Which Comes First? An Examination of Associations and Shared Risk Factors for Eating Disorders and Suicidality. Current psychiatry reports, 20(9), 77.

Authors' response:

Authors are grateful for having received very useful references that have been incorporated in the introduction section. Please see highlighted text in the second paragraph under introduction section.

• Further, although the authors state they have not made any casual conclusions, the way the paper is set up and the literature reviewed implicitly assumes DE precedes SI, however, some research has called this into question.

Thank you again for raising this important point. We have revised the manuscript accordingly. Please see under discussion and conclusion.

• Additionally, the flow of both the Intro and Discussion was disjointed and would benefit from better transitions and organization.

Authors' response:

Thank you for your comment. We have made several changes to Introduction and Conclusion sections to reflect structural changes in the presentation of subject matter as well as inclusion of new references. Further changes are made specifically to address points raised by the reviewers (see below) and are all in highlighted text.

• On line 23, page 3, it may be unclear to the reader that 'Disorders of feeding and eating' is pertaining to the categorization of eating disorders in the DSM so this should be clarified.

Authors' response:

Thank you very much for your comment. We have edited the text accordingly. We have also added a new reference for DSM-5.

Materials and methods

• Was BMI considered in creating the "poor appetite/undereating" and "uncontrolled appetite/overeating" groups? It seems these groups could be more rigorously defined if they also included BMI. For instance, only those with a BMI of less than 18.5 might be considered "poor appetite/undereating".

Thank you very much for this comment. Authors have looked at this issue closely and would like to make a few points that may help clarify the issue.

Authors would like to emphasize that EAT-A and EAT-B groups were defined solely based on presence or absence of disordered eating (DE) traits as investigated by the EAT-7 question items, and not based on the presence or absence of a cluster of wider related traits, psychological (mental distress), biological (BMI) or else (SES). EAT-A ("poor appetite/undereating") does not refer to a single trait (change in appetite) but rather to a cluster of DE traits. Detailed explanation on how DE is assessed and grouped can be found in the Appendix.

Furthermore, authors thought that by excluding participants with "poor appetite/undereating group" (EAT-A) but BMI > 18.5 one would lose the chance of detecting independent associations between EAT-A and SI (that are not confounded or mediated by other measurements such as BMI). As outlined in the last paragraph under "Discussion" section in the manuscript, due to desired masculine body shape, drive for thinness does not seem to explain emergence of DE traits amongst males as well as it does amongst females, hence making BMI less indicative of presence or severity of DEs (EAT-A and EAT-B).

However, as the reviewer rightly points out, one could consider performing further analyses on groups defined by the presence or the absence of a few traits clustered together, for example as suggested here in participants with EAT-A and lower BMI. In fact, authors had initially considered investigating many other paired sets of exposure variables but given small number of participants in each cluster and lack of statistical power, drawing meaningful conclusions seemed rather immature, if not impossible. Authors were also aware of issues around multiple testing and inflated type I error, again given rather smaller participants in each cluster. For example, out of 338 participants with "poor appetite/undereating", only 5 persons had BMI less than 18.5. Majority of participants, with or without DEs (EAT-A or EAT-B) were in fact normal weight, making it impossible to run conclusive or indicative analyses. Authors have therefore decided to depart from the original idea of clustering EAT-A or EAT-B groups with BMI grouping, but have suggested that clustering would help narrowing down research questions and is warranted in future longitudinal studies on larger populations, from community or clinical settings. For instance, authors have suggested conducting a follow-up study on a larger dataset from the HUNT Study including adolescents from the very recent survey, the HUNT4 Survey (data collection finished in February 2019), as well as the HUNT1 and 3, may reach more conclusive results.

The following paragraph has been added to Discussion section:

One interesting line of enquiry is to look at various DE traits clustered in smaller groups based on participants' BMI or body size perception, which was not possible to perform in the current study given small number of participants in each cluster. Conducting a follow-up study on a larger dataset from the HUNT Study including adolescents from the most recent data collection, the HUNT4 Survey (2017-19), may reach a higher statistical power and hence more conclusive results.

• For Body Size Perception, were these validated against actual BMI? For instance, one could be normal weight but feel very fat whereas another person could be more accurate in their perception (i.e., have an overweight BMI and perceive self as overweight). Might there be meaningful differences between these groups?

Authors' response:

Thank you for your comment. There was in fact some discrepancy between participants' actual BMI and body size perception (see Supplementary table 2) where individuals within the obese and overweight categories would inaccurately perceive their body size by underestimation (consider themselves as "like others", "quite thin" or "very thin"), individuals within the normal weight category could inaccurately perceive their body size by underestimation ("quite thin" or "very thin") or overestimation ("quite fat" or "very fat"), and underweight individuals would inaccurately perceive their body size by overestimation ("like others", "quite fat" or "very fat"). Authors examined whether observed increase in the OR for SI in individuals with BSP that deviated from the norm (not "about the same as others") remained robust to adjustment for BSP accuracy. This would then include both those that over- and under-estimate their body size independent of their actual BMI. We decided not to discriminate between these various groups as we presumed both to potentially impact suicidal ideation.

BSP accuracy rates showed interesting gender differences in our population. In both genders, a general trend of normalization of own's BSP towards population norms was observed in both genders. 64.26% of male and 50.5% of female participants who perceived their body size as "very fat" or "quite fat" were in fact normal or underweight (BSP inaccuracy due to overestimation of BSP, data not shown). A reverse gender pattern was observed in 15.21% of male and 26.59% of female participants who perceived their body size as "quite thin" or "very thin" in fact had normal BMI and above (BSP inaccuracy due to underestimation of BSP). One can argue that BSP inaccuracy might in itself

associate with high mental distress due to cognitive dissonance caused by discrepancy between “one’s perception of self” and “others’ perception of one’s self”. The direction of BSP inaccuracy, as under- or overestimation of BSP, in conjunction with gender specific societal body ideals might explain some gender differences in association analyses between BSP and SI as reported under result section (see also Table 3 and Table 4). One can also argue that if the direction of BSP inaccuracy qualifies individuals to a more favorable position in relation to what society regards as normal or ideal. For example, BSP inaccuracy by underestimation might cause less mental distress in female individuals who are obese or overweight but perceive their body size as normal or underweight, in effect rebranding own’s body size perception in a more attractive fashion (following agreed societal norms).

The association analysis between BSP and SI was therefore repeated, in multivariate logistic regression model and in gender stratified groups. Adjustments were made for age, mental distress and SES as well as for BSP accuracy. Authors found a general trend for increase in the odds for SI given subjective perception of body size was inaccurate, in both genders. However, the results were statistically significant only in girls (OR: 2.88, 95% CI 1.74 – 4.76, p-value < 0.001) who had inaccurately perceived their body size as “very fat” (data is not shown). In both genders and across all BMI groups, most participants (66.8% in boys and 68.5% in girls) had inaccurate perception of their own body size.

Due to lack of statistical power conclusions should be drawn with caution from our findings, but authors have examined possible associations between BSP, BSP inaccuracy, DEs and intention to lose weight in a concurrent study, the results of which require lengthier exploration in a separate scientific paper, which is currently in pre-publication process. Authors have therefore emphasized in the manuscript. Please see Line21 , page 18 to Line 10, page 19..

Some discrepancy between participants’ actual BMI and body size perception was observed (see Supplementary table 2) where individuals inaccurately perceived their own body size by means of under- or overestimation. The direction of BSP inaccuracy, as under- or overestimation of BSP, in conjunction with gender specific societal body ideals might have some real implications in the degree with which BSP inaccuracy might further associate with mental distress and SI. But one can argue that inaccurate BSP might not necessarily lead to higher mental distress, for instance if the direction of BSP inaccuracy qualifies individuals to a more favorable position in relation to what society regards as normal or ideal. BSP inaccuracy by underestimation might cause less mental distress in female individuals who are obese or overweight but perceive their body size as normal or underweight, in effect rebranding own’s body size perception in a more approved fashion (following agreed societal norms). Possible associations between BSP, BSP accuracy, BMI and intention to lose weight require full exploration that is beyond the scope of current study but is being investigated in a parallel study conducted (by the authors) on determinants of dieting in a Norwegian community sample (The HUNT Study.

Results

• Given that SI was zero inflated were Poisson or negative binomial models considered?

Authors’ response:

Thanks very much for your comment. Authors did not consider running Poisson or a negative binomial regression analyses given that our dependent variable (SI) dichotomous variable consisting of either 0 or 1.

• Line 12, page 11 – please clarify that the comment about higher odds in boys as compared to girls was when all relevant variables were adjusted for (and not necessary the case across all adjustment categories).

Authors' response:

Thank you for bringing this into our attention. We have amended the text accordingly. Please see highlighted text in Line 2, page 13.

Discussion

• "intricate interplay" appears to be an overstatement given that predictors were not considered together or in an interactive fashion.

Authors' response:

Thanks for your comment. We have changed the text slightly and avoided using terms that might suggest an interaction analysis having taken place.

• This sentence needs clarification as it is not true for all adolescent boys, "Adolescent boys showed a vulnerability for having SI and require special attention in risk assessment and treatment approaches."

Authors' response:

Thanks for bringing this to our attention. We have edited the text accordingly.

• One of the main findings, that adolescent boys with DE had elevated SI was glossed over and not discussed. Authors are encouraged to dig into the literature to better situate and explain this finding.

Murray, S. B., Nagata, J. M., Griffiths, S., Calzo, J. P., Brown, T. A., Mitchison, D., ... & Mond, J. M. (2017). The enigma of male eating disorders: A critical review and synthesis. Clinical Psychology Review, 57, 1-11.

Authors' response:

Thank you for your comment. Authors revisited the literature and made several references to what implications they may have in interpreting our results or in providing an indication for future research. Please see highlighted text in last paragraph, page 17 and first paragraph, page 18 where authors have reflected on this issue.

• Overall, as noted in the Intro, the Discussion is underdeveloped and there are several vague statements, e.g., "Future research in clinical populations seems warranted." What kind of research? What kinds of populations? What designs>

Authors' response:

Thank you. We have changed the text to reflect the comments made by the reviewer. Please see the highlighted text, closing paragraph under "Strength and limitation" section towards the end of manuscript (Line7-13, page 20) as well as the highlighted text in "Conclusion" section.

General comments

• Authors should proof read paper as several writing errors were found throughout (e.g., 'takin' on line 11, page 5 should be 'taking'; the period in line 19, page 15 should be changed to a comma)

Authors' response:

Thank you very much for bringing this to our attention. We have edited the text.

• Instead of stating that participants 'have' disordered eating (e.g., line 15, page 5) a better description would be stating participants engage in disordered eating.

We are very thankful for your useful comment and agree on the importance of how a health condition can be variedly defined in other contexts such as social psychiatry and humanistic studies. We were however fearful that by using the verb, "engage" we would indirectly imply a degree of intentionality in formation of DE related traits. We contemplated on using alternative words and failed to come up with a word that is more widely used and accepted than the verb, "have" in describing existence of certain symptoms in an individual in a medical context.

Reviewer: 2

Reviewer Name: Enrique Garcia Bengoechea

Institution and Country: University of Limerick, Ireland

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

Please leave your comments for the authors below

General Comments

This study clearly addresses an important topic and in my opinion the findings offer valuable insights and implications in terms of public and mental health, for adolescents in general and adolescent boys in particular. I've missed some clarity regarding the definition and use of key terms in the Introduction and the specific aims of this study. I also have some questions for the authors regarding the use of separate logistic regression models for each exposure factor and the implication for interpretation of findings.

Authors' response:

We are very grateful for your insightful comments which authors have reflected on and where needed implemented in interpretation as well as representation of study findings. In reply to the specific comment on study aims, we have now amended the text to highlight specific aims of the study. Please see our closing paragraph under introduction (highlighted in yellow, with added references). Questions on logistic regression and its interpretations are answered below. Thank you.

Specific Comments

Strengths and limitations of this study

P. 3, L. 5. In line with the limitations stated at the end of the manuscript, consider adding the study's cross-sectional design to the single item measure alluded to here.

Authors' response:

Thank you for your comment. Authors have changed the text to include the point. Please see highlighted text in the closing paragraph under Strength and limitations stated at the end of manuscript.

Introduction

P. 3, L. 19. The acronym 'EDs' is used for the first time, but it should have been used before the first time the term 'eating disorder' appears in the text (P. 1, L.17).

Authors' response:

We have changed the text accordingly. Please see Line 19, page 2 under "Keywords". Thanks for your comment.

P. 3, last paragraph and P. 4, first paragraph: Terms such as 'eating disorders (EDs)', 'disorders of feeding and eating', and 'disordered eating (DE)' are used, but it is not fully clear to me what these terms refer to. More conceptual/terminological clarity is needed here, particularly in terms of the differences between 'EDs' and 'DE'.

Please see below next point for authors' response.

P.4, L. 5: Related to my previous point, the acronym 'DEs' is used for the first time. Since the term is not fully spelled out, should I assume it is used to mean 'disordered eating(s)'? Again, my point here is about conceptual and terminological clarity and precision in the Introduction, which I think could and should be improved. Incidentally, through the paper the acronyms 'DE' and 'DEs' seem to be used interchangeably, which I have found somewhat confusing.

Authors' response:

Thank you for raising this important point. We have revised the text to explain more about the terms EDs, DEs and 'disorders of feeding and eating' (Line: 16, page:4 to Line 3, page:5). We hope the differences are now sufficiently clarified. We would like to draw attention to the Appendix where due explanation is given with regards to question items from EAT-7, grouping of participants with DE to EAT-A and EAT-B, as well as information on psychometric properties of EAT-7.

Authors would like to also emphasize that wherever previous research results were drawn on clinical data on patients with clinical diagnoses, using EDs seemed more reflective of the study population than using DEs. Since this might be confusing to the readers, authors have revised the manuscript to further clarify that EDs and DEs are not used interchangeably. Please see highlighted text (Line:1-3, page: 7). Thank you again for bringing up this important point.

P.4, last paragraph before Materials and Methods section: I can only see one stated purpose of the study ("first examine the prevalence of DEs and SI in a sample of more than 7,000 Norwegian adolescents…") and then a number of hypotheses about associations between variables that is related, as it appears, to a purpose that has not been stated explicitly. Please provide more clarity about the specific aims of this study before stating any hypotheses. In addition, please provide references about the "evidence available from adults" on which the stated hypotheses are based.

Authors' response:

Authors are very thankful for the useful comments. Authors have made several changes to Introduction section to reflect on the issues and hope that manuscript is now sufficiently clear on study aims and hypotheses. Please see closing paragraph under the introduction section.

Materials and Methods

P. 4, L. 19: The setting where YH1 and YH1 were conducted is stated. Why this is not the case for YH2?

Authors' response:

Thank you so much for your valuable comment. Since our analyses were done on data derived only from the YH1 and YH3 populations, we did not provide information on the settings for YH2 which was in fact a follow up on YH1 survey. Taking on your view, we have amended the text very slightly to avoid potential confusion. Please see opening paragraph under "Study design and population", "Materials and methods".

P. 4, L. 20: "Data was collected through self-reported questionnaires". All data? Just after this, there is reference to "clinical measurements". Please be more specific about what kind of data were collected through self-reported questionnaire. (Please see under next point for authors' response)

P. 5, L. 2: "participants older than 15 years of age". Is this correct or do you mean "participants 15 years of age and older"? It is not the same, in my view.

Authors' response:

We are so grateful to the reviewer for having brought up these points. We have edited the text accordingly. Please see Line 21-22, page 5.

P. 5, L. 10-11: Given its key role in the study, please provide information on the origin and, if available, validity and reliability of the single-item suicidal ideation item used in the study.

Authors' response:

Thank you very much for your valuable comment. We were aware of problems arising from using a single-item question on SI and have mentioned this as a limitation to our study. However, as the reviewer has diligently pointed out, providing more information on the validity and reliability of single-item SI question as well as possible Type I and II errors can add value to the paper.

As been suggested by Millner and colleagues (10.1371/journal.pone.0141606), “larger effect sizes and larger samples will reduce false conclusions by preventing or reducing misclassification due to a decrease in the chance for statistical errors”. We have therefore referred to related literature and argued that Type I and II errors are less likely to have influenced our results in a meaningful way, firstly due to the relatively ‘large effect size’ observed that reduces the chances for a Type II error and secondly due to a ‘large sample size’ that likely have reduced the possibility of a Type I error. We added an explanatory paragraph to Statistics section (highlighted text).

We have also emphasized (under the discussion) that to reduce possible miscalculations or misinterpretations of statistical analyses, providing a wider range of response alternatives (to examine SI) should be encouraged in future research. Interestingly, use of single-item questions in future nevertheless is encouraged since it provides an opportunity to validate and compare newer research finding with the older reports. To overcome miscalculation or misinterpretations errors, follow up questions should be used to compliment information derived from a single-item question on SI.

Once again, we are so very grateful to the reviewer for having raised this point and hope that our response has sufficiently clarified this issue for readers.

P. 5, L. 15-19: I may be missing something, but, based on the information provided, it is not clear to me how individuals with disordered eating were identified.

Authors response:

Thanks for your comment. Identification process (for individuals with DE) and subsequent grouping in EAT-A and EAT-B is described in detail in the Appendix.

P.6, L. 2-3: “about the same as others” and “like others” seems to be used interchangeably here and though the paper. For clarity purposes, please be consistent with the use of terminology.

Authors’ response:

Thank you for your comment. We have amended the text and used “about the same as others” that matches the exact wording in the HUNT questionnaire.

P. 7, L. 18-20. Associations between exposure factors and suicidal ideation were examined in separate models for each exposure factors. So the models are “multivariate” in a way (i.e. adjusted by a number of possible confounders). However, in the absence of mutual adjustment among exposure factors, how can we have an estimation of the “unique” effect of each of these factors on suicidal ideation? Please elaborate on the implications of the analytical strategy used for the interpretation of the results.

Authors’ response:

Thank you very much for raising this point. Authors agree with the reviewer that in the absence of mutual adjustments amongst exposure variables, the unique effect of each exposure variable remains

unclear without further investigations. Authors have included this in revising the manuscript ("Discussion" and "Conclusion" sections) where study limitations or direction of future research was discussed. Authors have also examined collinearity between exposure variables and added the following to the manuscript:

Collinearity between exposure variable was examined in linear regression. The authors found no evidence of multicollinearity as assessed by tolerance values greater than 0.1. Inspection of correlation coefficient showed no evidence in support of correlations (all values were under 0.7).

Please see Line 11-14, page 9.

Please also see highlighted text on page 5 of this document for authors' response to the comments made by the first reviewer on exploring patterns of BSP in various BMI groups and whether they might affect interpretation of our findings in any meaningful way.

P. 7, L. 19: For consistency purposes, please use the acronym "DE" instead of "disordered eating"

Authors' response:

Thanks for your comment. We have edited the manuscript accordingly and replaced "disordered eating" with "DE", wherever appropriate. Authors have not changed the term used in the abstract, tables or section headings since it is easier for a reader to read and understand these sections without having to read the manuscript itself.

Results

P. 10, L. 10: It is reported that "Majority of participants (67.1%) were comfortable with their weight and did not engage in weight reduction measures (Table 1)". Again, I may be missing something, but was data on whether participants actually engaged in weight reduction actions available?

Authors' response:

Thanks for your comment. "Intention to lose weight" was assessed by participants' answer to the following question, "Are you trying to lose weight?" to which participants could answer: 1) "No, I am comfortable with my weight", 2) "No, but I need to lose weight" or 3) "Yes". We thought it's logical to conclude that participants who said they were trying to lose weight, actually did something to change their weight (by engaging in any means they thought appropriate, be it dieting, exercise or else). However, authors appreciate that "engaging" might be perceived differently than in the manner authors intended, hence removing "actually engaged" from the manuscript. Please see revised text in the manuscript.

P. 11, L. 1-2: Based on the information about the SCL-5 provided in the Methods section, I believe "high" levels of mental distress would be more appropriate than "higher" levels of mental distress.

Authors' response:

Thank you for your comment. We have edited the text accordingly.

P. 12, L. 4-5: Please state how did you test for moderator effects of sex of participants.

Authors' response:

Thank you for your comment. Authors have tested for moderator effect of sex in a two-way ANCOVA model with interaction terms. We have edited the text, please see the highlighted text for changes made to the manuscript.

Discussion

P. 15, L. 3-5: Coming back to my previous point on the analytical strategy used, considering that the effects of disordered eating, body size and weight concerns on suicidal ideation have been examined in separate models, is it accurate to refer to the "intricate interplay [of these factors] with one another"?

Authors' response:

Thanks again for pointing out that the wording needs change, here. We have edited the text as advised.

P.15, L. 18-19. Check sentence (it seems a comma would work better than a period).

Authors' response:

Thank you. We have corrected the text accordingly.

In general: In the results section there is mention to a decrease in prevalence of uncontrolled appetite/overeating from YH1 to YH3 and also to changes in the distribution of weight status across survey administrations. Considering that both YH1 and YH3 surveys were conducted a sizeable number of years ago, I have missed in the Discussion some mention of the potential effect of the time elapsed in terms of prevalence of the variables considered in the study and the implications of this for the interpretation and possible application of the findings.

Authors' response:

Thank you very much for making this very important point. We have investigated whether there was a meaningful difference between the two study cohorts (YH1 and YH3). This is summarized in the manuscript, last paragraph under Statistical analysis section. It reads as:

Independent-samples t-tests and Chi-square tests of independence (both significant at the 0.05 level) were performed to determine whether participants from YH1 and YH3 differed in ways that would affect the validity of our results derived from pooled data. Participants were compared on all exposure variables. Wherever no statistically significant differences were observed, the association analyses were done on pooled data from YH1 and YH3 cohorts. Otherwise, association analyses were done separately in YH1 and YH3 as well as on the pooled data. Comparing the results from these separate analyses detected no meaningful difference. Results are therefore reported for pooled data only.

VERSION 2 – REVIEW

REVIEWER	April Smith Miami University, U.S.A.
REVIEW RETURNED	31-May-2019

GENERAL COMMENTS	Thank you for allowing me to review this revision. Overall the authors have done a nice job with their revisions. I just had a few more minor edits/suggestions. Abstract Specially --> especially Introduction This sentence is a bit unclear: “authors examined 1- whether DE traits 8 such as DEs or weigh and shape concerns are associated with SI amongst adolescents”. What are DEs and how to they differ from DE traits? Weigh should be “weight.” Perhaps consider re-wording as follows, “authors first examined whether DE traits, such as weight and shape concerns, are associated with SI amongst adolescents…” Analyses I had a hard time following this sentence, “Inspection of correlation coefficient showed no evidence support of correlations (all values were under 0.7).” -The authors now report a moderation analysis (that was not significant), but it was unclear to me why they only examined the BSP item “very fat” Conclusion -Again, I’m not sure what the abbreviation DEs should mean, “disordered eatings”? As such, I think you could revise this sentence, “In line with previous reports [27], we found that DEs were far more prevalent than clinically diagnosed EDs” to “In line with previous reports [27], we found that DE was far more prevalent than clinically diagnosed EDs”. Please change throughout the discussion. -These sentences get a bit long and hard to follow. I’d suggest breaking up and reworking for greater clariy: “We could not find evidence in support of BMI having a similar explanatory role for higher OR for SI amongst our male participants with DE traits, possibly partly due to differences in what these young individuals set themselves upon to achieve by dietary restraints or other means to lose weight, with female population more concerned about their weight[52] whilst male population shows more concern for looking masculine and lean[53], which in effect renders BMI less indicative of presence or severity of DEs amongst males. Our findings were in line with previous reports of existing but somewhat overlooked higher vulnerability to mental distress amongst adult male sufferers of DEs [54] that, as previously outlined in great details is reflective of higher prevalence of a wider array of comorbid psychiatric disorders [17] that can potentiate an existing association between DE traits and SI.” -“Furthermore, male populations seem to be more reluctant in seeking help for their symptoms [17] which can further potentiate existing associations between DE traits and SI by the factor of severity of unidentified and hence unaddressed DE traits or other comorbid psychiatric disorders.” Perhaps reword to, “Furthermore, male populations are more reluctant to seek help for their symptoms

	[17] which can further exacerbate the associations between DE and SI as these individuals will not be receiving help for either condition."

REVIEWER	Enrique Garcia Bengoechea University of Limerick, Ireland
REVIEW RETURNED	27-May-2019

GENERAL COMMENTS	The authors have addressed appropriately the comments and suggestions I made. Thank you. Just two minor points: In the revised text (Line 11-14, page 9), I suggest changing "no evidence in support of correlations…" to "no evidence in support of high correlations". Finally, my last point was actually about the extent to which trends noted from YH1 to YH3 in the present study are applicable today given, in particular, that more than 20 years have passed since YH1. In other words, my question was about the extent to which the data on prevalence of a number of factors in this study reflect the current situation. If relevant, please address concisely this circumstance in the Discussion section.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: April Smith

Institution and Country: Miami University, U.S.A.

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

Please leave your comments for the authors below

Thank you for allowing me to review this revision. Overall the authors have done a nice job with their revisions. I just had a few more minor edits/suggestions.

Authors' response: Thank you very much for your comments. Please see below authors' response to each point.

Abstract

Specially --> especially

Authors' response: Thank you. We have edited the text.

Introduction

This sentence is a bit unclear: “authors examined 1- whether DE traits 8 such as DEs or weigh and shape concerns are associated with SI amongst adolescents”. What are DEs and how to they differ from DE traits? Weigh should be “weight.”

Perhaps consider re-wording as follows, “authors first examined whether DE traits, such as weight and shape concerns, are associated with SI amongst adolescents…”

Authors’ response: Thank you very much for your comment. We have edited the text to clarify which exposure factors were included, namely DE, weight and shape concerns. By DE, we meant where adolescents belonged to poor appetite/undereating or uncontrolled appetite/ overeating groups, defined by their scores on EAT-7 question items, as outlined under Materials and methods section. We hope the text is now clearer to the readers. Thank you again for your comment.

Analyses

I had a hard time following this sentence, “Inspection of correlation coefficient showed no evidence support of correlations (all values were under 0.7).”

-The authors now report a moderation analysis (that was not significant), but it was unclear to me why they only examined the BSP item “very fat”

Authors’ response: Thank you very much for this comment. Here, the moderation analysis is done only as a continuation of association analysis done between BSP group, “very fat” and SI that was significant only in girls. The authors were interested to see whether the sex difference was due to due to a sex interaction in that particular group of research participants.

Conclusion

-Again, I’m not sure what the abbreviation DEs should mean, “disordered eatings”? As such, I think you could revise this sentence, “In line with previous reports [27], we found that DEs were far more prevalent than clinically diagnosed EDs” to “In line with previous reports [27], we found that DE was far more prevalent than clinically diagnosed EDs”. Please change throughout the discussion.

Authors’ response: Thank you for your comment. By DEs we meant poor appetite/undereating or uncontrolled appetite/overeating defined by participants’ score on EAT-7 question items, as outlined in detail under Materials and methods section. Authors have revised the text as suggested.

-These sentences get a bit long and hard to follow. I’d suggest breaking up and reworking for greater clariy: “We could not find evidence in support of BMI having a similar explanatory role for higher OR for SI amongst our male participants with DE traits, possibly partly due to differences in what these young individuals set themselves upon to achieve by dietary restraints or other means to lose weight,

with female population more concerned about their weight[52] whilst male population shows more concern for looking masculine and lean[53], which in effect renders BMI less indicative of presence or severity of DEs amongst males. Our findings were in line with previous reports of existing but somewhat overlooked higher vulnerability to mental distress amongst adult male sufferers of DEs [54] that, as previously outlined in great details is reflective of higher prevalence of a wider array of comorbid psychiatric disorders [17] that can potentiate an existing association between DE traits and SI."

Authors' response: Thank you very much for your comments. We have edited the text and hope it flows more smoothly now.

-"Furthermore, male populations seem to be more reluctant in seeking help for their symptoms [17] which can further potentiate existing associations between DE traits and SI by the factor of severity of unidentified and hence unaddressed DE traits or other comorbid psychiatric disorders." Perhaps reword to, "Furthermore, male populations are more reluctant to seek help for their symptoms [17] which can further exacerbate the associations between DE and SI as these individuals will not be receiving help for either condition."

Authors' response: Thank you very much. We have amended the text as suggested.

Reviewer: 2

Reviewer Name: Enrique Garcia Bengoechea

Institution and Country: University of Limerick, Ireland

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

Please leave your comments for the authors below

The authors have addressed appropriately the comments and suggestions I made. Thank you. Just two minor points:

Authors response: Thank you very much for your comments. Please see below authors' response to each point.

In the revised text (Line 11-14, page 9), I suggest changing "no evidence in support of correlations…" to "no evidence in support of high correlations".

Authors response:

Thank you for your comment. The text has been amended as suggested.

Finally, my last point was actually about the extent to which trends noted from YH1 to YH3 in the present study are applicable today given, in particular, that more than 20 years have passed since YH1. In other words, my question was about the extent to which the data on prevalence of a number of factors in this study reflect the current situation. If relevant, please address concisely this circumstance in the Discussion section.

Authors' response: Thank you very much for your comment. We have made several changes to the manuscript to reflect on this issue. Please see highlighted text in the closing paragraph under Strength and limitations towards the end of manuscript.