Author's response to reviews

Title: The changing demographic profile of eating disorder behaviors in the community

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Author's response to reviews: see over
Dear Ms Pafitis,

Thank you for the review of our manuscript. We appreciate the positive response of the reviewers, Dr Racine and Prof Stein, as well as the opportunity to respond to their thoughtful comments. Please find our responses below and the revised manuscript attached.

1. Dr Racine stated as one of her main concerns: “My other main concern was related to the cross-sectional nature of the data and the use of the term ‘change’ rather than ‘difference’. Because these are data from sequential cross-sectional surveys administered in 1998 and 2008, it is not exactly accurate to say that the prevalence of disordered eating changes over time (or that there are changes in health-related quality of life). I think it would be more accurate to describe the results as differences between the 1998 and 2008 survey responses. Along these lines, the fact that these data are cross-sectional and an acknowledgement of how this impacts the interpretation of the results should be included in the limitations section of the discussion.”

We agree with this criticism and have modified our language throughout the manuscript to remove inappropriate use of the term ‘change’ and replaced it with terms such as ‘difference’ as suggested in the Aims and Results, and when directly discussing our findings in the Discussion. The following has also been added to the limitations section of the Discussion (p13., paragraph 2):

“This study was conducted using two sequential cross-sectional surveys, which although using a very stringent selection process, meant that only differences between the samples rather than within-sample changes could be assessed. Further, although efforts were made to control for sample differences in the demographics that were measured, other possibly influential factors were not measured (e.g. acculturation and generational status) and so could not be controlled in analyses. A replication of this study using a longitudinal sample would provide an account of changes within a representative sample and greater confidence of actual temporal trends in the population.”
2. Dr Racine suggested: “The abstract should specify that, for example, binge eating was associated with greater mental health impairment in 2008 compared to 1998 in males (but not females), as it was the sex by survey year interaction that was significant. The way that the results are written, it is unclear that the term “greater” refers to the difference between 2008 and 1998.”

We agree, and the Results section of the Abstract has now been adjusted for easier interpretation:

“Results: Below-median annual household income was associated with increased prevalence rates from 1998 to 2008 in binge eating, extreme dieting, and purging. Male gender was associated with increased prevalence rates in extreme dieting and purging. Age ≥ 45 years was associated with increased prevalence rates in purging. In 2008 versus 1998, binge eating was associated with greater mental health-related quality of life impairment in males but not females; and greater physical health-related quality of life impairment in regional but not metropolitan areas. Extreme dieting was also associated with greater physical health-related quality of life impairment in 2008 versus 1998 in the lower but not the higher socioeconomic sector.”

3. Dr Racine requested: “More information is needed under the “Sample selection and interview procedure” section in order for the reader to understand how sample definitions influence statistical analyses. Specifically, how were the metropolitan versus regional “collector” districts defined? Perhaps the authors could provide information such as the population size requirements for these different residency categories.”

We have now broadened this section to read (p5., paragraph 3):

“In both 1998 and 2008, metropolitan and regional “collector” districts in South Australia were identified based on a probability proportional to size sampling procedure, according to Australian Bureau of Statistics Census data collected in 1996 and 2006. Three hundred and forty metropolitan collector districts (each including about 200 dwellings) were selected from those used in the Census. For the regional sample, towns of at least 10,000 in population as well as a balanced sample of towns of at least 1000 in population were identified from the Census. Within each collector district, a starting point was randomly selected and using a predetermined process based on a “skip” pattern of every fourth household, 10 dwellings were chosen to conduct interviews in. The person to be interviewed within each dwelling was the person who was older than 15 years and had their birthday most recently. The samples were non-replacement, and up to six visits were made to conduct an interview with the designated participant. Interviews were conducted from March until April 1998 for the 1998 survey and from February until July 2008 for the 2008 survey.”

4. Dr Racine requested: “More information about the interview questions used to assess eating disorder symptoms is needed. I would suggest including the specific questions in
this section, as done by Hay et al. (2008). Also, were participants asked to give a representative example of a binge episode or were they provided with any information to help with the definition of “an objectively large amount of food”?

We agree that access to the full wording of the questions would assist the reader. In the Methods we explain what each of the questions regarding eating disorder behavior asked. Rather than including the full wording in the manuscript itself (in order to keep this section succinct and because the rather lengthy wording has been previously published in an open access journal), we have created an online supplement which includes the questions, and we have also referred to the previous publication that published the full wording of the questions (p6., paragraph 1):

“The specific wording of the questions, which included detailed explanations of the behaviors being assessed, can be accessed in the online supplement and has also been previously published in an open access journal [18].”

5. Dr Racine asked: “Why were $\chi^2$ tests and multivariate logistic regression analyses used to compare the prevalence of eating disorder behaviors in 1998 and 2008? I am assuming that the multivariate logistic regression analyses included the covariates listed in this section and that the OR and second p columns in Table 2 present these multivariate logistic regression results. However, this is not clear from the text or the table. The analysis section should more clearly state why both $\chi^2$ and multivariate logistic regressions were employed, and the note in Table 2 should indicate that the OR was adjusted for X,Y,Z, covariates (if this is indeed the case).”

In the Statistical Analysis section of the Method we had already included mention of the covariates used in both the logistic regressions and MANOVAs (p7., paragraph 1): “In these analyses, the MCS was included as a covariate when the dependent variable was the PCS, and vice versa. Other covariates used in the multiple logistic regression and MANOVAs (except where they were a variable of interest) included age, education, income, and body mass index, which were previously found to significantly differ between the 1998 and 2008 sample [19] as well as employment and country of birth, which differed significantly between men and women.” As requested, we have now also included in this section a clarification of the purpose of employing both $\chi^2$ and multivariate logistic regressions (p7., paragraph 1):

“To assess the prevalence of ED behaviors according to sex, age, residency, and annual household income, $\chi^2$-tests were employed. Further, in order to allow for statistical controls of other ED behaviors and demographics, multivariate logistic regressions were also conducted.”

The title of Table 2 has now been amended to “Comparison of the prevalence of eating disorder behaviors in 1998 and 2008: Results of Chi-Square Tests and Multivariate Logistic Regressions”, with the following added to the table footnote: “† multivariate
logistic regressions included age, education, income, body mass index, employment, and country of birth as covariates;”

6. Dr Racine asked that we: “Please describe “the method by Altman and Bland” and, when first presenting these results in the text, remind the reader that the z scores are derived from this method (if this is true).”

We agree and have added the following information regarding the Altman and Bland method (including a reference to a paper that outlines the method in detail), to the Statistical Analysis section (p7., paragraph 1):

“To statistically compare the 1998-2008 odds ratios of ED behavior prevalence between demographic sub-groups (e.g. metropolitan vs. regional), the method described by Altman and Bland [30] to compare parameter estimates from separate analyses was used.”

We have also done as requested and reminded the reader at first mention in the Results that the z scores are derived from the Altman and Bland method (p8., paragraph 2):

“Using the Altman and Bland method, the odds ratios calculated across 1998 and 2008 data for objective binge eating were found not to differ significantly between men and women, indicating a similar rate of increase across the sexes (z = -0.83, p = 0.41).”

7. Dr Racine pointed out: “I strongly believe the results section would be significantly improved if the discussion of the results in the text and the presentation of the results in Table 2 followed the same order. Specifically, the text is organized based on demographic factors, and the table is organized based on eating disorder behavior. It appears to me that the overall pattern of the results suggests that purging is less likely to show greater differences across time in under-represented compared to over-represented groups and, for this reason, it might make the most sense to re-organize the text to discuss the results by eating disorder behavior”. Prof Stein similarly commented: “The results are presented in a very disorganized manner, significantly detracting from the readability of the manuscript. It may be easier to read if the order of the independent variables in tables can be followed the same order as they were described in the text. In addition, use of figures to display major findings would improve readability.”

As recommended, we have rearranged the Results to be organized by eating disorder behavior rather than demographic variables. We have also added a figure to visually illustrate the changes in prevalence of eating disorder behaviors over time within and between demographic sectors.

8. Both reviewers point out the need to indicate that the values in Table 3 are means with standard deviations in brackets.
We thank the reviewers for noticing this omission and we have now included ‘M (SD)’ in the appropriate row headings. The title of Table 3 has also been modified slightly to: “Health-related quality of life associated with eating disorder behaviors according to year, sex, age, residency, and household income”

9. Dr Racine suggested: “There is no need to include the significance values of the results in the Abstract”.

We have now removed p-values from the Abstract.

10. Dr Racine stated as a discretionary revision: “In setting up the rationale for the study, it would be helpful to explicitly state why it is important to understand whether the prevalence of eating disorders in underrepresented groups is stable or is increasing”.

The following sentence has been added to the Introduction (p3., paragraph 1):
“Understanding temporal shifts in the prevalence of disordered eating, and the specific sectors of the community within which these shifts are occurring, will enable an informed and targeted approach to the design and implementation of future prevention and treatment campaigns.”

11. As a discretionary revision, Dr Racine requested that we “define the term “residency” when it first appears”.

We have now altered the first sentence of the Aims section to read (p4., paragraph 3):
“The aim of the current study was to examine the rate of increase of ED behaviors across the demographic features of sex, age, residency (i.e. metropolitan vs. regional), and household income.”

12. Dr Racine added as a discretionary revision that: “In the discussion of findings by sex, the term “self-induced vomiting” should be changed to purging, as the study examined all forms of purging combined”.

To add clarity we have altered the sentence referred to (p12., paragraph 2):
“Our findings demonstrate that in addition men may also be ‘catching up’ to women in the prevalence of severe weight and shape control symptoms, such as the dietary practice of fasting for long hours and the purging practices of self-induced vomiting and laxative abuse.”

13. Dr Racine suggested: “Perhaps the authors could add a paragraph that speculates as to why the greatest increases in eating disorder behaviors occurred in traditionally underrepresented groups. A discussion of possible mechanisms for this effect could help pave the way for future studies and the development of prevention and intervention efforts.”
We have now expanded the section where we discuss possible explanations for the impact of disordered eating in males versus females (p13., paragraph 1):

“These issues, the inadequate access to resources and narrow demographic target of existing resources (i.e. young females), could partly explain less treatment seeking and under-detection of EDs in males [2, 3] and other under-represented sectors of the community. Ultimately this may have contributed to the increased prevalence and associated health-related quality of life impairment found in this study amongst males and older, regional, and poorer people.”

14. Dr Racine suggested as an edit for Table 1: “including an ‘Obese’ BMI Group to help further characterize the sample for comparisons to other studies”

We have now added an Obese group to Table 1.

15. Prof Stein commented: “The reason for using the data weighted from 1996 and 2006 rather than 1998 and 2008 was not addressed”

Data were weighted according to the most recent Census (i.e. 1996 Census for the 1998 sample and 2006 Census for the 2008 sample). The first sentence of the Statistical Analysis section has now been modified to read (p6., paragraph 3):

“Data were weighted according to the most recent Australian Census figures (1996 Census for the 1998 survey and 2006 Census for the 2008 survey).”

16. Prof Stein commented: “The authors categorized age into three groups but did not provide a rationale for converting the continuous variable to categorical variable. Cut-points appear to correspond to eating disorder onset and persistence data but justification for the cut points is not provided in the manuscript.”

Age and income were converted into categorical variables to allow them to be subjected to analysis using logistic regression and also to aid their comparability with other categorical variables considered in this study, sex and residency. In regards the age cut-offs, the three age groups represent early, middle, and later adulthood and also broadly map onto eating disorder onset and persistence data. This has now been explained in the Statistical Analysis section of the Method (p6., paragraph 3):

“To convert age into a categorical variable, three age groups were formed: 15-24, 25-44, and > 45 years. These groups arbitrarily represent early, middle, and later adulthood and also broadly map onto eating disorder onset and persistence data [e.g. 30].”

17. Prof Stein pointed out: “Since the study did not use a longitudinal design, the samples in 1998 and 2008 may well differ in terms of generational status and level of acculturation.
These variables were not controlled in the analyses. This issue needs to be address in analyses or noted a study limitation (if data are not available).”

We agree and have now added (further to the additions made under point 1 above) to the Discussion (p13., paragraph 2):

“Further, although efforts were made to control for sample differences in the demographics that were measured, other possibly influential factors were not measured (e.g. acculturation and generational status) and so could not be controlled in analyses.”

18. Prof Stein commented: “The significant level in the prevalence by age and prevalence by residency was 0.10, which is not the standard criterion of significant level (p=0.05) nor was not consistent with the standard used for the rest of the results. No explanation of the changed criterion of significance was provided. This significantly detracts from the contribution of the manuscript.”

We have now retained the significance level at p < 0.05.

19. Prof Stein pointed out: “The authors described that the extreme dieting approached significant difference in odds ratios, but the p value was 0.10 that is not considered as significant difference.”

We point out that this effect “approached” significance, which means that although the effect was not significant there was an observed trend that was almost significant (i.e. within 0.05). We have now clarified this to read (p10., paragraph 1):

“The difference between odds ratios based on residency approached but did not reach significance (z = 1.65, p = 0.10), indicating a trend for a greater rate of increase in the metropolitan areas.”

20. Prof Stein commented that in the Discussion: “The authors mentioned that the ED behaviors increased “most rapidly” from 1998 to 2008 but did not explain the comparison group/years.”

We have now modified the beginning of the Discussion to read (p11., paragraph 3):

“This study provided the first known investigation into demographic differences in the prevalence and associated impairment of ED behaviors over time. This was achieved by comparing the proportion of participants who reported behaviors in surveys conducted on samples of the South Australia population in 1998 and 2008. Overall the findings from this study indicated that ED behaviors increased most rapidly from 1998 to 2008 in those demographic sectors that were previously characterized as being less eating disordered.”
21. Prof Stein commented: “Some significant changes of ED behavioral prevalence from 1998 to 2008 were only based on the small sample size, especially in purging, which may limit the reliability of the finding. This needs to be address in the limitation.”

We agree and have added as a limitation to the Discussion (p13., paragraph 2):

“Finally, a few of the analyses conducted were based on relatively small n’s (especially those concerning purging behavior), which reduces confidence in the reliability of these particular results.”

Yours sincerely,

The Authors