PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Examining the prevalence of disordered eating in a cohort of young Australians presenting for mental health care at a headspace centre: Results from a cross-sectional clinical survey study.
AUTHORS	Burton, Amy; Hamilton, Blake; Iorfino, Frank; La Monica, Haley; Scott, Elizabeth; Hickie, Ian

VERSION 1 – REVIEW

REVIEWER	Wade, Tracey Flinders University
REVIEW RETURNED	17-Mar-2022

GENERAL COMMENTS	The examination of disordered eating in a headspace setting is welcome in terms of integrating disordered eating into mental health settings. However, the paper is not very informative or interesting apart from the findings that almost half the group report engaging in disordered eating on a regular basis. This information is probably not sufficient for publication per se. 1. I note that the strengths and limitations section mention no limitations. In terms of balance, one limitation should be outlined. 2. Pages 4 to 5 – it is worth mentioning that disordered eating (DE) is not detected because (a) the information is not offered (due to denial and stigma), and (b) clinicians in primary health care usually don't ask about disordered eating. The authors recognise (a) but not (b). Asking will usually elicit the behaviours – see Fursland, A. and Watson, H.J. (2014), Eating disorders: A hidden phenomenon in outpatient mental health? Int J Eat Disord, 47: 422-425. 3. Given body mass index is important in the diagnosis of an ED, is this information collected routinely at the headspace? 4. One formal study of an early intervention program for eating disorders in 2 headspace centres does exist and worth referring to - Radunz M, Pritchard L, Stein E, Williamson P, Wade TD. (2021). Evaluating Evidence-Based Interventions in Low Socio-Economic-Status Populations. Int J Eating Disorders, 54, 1887-1895. 5. Page 6 – how many young people did not give permission for their information to be used? 6. Page 7 – some mention of validity of self-report EDE-Q behaviours versus face to face should be included, the papers by Kelly Berg are informative for this purpose. 7. Page 8 – can authors also report socioeconomic status using postcodes and the SEIFA? is there any information available about ethnicity or culture? 8. I would strongly suggest the removal of "possible DSM-5 feeding and eating disorders from Table 3. Apart from requiring item 4 also to be present (typically ≥ 4) for a diagnosis, it is

	impossible to differentiate meaningfully between diagnostic groups with this information, and at least one of the groupings is incorrect (BED does not involve restriction, a combination of binge-eating and restriction is BN). 9. I would suggest removal of the correlations section, it adds nothing to what we already know and has been reported many times. Similarly, differences based on sex or gender are not novel or informative and should be removed. 10. I consider the following analysis to be more interesting and informative and might argue for increased need to monitor EDs in primary health care settings: compare the young people who reporting engaging in disordered eating practices on a regular basis (i.e., at least weekly episodes for binge eating or purging and ‘regular’ use of dietary restriction) in the 3 months prior to survey completion in addition to scoring ≥ 4 on the weight/shape concern question to the remainder of the sample and compare with respect to suicidal thoughts and behaviours, incidences of deliberate self-harm, physical health, substance use, and mental health symptoms and severity which includes mood, anxiety, psychosis. Reporting means (SD) and odds ratios in a table would help us to understand the further burden EDs place on vulnerable young people. Minor issues 1. In text referencing needs checking, in places author’s names rather than the number of the reference is used. 2. Page 5, lines 4 to 10 – I suggest removing conjecture and anecdotal reports. 3. page 5, lines 36-37 – this does not make stand-alone sense. Please elaborate. 4. Page 5, lines 47-48 – “The centre achieves high levels of patient satisfaction with a composite score of 4.3 of 5, slightly above the average of other centres (4.1)[17].” – please remove, this is not meaningfully over variation of data range.

REVIEWER	Harrop, Erin N. Addict Behav Res Ctr, Graduate School of Social Work
REVIEW RETURNED	07-Jun-2022

GENERAL COMMENTS	BMJ Open: Prevalence of disordered eating in young Australians presenting for mental health care at a headspace centre This is a well-written manuscript describing the frequency of disordered eating behaviors at a headspace center in Australia. Strengths of this manuscript include the clarity of the writing, the attention to subsyndromal presentations of disordered eating, the inclusion of gender diverse populations, and analysis of limitations. I have several minor suggestions that may improve the quality of this paper. --In the limitations section, you note that you used questions from the EDE (clinician led exam) despite not having a clinician perform this interview, and you do a nice job of how this may result in the over-reporting of binges due to the possible inclusion of subjective binges. I would also note that the purging question is limited by not being attuned to frequency of purging (those purging less than once a week would be missed, and it is impossible to tell the severity of purging behaviors. I would also note that restriction may be under-reported as often people with EDs are less aware of how strict/severe their food restriction is, and without a clinician to clarify this, under-reporting here would be likely.

	--Thank you for your point about how ED patients may be more likely to complete the survey with consent compared to those without disordered eating. This is a good point. To better inform on selection bias, please clarify how consenters compared to non-consenters for the study in terms of demographic factors. Did any demographic factors reach statistical difference? --Results section, Dietary restriction, second paragraph. There is a repeated clause of a sentence that is unnecessary. When reporting differences in sex-at-birth, you should not include "other gender" folks here, as you already report these stats earlier in the paragraph --Table 3: Regarding possible DSM-5 Diagnoses: Top row (binge, purge, restrict) could also be OSFED folks, specifically AAN, PD, or BN low frequency. Please add. You could also add OSFED categories to row 2 (BN low freq), and row 3 and 5 (BED low freq). --I would recommend taking out the Correlations section of the results and move to supplementary materials. These behaviors are usually related, so I am unsure what benefit this offers to the paper/field --Title/language: I am not a stats expert. As such, I am unsure if authors should be using the term "prevalence" to describe the percentages of participants with ED symptoms, or if "frequency" would be a more correct term. --In the summary/conclusion the age range is 14-26 years, but earlier in the paper they mention both 15 years and 25 years. Please ensure all ages are consistent with methods. --The term "other gendered" seems a bit stigmatizing. Since this term is capturing multiple identities (nonbinary, trans, agender), I am wondering about using the term "gender diverse" individuals instead? --I appreciate the land acknowledgement.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Dr. Tracey Wade, Flinders University
Comments to the Author:
The examination of disordered eating in a headspace setting is welcome in terms of integrating disordered eating into mental health settings. However, the paper is not very informative or interesting apart from the findings that almost half the group report engaging in disordered eating on a regular basis. This information is probably not sufficient for publication per se.
We thank the reviewer for their time in reading our manuscript and providing feedback and suggestions. We acknowledge that our paper's findings are somewhat limited in scope, however we note that it is an important contribution to the field to provide an examination of disordered eating in a headspace setting and our finding that almost half the group report engaging in disordered eating on a regular basis highlights the importance of further planned studies in this area. We hope that our revisions have satisfactorily addressed the reviewer's major concerns.

1. I note that the strengths and limitations section mention no limitations. In terms of balance, one limitation should be outlined.
Thank you for noting this, we have added the limitation of our study being based on self-report data.

2. Pages 4 to 5 – it is worth mentioning that disordered eating (DE) is not detected because (a) the

information is not offered (due to denial and stigma), and (b) clinicians in primary health care usually don’t ask about disordered eating. The authors recognise (a) but not (b). Asking will usually elicit the behaviours – see Fursland, A. and Watson, H.J. (2014), Eating disorders: A hidden phenomenon in outpatient mental health? Int J Eat Disord, 47: 422-425.
Thank you for this suggestion, we had now mentioned this important additional reason for the lack of detection of disordered eating into the introduction.

3. Given body mass index is important in the diagnosis of an ED, is this information collected routinely at the headspace?
This information is collected by GPs or treating clinicians and stored in the clinical file, it is not collected as part of the questionnaire dataset at assessment and therefore was not available as part of the dataset used for the analyses outlined in this paper.

4. One formal study of an early intervention program for eating disorders in 2 headspace centres does exist and worth referring to - Radunz M, Pritchard L, Stein E, Williamson P, Wade TD. (2021). Evaluating Evidence-Based Interventions in Low Socio-Economic-Status Populations. Int J Eating Disorders, 54, 1887-1895.
Thank you for bringing this paper to our attention. Its great to see this work being done. We have cited it in our manuscript as suggested.

5. Page 6 – how many young people did not give permission for their information to be used?
Unfortunately the dataset we were provided with for the purpose of the planned analyses reported in this paper do not allow us to assess this. In accordance with the opt-out consent process approved by the Northern Sydney LHD HREC, we were only provided with the data of the participants who consented to their data being used for research and have no information at all available on those who did not.

6. Page 7 – some mention of validity of self-report EDE-Q behaviours versus face to face should be included, the papers by Kelly Berg are informative for this purpose.
Thank you for this suggestion. We have now included references to studies by Kelly Berg’s papers in both the method section and the discussion of limitations.

7. Page 8 – can authors also report socioeconomic status using postcodes and the SEIFA? is there any information available about ethnicity or culture?
Unfortunately the dataset we were provided with for the purpose of the planned analyses reported in this paper do not allow us to assess this. No data provided on individual participant postcodes, ethnicity or culture. All available demographic information for the sample is included in Table 1 and we have reported on the general demographics of the population who present to headspace Camperdown in our introduction section. We agree that this is helpful information and will keep this in mind for future planned studies in this population.

8. I would strongly suggest the removal of “possible DSM-5 feeding and eating disorders from Table 3. Apart from requiring item 4 also to be present (typically ≥ 4) for a diagnosis, it is impossible to differentiate meaningfully between diagnostic groups with this information, and at least one of the groupings is incorrect (BED does not involve restriction, a combination of binge-eating and restriction is BN).
We have removed this column from the table as recommended.

9. I would suggest removal of the correlations section, it adds nothing to what we already know and has been reported many times. Similarly, differences based on sex or gender are not novel or informative and should be removed.
We have removed the correlations section from the paper as recommended. We have also removed

the section on differences based on sex at birth (moving these to Supplementary information). We have decided to retain the differences based on gender as the other reviewer has noted the inclusion of data on disordered eating symptoms in gender diverse young people as a strength and we do believe that this is an important and interesting addition to the literature to retain in this paper.

10. I consider the following analysis to be more interesting and informative and might argue for increased need to monitor EDs in primary health care settings: compare the young people who reporting engaging in disordered eating practices on a regular basis (i.e., at least weekly episodes for binge eating or purging and 'regular' use of dietary restriction) in the 3 months prior to survey completion in addition to scoring ≥ 4 on the weight/shape concern question to the remainder of the sample and compare with respect to suicidal thoughts and behaviours, incidences of deliberate self-harm, physical health, substance use, and mental health symptoms and severity which includes mood, anxiety, psychosis. Reporting means (SD) and odds ratios in a table would help us to understand the further burden EDs place on vulnerable young people.
We agree with the reviewer that these are indeed interesting and informative analyses to run and have these planned for future studies (we have added this also here in the current paper as an important area for future research). Unfortunately the dataset we were provided access to for the purpose of the present paper was very limited (just demographics and the eating and body image concerns data) and does not allow us to complete these important and interesting analyses.

Minor issues
1. In text referencing needs checking, in places author's names rather than the number of the reference is used.
We have checked over the in text referencing to ensure there is a number for the reference provided throughout the manuscript

2. Page 5, lines 4 to 10 – I suggest removing conjecture and anecdotal reports.
This has been removed as recommended.

3. page 5, lines 36-37 – this does not make stand-alone sense. Please elaborate.
This statement has been removed.

4. Page 5, lines 47-48 – "The centre achieves high levels of patient satisfaction with a composite score of 4.3 of 5, slightly above the average of other centres (4.1)[17]." – please remove, this is not meaningfully over variation of data range.

This has been removed as recommended.

Reviewer: 2
Dr. Erin N. Harrop, Addict Behav Res Ctr
Comments to the Author:
BMJ Open: Prevalence of disordered eating in young Australians presenting for mental health care at a headspace centre
This is a well-written manuscript describing the frequency of disordered eating behaviors at a headspace center in Australia. Strengths of this manuscript include the clarity of the writing, the attention to subsyndromal presentations of disordered eating, the inclusion of gender diverse populations, and analysis of limitations.
I have several minor suggestions that may improve the quality of this paper.

We thank the reviewer for their kind words and their time in reading our manuscript and providing feedback and suggestions. We hope that our revisions have satisfactorily addressed the reviewer's concerns.

--In the limitations section, you note that you used questions from the EDE (clinician led exam) despite not having a clinician perform this interview, and you do a nice job of how this may result in the over-reporting of binges due to the possible inclusion of subjective binges. I would also note that the purging question is limited by not being attuned to frequency of purging (those purging less than once a week would be missed, and it is impossible to tell the severity of purging behaviors. I would also note that restriction may be under-reported as often people with EDs are less aware of how strict/severe their food restriction is, and without a clinician to clarify this, under-reporting here would be likely.
Thank you for noting these additional limitations, we have provided extra commentary on these in this section.

--Thank you for your point about how ED patients may be more likely to complete the survey with consent compared to those without disordered eating. This is a good point. To better inform on selection bias, please clarify how consenters compared to non-consenters for the study in terms of demographic factors. Did any demographic factors reach statistical difference?
Thank you for this suggestion, this would indeed be interesting to look at. Sadly the dataset we were provided with for the analyses reported in this paper do not allow us to assess this. We were only provided with the data of the participants who consented to their data being used for research and have no information at all available on those who did not.

--Results section, Dietary restriction, second paragraph. There is a repeated clause of a sentence that is unnecessary. When reporting differences in sex-at-birth, you should not include "other gender" folks here, as you already report these stats earlier in the paragraph
Thanks for noting this error. This has now been corrected.

--Table 3: Regarding possible DSM-5 Diagnoses: Top row (binge, purge, restrict) could also be OSFED folks, specifically AAN, PD, or BN low frequency. Please add. You could also add OSFED categories to row 2 (BN low freq), and row 3 and 5 (BED low freq).
Thanks for these suggestions, I have removed this column on advice from the other reviewer.

--I would recommend taking out the Correlations section of the results and move to supplementary materials. These behaviors are usually related, so I am unsure what benefit this offers to the paper/field
We have removed the correlations section from the paper and moved these to Supplementary information as recommended.

--Title/language: I am not a stats expert. As such, I am unsure if authors should be using the term "prevalence" to describe the percentages of participants with ED symptoms, or if "frequency" would be a more correct term.
Thank you for noting this, we confirm that the preferred term for the title is prevalence as the aim of our study was to determine the prevalence of disordered eating in this cohort. However we acknowledge that the results do present information on both the frequency of particular symptoms and the prevalence of disordered eating in this cohort and so have included the term frequency in the introduction and discussion.

--In the summary/conclusion the age range is 14-26 years, but earlier in the paper they mention both 15 years and 25 years. Please ensure all ages are consistent with methods.
Yes, this one is tricky to address. The sample used was in the age range of 14 – 26 years. The age range reported for the sample throughout the manuscript as being 14-26. The reason for the

confusion is that you are right that the age of 15 is mentioned in the method as the procedure is for all young people aged 15 years and over to be invited but sometimes those younger than 14 years will complete it due to either being very close to turning 15 or simply an administrative error. The centre is designed to cater for those up to 25 years old (thus the mention of 12-25 years earlier in the paper), but our sample did include the data of some 26 year old individuals who were attending for care at headspace. To attempt to reduce the confusion we have removed the specifics of the age of the sample from the summary/conclusion.

--The term “other gendered” seems a bit stigmatizing. Since this term is capturing multiple identities (nonbinary, trans, agender), I am wondering about using the term “gender diverse” individuals instead?
This is a great suggestion which we have actioned. The term ‘other’ gender came from the options in the survey itself – male, female, other. We agree with the reviewer that the term gender diverse is less stigmatising and have implemented this throughout.

--I appreciate the land acknowledgement.