Reviewer's report

Title: Prevalence of Attention Deficit/Hyperactivity Disorder Among Adults in Obesity Treatment

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Reviewer: Dr Lisa J. Rapport

Level of interest: A paper of considerable general medical or scientific interest

Advice on publication: Accept after discretionary revisions

The author presents data describing the prevalence of comorbid ADHD among patients receiving treatment for obesity at a bariatric clinic. The design employed was archival review of medical records for 215 patients treated over a one-year period. The topic is important and would be of interest to a wide readership. In general, the manuscript is well written. The introduction provides a sufficient review of relevant literature to establish the rationale and purpose of the study.

The major limitation of the manuscript concerns the diagnosis of ADHD in this population. The author does not provide sufficient information to evaluate the validity of the diagnostic process. For example, the author should provide much more detail regarding the "consistent interview process addressing a broad range of symptoms of the disorder" (p. 8). The degree of structure, the format, and the comprehensiveness of the interview process are unclear. If the interview employed a semi-structured format, perhaps the author would consider including the measure as an appendix or describing in the text how the interview process covered the diagnostic criteria in a comprehensive manner. The Participants section implies that data were collected reflecting the presence or absence of specific diagnostic criteria (see p. 8, lines 3-7 of para 2). If data for individual diagnostic criteria are available, including this information in the manuscript would enhance confidence in the diagnostic procedure.

The author presents support for his modification of the ADHD diagnostic criteria (i.e., onset of symptoms prior to age 13 versus age 7). However, extending the inclusionary criteria increases the prevalence of ADHD in this sample and makes it difficult to compare the results of this study to prevalence rates that used the standard diagnostic criteria. Perhaps the author would consider reporting separately the rate of ADHD in this sample using both the standard and the extended criteria. Also, additional information should be provided on the criteria used to classify patients in the "ADSx" group (i.e., the "subthreshold" patients with ADHD symptoms not meeting diagnostic criteria for ADHD). This information is especially important considering that the author chose to modify some of the criteria for the ADHD diagnosis and not others. The results would be strengthened by including data that compare specific ADHD symptoms within and across the groups (e.g., inattentive versus impulsive symptoms).

It is unclear whether this ADHD diagnostic interview was implemented prospectively during the year with this special project in mind and/or whether the diagnostic process for these patients included measurement of other comorbid psychiatric conditions (e.g., depression, anxiety disorders, etc.). Results indicating a high prevalence of ADHD would be most convincing in the presence of
dissociations from other psychiatric disorders. In essence, the author could provide some evidence of discriminant validity for the diagnostic process and specificity for ADHD as a comorbid condition that is unique to bariatric patients. For example, an alternative explanation for the high prevalence of ADHD is that this patient population maintains a positive response bias in general. Perhaps these patients would endorse a variety of psychiatric symptoms and meet the criteria for a broad range of disorders. Inclusion of a general measure of response bias or neuroticism (i.e., negative affectivity) also could address whether patients might be inclined to over-endorse a broad range of symptoms. The issue of potential endorsement bias should be addressed in the Discussion.

Perhaps the author could address the sex ratio of ADHD cases in the sample. For example, from Table 1 we can observe that approximately 41% of men and 26% of women met the author's criteria for ADHD. These results could be interpreted in the context of expected differences in the sex ratio of ADHD. The observation that the proportion of men with the disorder exceeded the proportion of women with the disorder is consistent with the body of literature on the prevalence of ADHD among adults and provides some additional support for the author's hypothesis. However, an additional 27% of men and 34% of women were classified as "subthreshold" cases of ADHD. Thus, the operational definition of the subthreshold group also has meaning as it pertains to expected differences in the proportion of men and women with ADHD symptoms. An additional issue for the author to consider: Although males maintain a much higher rate of ADHD than females, a body of literature suggests that women, when affected, present with more severe symptoms of ADHD. Is this the case in this sample? It also would be helpful for the author to place the results for gender and ADHD in the context of the bariatric literature. This issue seems pertinent to the present study considering the predominance of women in the sample (90%).

In general, it would be helpful for the author to provide estimates of effect size for group comparisons (e.g., Cohen's d or eta squared) in addition to p values. The sample is large enough to produce significant p values in the presence of smallish effect sizes. For example, given the data in Table 1, the effect size for the significant difference between AD versus NAD on BMI Change is d .50 (i.e., small). Standard deviations should be provided with the means for BMI Change among patients in the Obesity III category (p. 11, para 3) so that effect size can be evaluated.

Some of the standard deviations for the variables BMI Change, number of visits, and months of treatment are larger than their respective means, which suggests some significant skew in the distributions. Because the assumptions of the parametric model are violated, nonparametric comparisons of the groups are most appropriate for these variables.

The OB+ADHD group averaged more visits and more months in treatment than did the NAD group, which the author interprets as evidence for "marked inefficiency" that is typical of ADHD. Although these results are interesting and important, the term "efficiency" seems to be a misnomer in this context. The author interprets the findings for group differences in number of visits and months in treatment individually; however, a better index of "efficiency" might take into account the combined effects of both of these variables (e.g., visits / months of treatment). In this context, the patients with ADHD (M = 1.46 visits/month) averaged approximately the same schedule as did the patients without ADHD (M = 1.38 visits/month). These data provide additional support for the author's contention that impersistence was not the cause of poor results for patients with ADHD. However, I do not believe that these specific results provide convincing evidence that the difference in treatment efficacy is caused by specific ADHD symptoms such as inefficiency. A third variable could account for the more severe presentation of persons with both ADHD and Obesity III.

The Discussion does well in summarizing the essential findings, comparing the results to prior research,
and proposing possible neurobiologic explanations for the link between ADHD and intractable obesity. However, I believe that some of the proposed theoretical links and conclusions may be overstated given the data. For example, the statement, "in OB+ADHD it is symptoms of ADHD rather than level of obesity that produces impediments to treatment success" (p. 12) asserts a causal relationship that cannot be assumed given the correlational design. Similarly, the statement "results hint that attentional dysfunction may play an important role in the onset and maintenance of obesity" (p. 16) implies a directional relation and causal mechanism. The implications of ADHD treatment in the context of obesity resulting in "fewer surgeries," "improved quality of life," and "normalized brain function" also seem a bit overstated as does the comment, "no doubt, obesity could only promote the profound impairments so commonly found in ADHD" (p. 17). As an aside, I believe that the 80% prevalence statistic for the presence of ADHD symptoms among homeless veterans (p. 15; Lomas & Gartside, 1997) is misstated.

The author avers that "the application of established diagnostic criteria and consistent procedures" reduces the risk of interviewer bias "that could skew the results" (p. 16). Given the lack of detail regarding the diagnostic procedures, this statement should be removed. Instead, the author should simply acknowledge the limitations of employing a single interviewer and an idiosyncratic method of measurement.

It would be helpful for the author to comment on whether this sample of bariatric patients seems representative of the larger population. Can the author evaluate the characteristics of this sample in comparison to other samples of bariatric patients (e.g., demographic and treatment outcome variables)?

In sum, the study presents unique data that address an important but previously overlooked phenomenon. The theoretical basis for a neurobiologic link between ADHD and obesity is sound. The archival design and diagnostic procedure present some limitations; however, the study has significant value as an initial, exploratory step in this line of research.

Competing interests:

None declared.