Author’s response to reviews

Title: Challenges in Defining the Rates of ADHD Diagnosis and Treatment: Trends over the Last Decade

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Dear Prof. Santos

Thank you for considering this article for publication, and for the thorough and thoughtful reviewers’ comments. In general we have found the reviewers' comments most useful and have addressed all of them below.
Reviewer #1:

Abstract:

1. please define abbreviations such as ADHD at first use - Done.

2. What is meant by "tight definitions"? – Changed to "Physician's diagnosis"

3. Rather than ADHD prevalence it would be better to say "the prevalence of ADHD diagnoses". Changed

4. It is not common practice to formulate a hypothesis in a conclusion section. Please reformulate. Done as suggested

5. What is meant by possible environmental effects? This paper did not address environmental factors so I suggest to remove this from the conclusion. – Thank you for this comment. We have addressed the role of changed environment in article 4.3 of the original draft submitted. However, as per your recommendation, we have removed it from the abstract's conclusions.

Method:

1. please explain the abbreviation HMO - Done.

2. I suggest to delete the sentence "This section is dedicated to the challenges of ascertainment of ADHD diagnosis based on the different available key parameters." As it is redundant. – Deleted as suggested

3. Also I suggest a more neutral title for section 2.3 "Case ascertainment" ("the challenge of" can be removed). Done
4. Why is a definitive diagnosis warranted when there was medication purchase? Please explain the rationale. Cannot there be undue prescription of ADHD medication? Later it is written that a definitive diagnosis is also warranted when there was no prescription. Please provide a clearer definition. Later the authors speak of yet another category "leading diagnosis" This all is not very clear. Please clean it up and provide clear definitions for the various categories. Also acknowledge that there may be cases prescribed ADHD medication without presence of an ADHD diagnosis. Thank you for this comment. We have changed this to better clarify the rationale behind our case selection and the "two-medication purchase" criteria, as well as touched upon the subject of possible undue prescription and its relative impact on the total cohort.

5. The section 2.6 does not provide a statistical analysis section but rather a power size calculation. Please correct. – Statistical analysis has been added.

Discussion
1. Would remove the subheadings. Respectfully, we feel that the subheadings allow us to properly focus the reader toward the main elements of our discussion while forming a coherent whole picture in the reader's mind. We believe this adds to the clarity of the presentation. 2. The first sentence "This discussion will focus on challenges in understanding the recent increase in ADHD prevalence" is redundant and can be removed. Done

3. What is meant by positive DSM IV criteria? How is it different from ADHD? Thank you for this comment. We changed the wording to clarify that we meant "fulfilling" the DSM criteria for a diagnosis. For example, currently the physician must document exactly which criteria were met for an autistic spectrum disorder diagnosis, while an ADHD diagnosis does not require the physician to elaborate on precisely which criteria were met.

4. Why is over-diagnosis less likely? Isn't this a straightforward explanation? A prevalence rate which is double that of the worldwide prevalence published recently certainly makes over-diagnosis very likely! We truly appreciate this input, and have worked to correct this oversight from the original draft submitted. The paragraph concerning over-diagnosis has been revised and
should now be properly articulated, and acknowledge the issue of over-diagnosis, as originally intended.

Conclusion:

I think it is less likely that there is an increase in ADHD prevalence but would rather speak of an increase in the number of ADHD diagnoses. Done.

Kevin Antshel (Reviewer 2)

1. Most fundamentally, the authors need to convince the reader (at least this reader) why yet another population-based study from a different country is needed. Nearly all of the data cited in the Introduction suggests that the prevalence rate and ADHD treatment engagement is increasing. What do these data add to the already existing voluminous literature on this topic? What is the innovativeness of this study? Thank you for this comment. We believe this study is unique in several important aspects. We have taken a deeper look as related to the quality of the diagnosis of ADHD. In this effort, the reviewed cases are all diagnosed by specialist physicians (Psychiatrists, Neurologists, and ADHD qualified pediatricians), and yet there is still a significant rise in ADHD diagnoses; Hence our recommendation for more rigorous adherence to diagnostic criteria, as well as documentation of these criteria as well as the functional impairment caused by the diagnosed ADHD. In addition, the increase in girls' diagnoses, as well as higher prevalence in above-average socio-economic groups further illustrate newer trends which merit this study. It is our belief, that the changed attitude of physicians, parents and children to ADHD diagnoses, should give rise to an important renewed societal discussion.

2. The authors note that studies using different methodological approaches have cited various prevalence and incidence rates. The various methodological approaches vary due to, amongst other variables, the country in which the study was conducted. Thus, any study on this topic that only focuses on one country is naturally limited. This issue is related to the first issue and speaks to the perceived low level of innovation present in this
study. Thank you for this comment; we have addressed this issue in tandem with your first comment above.

3. The authors stated goal to "better understand the reasons behind any significant changes in the number of cases of ADHD being diagnosed and pharmacologically treated" is important yet affected negatively by the extrapolation process. The extrapolation process accounts for roughly 25% of the cases. By extrapolating the cases, it is not possible to discern which individual level variables (e.g., SES) are responsible for the significant changes in the number of cases of ADHD being diagnosed and pharmacologically treated. Thank you for this comment. We acknowledge this issue, and have elaborated on this aspect under the "limitations" subheading.

4. The SES finding is one of the more novel findings and could be allocated more emphasis in the discussion. Presently, it has 2 sentences on page 14. Likewise, the disappearing of the large sex ratio typically observed in ADHD is interesting yet given little focus. Thank you for this insightful comment. With regards to the sex ratio, we have further emphasized it in the abstract, as well as given more breadth to the SES finding.

5. The possibility of overdiagnosis is refuted on page 15 using circular logic (diagnosis is made by experts over a period of 10 years). In the United States, most pediatricians do not adhere to AAP guidelines (see Jeff Epstein's work). These 'experts' are not adhering to practice parameters. The timing of the pediatricians becoming qualified to make ADHD diagnoses in Israel and the sharp increases in ADHD diagnoses is hard to overlook. How can the authors confidently state that overdiagnosis is not a factor? If the parents are more likely to seek out a diagnosis (due to reduced stigma), how can this not lead to overdiagnosis? Do the authors truly believe that 1 of every 4 adolescent males in Israel has ADHD and the associated functional impairments that come with ADHD? If yes, this represents a public health 'epidemic' – We truly appreciate this input, and have worked to address this point. The paragraph concerning over-diagnosis has been rephrased, acknowledging the issue of over-diagnosis, as originally intended.

Thank you for your help in improving the quality of our presentation.

Sincerely,
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