

Response to the reviewers

To Reviewer 02741591:

C1: The methodology should be better wrapped up.

R1: We have revised the methods section.

To Reviewer 00597793:

C1: The paper in general is OK but there is one problem. The authors state that they wish to EXPLAIN why some people with OSA develop daytime somnolence. They so not do so. Rather the paper is descriptive and says that day time somnolence is associated with MetS. Please re-do the INTRO to make the paper more congruent with its subject matter. Saying there is hypocapnia because of MetS is not an explanation

R1: Thank you for your comments. Please note that we have not stated that we aim to explain why some people with OSA develop daytime somnolence. Even if daytime hypercapnia were to be used instead of daytime somnolence, we have also not stated that we aim to explain why some people with OSA develop daytime hypercapnia.

To Reviewer 02476743:

C1: Please describe more details to calculate the power of this study.

R1: We have added details for sample size estimation as follows (P11, L11-16):
The null hypothesis for this study was that the proportions of MetS in patients with and without daytime hypercapnia were equal among obese patients with severe OSA. Because there are no previous data regarding proportions of MetS in patients with and without daytime hypercapnia, a medium effects size (i.e., 0.3) was applied. Assuming a 2-tailed Type I error of 0.05, we required a total sample size of 88 with a power of 0.8.

C2: Describe any efforts to address potential sources of bias, if applicable.

R2: We have added the following brief statement regarding this issue (P15, L6-9).
Because our center is one of the biggest sleep centers in Japan, the possibility of a referral bias may exist. Therefore, this should be taken into account while interpreting the prevalence of daytime hypercapnia among obese patients with OSA in the present study.

C3: What is the originality and strengths of this review? How physicians or policy makers could deliberate with patients or people based on the key findings of this manuscript?

R3: Firstly, this is not a review but an original investigation. Originality and strengths of the present study are demonstrated by the fact that we are the first to show an association between the presence of MetS or increased metabolic score and the risk of daytime hypercapnia in obese patients with severe OSA. We have emphasized this in the revised manuscript (P14-, L7-12).

C4: More discussion regarding the clinical practice of their findings would be important for the specific journal.

R4: We have revised the discussion section accordingly.

C5: Please consider the comparison with the other epidemiological studies in other areas using table so make clear the significance of this study.

R5: We have added a table to summarize previously reported prevalence of daytime hypercapnia in Japanese obese patients with OSA (Table 4).

C6: Some references should be updated.

R6: As per your comment, we have updated the references.

C7: Totally, I would like to congratulate the authors for the enthusiasm invested in this study. However, the manuscript does not reach the level of quality required for publication as original article without major revision in World Journal of Respiriology.

R7: Thank you for your comments. We believe that we have been able to address the concerns; please review our revised manuscript.