Regarding: response to the reviews of our research article "**The effects of diachronous surface uplift of the European Alps on regional climate and the isotopic composition of precipitation**" by Boateng et al.

Dear Prof. Gabriele Messori,

We would like to thank you for the time and effort in handling our manuscript, and we extend our gratitude to Reviewer 1 (J. E. Saylor) for their invaluable suggestions that have significantly improved the quality of our manuscript. We also appreciate their dedication in reviewing it for the third time. We have meticulously proofread the manuscript and have addressed all the minor comments. The most significant changes are as follows:

1. We have added a few sentences to further clarify why the estimated lapses are not meant to constrain past vertical changes but rather to compare them with different configurations. This comparison highlights the non-stationarity of the lapse rate through time and space and violates the assumption of paleoaltimetry. This is because our sensitivity experiments solely address changes related to topography without considering corresponding global paleoenvironment changes such as paleogeography, atmospheric CO_2 levels, etc. We intend to address these additional changes in future studies, as emphasized in the manuscript.

2. We have also taken additional steps to clearly highlight the sections where we report the $\Delta\delta^{18}O_p$ (i.e., changes between the low- and high-elevation regions) and the differences in $\delta^{18}O_p$ values between the CTL and modified topographic experiments.

We have provided detailed information regarding the manuscript's revision in our point-by-point response to the Reviewer's comments. We deeply appreciate your efforts, as well as the reviewer, in helping us enhance our manuscript.

The submission file comprises our point-by-point response to the Reviewer's comments and the revised manuscript (with tracked changes) specifying all the modifications made in accordance with the minor comments. Please do not hesitate to contact us if further clarifications are required.

Sincerely,
Daniel Boateng (corresponding author), on behalf of all co-authors

Reviewer's comments are repeated in black. Authors' replies are highlighted in blue font, and the revised texts in the manuscript are in quotation marks with blue italics font.

Review by J.E. Saylor

Summary

This is my third review of the manuscript by Boateng et al. The authors have addressed all of the comments raised in the previous reviews. I understand the authors' motivations for reporting confidence limits rather than prediction limits for the calculated isotopic lapse rates and the justification makes sense. Nevertheless, I appreciate that they include the confidence limits and thereby make this research as transparent as possible and also as useful as possible to future researchers.

Recommendation

I recommend that this manuscript be published after the authors address the minor comments below.

General comments

My comments below indicate that there are a disturbing number of minor issues with the manuscript considering that it is on its third round of reviews. I recommend that the authors carefully revise the text, critically looking for minor problems and internal inconsistencies.
We appreciate the reviewer for bringing up these minor issues, and we have diligently addressed them.

Detailed comments

Line 18: Consider replacing “for stable” with “to be detected using stable”.
Lines 109 & 111: No caps for “middle” in “middle Miocene” since it is not a formal epoch or age. Do a universal search and correct throughout the manuscript.
We have corrected the above comments.

Line 135–139: This is tricky because the atmospheric circulation interacts with topography as shown by this study. I am not sure what to recommend except to explicitly acknowledge the feedbacks between atmospheric circulation and topographic change.
We have extended the sentence to indicate that atmospheric circulation can change due to global and regional paleoenvironmental factors, including topography (see line 139).

Line 178: Delete, “The reader is advised that”. It is condescending.
This has been corrected.

Line 183: What is the ECHAM5-wise model resolution?
We have added the model resolution (see line 183).

Line 252: Establishing lapse rates for paleoelevation studies may not be the goal of this study, it is an obvious application of this research. Would such an application be valid? If not, why not? This doesn’t affect the current study, but an explanatory statement may help future researchers use your research appropriately.
We have added a sentence to clarify why estimates may be less suitable for lapse rate estimations, given that the experiment exclusively considers changes in topography without accounting for their corresponding global paleoenvironmental changes (see lines 250-253).

Line 259: I am confused by this statement. Are the authors stating that the lapse rates and associated uncertainties should not be used in an empirical paleoelevation study? If so, the

reasoning is not clear but is quite important to how this study is used in future research. Can you expand on the reasoning for this statement or clarify this statement?
We have extended the sentence to encompass the assumed conditions under which the lapse rate can be employed for paleoelevation calculations in future studies (see line 258).

Line 294: Is the offset systematic or random? I have never found the overlays such as those presented in Figure 1B a particularly helpful way to visually evaluate the data. I would recommend a more simple plot such as a biplot of GNIP d18O vs model d18O for each of the GNIP locations. It seems like that would be much easier to evaluate the deviation of the model from empirical data.
These deviations are systematic and have also been highlighted in previous studies (e.g., Langebroek et al., 2011; Werner et al., 2011). The reason for this comparison is to check how realistically the model simulates the spatial patterns of $\delta^{18}O_p$ and, therefore, evaluating the GNIPs with the regional patterns of the ECHAM5-wiso predictions help show the consistency of the model representing the spatial variability. Therefore, we would like to keep this type of plot in this manuscript.

Line 385: I see ~-6 per mil change between the CTL and W2E1 in Figure 5C at ~8 degrees E, but I am not sure that I see -8 per mill change anywhere. Can you clarify where this -8 per mil comes from?
We thank the reviewer for pointing this out. In this sentence, we are referring to the changes between the low- and high-elevation regions. We have revised the sentence to specify the $\Delta\delta^{18}O_p$ of up to -8 per mil, as illustrated in Fig. 5.

Line 387: But these values are slightly lower in Figure 5B (~-11, ~-9, and -6 per mil). How should the reader understand the difference between text and the figures?
We initially reported the values for the longitude transect. We have now updated the text to include the latitude transect and, as a result, correctly report the suggested changes.

Line 678: Doesn't this contradict the argument made in lines 135–139?
Line 678 pertains to the modified topography conditions, while lines 135-139 are related to the atmospheric control of present-day European climate.

Line 690: Shouldn't this be a d18O value (rather than Dd18O value) based on Figures 6 and 7?
We refer to $\Delta\delta^{18}O_p$ values here.

Line 773: Should this be Dd18O or d18O (see comment above on line 690)?
We refer to the $\delta^{18}O_p$ values difference here.