

## **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.

### **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.

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.

Line 178: Delete, “The reader is advised that”. It is condescending.

Line 183: What is the ECHAM5-wise model resolution?

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.

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?

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.

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?

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?

Line 678: Doesn't this contradict the argument made in lines 135–139?

Line 690: Shouldn't this be a d18O value (rather than Dd18O value) based on Figures 6 and 7?

Line 773: Should this be Dd18O or d18O (see comment above on line 690)?