2nd review by J.E. Saylor

Summary

The authors have addressed the reviewers concerns in part. However, there are still significant gaps in the data analysis and presentation as outlined below.

Recommendation

My primary reservations regarding the manuscript from the first round still stand. I do not recommend publication of this manuscript in its present form. I recommend that it undergo an additional round of major revisions before being reviewed a third time.

General comments

As in the first version, I do not think that the Abstract or Conclusions sufficiently lay out the results and associated caveats. For example, it is insufficient to indicate "changes" in the model output associated with changing topography without indicating whether those changes are within uncertainty. That the model would change with changing topography is a facile statement. Since most readers will read only the abstract, the abstract must convey both the results and some sense of whether the results are significant. For this study, I suspect that some of the results are significant but others (e.g., temperature, see comment on Lines 540, 543, and 552 below) are not.

The authors have provided uncertainties associated with their lapse rates in the form of 95% confidence intervals (presumably of the lapse rate linear fit). This is useful, but a more appropriate uncertainty would be the 95% prediction interval. Given that the lapse rate is empirically calculated, the most relevant question is whether an additional data point would be consistent with the calculated lapse rate (i.e., the prediction interval, not the confidence interval). In other words, if you want to know whether an observation is consistent with a model you want the prediction interval. As I stated previously, I still suspect that these lapse rates are indistinguishable in terms of their prediction intervals.

The treatment of temperature changes attributes very small changes in lapse rate to non-adiabatic processes. However, the small changes are within uncertainty of the adiabatic lapse rates. Therefore, although it is possible that these are non-adiabatic changes, it is at least equally likely that they are simply the result of the adiabatic lapse rate. It is impossible to distinguish these two scenarios as far as I can tell. I would favor a conservative approach in which all changes that are within uncertainty of the adiabatic lapse rate are attributed to the adiabatic lapse rate. Nevertheless, the manuscript needs to clearly state what the model results support, and what is interpretation. As it stands, these two are conflated as indicated by statements such as Line 552 in the manuscript (see also comment on Line 552 below).

Detailed comments

Lines 15–21: This could be condensed into 1–2 lines. For example, most of the motivation is irrelevant for the Abstract. Save the space for communicating your results and interpretation.

Line 56: The theoretical Rayleigh distillation curve is non-linear (Rowley, 2007).

Line 540: The manuscript already states that it is reasonable to attribute 80% of the temperature change to the adiabatic lapse rate. Rephrase this sentence. I suspect it is a hold-over from the first version.

Line 543: This approach absolutely needs to be dropped. If there is a potential range of temperature lapse rates, then you need to calculate the potential range of adiabatic temperature decreases based on that range in lapse rates. It is invalid to only use one value (the mean?) and then conclude that some small fraction of the total observed change must be due to non-adiabatic climate change. The best that could be argued is that the misfit _might_ be due to non-adiabatic temperature changes. But it might not be…

Line 552: No. The results suggest nothing of the sort. They suggest that all of the change can be attributed to adiabatic temperature changes, but that a small fraction _might_ be attributable to non-adiabatic changes. The signal is within the range of the noise.

Line 677: I wonder if it would be worth highlighting the fact that the only way to get d18O values more negative than ~-8 per mil is to have topography that is higher than modern.

Line 685: I don't see the 1 per mil per km. 0.5 per mil per km might be possible based on average values, but, again, the uncertainties are important.

Line 692: Without specifying what the effect of the model shortcomings are it is virtually impossible for most readers to consider these limitations in any meaningful way. I am not sure what to advise here, because I assume the effects of the model limitations are unquantified (and perhaps unquantifiable until better models are built). Nevertheless, this section reads very much like a disclaimer.

Line 720: The limitations of this section come back to the uncertainties, which in this case should certainly be the prediction intervals. The primary question is whether, given a new data point or data set, you could distinguish between the proposed models. I suspect that given the spread in the data used to calculate lapse rates the answer is no. However, the authors need to demonstrate that that is not the case.

Line 753: As I indicated in my first review, the conclusions and abstract need more detail and caveats associated with the conclusions given the fact that these are the sections that most people will read. Stating that the changes are "distinctly different" does not communicate any of the nuances of potentially overlapping lapse rates in d8O or temperature.