Response to Reviewer 1 (J. E Saylor)

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.

We thank J. E Saylor for the constructive comments and time for highlighting parts of the manuscript that require changes and clarification.

The authors present a sensitivity study of the effects of diachronous elevation changes in the Alps on regional climate and, specifically, the stable isotopic composition of precipitation. The sensitivity study is conducted using the ECHAM5-wiso isotope-enabled General Circulation Model. They explore a range of end-member models, ranging from no Alpine topography, to double modern elevations in the western and eastern Alps individually or uplift of the full Alps to twice their modern elevation. The authors conclude that there are significant changes in isotopic lapse rates (in addition to changes in isotopic ratios as a result of topographic change but not due to changes in lapse rate). The authors also identify changes in temperature, precipitation amounts, and atmospheric circulation related to uplift. The authors conclude that uplift of the western Als has a greater effect than uplift of the eastern Alps and that diachronous uplift can be assessed from the geologic record, given appropriate archives.

The reviewer highlighted in the summary that the manuscript concludes with significant changes in the isotopic lapse rates in response to the topographic changes but not due to the changes in "lapse rate". In order to avoid possible misunderstandings, we would like to point out the following: If we refer to the "lapse rate" as the expected changes in isotope ratios due to the topographic increase or decrease, then the manuscript highlights that the simulated changes are driven by the combined influence of the linear feedback (such as altitude effect) and other non-linear responses such as redistribution of precipitation, atmospheric circulation changes, vapor transport, non-adiabatic temperature effects, etc. (For similar findings, see Ehlers and Poulsen, 2009; Insel et al., 2010; Feng et al., 2013; Lee and Fung, 2008; Risi et al., 2013; Botysun and Ehlers, 2021). On the other hand, if we refer to the "lapse rate" as temperature lapse rate, then our analysis indicates a significant decrease (increase) in localized near-surface temperature in response to an increase (decrease) in elevation. Considering the expected changes in temperature due to the elevation changes, we attribute the highly localized changes primarily to the adiabatic temperature lapse rate changes with only minor contributions from the non-adiabatic temperature changes. We demonstrate the additional changes aside from the direct lapse rate response for temperature and $\delta^{18}O_p$ in our replies to the detailed comments section.

Recommendation

There are significant unexplored limitations to the dataset that need to be addressed before the manuscript can be published. These limitations may undermine, or at least put caveats on, the authors' conclusions that should be incorporated into the Abstract, Discussion, and Conclusions. I recommend that the authors address the considerations below in a major revision prior to publication.

We thank the reviewer for suggesting that we incorporate some uncertainties associated with our analysis. We describe in our responses below how exactly we addressed the reviewer's concerns about the study's limitations.

General comments

The authors need to calculate and present uncertainties associated with their lapse rates. It is difficult or impossible to determine if the difference in lapse rates is significant without some estimate of the uncertainty associated with the values.

We agree with the reviewer's assessment and updated the manuscript accordingly. We have added uncertainties associated with the lapse rates in all the related figures (Figures 6 and 7).

The authors conclude that changing the topographic configuration changes the d18O values across the region. This goes without saying and is the basis for paleoaltimetry.

We believe there has been a small misunderstanding due to a lack of clarity in the text. Even though the manuscript also addresses the question of how much (and where) different scenarios of differentiated west-to-east surface uplift would affect $\delta^{18}O_p$, one of our goals is to help determine whether the estimated $\delta^{18}O_p$ changes in response to the different configuration of the Alps would be significant enough to be reflected in the paleoaltimetry or paleoclimate records. In that case, paleoaltimetry can be regarded as a valid tool to reconstruct paleoelevations and help understand the geodynamic evolution of the Alps, which is still debated. We have clarified this goal (lines 84-85).

"The simulated $\delta^{18}O_p$ signal can help determine if the changes are significant enough to be reflected in paleoaltimetry records, which would ultimately help to reconstruct the geodynamic evolution of the Alps."

Furthermore, the expected $\delta^{18}O_p$ changes we calculate include non-adiabatic changes that would be missed in classic methods used for paleoaltimetry. These changes constitute an important part of our conclusion and provide significant added value to well-established paleoaltimetry methods. Please see the responses below for more details on this.

The question is whether the d18O values change more or less than is expected given a certain amount of topographic rise. The authors have not demonstrated that that is the case based on my evaluation of Figure 5, 6 or 7.

This is a very valid and important concern. We addressed this as follows: since the expected $\delta^{18}O_p$ in response to certain topographic rises can be determined with a specific lapse rate, we adopt the present-day isotopic lapse rate of 2.0 ‰/km (as estimated from the isotopic observation across the Central Alps by Campani et al. (2012)). This modern isotopic lapse rate has been used to infer the past elevation of the Central Alps in the Middle Miocene (e.g., Krsnik et al. 2021). We calculate the difference between the simulated annual $\delta^{18}O_p$ changes and the expected changes in $\delta^{18}O_p$ based on the topographic changes using this particular lapse rate. The results show up to 4 ‰ differences in $\delta^{18}O_p$ values across the region of modified topography, and differences of ca. 2 ‰ for adjacent low-elevation areas. This indicates that part of the $\delta^{18}O_p$ changes in the model is due to effects that are not (directly) related to altitude changes. These additional changes in $\delta^{18}O_p$ would have been missed using a simple lapse rate to calculate the changes based on the topographic rise. We added these results demonstrated for some of the topographic configurations to the supplementary material (Figure S16).

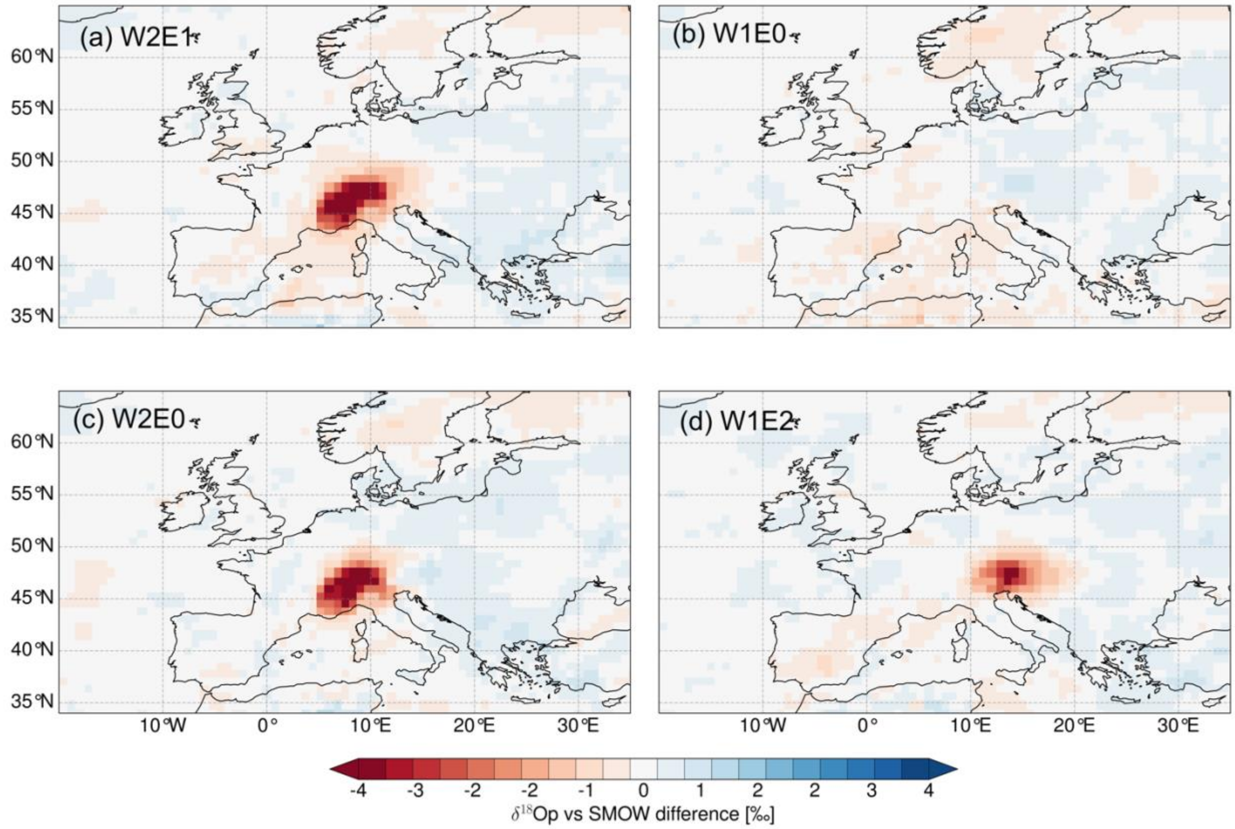

Fig. R1: Annual long-term difference between the simulated and expected change of $\delta^{18}O_p$ values in response to elevation changes across the Alps (e.g., W2E1 (a), W1E0 (b), W2E0 (c), and W1E2 (d)). The expected changes are calculated using a modern isotopic lapse rate based on long-term isotopes in precipitation measurements (Campani et al., 2012). The differences highlight the signal that would be missed if only the fixed lapse rate were used. For instance, negative difference indicates that paleoelevation would be overestimated due to their associated shallow lapse rate.

There are places where the authors apparently favor non-uniformitarian interpretations based on data that are equivocal (see comment on Line 519 below). It seems that the most conservative interpretation should be favored where possible and unless the data require alternative interpretations.
Please see our response to line 519 below.

Detailed comments
Lines 15, 117: Delete the “e.g.,”
This has been corrected

Line 22: What is "significant"? This should be presented in terms of absolute lapse rates and their uncertainties.
The uncertainties are now provided throughout the manuscript (Figures 6 and 7). Moreover, we modified the text (e.g., in lines 23-26) to highlight that what is significant depends on the topographic rise and configuration.

"The simulated responses to the varied topography suggest changes in the spatial patterns of $\delta^{18}O_p$, the elevation-dependent rate of change in $\delta^{18}O_p$ ("isotopic lapse rate"), near-surface temperatures, precipitation amounts, and atmospheric circulation patterns. However, the magnitude and spatial patterns of the simulated changes varied, depending on the topographic configuration and the extent of the surface uplift."

Line 25: Obviously the absolute values change and that that change will vary if part or all of the orogen is uplifted, the question is whether the underlying lapse rates in isotopic ratios or temperatures change.
Yes, changes in elevation would change the $\delta^{18}O_p$ values. However, considering the size of the Alps and its geographic location with respect to moisture transport, the underlying question is whether varying the topography of the different sections would result in different (uneven or unexpected) spatial patterns and lapse rates. For instance, does changing the elevation of the West-Central Alps affect the lapse rate of air mass transporting towards the northern transect? We have demonstrated in the lapse rate estimates (Fig. 6 and 7) that the magnitude of changes varied spatially in response to the different topographic configurations.

Line 70: Rephrase as "remains an open question."
This has been corrected.

Line 103: It would be useful to have a succinct statement of the modern elevations and the basis for selecting the elevations selected for the experiments. The latter is disseminated through this section, but it would be useful to have it stated concretely and in one location.
We thank the reviewer for this insight. Additional sentences have been added to explain further the reasons for the topographic experiments (lines 125-130).

"Therefore, to explore the plausibility of stable isotope paleoaltimetry estimates in addressing the west-to-east surface uplift scenarios, we use the different topographic configurations in sensitivity experiments to quantify the expected isotopic signal. With the present-day mean elevation of peaks of ca. 2500 m across the Alps, increasing its elevation by 200% would reflect the paleoelevation reconstructions across the West-Central Alps in the middle Miocene (Campani et al., 2012; Krsnik et al., 2021). However, due to the lack of quantitative paleoelevation estimates across the Eastern Alps, we incrementally increase the elevation across that transect to explore all the potential surface uplift magnitude back in time (see Sect. 3.2 for more details about topographic configuration)."

Line 108: What is deemed unlikely about the topographic development? It seems like this sentence needs an additional clarifying phrase. Line 108: Should this be, "between 200 and 100 km"?

It’s considered unlikely since significant continental lithosphere subduction under the European plate in response to the subsequent collision of the Adriatic and European plates at ca. 35 Ma after its commencement in the late Cretaceous (Schlunegger and Kissling, 2015) resulted in post-collisional crustal shortening of 100-200 km (and presumably some thickening) (Lippitsch et al., 2003; Rosenberg et al., 2015). However, we have modified the section to focus more on paleoaltimetry estimates.

Line 108: What is meant by “post crustal shortening”?
We thank the reviewer for pointing this out. We meant “post-collisional crustal shortening and refer to it as the subsequent collision that occurred between the European and Adriatic plates in the Oligocene after its initiation in the late Cretaceous to early Tertiary period.

Line 310: What about W0E0?
The W0E0 is not shown since it was similar to the results already presented by Botsyun et al., 2020. We have added a statement about this in lines 340-343 to refer readers to Botsyun et al., 2020.

“The experiment with no Alps (W0E0) predicts an increase in $\delta^{18}O_p$ values up to 8 ‰ (not shown) and was similar to the results presented by Botsyun et al., 2020. Therefore, we do not further discuss its results and refer the reader to Botsyun et al. (2020) for more details.”

Line 314: I recommend rephrasing as, “The topographic scenarios predict significant localized cooling or warming where the topography is raised or lowered, respectively.
We followed this recommendation and changed the text accordingly.

Line 315: Are these adiabatic or non-adiabatic cooling or warming?
The presented values are differences in the near-surface temperature, which are a result of both (a) adiabatic warming or cooling directly linked to elevation increase or decrease, and (b) non-adiabatic warming or cooling associated with climate change (due to changes in atmospheric circulation, radiative and/or surface heating)

Line 330: Consider annotating the legend with text to guide the reader, such as “Warmer than control,” or “Cooler than control.” Consider something similar for figure 2 and 4.
All the simulated difference figures (Figures 2, 3, and 4) are adjusted accordingly.

Line 370: It is quite difficult to correlate between the legends and the curves. I think this is in part because the topography is semi-transparent and so the colors are washed out. As the colors selected in the legend are somewhat similar, it makes it hard to tell the difference when they are semi-transparent. I recommend making all lines 100% opaque and perhaps labelling individual curves to aid visual correlation.
We thank the reviewer for pointing this out. We have added solid (opaque) lines to the topographic profiles and added their respective colors as the shading.

Line 370: Include these cross-sections or swaths on figure 2.
This is a very helpful request made by the reviewer. Thus, to identify better the cross-sections used for the profiles, we have added the swaths on the original topography used for modification

on the same Figure 5. This would help the reader to get all the information related to the isotopic profiles visually.

Line 370: What about W2E2? How do the lapse rates change for that scenario?
The profiles were presented in the supplementary for the W2E2 and W0E0 (in Figure S8) since we mainly focus on the stepwise surface uplift responses.

Line 380 and figure 6: What are the uncertainties in these lapse rates? I suspect that a lapse rate of -2.08 per mil per km is virtually indistinguishable from -1.83 per mil per km. Ditto for - 3.11 per mil per km and -2.96 per mil per km.
Even though the correlation coefficients (R^2), which quantify the accuracy of the regression fitting used to determine the gradients (or lapse rate) were presented, we additionally estimate the slope error and further show the associated uncertainties of the regression fit with the threshold of 95% confidence interval with the bootstrapping technique. Overall, the isotopic lapse rate uncertainties range from 0.09 to 0.24 ‰/km, which are insignificant compared to the magnitude of the lapse rate estimated (ca. 1.5-3.4 ‰/km) (see line 400). We have added the uncertainties associated with the estimates in Figures 6 and 7. Moreover, we also mentioned that changes in the lapse rates depend on the topographic rise, configuration, and the transect used for the lapse rate estimation (see lines 690-695). For example, the surface uplift of the West-Central Alps has a higher spatial impact due to the predominant moisture trajectory path from the North Atlantic Ocean towards the Alps. In that case, estimates across the northern transect would experience changes in lapse rate due to the redistribution of precipitation caused by the orographic barrier. For example, the isotopic lapse rate difference between the W2E1 and CTL across the north transect is estimated to be 1.19 (±0.1) ‰/km (Fig. 7B). Such an estimate of difference is significant and can lead to paleoelevation uncertainties of about 0.5-1 km depending on the lapse rate used (see L915).

"However, compared to the difference in lapse rate of 0.2±0.1 ‰/km estimated across the southern flanks indicate that the impact on the isotopic lapse rate changes depends on the topographic rise and configuration. In this scenario, the northern transect lapse rates estimate a higher magnitude of change of lapse rate since the higher topography established across the west-central Alps redistributes precipitation due to the orographic barrier to the moisture trajectories paths from the North Atlantic Ocean, which cause dryness toward the north (Fig. 4 B)."

Line 390: I'm not sure that linear lapse rates are appropriate here. From Figure 5 it looks like there are very different lapse rates between 0–500 m and >500 m. A back-of-the-envelope calculation of lapse rate for the W2E1 and control scenario yields similar lapse rates above 500 m. CTL: (-7.5 - -4.5)/(1.25-.5) = -4 per mil / km; W2W1: (-14 - -4.5)/(3-.5) = -3.8 per mil / km
We rely on empirical isotopic lapse rates using least square regression. Using a specific profile would not reflect the wide-area lapse rate. Besides, different points along the profile would also lead to different estimates. We refer the reviewer to previous studies that have used a similar technique (e.g., Rowley, 2007; Poage and Chamberlain, 2001; Feng and Poulsen, 2016). We deem these techniques more suitable for a regional study such as ours.

Line 519: This 2 C is well within the range of lapse rates cited above (4.1–5.9 C/km). Without further examination of the data, it seems like an over-interpretation to invoke non-adiabatic processes here. In other words, the non-adiabatic processes must be demonstrated and not simply invoked. No such demonstration is offered here.
We thank the reviewer for pointing this out. To quantitatively support our discussion further, we first estimate the expected temperature change attributed to adiabatic cooling or warming using a lapse rate of 5.6 °C/km, as estimated for the Alps by previous studies (e.g., Kirchner et al., 2009; Rolland, 2003) and our own simulation. We then subtract the adiabatic cooling or warming from the total temperature change. For example, the W2E1 experiment estimates non-adiabatic cooling of up to -2°C across the Alps and its adjacent regions. We have added this work to the text (see lines 545-555) and added a supporting figure in the supplementary material (Fig. S17).

"To validate the potential non-adiabatic cooling or warming due to the regional climate change, we first estimate adiabatic temperature change using a lapse rate of 5.6 °C/km, which is within the range reported values in previous studies using the CTL experiment (Kirchner et al., 2009; Rolland, 2003). We then subtract this adiabatic temperature change from the total temperature changes to determine the contribution of non-adiabatic processes to the total changes. Overall, we notice non-adiabatic cooling of up to -2 °C across the Alps in response to topographic rise, and ca. -1 °C in the adjacent remote regions (Fig. S17). Previous studies on major mountain ranges such as the Andean Plateau and North American Cordilleran also suggest additional non-adiabatic temperature changes in response to surface uplift (Ehlers and Poulsen, 2009; Feng and Poulsen, 2016). The non-adiabatic warming or cooling in response to the different topographic scenarios may be as a result of the associated regional climatic changes, such as changes in tropospheric dynamics, local atmospheric humidity, and atmospheric circulation patterns (sections 5.3 and 5.4)."

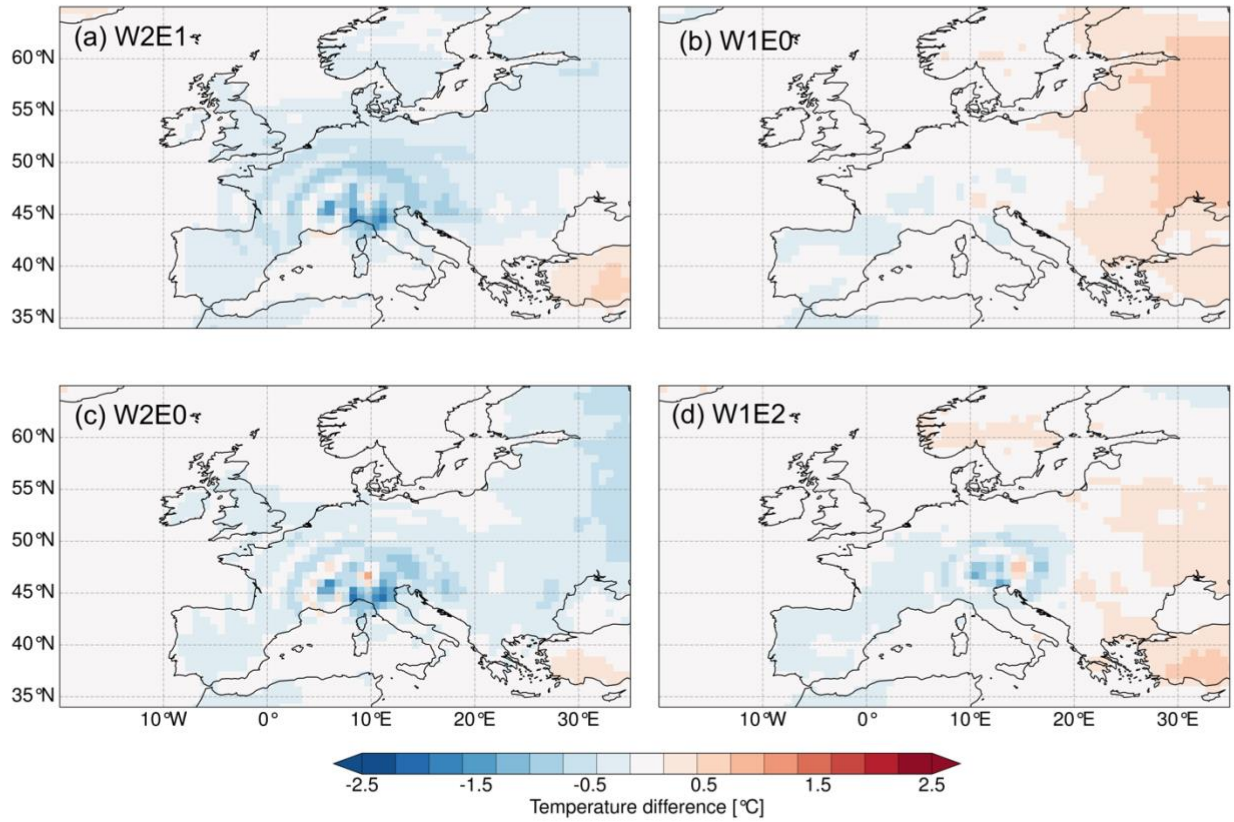

Fig. R2: Annual changes of non-adiabatic near-surface temperature between the topographic scenarios (e.g., W2E1 (a), W1E0 (b), W2E0 (c), and W1E2 (d)) and CTL. The adiabatic lapse rate of 5.6°C/km was used to calculate the expected adiabatic cooling or warming due to the elevation changes and then subtracted from the total temperature changes.

Line 520: Yes, plausible and the simplest explanation prima facie.
This has been addressed in the previous comments (response to line 519 comment).

Line 523: What remainder of the signal? I am not sure what is being referred to here.
We referred the remainder of the signal to the temperature difference between the total temperature changes and the temperature change due to the adiabatic lapse rate. We have modified the text (line 554).

Line 525: "All of the signal can be explained via adiabatic processes. Other processes appear to be insignificant." The "small contribution" has not been demonstrated and should not be invoked without caveats.
This has been clarified in the previous comment (response to line 519 comment).

Line 554: I can see the higher atmospheric origin (maybe…) but the paths are not convincingly longer than the CTL experiment. Also, specify that you are referring to the topography of the western Alps (obviously W2E0 has topography both raised and lowered).
We thank the reviewer for highlighting this, and we agree. We have modified the sentence and added the reference figures to support the discussion.

Line 555: An origin at higher atmospheric levels when the topography is lowered seems to be the opposite of what was stated in the previous sentence. Reference the relevant figure.
Agreed. This has been corrected.

Line 557: They also lower the elevation of the vapor source if I am interpreting the figures correctly.
Agreed. This has been corrected.

Line 617: To what is the 8 per mil additional? Perhaps replace "an additional" with "a"?
We deleted the "additional" to resolve the complication of the sentence.

Line 617: But are these differences in d18O values unexpected, or are they what would be predicted based on increasing topography without significantly changing the lapse rates? It looks like the latter based on my evaluation of Figure 5, for example.
This has been addressed in the previous comments (response to line 519 comment).

Line 630: Where is this difference in summer versus annual lapse rates shown? The text needs to refer to specific figures and panels to support statements like this.
These were not shown in the main manuscript. Thanks for pointing this out. We also agree that these need to be referenced. The appropriate figures presented in the supplementary (Fig. S9 and S10) are added to the sentence.

Line 640: Again, is the change significant? What are the uncertainties? Are the uncertainties greater than the calculated change in lapse rates?
We thank the reviewer for raising such a valid point. We have shown in our response to line 380 that the estimated lapse rate uncertainties are not significant compared to the lapse rate magnitudes.

Line 645: Where is this shown?
The figures of the lapse rate estimates are now referenced in the updated sentence.

Line 682–684: Whether they differ or not depends on the uncertainties associated with these measurements
We have addressed this in the previous comment (line 380) and highlighted that the uncertainties are not significant.

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