Author response: We would like to thank the referee for the time and effort he dedicated in reviewing our manuscript. We appreciate the referee's insightful comments and suggestions and carefully addressed them. Please see below, in blue and italic font, for a point-by-point response to the reviewer's comments. Provided page numbers refer to the revised manuscript file with tracked changes.

RC2: 'Comment on se-2021-59', Peter van der Beek, 30 Jun 2021 reply

Krsnik et al. present new stable and clumped oxygen / carbon isotope data from three early – middle Miocene sections in the North Alpine foreland basin, which they combine with existing data from a high-elevation site (Simplon Fault Zone) and isotope-enabled climate models to refine earlier estimates of middle-Miocene paleo-elevation of the Swiss central Alps. They find that this paleo-elevation was probably significantly higher than the present-day, confirming early data that were generally considered with some skepticism. This is a good paper with interesting new data that will make a nice contribution to *Solid Earth*. I would recommend acceptance after moderate revision as there are a few aspects that could be made clearer:

1) First, the manuscript does not make it entirely clear what is new and what is existing data. It appears that the oxygen- and carbon-isotope data from the three sections were collected specifically for this paper, while the hydrogen-isotope data from the Simplon Fault Zone are from Campani et al. (2012). However, what about the clumped-isotope data? Some of these appear to be from Methner et al. (2020). Was additional data collected for this manuscript? A data table that explicitly states the origin of the data would be useful.

- *Following the suggestion of the reviewer we prepared a table clearly displaying which data was obtained specifically for this study and which data was provided by Campani et al. (2012) and Methner et al. (2020). This table can be found in the Supplementary data (SI6).*
- *We also made sure to provide a more precise referencing to these studies.*

2) Similarly, it is not clear whether the paleoclimate simulations were run specifically for this study or whether they were taken from Botsyun et al. (2020). It is totally OK to reuse data or models but their origin should be clear.

- *Paleoclimate simulations from Botsyun et al. (2020) were not run specifically for this study. We state the origin of these climate simulations in lines 262–263 in the manuscript.*

3) Second, I feel that the sections, data and time constraints could be described a bit more clearly. In particular, Fig. 3 (which should be Fig. 2 – see below) does not contain a lot of information: it would be good if this figure showed stratigraphic names, specific age markers discussed in the text (with their age), the tie to the paleomagnetic time scale, etc. Carbon-isotope data are discussed but not shown at all; these could be plotted in the panels of Fig. 3 adjacent to the oxygen data. Similarly, it would be useful to show the locations of the samples collected for clumped-isotope analysis on the logs and report the inferred paleo-temperatures in the figure.

- *We appreciate this comment and implemented all of the suggestions of the reviewer as it really improves the figure.*
- *Following the suggestions we changed the order of Fig. 2 and Fig. 3.*
- *We added more information to the (new) Fig. 2 including bentonite horizons and mammal sites with their ages, respectively mammal zones, and locations of samples for clumped isotope analysis with corresponding (Δ_{47}) temperatures. We added two additional columns displaying the regional magnetostratigraphy used for calculation of soil carbonate ages and showing stratigraphic names of the OSM sediments, respectively. Furthermore, we included $\delta^{13}C$ records for all three Swiss Molasse Basin sections.*
- *Additionally, and not related to the suggestions of the reviewer, we changed the order of displayed Swiss Molasse Basin records from "Fontannen-Jona-Aabach" to "Fontannen-Aabach-Jona" according to their geographical locations for the purpose of better readability.*
- *In addition to Fig. 2 in the manuscript we prepared a more detailed figure for the Supplementary Material displaying magnetostratigraphic logs and their correlation to the paleomagnetic time scale for each section (Fig. SI1 in suppl. Material SI5).*

4) I would also like to see a somewhat more complete description of the paleoclimate models: what is the "ECHAM5-wiso GCM"? I don't think one can assume the average reader of *Solid Earth* to be acquainted with these acronyms. What is meant by a "pre-industrial model setup"? Does this only apply to the paleogeographic or also to the climatic (i.e., atmospheric pCO_2) boundary conditions? If pre-industrial pCO_2 was used instead of an estimated middle-Miocene condition, what would be the influence on the model predictions? Would they be realistic? Could a "distant region" for which middle-Miocene stable-isotope data are available be included and used to calibrate the model? Overall, this model description section needs a bit more explanation and justification.

- *As suggested by the reviewer we complemented section 3.4 ("Paleoclimate simulations") by adding relevant information about the climate model ECHAM5-wiso. This comprises details on the origin of the ECHAM5-wiso, resolution of the model setup, and boundary conditions of the pre-industrial model setup.*
- *It is beyond the scope of this contribution to conduct further experiments with Miocene boundary conditions. Current efforts by the original author (S. Botsyun) aim at complementing climate model runs with Miocene boundary conditions. This, together with model validation against proxy data at different locations (including "distant regions"), is part of a future project.*

5) I suppose the paleoclimate models make predictions of the (summer – JJA) temperatures at the fan sampling site**.** It would be interesting to report these and compare them to the estimates obtained from the clumped-isotope analysis; on the one hand to provide independent support for these fairly elevated temperature estimates and on the other hand to calibrate / assess the model outcomes.

- *Clumped isotope analyses provide temperatures prevalent during the time of soil carbonate formation, which is an essential factor to calculate mineral-water isotope fractionation. We, therefore, use (Δ_{47}) temperatures of soil carbonates for calculation of $\delta^{18}O$ in Swiss Molasse Basin soil waters and ultimately precipitation. We agree that* *obtained Swiss Molasse Basin (Δ_{47}) temperatures appear rather high and we acknowledge that the role of Δ_{47} based soil temperatures as a proxy for air temperatures is currently being debated. Nevertheless, we want to highlight that we use the obtained (Δ_{47}) temperatures solely as a means for reconstruction of the mineral-water fractionation temperature of the formed soil carbonate. For the following reasons we do not expect a close match between the temperatures simulate in (Botsyun, S., Ehlers, T. A., Mutz, S. G., Methner, K., Krsnik, E., & Mulch, A. (2020). Opportunities and challenges for paleoaltimetry in "small" orogens: Insights from the European Alps. Geophysical Research Letters, 47, e2019GL086046. https://doi.org/10.1029/2019GL086046) and the (Δ_{47}) temperatures presented here:*

- *1) In the absence of a Miocene model setup, climate model simulated summer mean (June-July-August) temperatures for the Northern Alpine Basin were obtained with pre-industrial boundary conditions and yielded ~16°C (Supplementary Material of Botsyun et al., 2020, Fig. S4). The pre-industrial ρCO_2 (280 ppm) boundary conditions restrict the upper temperature limit and therefore the simulated temperatures are lower than one would expect for the Middle Miocene. Because of the pre-industrial model setup, simulated temperatures and measured (Δ_{47}) temperatures of Middle Miocene soil carbonates record different climatic conditions and, in our case, are not directly comparable. As shown in the study of Methner et al. (2020), temperatures changed dramatically towards lower temperatures during the Middle Miocene Climate Transition when compared to the Middle Miocene Climate Optimum covered here. The current model setup is not able to resolve such temporal differences.*
- *2) A comparison between modeled and measured (Δ_{47}) temperatures would require consideration of the 2-3 times higher ρCO_2 for the Miocene. We therefore expect simulated Middle Miocene temperatures to be up to 6–12°C higher, if climate sensitivity (temperature increase in response to a doubling of pCO_2) of 1.5-6°C is taken into account. This would reduce the difference between modeled and measured (Δ_{47}) temperatures for the Swiss Molasse Basin.*

6) When discussing the results and their implications, a fuller assessment of uncertainties could be made, in particular considering the uncertainties in lapse rate. Why not first give the full range of possible paleo-elevations considering the different lapse-rate models and then potentially discuss a preferred option?

- *We very much appreciate this comment and followed the suggestion of the reviewer.*
- *Besides calculating paleoelevation based on the isotope lapse rate of -2.0‰/ km ±0.04‰ (Campani et al., 2012), we now discuss the impact of a more conservative choice of isotope lapse rates (taken from Botsyun et al., 2020) and provide maximum Δz (m) and minimum Δz (m) based on the uncertainty of $\Delta(\delta^{18}O_w)$ for both lapse rates (lines 409-411 in revised manuscript).*
- *In doing so, we realized that a copy-paste mistake was present in the original manuscript whenever referring to the uncertainty of $\Delta(\delta^{18}O_w)$, which was falsely given with "±0.5‰". We now give the correct error of "± 1.5‰" (which was already reported in Table T5 in Supplementary Material SI7, and correctly used to calculate elevation uncertainties. We now also present this error in panel b) of Fig. 6.*
- *As elevation uncertainties were calculated with the correct parameter, this mistake has no implications for the paleoelevation calculations, therefore no revision of the stated paleoelevation is needed.*

7) Also, it seems that the lapse rate predicted by the paleoclimate model is significantly higher than the observed modern lapse rates, whereas it is argued in lines 348-349 that the mid-Miocene lapse rates should probably be lower than the modern. Why is this – is it linked to the climatic boundary conditions used in the model (see above)?

- *This is correct. Please also see our response to remark 5): The model from Botsyun et al (2020) does not reflect Miocene boundary conditions. We would expect lower lapse rate value for a warm (and more humid) atmosphere as has e.g. been suggested by Poulsen, C. J. and Jeffery, M. L.: Climate change imprinting on stable isotopic compositions of high-elevation meteoric water cloaks past surface elevations of major orogens, Geology, 39(6), 595–598, 2011.*

8) Finally, while the presentation of the results and their interpretation in terms of paleo-elevation is fairly rigorous (as far as I can judge), the final part of the discussion (section 5.5) suddenly becomes quite vague, arm-wavy and speculative. For instance, it is unclear if the authors are arguing for high elevation in the Lepontine dome or in the Aar massif at 14 Ma**.** It is important to clarify the spatial scale to which the paleo-elevation estimate pertains – and would this number constrain the average or the maximum elevation in this region? I feel this discussion could be improved by integrating the drainage development as constrained by provenance data. As long as there was a direct connection between the Lepontine dome and the studied fans in the foreland basin, the Aar massif could not have been elevated – this is a very important piece information that should be better integrated in the scenario. It has been argued in the French western Alps that the Internal Zone (southeast of the Penninic Front) was elevated substantially earlier than the External Crystalline Massifs (e.g., Fauquette et al., *Earth Planet. Sci. Lett.* 2015); a similar scenario appears to apply to the central Alps from the present data. Making such linkages would help developing a more holistic view of Alpine paleotopography.

- *We relate this criticism to the rather confusing way of how we have structured this part of the discussion. We wanted to make the point that (i) the relatively high elevation for the area surrounding the western margin of the Lepontine Dome (inferred from our data) contrasts to evidence for a low-elevation topography in the area of the Lepontine Dome itself (such as a low sediment supply to the basin following tectonic unroofing), and that (ii) the time with these inferred elevation contrasts coincides with the period when the reorganization of the drainage network started. As a consequence, while the Central Alpine landscape was most likely cylindrical between the Late Oligocene to Early Miocene and was most likely characterized by a regular spacing and a constant relief between the valleys, the post 20 Ma Central Alpine landscape became more complex and most likely non-cylindrical.*
- *We revised section 5.5 ("High (and highly variable) mid-Miocene Central Alps?"; lines 433–446 in revised manuscript) and section 6 ("Conclusions"; lines 508–524) to provide more clarity.*

9) Apart from these main issues, I have a number of more minor editorial comments, which are listed below tied to line numbers. Overall, the manuscript is well written and easy to read. A few references are missing from the reference list and a more generous use of commas could be made.

1 (Title): whereas the manuscript discusses the mid-Miocene paleo-elevation of the Central Alps, there is little discussion of paleo-relief. I would suggest that this is either added more prominently to the discussion (if the data allow constraining some measure of paleo-relief) or the title is modified.

- *We changed the title according to the reviewer's suggestion and deleted "and high relief" from the title.*

22: the acronym SFZ has not been explained at this stage. In general, please try to minimise the use of acronyms as they detract from the reading in exchange for only a limited gain in space.

- *We changed the text according to this suggestion and wrote out the term Simplon Fault Zone.*

36-38: this phrase ("The European Alps are …") seems somewhat out of place here and should be moved or modified / expanded.

- *According to this suggestion we moved this phrase to lines 28–29 in the revised manuscript.*

39-43: this paragraph could benefit from being a bit more specific. Where were the cited paleo-elevation estimates obtained, based on what methods? Also, Kocsis et al. (2007) seems to be missing from the reference list.

- *According to this suggestion we revised this paragraph and added more details (see lines 47–58 in the revised manuscript).*
- *We added Kocsis et al. (2007) to the reference list.*

43-45: this appears a bit like setting up a strawman argument; Hergarten et al. (2010) is a very problematic study that is stained by serious flaws in the reasoning. I do not think this is needed or even appropriate as a justification for the current study.

- *According to the suggestion of the reviewer we removed this phrase and the associated reference.*

69: Handy et al. (2010) appears to be missing from the reference list.

- *We added Handy et al. (2010) to the reference list.*

73: SMB, NAFB – see previous comment regarding acronyms; I don't think these are useful here.

- *We followed the suggestion of the reviewer and reduced the use of the acronyms SMB, NAFB, and SFZ.*

89: it would be useful to add a discussion of the evolution of drainage patterns and the implications for (surface) uplift of the Aar massif to this paragraph, as these will aid in sharpening the discussion in section 5.5.

- *See reply to remark 8).*

118: Fig. 3 is called before Fig. 2 and it would be logical to change the order of these figures.

- *We changed the order of Fig. 2 and Fig. 3.*

135-138: it would be useful to show the stratigraphic levels of the dated bentonites as well as the mammal sites (with their corresponding mammal zone) on the logs.

- *We followed the suggestion of the reviewer and added stratigraphic positions of the bentonites (with their dated ages) and mammal sites (with corresponding zones) to Fig. 2. Furthermore, we added sampling sites of the samples used for carbonate clumped isotope measurements and the magnetostratigraphy used for sample age calculation (see reply above).*

Also, a line of explanation about how a conglomerate in one section can be correlated to a limestone in the other would be welcome.

- *We changed the text according to this suggestion and added a brief explanation about the correlation between the Hüllistein conglomerate and the Meilen Limestone (see lines 164–167 in revised manuscript).*

150-151: see above comment. Also, were magnetostratigraphic analyses performed on these sections? If so, why not show the magnetostratigraphy as well? The age constraints are important here so it would be good to clearly show these constraints on the figure.

- *We changed Fig. 2 according to this suggestion and added the magnetostratigraphy.*

160: “magnetostratigraphy” rather than “paleomagnetostratigraphy”. Also, this was not discussed in section 2, but should have been if such data are available (see above comment).

- *We replaced “paleomagnetostratigraphy” by “magnetostratigraphy” according to this suggestion. Furthermore, we added a brief discussion about the magnetostratigraphic constraints to each of the three sections (see lines 150–153; 167–168; 181–182 in revised manuscript).*

181-183: these two sentences would read a bit more easily if the starting subphrase was moved to the end of the main phrase (e.g., “Ascending air masses undergo adiabatic cooling and rain out with increasing altitude, which leads to …”; and similarly for the following phrase).

- *We rephrased the sentences according to this suggestion (see lines 217–219 in revised manuscript).*

183: add “altitudinal” to “lapse rates” for clarity.

- *We added “altitudinal” according to this suggestion.*

225: see major comment on description of climate simulations.

- *We appreciate this comment very much and give a detailed reply within the major comments. See comment on remark 4).*

230: it is not clear what the "(250 m)" pertains to.

- *In the Botsyun et al (2020) paper the "0 % Alps" topography has the Alps set at 250 m above sea level; hence the "(250 m)" term. We tried to clarify and added "topography set to 250m" (see lines 275–276 in revised manuscript).*

233-234: "enhances assessment of paleoclimate changes" is quite vague – can you elaborate? Is there any data available for such a distant location that could help constraining the model?

- *See comment on remark 5).*

240-242: this phrase doesn't read very well; maybe try a construction with "Although …"?

- *According to this suggestion we rephrased this sentence to improve readability (see lines 290–291 in revised manuscript).*

248-251: is this new or existing data? Can it be shown on the log and/or a separate data figure?

- *$\delta^{18}O$ and $\delta^{13}C$ data for the Fontannen section has been provided by Campani et al. (2012). Δ_{47} based temperatures are from Methner et al. (2020).*
- *We added the origin of the data in the text (see lines 297–298 in revised manuscript) and the figure caption of Fig. 2.*

287: why would the proximal part of the fan be at "more than" 300 ± 100 m above sea level, when it appears that the uncertainties have already been included in this calculation?

- *We changed the text according to this suggestion and deleted "more than".*

290: "(mainly also because of the occurrence of paleolakes …)" is a bit of a mysterious addition to this phrase – either explain this or remove it.

- *We changed the text according to this suggestion and removed the sub-clause "mainly also because of the occurrence of paleolakes …" (see lines 348–349 in revised manuscript).*

293 (and 305): Fig. SI3 could easily be made part of the main paper, which is not very long in any case. Having this figure in the main paper would facilitate assessing this argument.

- *We appreciate this comment and acknowledge the proposal of the reviewer. The aim of our study is to provide paleoelevation calculations for the mid-Miocene Central Alps, which are based on measuring $\Delta(\delta^{18}O)$ between two sites. Pedogenic carbonate $\delta^{13}C$ ratios are not an essential part for these calculations and it is beyond the scope of this study to examine in detail the significance of Swiss Molasse Basin carbon isotope compositions which are driven by complex processes within the soil.*
- *Therefore, rather than including a $\delta^{13}C/\delta^{18}O$ cross plot in the manuscript, we prefer to add the $\delta^{13}C$ data to Fig. 2 (as suggested by the reviewer in remark 3), and furthermore*

provide Swiss Molasse Basin pedogenic carbonate $\delta^{13}C$ values in a separate data table in the Supplementary Material (SI7). We give the $\delta^{13}C/\delta^{18}O$ cross plot in the Supplementary Material (SI5).

305-306: argument c) has not been developed previously and it is thus not clear why using this section location would underestimate paleo-elevations. Please provide an explanation.

- *We changed the sentence and included an explanation according to this suggestion (see lines 365–366 in revised manuscript).*

322-325: the oxygen-isotope data from volcanic ash horizons could be plotted in Fig. 6 for simpler comparison with the data presented here.

- *According to the suggestion of the reviewer we plotted $\delta^{18}O_w$ values derived from volcanic ashes in the Fig. 6a.*

330: Equation (1) appears pretty obvious; it is not clear why this equation is given and not others that are maybe less straightforward (e.g., for the isotopic fractionation of the lapse rates).

- *According to the suggestion of the reviewer we deleted the equation since it is not essential for understanding the paleoelevation calculation (see lines 393–395 in revised manuscript).*

331-353: see major comments on assessment of uncertainties and the model-predicted lapse rate above; these could be discussed here.

- *We appreciate this comment and revised the text according to this suggestion. See comment on remark 6).*

350: please provide the present-day (average or peak) elevation of the relevant area for direct comparison with this number.

- *We added the elevation of the neighbouring peak (Monte Leone with 3553 m.a.s.l.) according to the suggestion of the reviewer (see line 422 in revised manuscript).*

357-359: the comparison between Figs. 5b and 6b is not straightforward and I am wondering whether there would be a more efficient way of showing the model – data comparison?

- *According to the suggestion of the reviewer we revised Fig. 6 and included an additional panel showing climate modeled Swiss Molasse Basin $d^{18}O$ data for the case Alps150.*

364-370: a fairly big interpretational step seems to have been taken here. This section could be rewritten to take a more linear course from the paleoelevation estimates to implications for paleo-topography in the Alps to potential geodynamic implications.

- *We revised section 5.5 ("High (and highly variable) mid-Miocene Central Alps") and moved this paragraph to the introduction (see lines 31–37 in revised manuscript).*

389-393: see major comment on drainage development above: when was the connection between the Lepontine dome and the fans cut off by surface uplift of the Aar massif? This is an important constraint on the evolution of topography. By the way, Bernard et al. (in press) has now been published.

- *See comment on remark 8)*
- *We revised the reference and included the year of publishing.*

398-400: OK here is some of that discussion – this should just be made clearer and stated more upfront.

- *We revised this section (see lines 433–446 and 454–464 in revised manuscript).*

403: whether mean elevation increased or decreased related to extensional denudation of the Lepontine dome footwall would depend on the considered scale: some of the metamorphic core complexes in the western USA stand up to 2 km above their surroundings. The spatial resolution of the paleo-elevation estimate is key here

- *We completely agree with the reviewer here. The elevation of a region undergoing extensional denudation will (amongst other parameters) depend on the rate at which temperature anomalies in the exhumed footwall are being relaxed. Ultimately, the end result of extensional detachment faulting and thinning of (buoyant) continental crust should be a lowering of elevation compared to the pre-extensional stage. It is hence tricky to infer relative elevation differences between neighboring regions undergoing differential amounts of extensional deformation. Given the rather high elevations obtained here and the absence of evidence for low-δD meteoric fluids in mylonites further East (e.g. the Brenner fault zone; see Table 3 in "Mancktelow, N., Zwingmann, H., Campani, M., Fügenschuh, B. and Mulch, A.: Timing and conditions of brittle faulting on the Silltal-Brenner Fault Zone, Eastern Alps (Austria), Swiss J. Geosci., 108(2–3), 305–326, doi:10.1007/s00015-015-0179-y, 2015.") led us to suggest that the overall effect of extensional faulting may have been represented by lower elevations when compared to the Simplon region.*
- *See also reply to comment on line 406.*

406: it is not clear what evidence was provided for "co-existence of regions with different elevations on a small spatial scale …".

- *Our conclusion is based on the estimated paleoelevation (as inferred from our data) of >4000 m for the region surrounding the Simplon Fault Zone (SFZ), which is in close proximity (~45 km to the W) to the Lepontine Dome. For the latter decreased sediment discharge rates were suggested for the same time interval (Kuhlemann, J., Frisch, W., Dunkl, I. and Székely, B.: Quantifying tectonic versus erosive denudation by the sediment budget: The miocene core complexes of the Alps, Tectonophysics, 330(1–2), 1–23, doi:10.1016/S0040-1951(00)00209-2, 2001.). Consequently, we link the decreased sediment fluxes with a low-elevation topography in the area of the Lepontine Dome when compared to >4000 m inferred for the SFZ.*

410-412: again, the key question is the spatial scale on which the paleo-elevation estimate constrains paleo-topography, and what aspect of the topography (mean, maximum?) is actually constrained.

- *δ-δ paleoaltimetry provides constraints on the mean elevation of the catchment of precipitation which falls in the area of the high-elevation site. Within the catchment, runoff collects at the lowest topographic point, and the measured $\delta^{18}O_w$ estimate represents an integrated signal originating from different elevations within this area. In our case, the inferred paleoelevation estimate represents the mean elevation of the mid-Miocene paleo-catchment of the Simplon area.*
- *We clarified this in the text (see lines 405–407 in the revised manuscript).*

426: is the evidence for *uplift* or *exhumation* of the Aar massif at ~20 Ma?

- *We replaced the term "uplift" with "exhumation".*

429-430: it would be helpful to place this number into perspective by quoting the relevant present-day elevation measure.

- *According to the suggestion of the reviewer we included the present-day elevation of this section (see line 509 in the revised manuscript).*

Fig. 2 should become Fig. 3. Labelling each photo individually (a – h) would help identifying the panels.

- *We changed the order of Fig. 2 and Fig. 3 and labelled each photo individually as suggested by the reviewer.*

Fig. 3 should become Fig. 2. This figure should include the chronostratigraphic constraints (age markers, magnetostratigraphy if existent) and stratigraphic names (mentioned in the text). It would be helpful to add the carbon isotope data (using a double scale and a slightly different colour or symbol) as well as at least the locations of the samples used for clumped-isotope analysis.

- *See comment on remark 3). We followed the suggestion of the reviewer and added age markers, magnetostratigraphy, stratigraphic names, and locations of samples used for carbonate clumped isotope analysis.*

Fig. 6: panel (b) needs a legend for the different lapse rates. The green bar indicating the paleo-elevation estimate should take into account the uncertainties in both $\Delta(\delta^{18}O_w)$ and in the lapse rates.

- *We appreciate this comment and revised panel b) in Fig. 6 according to the suggestions of the reviewer as we think that this improves the figure very much.*
- *We very much appreciate the argument regarding the uncertainty in $\Delta(\delta^{18}O_w)$ since this is an essential factor when calculating paleoelevation. Therefore, we give inferred paleoelevations including the full error span in $\Delta(\delta^{18}O_w)$ based on all four isotope lapse rates (Table SI5 in Supplementary Material). For the preferred Swiss Molasse Basin section Jona the uncertainty in $\Delta(\delta^{18}O_w)$ of ±1.50 ‰ (propagated error in stable and clumped isotope analysis) and error of the chosen isotope lapse rate result in a total*

error of ±1.54‰ which equals ±770 m if choosing the lapse rate after Campani et al. (2012).

- *For the purpose of improved visualization we omit the graphic representation of inferred paleoelevations based on all four isotope lapse rates, and show only calculated maximum Δz (m) and minimum Δz (m) according to the preferred lapse rate after Campani et al. (2012).*
- *Changes in Fig. 6:*
    - *We added a green vertical bar indicating the uncertainty in* $\Delta(\delta^{18}O_w)$ *and black horizontal lines clearly displaying the calculated maximum Δz (m) and minimum Δz (m) according to the error in* $\Delta(\delta^{18}O_w)$.
    - *We included a legend to panel b) presenting the different isotope lapse rates used for paleoelevation calculation.*

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