Solid Earth Discussions

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Comment on se-2021-59

Peter van der Beek (Referee)

Referee comment on "Miocene high elevation in the Central Alps" by Emilija Krsnik et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-59-RC2, 2021

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:

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.

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.

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.

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.

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.

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? 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)?

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.

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.

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.

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

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.

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.

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

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

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.

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

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

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.

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).

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).

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

225: see major comment on description of climate simulations.

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

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?

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

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

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?

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.

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.

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.

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

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).

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

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

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?

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 paleo-elevation estimates to implications for paleo-topography in the Alps to potential geodynamic implications.

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.

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

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.

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

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.

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

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

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

Fig. 3 should become Fig. 2. This figure should include the chronostratigraphic constrains (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.

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.