Solid Earth Discussions

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Comment on se-2021-59

Jay Quade (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-RC1, 2021

This is an excellent paper that builds upon the foundations of Campani (2012) and Methner (2020) to reconstruct paleoelevation of the Swiss Alps in the mid-Miocene optimum. This review took me extra time because I had to read those papers. This paper fills in the picture by studying low-elevation paleosols from three mid-Miocene sections from the foreland basin. The dating of these looks exceptional, but for the purposes of assessing the diagenesis history, a clear gap on the paper was the lack of discussion on the burial depths and burial temperature history of the molasse basin from other published sources.

If this were my paper I would add depth of soil carbonate nodules below the top of the paleosol. Depth has a big influence on d13C, d18O, and D47 values . In the general the paper does not record soil carbonate distribution with depth, and how this varies among soils. But they took so many samples that I assume that that got a representative suite from shallow to deep, which serves the purpose of the paper well enough

Some things that struck me about the results, which might be expanded upon in the revised version, was how high the d13C values of the soil carbonates, averaging -2 to -3‰±1‰. In the absence of C4 plants and high pCO2 (neither are indicated for this period), this indicates fairly modest respiration rates and dry desert conditions of formation, typical of sagebrush covered steppe or drier in the Great Basin (see Quade et al 1989 GSA Bull. Systematic variations…). I wonder: are there other indicators of such aridity in the molasse basin of the mid-Miocene, such as evaporites? I am surprised it was as dry as the Great Basin, given the region is so wet today. Perhaps this reflects some strong rainshadow effects, although I would have thought storms came out of the west,

then as now. I find this really intriguing.

This brings me to my chief concern about the paper's conclusion that paleoelevation was ~4200 m. That is: if the setting was that desertic, how can one be confident that evaporation does not influence even the lowest d18O values. I understand that the authors tried to minimize this by using only the lowest 25% quartile. That should help. But were this my paper, I would think this through very carefully, and perhaps be more conservative. Evaporation would expand the difference between isotopic values from low and high elevations and lead to overestimates of paleoelevation. In two papers from 2007, I tried to assess the effects and limits of evaporation on isotopic values from soils in dry climates (there are probably better papers on there on this topic that I am unaware of)

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Quade, J., Garzione, C., and Eiler, J., 2007, Paleosol carbonate in paleoelevation reconstruction, *in* M. Kohn, ed., Paleoelevation: Geochemical and Thermodynamic approaches. Reviews in Mineralogy and Geochemistry, Mineralogical Society of America Bulletin, v. 66, p. 53-87.

Quade, J., Rech, J., Latorre, C., Betancourt, J., Gleason, E., Kalin-Arroyo, M., 2007, Soils at the hyperarid margin: the isotopic composition of soil carbonate from the Atacama Desert. *Geochimica et Cosmochima Acta* 71, 3772-3795.

This manuscript cites the first paper but I am not sure they fully internalized the meaning of the results, because mid-elevation (say up to 2000 m) soil carbonate from the Great Basin is the best analog for carbon isotopes in the molasse basin, and those soils show

pretty strong (but variable) evaporation effects. In short, I come away with the feeling that there was some evaporation effect even in the lowest 25% quartile of d18O values, and therefore that 4200 m is a maximum estimate.

I don't expect the authors to change the manuscript on this point. They are free to disagree. But I urge them to think more carefully on this point and revise the manuscript if they see fit to do so, or not at all.

I was surprised by the really high soil T°C (47) found by the Methner paper; it will be interesting if this can be reproduced elsewhere for the MMO.

Here are a few line-by-line comments and edits

14: omit , however; omit geochemistry

16: sea-level here and elsewhere, where used as an adjective

22: state the range of dD values

76: molasse

96-97: were predominantly composed

103: is the Molasse Sea a formal name? otherwise no caps

114: astronomically tuned

156: What typical depths are the nodules below the top of the paleosol?

205: VSMOW

235: While mostly true, in some settings soil carbonates can form in the cool season, if summer are wet. Huntington's group at Washington has documented this. This scenario should be considered.

242 onward: here and in following paragraphs it is essential to insert VSMOW where referring to d18O values, since in many papers, VPDB is the convention for carbonates This will clear up any confusion.

295: yes, I agree here, but to develop +1.2‰ carbon isotope values requires dry conditions (mid-elevations of the Mojave Desert are good analogs), near-surface depths of nodule formation, or high pCO2. The last is not indicated from mid-Miocene records elsewhere, although perhaps the mid-Miocene optimum should be lookd at more carefully. That leaves some combination of the forst two explanations: moderate, desertic vegetation cover and a mix of soils depths 0-100 cm deep). Quade et al., 1989, 2008 (on paleoaltimetry) and Breecker et al, 2009 are the authoritative papers on this.

300: this covariance is also observed in modern soils (Cerlling 1984) and other papers

305: how do you know the Jona section is the best? Explain. From Fig. 2, the Jona section looks the most variable isotopically, and therefore the most impacted by evaporation.

323: which mineral pairs? Clarify how this was done.

328: no new paragraph?

352: good! Few people know about the Sharp paper, but it is ahead of its time

394: million years