

Solid Earth Discuss., referee comment RC1  
<https://doi.org/10.5194/se-2021-12-RC1>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Comment on se-2021-12

Anonymous Referee #1

---

Referee comment on "Tectonics, Climate and Topography: Oxygen stable isotopes and the early Eocene growth of the Pyrenees" by Louis Honegger et al., Solid Earth Discuss., <https://doi.org/10.5194/se-2021-12-RC1>, 2021

---

I read with great interest the paper by Honegger et al. The introduction is well written, and I agree that the topic of orogenic evolution is important. The authors have framed the problem as a Raileigh distillation exercise, whereby the d18O of carbonates can be used as a proxy for elevation. This is commonly done in continental tectonic using soil nodules (see for instance Huntington et al, and Jay Quads et al). This paper is however using marine carbonates previously shown to contain a record of orogenic effect on d18O, but they do this at a much higher temporal resolution than previously published.

Overall this is an easy to read, paper, and a nice narrative between oxygen and carbon isotopes and tectonics. I like the fact that the authors have independent magnetostratigraphy and that they use these as the only tie points in Analyseries: it gives me confidence that their age model is probably correct (lines 219-220)and that comparison with the global stack of Cramers et al is not a chicken and egg problem where isotope trends are matched first, and then their correlation discussed.

But as an isotope geochemist, I am not fully convinced with the fact that the data actually supports the authors story. The story *\*might\** be correct, but it might not. And many details were omitted, or simply not fully discussed in this manuscript. Therefore I recommend some major revisions on the stable isotope side..

The first block to me being fully convinced is the choice of sample, and the implications that are not discussed. The analytical targets are marine mudstones (Lines 170-175) deposited between a few tens to hundreds of meters. This raises a number of issues.

Perhaps one of the most difficult one is mineralogy. There is no indication that systematic quantitative x-ray analysis was conducted, and that the amount of difference carbonate species has been established. If not, then the authors are blind to what they are

measuring. This is highly relevant, because the acid fractionation factor and temperature relationship of different carbonate minerals vary. So if a mix in mineralogy existed in the fine-mud matrix (as is not uncommon), then some of the discrepancies in the magnitude of the signal could be explained by changes in mineralogy. I also note (line 187) that neither the acid temperature nor the acid correction factor are indicated in the methods, something that needs to be remediated.

For me, the second problem is of course diagenesis. The authors did discuss it somewhat, but I don't think that the question was fully addressed. For instance, on line 240-243 a statement is made that a lack of correlation between  $\delta^{13}\text{C}$  and  $\delta^{18}\text{O}$  can be used as evidence of the lack of meteoric diagenesis. The problem with this statement is that it ignores the work from Lohman in the late 80's, and the concept of meteoric water line. It is well-known that oxygen is more likely to be reset by meteoric processes whilst carbon remains constant unless you reach very high fluid/rock ratios. Thus, the absence of a correlation between  $\delta^{13}\text{C}$  and  $\delta^{18}\text{O}$  is not necessarily a good way to prove the absence of meteoric diagenesis, especially given the low  $\delta^{18}\text{O}$  values.

The large spread in  $\delta^{13}\text{C}$  that the authors mentioned is suspicious, i.e. could this tell us something about diagenesis of the host rock under variable water/rock ratios? Note that the oxygen isotope values are also very scattered, i.e. about 2 permil variation which is significant for marine values. As presented, I am not convinced that I am not looking at a marine trend (the overall trend) but with diagenetic overprinting on the absolute values and the scatter. Petrography, cathodoluminescence and perhaps some trace metal analysis would go a long way convincing me rather than the isotopic values alone.

The third problem in my opinion is that if these mudstones were deposited at water depth differing by hundred(s) of meter(s), then would you not expect a difference in recorded water temperature, and thus  $\delta^{18}\text{O}$  of the carbonates? At the very least, if one counts on the input of freshwater from the Pyrenees to explain the low  $\delta^{18}\text{O}$ , then one problem is how well mixed is the low-salinity runoff waters were with respect to the much higher salinity, epeiric platform water mass at 100-200 meters? I would think the chances of a fresh water lense forming would be high, and that the freshwater signal would be lost as you go deeper in the water column. This could also have implications on oxygenation of the basin. Water depth is a very important factor in the interpretation of the isotopic values, in my opinion.

The authors could also try to support their freshwater mixing hypothesis (320-330) with additional evidence, for instance a change in the faunal assemblage of the microfossils that indicates a decrease in salinity. Are there any studies showing this? Can they see this in their assemblages?

Overall the oxygen isotope curve does look similar in shape to the global curve, which indicates that the global ocean and the epeiric sea were well-mixed (Fig 5). But then if the epeiric sea is connected to the global ocean, I would have expected regional rain effects to create deviations from the global trend (irrespective of the absolute  $\delta^{18}\text{O}$  value) not just the same trend but slightly different (more negative) minimum values. Is it not a strange

coincidence that the global trend is captured, just with a lower  $\delta^{18}\text{O}$  minima and a offset in timing?

The difference in absolute value from Cramer et al will also be due in part to the fact that we are looking at epicontinental seas, with much warmer (essentially surface) waters, rather than deep-sea sediments (line 270). How much could this temperature effect explain the difference in  $\delta^{18}\text{O}$ ?

Overall, my problem I think is that the authors actually correctly identify the complexity of using  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  alone to disentangle processes at this scale, but yet in line 295 they still propose what they call the alternative hypothesis (that the trend is driven by the orogeny) as their chosen one. This hypothesis does appear more strongly supported by the data than the alternative hypotheses.

I believe that the points I raise above need to be addressed convincingly to warrant publication of the model. This will most likely require to either generate or use existing data on the mineralogy of the samples, and ideally their petrography. But I do hope the authors can address these concerns, because their paper is really interesting and if they convincingly demonstrate the validity of their interpretation then this will be a very significant contribution to the field.

Small additional comment:

Line 226: in this context, the term 'fresh' does not apply. Surely you don't mean benthic forams fresh out of the sea? I think you want to say well-preserved?