

Clim. Past Discuss., referee comment RC2
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Comment on cp-2021-79

Anonymous Referee #2

Referee comment on "Clumped isotope evidence for Early Jurassic extreme polar warmth and high climate sensitivity" by Thomas Letulle et al., Clim. Past Discuss.,
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This manuscript describes a compelling dataset of carbon, oxygen, and clumped isotope ratios from late Jurassic fossil shells, one at northern polar latitudes in the Toarcian and another from mid-latitudes in the Pliensbacia. Their sub-period designation is about the only commonality between the samples and the data, yet the authors discuss them in nearly the same breath. For this and other reasons articulated below I am recommending that this manuscript could dramatically improve following careful, major revisions.

The major strengths of the paper are in the quality of the stable isotope data, which by all accounts is very high and uses the most up-to-date clumped isotope methods, correction schemes, and temperature calibration curves. However, I am routinely irked by the habitual reporting of clumped isotope temperatures to the tenth decimal place, which hardly seems to matter when the reported error is several degrees. Beyond this, I fear the manuscript falls into a common trope of being too quick to overlook the possibility of clumped isotope resetting during burial and too ready to extrapolate results across paleolatitudes and Phanerozoic timescales with grand paleoclimate ambition. In their revision I would encourage the authors take a more logical, considered, and even skeptical approach. What if the shells are not as pristinely aragonite as their SEM and Raman data imply? The chalky and fractured nature of some of their samples from the photographs in Fig. 2 calls into question the ubiquity of their SEM and Raman-based conclusions. Similarly, is it possible that the burial temperatures are slightly warmer than the best estimates from the literature? Aragonite clumped isotope bond reordering is complex (relative to calcite and dolomite), poorly understood, and seemingly faster for a given thermal history than calcite (see Chen et al. 2019, GCA). The authors hardly dwell on this fact and its associated uncertainty. Also, might the Polovinnaya River samples be estuarine, and not marine? Terrestrial fossils from the same shale exposures indicate that it might be a possibility, or at least one that needs detailed recognition even if it is not the preferred interpretation. An estuarine or non-marine origin might not impact the importance of their clumped isotope paleotemperatures, but it complicates the calculated water oxygen isotope ratios in ways that are interesting and not exclusive of comparisons in Fig. 5a.

Finally, in the text and in Figure 5 there is a casualness with comparing datasets over nearly 150 million years of the Mesozoic and Cenozoic, with dramatically different global paleoclimates and continental configurations, that makes the discussion hard to follow. For example, there is a large leap between the Early Jurassic and the Early Eocene on lines 254-259 that converts latitudinally ambiguous precipitation oxygen isotope ratios from Eocene proxy datasets to the calculation of paleosalinity during the Early Jurassic arctic. The leap is so large that it seems to obviate their point. In instances like this (see below for more line-specific commentary) I would encourage the authors to stick with data-model comparisons and well-reasoned hypotheticals. This reframing would still allow for the multi-period comparisons shown in Figure 5 with an edited discussion that better conforms to the study motivations outlined in the abstract and introduction.

Line-edits:

17-18 – The connection with the previous sentence is not very clear; why the distinction in time interval?

19 – “mildly buried” is a confusing term.

24 – Correct “highlight” to “highlights”

36-38 – The authors could elaborate on this statement for better effect, I think. It may not be obvious to all readers how clumped isotopes are sensitive to burial.

39 – “ante-Cretaceous” is an uncommon phrase.

43 – Regarding “marine carbonate shells”, there is some ambiguity on their marine origin in the discussion and I think the authors should specify that they are aragonitic fossils. This is important for two reasons: 1) aragonitic is exceptionally susceptible to geochemical alteration by conversion to calcite and 2) the bond reordering kinetics for aragonite are such that they are more prone to 'solid-state' clumped isotope change than calcite or dolomite.

60-61 – “Exceptionally low burial” might be hyperbole. A T_{max} of 420 °C is indeed immature, but not exceptionally so.

61-66 – It would be useful for the authors to commit to a maximum burial temperature. Using models of bond reordering and a simple burial history curve, it would be possible to

estimate the possible change in clumped isotopes due to burial heating alone.

Figure 2 – Samples (a), (b), (d), (i), (j), (k), and (l) all *look* too weathered or fractured to demonstrate that they retain primary shell material. Later it is revealed that (i) is not aragonitic (Fig. 3h). How are the authors able to admit that (i) is not aragonite, but call (e), (f), or (j) “pristine”? Each of these samples have the same coloration in these images.

102-103 – Regarding the white color, this chalky appearance can indicate shell alteration (mineralogical conversion or geochemically).

Section 3.2. – There are subscript and superscript formatting errors throughout this section.

158 – Was the prismatic layer avoided when microsampling the shells?

Figure 3 – Why are the Raman spectra truncated $<200\text{ cm}^{-1}$ for (b) and (d) and not for (f) and (h)? Also, it is not clear here (or in the text) how the position of these images and spectra relate to the subsamples for isotope analysis.

194-196 – The temperature ranges cited are canonical values for calcite, not aragonite.

203-204 – Regarding the possibility that small shell-water interaction has been shown to change clumped isotope ratios with only modest change in oxygen isotopes, what might be specific, independent evidence that this sort of thing had occurred (or not) in these shells?

204-206 – I think the implication here, subtly, is that $31\text{ }^{\circ}\text{C}$ is something like a maximum burial temperature. Given that this is a fracture-fill carbonate without any other paragenetic sequencing context, it is equally possible that it's an exhumation temperature (i.e., a temperature experienced during fluid infiltration after maximum burial was reached).

219 – I think it is notable that this range narrows considerably after removing the coolest temperature. The remaining 7 of 8 shells have an average of $\sim 15\text{ }^{\circ}\text{C}$.

230-231 – Discussion of Jurassic food availability during the polar night, without any

additional information, is entirely speculative and is too extrapolative from their isotope dataset.

243-237 – As mentioned above, this is an apples-to-oranges comparison. The similarity in SST between two shell populations separated in time by over a 100 million years might be entirely coincidental.

257-259 – Also as mentioned above, I don't understand the relevance of Eocene high latitude precipitation values here. Who knows what they were in the early Jurassic?! As the authors show in Fig. 5, the modeled Jurassic poles were warmer than modeled Eocene poles, yielding lower latitudinal gradients in precipitation oxygen isotopes (see dashed lines for Eocene and Cretaceous data).

266-268 – The authors should reframe this statement to consider an alternative scenario in which this locality and these shells are not marine at all. What if -4.9 to -2.5 are estuarine or mostly freshwater oxygen isotope values?

Figure 5 – Are the model results new or replotting of published results? If it's the latter than proper attribution needs to be clear in the figure or the figure caption.