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Interactive comment on “The bivalve *Glycymeris planicostalis* as a high-resolution paleoclimate archive for Rupelian (Early Oligocene) of Central Europe” by E. O. Walliser et al.

Anonymous Referee #2

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Walliser and colleagues present 40 years of high-resolution intra-annual stable oxygen isotope data from fossil bivalves to help constrain paleoenvironmental conditions during the early Oligocene, an interval that the authors effectively argue to be an excellent analog for the near future (southern hemisphere ice only, somewhat higher CO₂). Fossil shells are well preserved, beautifully prepared, and copiously sampled, yielding impressive records of seasonal isotopic variation reflecting temperatures experienced by the organisms during their ontogenies. Water composition, so as to allow for temperature calculation, is estimated from the composition of marine mammalian phosphate in the same unit. The data are compared to those from other roughly coeval taxa from the same region, and with modern semi-restricted marine settings.

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While I continue to be captivated by these sorts of datasets, I am nonetheless skeptical of the degree to which you can interpret the data meaningfully in a broad paleoclimate context. The problem is that these data come from only one time slice and one place (and one taxon as well), and it is very unclear how to compare such results with data from other places, times, and taxa – for many of the reasons acknowledged in the manuscript. There are so many potential ways to make comparisons spurious, be it water depth, stratification, salinity, sampling resolution, season of shell accretion. . . all of these will affect seasonal extremes in isotope values, and none of them are what you want to actually study. Difficulty of comparison on equal footing is particularly true when fossil proxy data are compared to modern instrument records for SSTs. There is no way to know whether you are comparing apples to apples. This is the frustration of studies that generate data like these which are isolated in time. I find it much more useful to have the same type of data arrayed in space or time so that spatial or temporal patterns can become evident within an internally consistent dataset. Trends in the same kind of data from the same kind of environment are more robust than comparisons of snapshots of one kind with snapshots of another kind. E.g., does central Europe get less (or more) seasonal during the course of the Oligocene? Is seasonal range higher in the north than the south? By how much? Gradients are easier to compare directly with modern data, even if the mean values are not immediately comparable.

The paleotemperature estimates presented here hang upon several assumptions, not the least of which is the longstanding frustration of all oxygen isotope paleothermometry, that of the unknown composition of water. The authors argue that they can recover a reasonable estimate of water composition and that it does not vary throughout the year. There is a substantial degree of uncertainty here, which they acknowledge, but the final analysis just moves forward and doesn't really encompass or deal with that uncertainty in a systematic way. Estimating $\delta^{18}\text{O}$ from the composition of sirenian phosphate is a creative solution to the water problem, but I was unable to access any of the references cited – the cited Tutken equation was only published in a thesis, in German.

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Sirenians were not likely to be living/precipitating in the same water as the bivalves (nor were they collected from the same locations), as the former are in shallower water (they are benthic feeders) while the bivalves are benthic with estimated paleodepths around 30-40m, and so likely farther offshore. Sirenians would have experienced any freshwater lens in a more pronounced way than the bivalves. I also do not know the timeframe over which sirenian teeth mineralize, and so don't know how much that value could be biased from an annual average (though the reported values are fairly consistent, so encouraging). The dw estimated from sirenian teeth is -0.9 per mil, implying that the water was somewhat brackish (as global Oligocene average values are closer to -0.5). Given the 'extremely proximal' paleoenvironment and estimated dw, runoff likely contributed to the salinity of the water and salinity was likely variable throughout the year as well, as acknowledged on pg. 4088. This degree of uncertainty could shift mean temperatures by several degrees and seasonal extremes by more. Agreement with other published estimates is not enough to alleviate concerns, particularly when the published range is so great (top p. 4091); the authors argue the same later, beginning at the bottom of pg. 4101. However, Fig 7b does show good general agreement in the mean values, moreso than is apparent from the text – this should be emphasized a bit more. Is there reason to believe that similarity is more than coincidental, given the assumptions the other authors had to make as well? The seasonal range of under 4 deg C though is quite low for a mid-latitude Oligocene site, given similar published ranges from the early Eocene at lower latitudes (e.g., Sessa et al 2012).

I found myself still somewhat unclear about the main purpose of the paper after reading through it. Is the main goal to present Glycymeris as another potential skeletal archive of paleoclimate information, or to present a rigorous analysis of Oligocene climate? This should be clarified up front, and the text adjusted as appropriate to reinforce that goal. The first is certainly within range of the current manuscript, though perhaps not quite as interesting or broadly relevant as the second. The second, however, is a much more difficult proposition, and I am not convinced of the conclusions in this regard, for reasons outlined above.



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Regarding claims about interannual variation - based on the abstract, I was expecting to see a numerical analysis of interannual data to show decadal scale oscillations. But nothing was provided in the text other than the statement that seasonal ranges ‘seem to have changed periodically’. If this is all there is to it, and pattern cannot be substantiated statistically or otherwise, then it doesn’t merit appearing in the abstract. I agree it is interesting, and I have seen similarly suggestive patterns in other bivalve datasets, but that’s about all you can do with it unless there are more years in the data. I don’t think the authors should make so much of that observation. Fig 6 is perhaps not warranted.

One option to increase the power of that apparent pattern might be to at least demonstrate that the variation in seasonal range is non-random – that there are long runs of decreasing or increasing something (be it summer, winter, range, etc) that cannot be explained simply by randomly variable years. In other words, the climate system has memory. Perhaps there is some useful comparison to be made with modern shelf water temperatures in this regard. How variable are coastal temperatures from year to year in, e.g., the North Sea today, and is the pattern of variation similar in terms of amount of memory?

Another perhaps more satisfying but time intensive possibility - might there be a correlation between detrended annual increment widths and isotope values (seasonal ranges or extremes or annual temperature means) that could allow the authors to increase the number of consecutive years in their study? If width can be correlated to some climate variable in the 40 years for which there are isotope data, then there would be the potential for a longer time series if additional increments and potentially additional shells are measured. Shell records could be strung together to produce a single long composite record as well. The potential for periodic climate variation could then be statistically assessed. I’m not sure what the anticipated period driver might be – some proto NAO perhaps – but at least there would be a concrete test to compare modern pattern with the Oligocene pattern. Whether it is there or not should provide some

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insight, either way, about the Oligocene condition that would be directly comparable to today.

In the end, I am unclear what exactly we learn from this study as it stands about how the Oligocene compares to today. These are beautiful data, but I am unsure how to interpret them, given that I do not know to what I can meaningfully compare them, nor do I have confidence in the value and constancy of the assumed water composition, and hence temperatures. I recommend revisions to clarify the main point of the paper, tone down claims about interannual periodicity, and more fully address uncertainty associated with assumptions about water composition. If this is to be primarily a paper about Oligocene conditions, a broader context in which to discuss the results would be useful.

Some line-by-line comments are provided below:

I'd advocate plotting isotope data versus distance rather than sample number, unless the samples are equally spaced throughout. The stated 100-200 microns could be a fairly big range depending on how wide the growth increments are. It will make it easier to interpret changes in calcification rate through the year.

Were these shells sampled in the hinge or on the main valve? Not clear – can affect temporal sampling resolution.

A picture of the shell and a growth curve, might be helpful. The references to the taxon are in hard to access publications or in theses, and so the species can't be easily assessed.

p. 4092 – there are lots of references regarding Mn and other techniques for assessing preservation – cite a few of them (e.g., Grossman)

p. 4097 line 26 – 'exhibits' has extra 's'

pg 4099, line 10 – awkward wording; line 22 - Dorman and Gill 1959 is not cited in the references; line 28 – maybe more likely to simply dissolve, no?

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p. 4100, line 19 – see papers by Jones, Arthur etc on Mercenaria for a well-documented example of this

p. 4103, line 18 – this is an odd transition - the previous paragraphs are good, and clearly outline why comparisons among taxa are difficult to make - why then are the bivalve temps more robust than the rest, or the aggregate? needs a transition - maybe all estimates are robust for those taxa and individuals, but not for the goal of estimating actual SST (or benthic T) in one place all year round line 21 – not clear where these places are geographically in comparison – include them on the map, and include paleolatitudes line 25 - reverse the order of reported temps to make order the same as the Lebanon data line 26-27 - awkward wording - what is 'these thermal regimes'? Lebanon is considered subtropical here, yes? clarify

p. 4104 – I'm not entirely sure what the point of this paragraph is – it's all a pretty good stretch if precipitation is that high, because it's likely then that salinity was variable. The rationale is difficult to follow - are we comparing Olig data to places with similar basin configuration/stratification, similar latitude, or similar precip regime? All will introduce variation. A more expanded discussion of each type of comparison and what you might learn from it would be easier to follow. Similar basin types tell you that bottom water temps reflect air temp (yes?). Germany temps are more similar to France temps than to Lebanon, but both are quite a bit lower in latitude - be explicit about this and what you learn from it. Is there even a reason to mention Lebanon temps if they don't compare favorably? Then say how Oligocene Germany is thought to differ from France - it is wetter. What does this do to your expectations?

p.4106 line 3 – Sr/Ca is likely growth rate dependent – see work by Rosenthal; the end of this section is a weak way to end your discussion

The word 'noteworthy' is overused, and occasionally misapplied; 'faithfully' is also occasionally used in an odd way, e.g., 'temperatures were faithfully reconstructed' – this is difficult to determine!

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The genus and species names are not used correctly – the genus has a worldwide distribution, not the species

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Interactive comment on Clim. Past Discuss., 10, 4085, 2014.

10, C1932–C1938, 2014

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