

Dear Dr. Dutton,

We would like to thank all three referees for their helpful comments that further increased the strength and clarity of the manuscript. We have followed almost all suggestions and revised the paper accordingly. In the following, we provide details how we have addressed the issues raised by the reviewers.

Anonymous referee #1 (AR#1)

“... the discussion would have benefited from some hypotheses about what drives the fall growth stop. Given the annual temperature range reconstructed from $\delta^{18}\text{O}_{\text{bivalve}}$, i.e. temperature between 13.6 (winter average) and 17.3°C (summer average), it is quite unlikely that thermal stress (extreme temperatures) could explain this growth stop. I’d rather suggest that this growth stop could be formed during the main gametogenesis period of the year as this represents a high metabolic demand (energy can therefore not anymore be allocated to shell growth).”

Yes, most likely annual growth line formation in this species is indeed related to the reproductive cycle. We have expanded the discussion in section 4.2 accordingly.

*“I am also wondering why the authors did not use the structural information archived in the shell in the form of growth increment width. Measurement of growth increment width in *G. planicostalis*, followed with ontogenetic detrending, would have been useful to confirm the hypothesis of a quasi-decadal oscillation in extreme temperatures. I understand that it is difficult to sample carbonate all along the outer shell cross-section, from umbo to ventral margin, and that temperature cannot be reconstructed for the whole lifespan of the animal. But extreme temperatures can also lead to changes in growth increment width. The construction of SGI (standardized growth increments) chronologies, spanning the 67–84 years of growth of each specimen, could reveal oscillations related to quasi-decadal climatic oscillation. I think this is important information, easily available, that could definitely strengthen the hypothesis of quasi-decadal climatic oscillation in the late Rupelian.”*

This is exactly what we are currently doing in close collaboration with a numerical climate modeler: analyzing SGI chronologies and comparing them to high-resolution climate models. For this purpose, we are using a much larger number of shells than in the present study. According to preliminary data, shell growth fluctuated on time scales of ca. 6 to 8 and 10 to 14 years possibly indicating the presence of some paleo-NAO (also see comment by AR#2). However, we think it would be beyond the scope of this paper to include this aspect here.

We therefore decided to lower down our tone on decadal-scale variability and rather speak of inter-annual differences in seasonality.

Line 15, page 4097: the average $\delta^{18}\text{O}_{\text{water}}$ value calculated from sirenian tooth enamel is -0.9 ‰. A comparison with modern value would suggest that such water was typical of subpolar settings or the current Baltic Sea. But you state page 4090 (lines 24–29) that the Mainz Basin

had an overall warm climate comparable to modern subtropical climate zones of the Mediterranean during the Oligocene.

Today, these environments have a $\delta^{18}\text{O}_{\text{water}}$ value close to 1.5 ‰. How could you explain the difference between this value and the one you calculated from tooth enamel?

As we wrote in section 4.3, based on fossil assemblages, the climate of the Mainz Basin was similar to that of the modern Mediterranean Sea (= warm). This does not imply that all environmental parameters (salinity, $\delta^{18}\text{O}_{\text{water}}$) were the same. In contrast to the modern Mediterranean and Baltic Seas, the Mainz Basin experienced a strong exchange with the open ocean (here: the Nordic Seas). As a consequence, other than in the modern Baltic Sea, freshwater influx through rivers – despite higher precipitation rates ~30 Ma ago – did not have a strong effect on the isotope signature of the Mainz Basin; likewise, evaporation rates were lower than in the modern Mediterranean. This likely explains why the $\delta^{18}\text{O}_{\text{water}}$ value of the Mainz Basin (-0.9‰) was only 0.4‰ more negative than that of the open ocean during the Early Oligocene (-0.5‰). It should be added that the fauna was indicative of a fully marine environment with very little seasonal salinity fluctuations (including corals; information was added to the text).

Lines 12–13, page 4087: apart from the coasts of the Baltic Sea, I cannot see any other "densely populated coastal areas and ecosystems in Central Europe". I think the coasts of Western Europe must also be mentioned as they are much longer than the Baltic shoreline of Central Europe.

We agree and changed “Central Europe” to “Europe”.

*Line 17, page 4088: although I am no specialist of *Glycymeris planicostalis*, I hardly imagine a worldwide distribution for this species. All actual *Glycymeris* species are only restricted to relatively "narrow" geographic areas.*

Yes, that is true. The manuscript has been modified accordingly (genus *Glycymeris* instead of a particular species).

Page 4089: I don't see any sound reason for excluding the section "study area" from the Material & Methods section.

The manuscript has been modified accordingly. Section 2.1 = Study area

Lines 19–23, page 4089: It is relatively weird to me to read the main conclusion of the paper at the very end of the introduction. This should be deleted.

Done.

Lines 19–22, page 4090: replace "nannoplankton" with "nanoplankton"

The manuscript has been modified accordingly.

Lines 4–5, pages 4091: precise what you mean with "surface waters" (upper first meter? upper 10 m?) and "bottom waters" (what depth?).

In this sentence, we report data from the literature (recalculated with our $\delta^{18}\text{O}_{\text{water}}$ value). In the original publications, water temperatures were reconstructed from shark teeth and referred to as surface temperatures. As discussed in the text (page 4102, line 13) it is difficult to constrain the water depth in which sharks formed their teeth. However, the shark teeth belonged to a non-bottom dwelling open water species. Therefore, they have most likely recorded temperatures of the upper water column. Hence, we substituted “surface water” with “shallow water”.

The bottom water temperatures came from $\delta^{18}\text{O}$ values of foraminifera recovered from basinal sediments of the Mainz Basin (Bodenheim Formation). Water depth estimates suggest a maximum water depth of 150 m (Grimm 1994). According information was added to the text.

Lines 19–20, page 4091: According to Figure 1, it actually seems that your fossils come from the paleo-coastline of the Upper Rhine Graben, and not from the southwestern shore of the Mainz Basin.

The studied shells, originate from the outcrop “Trift” near Weinheim, which is the stratotype of the Alzey Formation, the costal facies of the Mainz Basin. In Figure 1B, we added the boundary between the Mainz Basin and Upper Rhine Graben.

Line 2, page 4092: delete "of" before Glycymeris.

Done.

Lines 11–16, page 4093: where were the carbonate samples drilled in the shell? You sampled the equivalent of 10 to 16 years of growth whereas the specimens lived up to 84 years old. I guess you didn't sample the shell for oxygen isotope analyses close to the ventral margin, i.e. you didn't sample the last 10–16 years of shell growth. I would rather think that you sampled the ontogenetically youngest years of shell growth, i.e. the shell portions close to the umbo region. Please mention it.

Samples were taken from the ventral margin. Sampling was performed in the ontogenetically youngest shell portions. Text has been changed accordingly.

Lines 7–8, page 4094: you must provide the reader more information about the method you used to get this 0.3°C accuracy in your temperature reconstruction. Which statistical descriptor did you use?

Error propagation method was applied. We averaged the precision errors of the mass spectrometric analyses of both the bivalve shells and sirenian teeth samples. Furthermore, the standard deviation of the reconstructed average $\delta^{18}\text{O}_{\text{water}}$ value was also included in the calculation of the error. Altogether, the combined temperature uncertainty is equal to $\pm 1.3^\circ\text{C}$. The manuscript has been modified accordingly, and this information was added at the end of chapter 2.3.

Line 17, page 4095: remove bracket before Coplen.

OK

Lines 20–23, page 4098: you should remove these sentences as your time-series is definitely too short to identify any decadal oscillation in your records. I think all discussion about this periodicity is purely speculative.

The manuscript has been modified accordingly.

• Line 17, page 4105: salinity must be expressed without unit (PSU, ‰, g/L). It is a dimensionless number. Remove PSU.

OK

Figure 2: please add information on the different photographs about their orientation.

Figure 2 has been modified accordingly.

Anonymous referee #2 (AR#2)

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 reviewer addresses a number of fundamental issues associated with ultra-high-resolution deep-time climate reconstructions. First of all, such studies are still relatively rare, but will most likely move into the limelight of paleoclimate research, because climate varies significantly on short, i.e. seasonal to multi-decadal time-scales. If we want to understand how the future climate will likely change on time-scales relevant on a human perspective, we need to increase the temporal resolution of proxy data and numerical climate simulations. For an excellent article that illustrates why paleoseasonality data are extremely relevant for paleoclimate research and that such data are recorded in accretionary skeletal carbonates (such as bivalve shells) the interested reader is referred to Ivany (2012).

We strongly agree with AR#2 that it is necessary to extend this dataset in both space and time (and cross-check with information from other archives) in order to better understand the climate patterns of the Oligocene. Indeed, this is what we are ultimately aiming at. However, this is not in the scope of the present paper. We understand that the goals of our paper need to be defined more precisely. In the revised manuscript we explained that we first needed to lay the groundwork for ultra-high resolution paleoclimate reconstructions in the Oligocene, because there are currently no paleoseasonal archives available from that time interval. Furthermore, we emphasized more clearly that *G. planicostalis* is a suitable proxy archive for such studies. Shells are extremely well preserved, grew during both seasonal extremes and recorded the ambient environment over many years.

As far the reliability of bivalve sclerochronological archives is concerned: undoubtedly, existing paleoclimate archives have collated a tremendous body of knowledge on climate cycles and trends in the past (e.g. Zachos et al., 2001). Yet, there are intrinsic limitations associated with the most frequently used archives. For example, temperature reconstructions based on foraminiferan test mostly rely on bulk analyses of several specimens (e.g. Lear et al., 2000; Billups and Schrag 2002). In the case of fossil foraminifera, it remains difficult to determine during which season of the year the tests were formed, how much time is represented by each shell (days, weeks) and at which water depth each shell was formed. This precludes “comparison on equal footing” with instrumental records. Most certainly, the specimens used in such bulk samples were not alive during the same year. In addition, the chemical composition of a single foraminifera test is extremely heterogeneous (e.g. Eggins et al., 2003). Paleoclimate data reconstructed from foraminifera are thus strongly time-averaged. The biggest advantage of using bivalve shells is the possibility to infer seasonal environmental amplitudes and extremes over a coherent time interval of several years or decades. A resolution of 20 samples per annual increment (= growing season) resulted in a temporal resolution of about two weeks per sample. The resampling technique ensured that similar time slices were compared to each other. In some species, the amount of time represented by each isotope sample can be further constrained by using daily microgrowth patterns. That way, a “comparison on equal footing” with instrumental records is possible.

Concerning the difficulty of interpreting the $\delta^{18}\text{O}_{\text{shell}}$ data: bivalves are sessile organisms that record environmental conditions at a given depth throughout lifetime (like a buoy). Other than many planktonic microfossils, the water depth can be estimated from the fossil assemblage. The rich benthic assemblage and the occurrence of corals suggest a water depth of 30 to 40 m for strata from which the (autochthonous) shells of *G. planicostalis* were collected (this information was added to the revised manuscript). Furthermore, these faunal elements are indicative of fully marine conditions without significant seasonal variations of salinity (and $\delta^{18}\text{O}_{\text{water}}$). At 30-40 meter depth, seasonal changes in salinity and $\delta^{18}\text{O}_{\text{water}}$ are much less pronounced than in the upper water column. Even in restricted environments which are strongly influenced by riverine discharge (e.g. Gulf of Trieste), $\delta^{18}\text{O}_{\text{water}}$ values remain stable over space and time below ca. 20 m depths (Stenni et al. 1995). Periods of stratification in the Mainz Basin have been recognized by Pross and Schmiidl (2002). However stratification only affected the deeper part of the basin, where the organic-rich pelites of the Bodenheim Formation were deposited, but not the shallow, well-mixed water bodies (Alzey Formation). We admit that our text was misleading and not precise enough.

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.

... 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 reconstruction of past $\delta^{18}\text{O}_{\text{water}}$ values via the oxygen isotopes composition of sirenian teeth is a well-established method. Many studies using this technique were published in high ranked journals (e.g. Clementz and Sewall, 2009). It is not the scope of the present paper to present a systematic discussion on the efficacy of this method. Like other large mammal teeth (e.g. Kohn, 2004), sirenian teeth are expected to mineralize continuously in a time frame of a year up to multiple years. Thus, they should record annual to multi-annual average $\delta^{18}\text{O}_{\text{water}}$ values.

We are aware of factors that could bias the annual average $\delta^{18}\text{O}_{\text{PO}_4}$ values such as freshwater or physiology. For these very reasons, we used an average value of specimens from different localities.

Estimating dw 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.

Another $\delta^{18}\text{O}_{\text{PO}_4}$ -to- $\delta^{18}\text{O}_{\text{water}}$ relationship for sirenians was published by Lécuyer et al. (1996). However, these authors only plotted the data without giving an equation. Furthermore, they

only used four $\delta^{18}\text{O}_{\text{PO}_4}$ values (three from the extant *Dugong dugon* from Djibouti and one from the extinct Arctic steller's sea cow; using an extinct species for calibration is not ideal) and merely assumed a regional $\delta^{18}\text{O}_{\text{water}}$ value (with an assumed error range of $\pm 0.5\text{‰}$). We therefore prefer to use the equation by Tütken presented in his PhD thesis (<https://publikationen.uni-tuebingen.de/xmlui/handle/10900/48514?locale-attribute=en>, page 132, Fig. 73). This equation ($\delta^{18}\text{O}_p = 0,86 \times \delta^{18}\text{O}_{\text{H}_2\text{O}} + 20,23$; $r^2 = 0,97$; $n = 10$) is well defined and based on the measurements of ten specimens (wild and kept in captivity) of the two extant species *Dugong dugon* and *Manatus manatus* from 4 different marine and fresh water settings covering a 10‰ range of ambient water values. For the low $\delta^{18}\text{O}$ value end member, a manatee from the Tiergarten Nuremberg, the local well water was measured, while for the other specimens ambient water values were taken from published measurements of the according water bodies in which the sea cows lived.

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.

The accompanying fauna of the studied shells clearly indicates fully marine conditions, not brackish. Furthermore, the presence of corals in that environment suggests that stenohaline conditions prevailed throughout all seasons. Even dinocyst assemblages, on which salinity reconstructions are based, clearly indicate fully marine conditions (Pross and Schmiedl, 2002). We have expanded the text in the description of the facies. Despite sirenians lived in much shallower water than the studied bivalves, the oxygen isotope values of their teeth were only 0.4 per mil more negative than the open ocean. During the Early Oligocene, the Mainz Basin/Upper Rhine Graben System was connected to open marine basins to the north and west (paleo-North Sea Basin and Paris Basin). Furthermore, based on fish communities, a connection to the Western Paratethys has been postulated (e.g. Pharissat and Micklich, 1998). A connection to the south (Tethys) is still under debate (e.g. Picot 2002, Grimm, 2006; Spiegel et al 2006). Sedimentological data indicate siliciclastic transport and water exchange between the different basins and the open ocean during the Rupelian (see Sissingh, 2003, for a schematic reconstruction of paleo-currents). All this precludes brackish conditions in the Mainz Basin (Berger et al., 2005a, b).

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 only way to test if our data are reliable or not is to compare them with published data. Temperatures reconstructed from foraminifera tests and shark teeth yield larger temperature ranges than *G. planicostalis* for reasons given in the text. The fact that our reconstructions lay within previously reported ranges further substantiates their reliability.

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).

The actual reconstructed temperature range is about 10°C, whereas 4°C is the average seasonal range of the resampled data. Mathematically resampled data smooth out seasonal extremes and should only be used for inter-annual comparisons. In Sessa et al. (2012), values were not resampled.

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.

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 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.

We agree that such ultra-high-resolution proxy data can only play off their full potential if combined with appropriate numerical climate models. For the studied time interval and region such models do not exist, but the current study is part of a larger research project that tackles this very question.

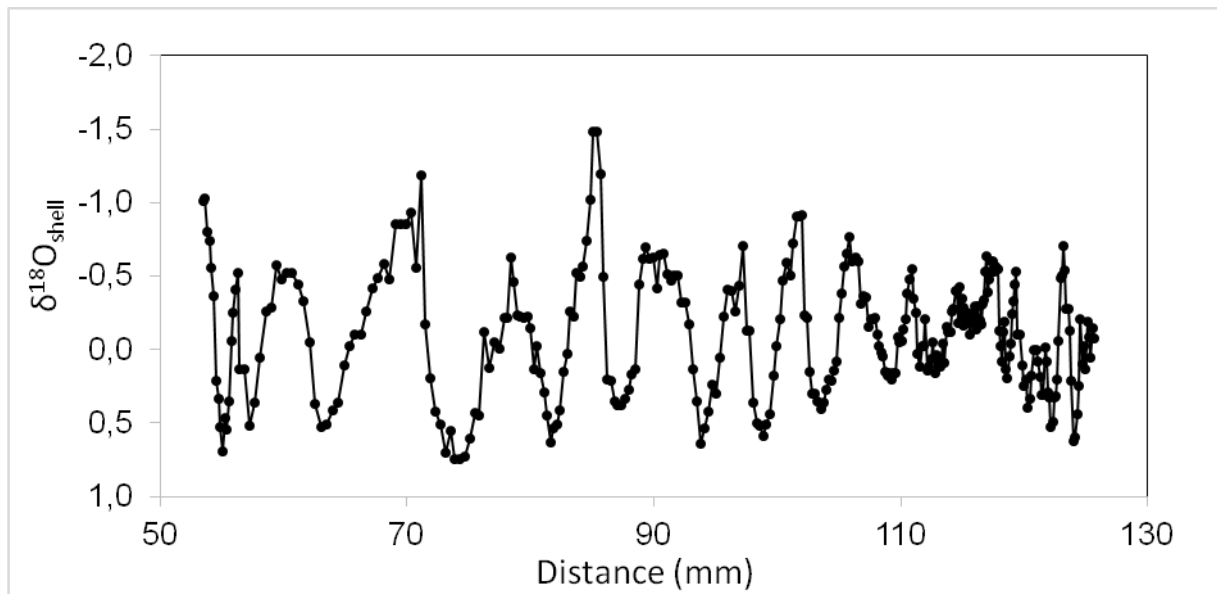
We also understand that the goals of this paper need to be better constrained. This has been done in the revised version. We refocused the manuscript on the evaluation of *G. planicostalis* as $\delta^{18}\text{O}_{\text{shell}}$ -based proxy archive for seasonal temperature oscillations in the Oligocene. Furthermore, we excluded any discussion on decadal climate oscillations from the revised manuscript. In the end, we showed how to $\delta^{18}\text{O}_{\text{shell}}$ -derived temperatures can be used to refine previous climate reconstructions based on other proxy archives, which have a much lower temporal resolution.

As for the suggestion to compute how SGI values and (re-sampled) isotope-derived temperature extremes are correlated, we do not think this will result in meaningful data. For a robust statistical analysis of how growth and temperature are related to each other, our isotope time-series are not long enough. As demonstrated by a recent study by Marali & Schöne (2015), the relationship between shell growth and temperature is often non-stationary. At times, the correlation can be strong and positive, at other times zero or even negative. To evaluate how the correlation evolves through time, longer time-series are essential. Also, the response of shell growth to temperature can change during ontogeny. Furthermore, as demonstrated by modern sclerochronological studies, temperature is not necessarily the main driver of shell growth rate. Food availability and food quality are often much more important.

Despite these considerations, we have done some regression analyses. Results partly meet our expectations: Only specimen #7 (ontogenetic years 6 to 15) shows a statistically significant ($p < 0.05$) positive correlation with summer ($R^2 = 0.63$) and winter temperature ($R^2 = 0.23$). If only the ontogenetic years 6 to 15 are considered in the regression analysis, specimen 2 also shows a strong ($p < 0.05$) positive relationship with summer ($R^2 = 0.43$) and winter temperature ($R^2 = 0.52$). The correlation in specimen 4 is not significant and very low.

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.

The aim of Fig. 4 is to show how seasonal oscillation varies from year to year. We decided to plot the data versus sample number because this representation style helps to make the inter-annual comparison easier. However, when the data were plotted versus distance, seasonal cycles would become increasingly narrow and difficult to discern (see below).



Distance plot for specimen EOW-MB-Wht-2.

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.

The shells were sampled in the ventral margin. We clarified this in the revised manuscript. In addition, we added a photograph of the entire shell to Fig. 2.

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

Grossman et al. (1996) has been added.

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

OK, deleted.

pg 4099, line 10 – awkward wording;

Sentence has been rephrased.

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

We removed the reference.

pg 4099, line 28 – maybe more likely to simply dissolve, no?

Dissolution and re-precipitation is only one way to transform polymorphs.

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

The manuscript has been modified accordingly.

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.

The manuscript has been modified to address this issue.

line 21 – not clear where these places are geographically in comparison – include them on the map, and include paleolatitudes.

The Map (Fig. 1) shows the paleogeographical settings of Europe during the Oligocene, not the modern configuration. Adding the position of the two modern localities would make things more difficult to understand.

line 25 - reverse the order of reported temps to make order the same as the Lebanon data

The text has been modified accordingly.

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?

The aim is to demonstrate how temperature information from the shell compares to previous paleoclimate reconstructions. Previous reconstructions stated that the paleoclimate conditions of the Mainz Basin were similar to areas in the southeastern, subtropical Mediterranean Sea. Our results demonstrate that the seawater was cooler than previously assumed/reconstructed. The temperature yielded by the fossil shells show that temperatures were similar to the modern northwestern Mediterranean Sea. It would be beyond the scope of this study to reconstruct the hydrological dynamics of the Mainz Basin.

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

There are several studies indicating that the incorporation of Sr (and other trace elements) into bivalve shells is controlled by a variety of different variables including biological fraction (vital effects), salinity (Wanamaker et al., 2008) etc. As demonstrated for *Arctica islandica* by one of us (BRS), after proper mathematical elimination of inherent age-trends (or growth rate related effects on Sr/Ca levels), a significant correlation exists between Sr/Ca and ambient water temperature (Schöne et al., 2010). Of course, this needs to be confirmed for *Glycymeris* spp. In ongoing tank experiments, we are currently studying this very issue with living specimens. To combine different proxies, specifically proxies from the shells, is certainly useful to further constrain $\delta^{18}\text{O}_{\text{water}}$ estimates, particularly because this was a major criticism.

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!

The text has been modified accordingly

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

OK, we refer to genus, not species.

Referee #3 Donna Surge

*When I began reading this manuscript, I was expecting a paleoclimate reconstruction for the Early Oligocene, but when I got towards the end of the manuscript it more seemed like an evaluation of whether *Glycymeris planicostalis* shells are useful to reconstruct climate conditions during this time. The authors should more clearly direct the focus of the paper up front.*

We agree and have therefore slightly modified the Abstract and Introduction in order to better specify the goals of this study.

What was insolation like at this time? How similar or different was it from today? This is potentially important when suggesting that the early Oligocene climate is similar to conditions predicted for the near future.

Although this should be considered in numerical climate models, the effect is small on these time-scales. According to computer models (Crowley, 1993), the solar flux was ca. 3 to 5% lower during the early Paleozoic (450Ma) than at present. Projecting this model into the early Oligocene would result in a 0.2 to 0.3% less intense sun than today. Conversely, a 2% increase in solar flux results in a global mean temperature rise that corresponds to a doubling of atmospheric carbon dioxide (Cockell, 2007). In order to keep the Rupelian world as warm as ours would have required only a few tens of ppm higher CO₂ levels than at present. Oligocene values were ca. 400 to 500 ppm (Zachos et al., 2008).

Where on the shell were the oxygen isotope samples collected?

Samples were collected on the ventral portion of the valve (outer shell layer). This information has been added to the manuscript.

The authors should be careful about making statements about quasi-decadal oscillations in their data series because the intervals sampled are too short to make such an assessment.

We have phrased this more careful in the revised version.

*First line on page 4092 is missing a word: From the of *Glycymeris**

The manuscript has been modified accordingly.

Page 4093, lines 10 and 22: Should be “oxygen isotope ratios” or “oxygen isotope composition” not “oxygen isotope values”. Fix throughout.

Here, we disagree. Delta is a value computed from ratios.

Page 4098, line 19: Should be ...average maximum (summer) values...

OK.

Page 4099, lines 10-11: Reword the beginning of the second sentence: Not only do the shells consist of aragonite...

Done.

Page 4103, line 25: Reverse the order of the temperature range so that the lower value is first.

OK

Page 4103, line 26: Should be: ...lay well within these...

OK

We hope you find our manuscript now acceptable for publication. If there are any questions, please let us know. We look forward to hearing from you.

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