Climate of the Past Discussions

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Reply on CC1

Andrew L. A. Johnson et al.

Author comment on "Sclerochronological evidence of pronounced seasonality from the late Pliocene of the southern North Sea basin and its implications" by Andrew L. A. Johnson et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-142-AC3, 2022

(CC1 comments in normal typeface; **responses in bold**)

In their manuscript, Johnson and colleagues present a nice dataset of seasonally resolved stable isotope transects through fossil bivalves to reconstruct seasonality during the mid-Piacenzian warm period. The study is timely, well thought-out and very relevant for the readers of Climate of the Past.

I enjoyed reading about the combination of this extensive, high-quality dataset and would like to compliment the authors on bringing the data together to give the reader an overview of the seasonality reconstructions from different individuals (Figure 8). The images of the *Aequipecten* shells (Figure 1) and the overview of the stratigraphic context of the shells (Fig. 3, section 3) are also a very useful addition to the field! On reading through the manuscript, I did encounter some aspects of the discussion which may require a bit more attention, or with which I did not fully agree, and I wanted to highlight these below so the authors could consider them in their revision. These comments are meant to improve the discussion of the nice dataset that is presented, which by itself is already a very valuable contribution to the field and certainly merits publication.

Preservation

The authors acknowledge that no preservation screening was done on the shell material (lines 389-391). In most deep time (pre-Quaternary) sclerochronological studies, I would consider such an investigation essential to demonstrate the reliability of isotope records. Just a few trace element analyses to test against incorporation of Mn or Fe during diagenesis (see Brand and Veizer, 1980), XRD profiles to test original aragonite preservation in the aragonitic species and/or SEM images to demonstrate original shell structure preservation would lend more confidence to the interpretations in the manuscript. That said, the authors do cite evidence of good preservation of specimens from the same or time-equivalent deposits and I know from personal experience that the preservation of these shells from the Lillo formation show excellent preservation, so I would not consider the lack of preservation screening in this study to be a big obstacle to interpretation of the results.

As noted in the comment, there is some published evidence of good preservation in the Pliocene sequence investigated (Valentine et al. 2011). It only extends to demonstration of original microstructures, but in both calcitic and aragonitic

bivalve species, the investigated example of the former being an individual contributing isotopic data to the present study (as AO7; we will point out that it was this specific specimen that provided the evidence of good calcite preservation in Valentine et al. 2011). We took the view that the existence of pronounced $\delta^{18}O$ cyclicality, of a wavelength similar to that in modern examples of the same or similar species, was enough to confirm preservation of the original isotopic signature – a position explained in other studies (e.g. Johnson et al. 2017) but not stated explicitly here. Moon et al. (2021) have recently shown that dry heating at 200 °C can shift isotopic values but preserve cyclicality. Our shells, however, could only have experienced heating by a few degrees (in burial to a depth of little more than 100 m), so the existence of cyclicality can still be taken to indicate an original signature. I find it difficult to believe that cyclicality would be preserved in any (presumably fluid-based) process that introduced Fe or Mn into the shell after death, and high concentrations of Mn (as revealed by cathodoluminescence) can in any case result from incorporation during shell growth (e.g. Barbin et al. 1991; Soldati et al. 2010). I would therefore be reluctant to reject any of the present cyclical isotopic data on the basis of a high associated Mn concentration. It is worth noting here that a Pliocene aragonitic shell from the UK showing excellent mineralogical and microstructural preservation (*Cardites quamulosa ampla* 7 of Vignols et al. 2019) provided CL evidence of a high Mn content but yielded a $\delta^{18}O$ profile essentially the same as those from three other examples of the species in which Mn content was low from CL evidence. We will briefly discuss the evidence of good preservation provided by cyclicality in the revised version of the manuscript.

Transfer functions

In the manuscript, the authors nicely discuss the effect of applying several different transfer functions for the d18O-temperature relationship and a range of potential d18O values of the sea water on their d18O curves. Overall, I think this discussion is very honest and useful in showing the uncertainty on these d18O-based reconstructions, however I do not agree with the notion that the validity of transfer functions can be rejected or supported based on the data (e.g. lines 489-491; lines 679-684). In my opinion, the validity of proxy transfer functions like those for d18O can only be tested using modern carbonates precipitated at (approximately) known temperatures. Inferring the correctness of a transfer function based on the "fit" of fossil data with expected temperature outcomes runs the risk of circular reasoning. The discussion in lines 659-684, where outcomes of the d18O-temperature seasonality are compared with temperature reconstructions from ostracod and dinoflagellate assemblages is especially problematic, since the authors later (rightfully) argue that such assemblage-based reconstructions may be subject to bias (lines 926-929). My suggestion would be that the authors present the range of temperature seasonality outcomes they obtain from their fossil d18O data using various transfer functions and d18O values of the sea water as an uncertainty range. It is of course fine to discuss which outcomes fit better with previous reconstructions (which have their own uncertainty), but to conclude from these comparisons which transfer functions are best seems to push the interpretation a bit too far.

I do not accept the charge of circular reasoning in the first half of the paragraph above and in the specific comment on lines 489–491 below. It results from a misinterpretation of the text in those lines. The $\delta^{18}O$ data referenced (in Johnson et al. 2021b) are from a modern shell and the temperatures calculated using the various transfer functions were compared with directly measured values (just as recommended above for validation purposes). In so far as the $\delta^{18}O$ data were mistakenly taken to be from Pliocene fossils and the temperature comparison was mistakenly taken to be with 'expected' Pliocene values, the text obviously

needs some expansion/clarification. I will attend to this.

While the above criticism is understandable (given the misinterpretation at its root), I am perplexed by the further criticism of comparisons between isotope- and assemblage-based estimates of Pliocene temperature in order to determine which of the former (based on different transfer functions) are more credible. It seems logical to give greater credence to estimates which are corroborated (even by 'uncertain' data) than to those which are uncorroborated.

Statistics

In places where the uncertainty of the data is assessed (e.g. line 471-472) or comparisons between different records are made (e.g. line 570-574), the manuscript could benefit from more detailed statistical evaluation. For example, it would be more transparent if the measured values of the isotope standards are provided in a supplement and the actual mean value and standard deviation on these measurements is given in the text (line 471-472). In descriptions of the records, terms like "noise" (e.g. line 525) should be better defined and perhaps quantified. Statements like "substantially less variation" and "moderate positive covariation" (lines 570-574) should be backed up with statistical tests and quantification of uncertainty. Finally, I think the discussion would benefit from statistical evaluation of the seasonality outcomes and their uncertainty. The comparison between temperature reconstructions, on which much of the discussion is based, is heavily dependent on the way in which seasonality is calculated and the degree by which differences between reconstructions are statistically significant. The authors discuss how their method for extracting seasonality from the extreme values of d18O records influences the outcome (e.g. section 5.1), but the study design using a large number of specimens (data in Fig. 8 and Table 2 and 3) should make it possible to calculate ranges and uncertainties for summer and winter temperatures, which can be used to test statistically if some species or combinations of assumed d18O of seawater and transfer functions are in agreement with previous temperature estimates (see paragraph above).

As the seasonality outcomes are the principal 'result' of the paper I will deal first with the comments concerning these (second half of the above paragraph). We certainly needed to address uncertainties, and did so very thoroughly according to both referees. Full statistical comparisons with the data for winter and summer temperature now and (from other evidence) in the Pliocene are simply not possible because in neither case is there information on variation about the given values (indeed the data from Pliocene dinoflagellates are not in the form of specific temperatures but of temperature ranges: warm/cool temperate). I have, however, reflected on the comparisons that we did make and realized that a slightly different approach would be preferable. In Section 6.1.2 we started by comparing the difference between interval means for summer and winter temperature with the difference between the modern summer and winter seafloor temperatures at 53° N, 03° E (the site used in the validation exercise mentioned in response to the 'Transfer functions' comments). I think this was fair. However, we then went on to compare the largest single-year temperature ranges from individual shells with the seafloor seasonality figure for 53° N, 03° E, pointing out that some Pliocene ranges were higher than the latter figure. While this was worth recording, it does not mean that Pliocene seafloor seasonality was different from present. The data for 53° N, 03° E constitute a representative 'snapshot'; on the evidence of data from elsewhere in the southern North Sea (Lane and Prandle 1996) seasonal temperatures probably vary by ± 2 °C at this site. The 'high' Pliocene ranges can be accounted to this variation. The 'high' ranges, supplemented by a 3 °C stratification factor, were used in subsequent comparisons with the surface seasonality figure for 53° N, 03° E. This was not appropriate: the difference between interval means for

summer and winter temperature should have been used, as in the comparisons of seafloor seasonality. While fairer, this approach makes very little difference to the figures for Pliocene surface seasonality, and revision of the data used will not require revision of the conclusions. It is worth noting here that in response to RC1 and RC2 the present seafloor temperature range at 53° N, 03° E has been indicated in Fig. 8, and the surface range has been included in a new plot (Fig. 9) showing individual and interval-mean summer temperatures incorporating the 3 °C ('minimum') stratification factor where appropriate - see the attached file. I think this visual representation of the Pliocene and modern data provides clear and convincing support to the argument in the text.

Regarding the other statistical comments (first half of the above paragraph), I will be happy to include additional information to the extent that it is possible and worthwhile. For instance, the strength of covariation between $\delta^{13}C$ and $\delta^{18}O$ over parts of the ontogeny of individual specimens (lines 572–573) can be expressed in the form of a few R^2 values - information which can be added to the text without interrupting the overall flow (see also the response to RC1). By contrast, the weaker cyclicality in $\delta^{13}C$ compared to $\delta^{18}O$ would require substantial addition to the text if documented statistically – one would need to present and discuss test results relating to each $\delta^{18}O$ cycle in each specimen. I hardly think this level of statistical support is needed for a descriptive statement which is manifestly true from the evidence of Figs. 6 and 7. I will, however, modify the text concerning $\delta^{13}C$ to make it clear that the 'variation' referred to (line 570) is within ontogeny (i.e. over intervals comparable to those of $\delta^{18}O$ cycles) rather than over the whole of ontogeny. I can provide raw measurements of isotope standards for about half the analytical runs (conducted in my research over the last four years). For the other half (conducted in the PhD research of Annemarie Valentine from 2009 to 2013) I have only the measurements of samples, although analytical reproducibility was similar (Valentine et al. 2011). I personally think it is a bit excessive to provide raw measurements of standards (even as supplementary data) but will supply those from the more recent runs if it is considered worthwhile. Finally, noise is usually defined as 'unexplained variability'. Here we apply the term to low-amplitude, low-wavelength excursions from the higher-amplitude, higher-wavelength (cyclical) pattern of $\delta^{18}O$ variation, but also to a single relatively high-amplitude, low-wavelength excursion coincident with a similar excursion in $\delta^{13}C$. This usage is explained in the caption to Fig. 6d (the reference to Fig. 6c in line 525 will be corrected) and I think readers will readily recognise the common 'excursion' element – i.e. unexplained variability in the form of departures from a pattern.

Minor comments:

Line 128-129: In some species (e.g. *Crassostrea gigas*), shell sections in early ontogeny have been shown to by precipitated out of isotopic equilibrium (e.g. Huyghe et al., 2021), so this may not always be the best part of the shell to target for reconstructions.

I was aware of the slightly earlier work of Huyghe et al. (2020) showing non-equilibrium (low) $\delta^{18}O$ values from the early ontogeny of *Crassostrea gigas* shells. Of the four species supplying $\delta^{18}O$ data for the present data, only *Aequipecten opercularis* was sampled over the first two years of growth. All the available evidence points to near-equilibrium isotopic incorporation in this phase of ontogeny, with year one providing the fullest and most accurate record of environmental temperature variation, as documented by Hickson et al. (1999) and Johnson et al. (2009, 2021b). These works, and those of Huyghe et al. (2020, 2021), could be discussed at this point, but it would perhaps over-elaborate the text. The works already cited provide adequate support for the

existing general statement.

Line 216-267: I really enjoyed reading this thorough review of the southern North Sea stratigraphy. I wonder if it would be beneficial to the reader to add rough paleo-depth curves to the sections in Fig. 3 to make the evolution of the paleoenvironment in these different areas easier to follow.

The purpose of Fig. 3 is to show the stratigraphic relationships of units rather than their environment. Addition of palaeodepth curves would interfere with this objective. While fairly precise (albeit divergent) estimates are available for the sequence in northern Belgium, only very rough estimates (e.g. 'mainly above storm wavebase' for the Oosterhout Formation; Slupik et al. 2007) are available for the sequence in the south-west Netherlands.

Line 405-407: Does this penetration of the resin into to shell affect the isotope analyses?

The specimens concerned (*Glycymeris radiolyrata*) were investigated at Mainz, where I was asked not to present resin-contaminated material for analysis – I think because of calibration issues. While sampling therefore took place below the resin-contaminated zone (see Fig. 5), in a few cases boreholes did very slightly extend into it. There was no effect on the analytical results: δ^{18}O values were consistent with the pattern of those from completely uncontaminated samples before and after.

Line 411: Figure 3 does not show the drilling of A. opercularis, but instead shows stratigraphy of the mPWP sections. Perhaps this should refer to Fig. 1? (although this figure also does not show the drill holes)

The reference here is to the specimens illustrated in the cited papers by Hickson et al. – note the use of 'fig.' rather than 'Fig.', a common practice for referring to illustrations in other works.

Line 480-482: The authors should briefly explain here why the global d18Osw values are rejected here.

They give unreasonably low temperatures from *Aequipecten opercularis* – e.g. 0.1 °C and 1.6 °C (for water δ^{18}O values of –0.6 ‰ and –0.3 ‰, respectively) from AO6, a specimen from the *Atrina fragilis* bed of the Oorderen Member. Given the warm temperate summer temperature indicated by dinoflagellate evidence from this horizon, such extremely low winter temperatures are not credible. Any further consideration of them would reduce the credibility of the isotope-based temperatures as a whole. I will amend the text at this point accordingly.

Line 489-491: See major comment above: The authors should explain why the Kim and O'Neil equation temperatures are "too low". I would be careful with this type of reasoning about transfer functions based on the "expected" temperature value.

See the response to the major comment.

Line 546: Provide a number for "a great deal" to quantify the difference in growth rate.

At the end of the sentence concerned I will add '(more than twice as fast as *A. islandica* and *P. rustica*, **and three to five times faster than** *G. radiolyrata*)'. **This is in terms of the number of δ^{18}O cycles in a given height interval. Some would not accept this as a measure of growth rate (preferring a statistic relating to the**

whole of ontogeny; see discussion in Johnson et al. 2021a) so it is best not to say anything precise when all that is needed is general support for the statement 'a great deal faster'.

Line 723: "overestimates" should be "overestimated"

No – 'overestimates' is the correct word, referring to the fact that the calculated temperatures would be higher than the actual maximum temperatures.

Line 750-752: See also major comment about the transfer function discussion: I wonder if this reasoning about the height of the stratification factor based on the temperature outcome and its comparison with modern temperatures is not sensitive to circular reasoning issues.

In line 750 reference is made to Section 3, where independent (dinoflagellate) evidence of summer surface temperature is provided. I admit that this does not indicate warmer temperatures than now at all horizons but dinoflagellates give no indication that temperatures were ever cooler than now. I therefore think it is entirely reasonable to infer a summer surface temperature higher than the present value 600 km north of the study area, and thus a higher stratification factor.

Line 791-792: See comment above: I think one can almost never test the accuracy of proxy transfer functions (or the validity of d18Osw assumptions) based on their outcome on fossil data. This type of discussion requires independent evidence and/or modern calibration studies.

We don't pass any judgement on transfer functions in these lines, but have done so elsewhere (see the point above about the greater credibility of data that are corroborated).

Line 926-929: If the assumption of ecological uniformitarianism does not always hold (with which I agree), the authors should be careful with their conclusions from comparison of temperature reconstructions with the outcome of ostracod and dinoflagellate assemblage studies elsewhere in the discussion.

We refer in lines 917–926 to evidence of niche change amongst bivalves. Until niche change is shown to be widespread and common I think we have to accept assemblage-based interpretations of palaeoenvironment founded on the ecology of modern representatives or close relatives of the species involved. There is certainly not much evidence as yet of niche change amongst ostracods and dinoflagellates.

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Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2021-142/cp-2021-142-AC3-supplement.pdf