Climate of the Past Discussions

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Reply on RC2

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-AC2, 2022

(RC2 comments in normal typefaces; **responses in bold)**

General Comments

Johnson and co-authors discuss seasonality, an under-investigated but essential dynamic in palaeoclimatology, from the southern part of the North Sea Basin (SNSB) during the last episode in Earth History when global climate was consistently warmer than today. They use stable isotope measurements on benthic marine molluscs (*Aequipecten opercularis*, *Pygocardia rustica*, *Arctica islandia* and *Glycymeris radiolyrata*) sourced from the Luchtbal, Oorderen and Merksem Members (and their lateral equivalents) from the Lillo Formation in North Belgium and the Southern Netherlands. The authors use the extreme inflection points of the δ18O ontogenetic profiles from the recovered molluscs to compute summer and winter seafloor temperatures using various equations against an assumed background of 0.0–0.4‰ average δ18Osw in the SNSB. The derived temperature difference taken at these inflection points is interpreted as a seasonality signal. Johnson et al. conclude that seasonality of the late Pliocene SNSB was on average more pronounced than it is now (3°C higher). Summer temperatures were found to be higher in the late Pliocene while winter temperatures were comparable to today.

I believe the subject matter is relevant to the diverse readership of Climate of the Past and closely matches the scientific remit of the journal. The manuscript is well-structured, underpinned by clear, objective and very precise writing. As a result, it is easy to follow the employed methodology, the authors’ interpretation of the results and how the conclusions were reached. The text is supported by a set of informative figures which are, despite the complexity of the incorporated data, kept simple and straightforward to interpret.

The authors present a very honest assessment of their results, easily exceeding what can be reasonably expected as the baseline for scientific scrutiny. This is exemplified by Figure 8 where the authors compare different computation methods in the literature and select the most suitable algorithm for their specific case.

Specific comments

*The age of the different members of the Lillo Formation was constrained using biostratigraphy and sequence stratigraphy: Luchtbal Member (3.71–3.21 Ma), Oorderen

Member (3.21–2.76 Ma) and the Merksem Member (3.21–2.76) (Figure 3 and De Schepper et al., 2009). This covers the mid-Piacenzian Warm Period, but also includes glacial events like MIS M2 (De Schepper et al., 2013), with a total fluctuation of 0.89‰ in the orbitally-tuned global stack of benthic δ18O (Lisiecki and Raymo, 2005) over this period. Evidence suggests that seasonality in proxy records is more pronounced in colder, glacial conditions (Crippa et al., 2016; Hennissen et al., 2015 and references therein). Is there a way of tying the seasonality results from the current study into the global climatic picture or should they be viewed as endemic snapshots of seasonality of the late Pliocene (which may be against a background which could range from MIS M2 to MPWP)? Are the reported seasonality values to be viewed as an average seasonality signal for the late Pliocene or is it more of a minimum estimate?

The age evidence as supplied by De Schepper et al. (2009) is indeed as stated but we incorporated the additional evidence of Louwye and De Schepper (2010), which indicates that the upper boundary of the Oorderen Member is no younger than 3.15 Ma – i.e. that this unit was entirely deposited within the Mid-Piacenzian Warm Period (MPWP). We will correct our omission of Louwye and De Schepper (2010) from the list of sources given in the caption to Fig. 3. It is of course true that there were fluctuations in deep-sea benthic $\delta^{18}O$ during the MPWP, and that these (in the order of 0.3 ‰ either side of the modern value) signify relatively cool and warm intervals. It seems reasonable to assume that the few horizons in the Oorderen Member with a 'cool' dinoflagellate biota represent the former and that the more numerous horizons with a 'warm' biota, including the horizon (*Atrna fragilis* bed) supplying five of the six Oorderen-Member specimens used in this study, represent the latter. From the evidence that seasonality is higher under cooler conditions these specimens can be viewed as providing a minimum estimate of average MPWP seasonality. The 3.71–2.76 Ma age limits for the Luchtbal Member encompass the MIS M2 glacial at c. 3 Ma. In so far as the projected mean sea-surface temperature from this unit is lower than from the Oorderen Member it may be that it was deposited under glacial conditions. However, the difference is only small and seasonality was no greater so it seems more likely that the data from the Luchtbal Member are representative of some part of the long, predominantly warm interval extending back from MIS M2 to 3.71 Ma. De Schepper et al. (2009) interpreted the unconformity above the Luchtbal Member as a consequence of the sea-level lowering associated with the MIS M2 glacial, confirming the view that the unit represents some part of the interval indicated. We will include at least some of this more refined discussion of the data in the revised version of the manuscript.

*Sclerochronologically derived temperature estimates offer an invaluable window into the (sub)annual temperature fluctuations that the biotic carriers were exposed to. Other techniques (e.g. foraminiferal Mg/Ca, alkenones and TEX86) offer estimates that are averaged over much longer time periods. Do these differences in temporal resolution complicate cross-proxy comparison? Can the sclerochronologically derived results be viewed as a tool to set the true range of seasonality recorded in other proxies that cannot capture this accurately but have the advantage of stretching observations over larger intervals?

Our isotope-based sclerochronological approach yields a very similar mean annual sea-surface temperature for the MPWP in the SNSB to that derived by averaging the alkenone and TEX_{86} temperatures of Dearing Crampton-Flood et al. (2020). We therefore agree with these authors that alkenone and TEX_{86} temperatures represent seasonal values (respectively, summer and winter) but conclude from comparisons with our seasonal sea-surface temperatures that they do not signify the seasonal extremes and hence yield an underestimate of the seasonal range. This difference in range estimate will persist when we

compare alkenone/TEX_{86} seasonality with our figure derived from mean (rather than individual) summer and winter sea-surface temperatures for the relevant stratigraphic unit (Oorderen Member), as intended in the revised version of the manuscript (see the response to CC1). The figure derived from mean values is formally comparable with the alkenone/TEX_{86} figure in that it integrates data over an uncertain but undoubtedly lengthy interval. However, even if annual average temperature fluctuated during this interval (highly likely on the evidence of instrumental data for recent centuries) this integrated figure is likely to give a good insight into seasonality in individual years, as indeed the present data show (seasonality from the pooled data is similar to that from each shell). As well as providing a more accurate picture of seasonality, the sclerochronological approach, applied to long-lived shells, can in principle provide a record of year-by-year variation in annual average temperature which is not recoverable from time-averaged data (as from alkenones, TEX_{86} and Mg/Ca of multiple forams). Unfortunately, as the records from *Glycymeris radiolyrata* in this study show, long-lived shells sometimes do not give a full picture of seasonal temperatures and in such cases cannot supply an accurate figure for annual average temperature. We will again include at least some of this discussion in the revised version of the manuscript. However, we do not want to stray too far into interpreting results from other proxies for fear that it might reduce the coherence of the interpretation of sclerochronological data.

Technical corrections

Line 460: measurements were made in two different laboratories and analytical errors were reported. Were replicate samples run to assess the inter-laboratory variation?

No individual shell samples were analyzed at both Keyworth and Mainz but Johnson et al. (2019) recorded the following in relation to analytical results from the two laboratories: 'For a few shells, part of the sample series was analyzed in one laboratory and part in the other; there was found to be excellent agreement (e.g., smooth continuation of trends) between the subsets of data.' This could be referenced as evidence of the comparability of results from each lab, but the existing statement about results from analysis of NBS-19 covers this point (lines 471–472). For the present study, all samples from each shell were analysed either in one laboratory or the other (see lines 460–462).

Line 532: insert comma after 'cycle'.

OK.

Line 573: is there a way of quantifying the covariation?

We will supply R^2 values (see also the response to CC1).

Figure 8: it may be informative to put in a vertical line (grey in background maybe) to indicate current summer and winter temperatures as a direct comparison to the measurements and calculations in each panel.

Rather than add further vertical lines to those already present we have chosen to depict the modern seasonal range by a grey bar of appropriate width, the seafloor range being shown in Fig. 8 and the fractionally larger surface range in Fig. 9 (new figure; see the response to RC1). Both figures are attached for perusal.

Table 3: the text is rather small and it may be better to put the entire table in landscape

format.

Agreed. As pointed out in the response to RC1, it will be possible to reproduce the table (and the text within it) at a larger size than in the pre-print, without altering its orientation. However, a 'side-on' (landscape) format may be better.

In conclusion, I believe this paper is an example of how ecological uniformitarianism can be employed to evaluate palaeoclimatological conditions and offer invaluable constraints for climate models.

References

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Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2021-142/cp-2021-142-AC2-supplement.pdf