Final Author Comment

Karatsolis et al. – cp-2022-60

We found this review constructive and helpful, and we therefore want to thank Anonymous Referee #2 for taking the time to read our work and suggest changes that will make the analysis more robust and the discussion clearer.

Comment on cp-2022-60

Anonymous Referee #2

Referee comment on "Late Neogene nannofossil assemblages as tracers of ocean circulation and paleoproductivity over the NW Australian shelf" by Boris Theofanis

Karatsolis and Jorijntje Henderiks, Clim. Past Discuss., https://doi.org/10.5194/cp-2022-60-RC2, 2022

The Karastolis and Hendericks study is an interesting contribution in a region and period that need to improve knowledge, particularly.

The use of CN offers a unique opportunity to characterize surface water masses and to monitor their evolution during the Mio-Pliocene transition interval.

The state of the art is well stablished and objectives of interest.

We thank the reviewer for the positive comments.

The chosen technique is appropriate, based on previous initiatives. The use of sphenoliths as main taxonomic group linked to stratification is fine, although should be more correct to include too discoasters (although its proportion is low). Others species clold be also considered in the interpretation. In fact there are some that are present in the plots but not mentioned/considered. If they are not taken into account, there is no point in including them.

Discoasters have been used to infer similar (more oligotrophic and stratified) conditions to those we are reconstructing in this study using *Sphenolithus* spp. (e.g., Imai et al., 2015). However, in this case, the relative abundances of *Discoaster* spp. are too low to be included in our analysis, which focuses only on common and dominant species. As an indication, Discoasters never exceed 1% of the assemblage in samples from Site U1463 and only do so in 2/105 investigated samples from Site U1464, in which case they still remain under 2%. This indicates, most importantly, that including Discoasters in the analysis would not alter the results and the interpretations we make. The relative abundances and fluxes of the rest of the common species (*Calcidiscus* spp., *Umbilicosphaera* spp., *Reticulofenestra* spp. >5μm) do help us understand better the extent of the changes in the nannofossil assemblage across the two intervals of interest (5.4-5.2 Ma; 4.6-4.4 Ma), as well as to put them in broader-scale context. See discussion in section 4.3: *Broader-scale changes in paleoproductivity and paleoecology* (L.435-443).

One aspect repeatedly used is seasonality. In the text it is discussed that with this resolution a direct correspondence with present day conditions it is not feasible.

In this sense, should be more correct talking about persistent conditions (e.g. stratification), rather than enhance seasonality.

In fact, the signal that is manifested is the dominant one in a sufficiently broad period to refer its dynamics to seasonality (although it is obvious that these processes may be the triggers), but there are not enough arguments to focus these changes on seasonal variation only.

I suggest a modification in this sense, avoiding such a direct reference to seasonality.

This is an interesting and valid point. Indeed, the timescales that we study make it impossible to directly infer and resolve seasonal variations, as we state in the manuscript. However, sustained changes in seasonal intensity could have had a long-term impact in regional ocean circulation and paleoclimate that, in turn, can be recorded in the sedimentary archive. For example, although we cannot resolve the intensity of the Leeuwin current (LC) during an austral winter thousands of years ago, an overall increase of sea surface temperatures (SST) offshore west Australia (LC pathway) can be used as an indirect way to infer its presence. In turn, since this current flows seasonally, we could propose that this SST change could be linked to a stronger winter season that could have intensified the flow of the LC. That being said, we understand the concern of building our argument primarily around seasonality and making it the main focus point of the proposed mechanism. To resolve this, we will tone down our claims regarding a direct link to changes in seasonality and focus more on the long-term changes in stratification and water column mixing, that can be substantially supported by the provided data.

Other aspect that must be considered in Discussion and Conclusions, is provide more clear information about the ITF and LC, marked as objective, trying to ling the signal observed with characteristics of LC (for example). In fact this is considered, but plots should be explained in this sense, in order to follow better the arguments. Discussion refers the features in a general way without focusing on the evolution of those water masses. In this sense, I cannot see clearly the utility of the H Index (not linked to these features) and the CA (also superfluous. With the available data (T) and stratification index, should be possible to identify/define the involved water-masses, and consequently its evolution along the considered period.

We will provide additional information regarding ITF and LC activity in the discussion and conclusion sections, as well as the possible link of their features to the temperature gradients and NSI signals we identified.

Indeed, the H-index and the correspondence analysis are not directly used to infer changes in stratification. However, they are useful in providing additional information regarding the broader-scale (and persistent) changes in paleoproductivity and paleoecology and therefore help us understand the global-scale processes recorded in the nannofossil assemblage. (see section 4.3 *Broader-scale changes in paleoproductivity and paleoecology*; L.423-435). For this reason, we believe that they should be presented as part of the results and included in the discussion.

Concerning the potential mechanisms related to paleoenvironmental aspects (3 options), the authors should consider the most reasonable possibility, taking into account the available data.

It is not entirely clear what the reviewer is suggesting in this sentence. The three possible mechanisms/ocean circulation scenarios are presented in section 4.1: *Water column mixing and*

nutrient availability on the NW Australian shelf, based initially on the changes in the NSI. Later, after also discussing the observed changes in the temperature gradients in the first part of section 4.2: *Paleotemperature and inferred ocean circulation patterns*, we present the most reasonable (to our understanding) scenario/combination of features, taking into consideration all the available information (L.395-405):

The three mechanisms that could have led to sustained changes in NSI are summarized here to facilitate the revision:

1. If the LC activity had intensified, it could have led to increased eddy formation that promoted enhanced productivity across the western Australian continental shelf.

2. An overall increase in convective mixing could have occurred across the continental shelf area due to more cooling in the upper water column and/or intensified storm activity during the winter period.

3. If the oligotrophic influence of the LC (which is understood as: warm, oligotrophic LC waters can inhibit upwelling activity and therefore lead to overall decreased primary productivity across the continental shelf) had weakened, productivity over the shelf may have had increased.

A sentence that will link our hypothesis to these three mechanisms, and then explain which one we see as the most reasonable scenario according to our analysis, will be added. The possibility that a combination of mechanisms was present (also suggested by Reviewer 1) will be discussed too.

The comments referred in line 395 is too speculative: should provide extra information to propose this mechanism, or afford the explanation in a more general way (mixing!).

We will expand this part of the discussion to further support our arguments, by linking the combination of observations in NSI and temperature gradients to the mechanisms we described in section 4.1 (see also previous comment).

Section 4.3 refer an interesting global feature. Here is considered lightly, being necessary a better explanation of the processes and records. The link with the rest of the text is not clear, need better justification.

We will expand this section to further explain the processes behind broader-scale changes in calcareous nannofossil paleoproductivity and paleoecology. In the previous sections, we focused on regional paleoclimatic and ocean circulation changes and how these are recorded in the nannofossil assemblage. In this section, we aim to investigate how broader-scale changes in paleoproductivity are reflected in the assemblage and therefore decouple them from the more regional ones. A sentence linking this section to the rest of the discussion and pinpointing the importance of looking at the bigger picture will be added in the beginning of this section.

References

Imai, R., et al., 2015. Evidence for eutrophication in the northwestern Pacific and eastern Indian oceans during the Miocene to Pleistocene based on the nannofossil accumulation rate, *Discoaster* abundance, and coccolith size distribution of *Reticulofenestra*, Mar. Micropaleontol. 116, 15–27.