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Interactive comment on "Highly variable Pliocene sea surface conditions in the Norwegian Sea" *by* Paul E. Bachem et al.

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Review of "Highly variable Pliocene sea surface conditions in the Norwegian Sea" by Paul Bachem et al.

Bachem et al present compelling new SST, %C37-4, and IRD records from the eastern Nordic Sea covering a two-million-year interval of the Pliocene, alongside a new Holocene SST record from a near to Site 907 site produced for comparison. These records complement planktic and benthic foraminiferal isotope records from the same core (ODP Site 642) published earlier this year (Risebrobakken et al., 2016, Paleoceanography). The new SST record is the first of its kind for this region, and is therefore key to improving our understanding of the high-latitude climate conditions that preceded the onset of large-scale northern hemisphere glaciation in the latest Pliocene.

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Discussion paper

The new record from Site 642 is compared with another recently published record from the western Nordic Seas (907), and potential tectonic and orbital/CO2 forcing mechanisms on SST in this region are discussed. Overall the records are very interesting and their presentation is clear. My main comment is that the discussion and presentation of ideas could be significantly improved, which would increase the impact generated by the publication of these nice new records. Specifically, I think that the new SST record should be more carefully compared to other existing records for the same time interval (thus revealing clues on potential mechanisms), and that suggested mechanisms for drivers of Pliocene climate variability during various sub-intervals should be more firmly grounded in what we know already in greater detail from Pleistocene climate variability. Below are my detailed comments.

Introduction

The cited reference for the first sentence about Pliocene warmth (Zachos et al., 2001) is not really appropriate. Perhaps cite some Pliocene temperature papers instead that quantify warmth relative to the present.

Discussion related to AMOC strength and North Atlantic water masses

The paragraph of the introduction that discusses AMOC (starting page 2 line 12) needs significant revision, because as it is, it misrepresents the strength of paleo-evidence for a strengthening in AMOC during the warm Pliocene, between 4.6 and 4 Ma. The idea that AMOC intensified at ~4.6 Ma was originally based on an increase in d13C values measured in benthic forams and an increase in sand content at ODP Site 999 in the Caribbean Sea (Haug & Tiedemann 1998). These proxy data were interpreted as indicating that after 4.6 Ma, the Caribbean was filled (over an intermediate depth sill) with northern-component water (UNADW) rather than more corrosive, low d13C southern-component water (AAIW) – i.e. the spatial extent of UNADW increased at this time. Bell et al.'s 2015 paper in Scientific Reports showed that there was no similar contemporaneous increase in the spatial extent of LNADW, and that NADW production was apparently strong both before and after 4.7 Ma. Therefore, by themselves, the Haug & Tiedemann (1998) and Steph et al (2010) records (from Caribbean ODP Sites 999 and 1000, respectively) do not provide strong evidence for an intensification of AMOC, rather an increase in the spatial extent of UNADW.

In the Pleistocene, the intermediate-depth Caribbean fills with more positive d13C water during glacials relative to interglacials because UNADW penetrates into the Gulf of Mexico when LNADW spatial extent reduces during cold stages (Site 502, Oppo et al., 1995, Paleoceanography), so the confidence placed in the Caribbean Pliocene data on their own as evidence for a stronger AMOC is puzzling to me. Spatial changes in water masses do not have to equate to changes in AMOC strength, e.g. the NADW cell can shoal, but circulation in it (i.e. AMOC) can still be strong. During the last glacial, evidence suggests that AMOC remained relatively strong for the most part (even during most Heinrich events; Bradtmiller et al., 2014; Bohm et al., 2015) despite a much-reduced spatial extent of LNADW in the deep North Atlantic at that time. In summary, the language used in the manuscript leaves the reader thinking that assessment of all available evidence could still lead to the conclusion that AMOC intensified at 4.6 Ma, and I don't think it can any longer with any confidence (that is, unless analysis of their data, following suggestions below, provides new supporting evidence for the original claim). If you follow this route, I think you need to change the introduction to set up the problem more fairly, i.e. that based on Bell's new work it is no longer clear if AMOC intensified between 4.6-4 Ma.

Also, the authors should note that BellÂăet al. published a paper in 2015 in QSR, which shows that the conclusions of Zhang et al. (2013) are incorrect because in d18O-d13C space, Site 704 is bathed by northern component water, and not southern component water, with a more positive d13C value. IÂăquote: "Zhang et al. (2013) proposed a scenario, based on model-data comparisons, whereby Southern Ocean ventilation increased, raising d13C values at Site 704, a site that has been important for inferring enhanced AMOC. A closer examination of our data, however, indicates a northern sourced influence on Site 704 d13C, thereby supporting an enhanced AMOC interpretation. This is because Site 704 data lies close to Site 1264 in d18O-d13C space, while Site 929, which is sensitive to the influence of SSW, lies closer to Pacific Site 849.".

Lastly, I would suggest you do not cite Sarnthein et al. (2009) on page 2 line 25, because this paper contains no new data in it that relate to AMOC during the mid-Piacenzian warm period, but one of the original papers (inÂăaddition to Raymo et al., 1996 already cited) that discusses evidence for anÂăenhanced AMOC based on spatial water-mass structures such as Ravelo andÂăAndreasen (2000).

These comments on how you discuss changes in AMOC strength are also relevant to some parts of the discussion (for example page 12, in reference to Steph et al 2010).

Methods/Results

p5, line 25: this value is not really a regional average, as it is based on one site. I suggest changing to "Holocene average at a nearby site" or similar. Note: perhaps also worth mentioning in the methods why you use this nearby site to get Holocene values for comparison rather that the same site (I guess it's not possible?).

Some description of seasonality of alkenone production/coccolithophore productivity in this region in the modern ocean would be useful here, with the methods. Perhaps this is why you only mention summer SSTs? Ok, now I see you discuss seasonality later in the "proxy interpretation" section of the discussion... I would suggest incorporating this whole section in the relevant parts of the methods, so that your discussion flows better and all caveats/assumptions are already dealt with and out of the way.

"Nevertheless, there is some doubt about the preservation of alkenone production seasonality signals in sediments" – this statement is a bit cryptic! Please expand and explain. Is summer SST very different to mean annual at your study site (i.e. would a summer bias make a big difference to absolute values)?

p6 line 13: number missing

p7 line 14: is this statement supported by biological oceanography data?

Overall, I think the results section is lacking a basic, clear description of the key features of your new records: the existence of large-amplitude changes in SST on xx and yy timescales at Site 642 in the warm Pliocene. This finding is supportive of the ideas put forward by Kira Lawrence et al (2009) based on Site 982 further south, that the warm Pliocene high northern latitudes were characterized by this large-amplitude surface variability in SST at this time. I think the Site 982 SST record should be included in a figure for comparison (as well as the Herbert et al. (2016) Site 907 record already included, and perhaps also the Knies et al (2014) Site 910 record mentioned), even though it only overlaps with the younger part of your new record. NB: I suggest you plot the Site 982 SST data from Lawrence et al. (2009) on its original LR04 age model. One group has challenged the validity of the LR04 age model for Site 982 during your study interval (Khélifi et al., 2013; CP). However, its LR04 age model has been shown to provide the best estimate of the age-depth relationship for this site (Lawrence et al., 2013; CP). In this regard, it is interesting to note that your new 642 SST record looks very similar to the 982 SST record when the latter is plotted on its LR04 age model. At the start of the SST discussion, I would then compare the new 642 record with both the Site 907 and Site 982 SST records, in terms of the amplitude of orbital-scale variability where they overlap, and the longer-term trends. This comparison should form the centerpiece of your discussion, since inferences on forcing factors, whilst interesting, remain mainly speculative at present.

Figures

Figure 1: It would be nice to include modern SST contours on this map, so that the reader can gain insight into zonal and latitudinal SST gradients in the region, and the effects of the various currents on SST (which are subsequently discussed a lot for the Pliocene).

Figure 2a: IRD records: It is confusing using different y-axis scales for the two IRD records... I would suggest putting them on the same scale (perhaps with a break in the axis at the high end so you can still see the smaller peaks clearly). Are these two records both on your new age model?

Figure 2b: the use of a dashed line makes it hard to see what is going on.

Figures 2 and 3: I suggest adding the LR04 benthic isotope stack to these graphs for reference, so the reader can more easily visualise where major SST changes and IRD peaks occur in relation to familiar Marine Isotope Stages.

Discussion

Holocene data: Relatively high variability (~4 degree range) is documented in the new Holocene Iceland Sea SST record, and this is not really mentioned because the authors go on to use a mean value for comparison with the Pliocene. Is this what one would expect within the Holocene (suggesting that local oceanography is very dynamic at this time?)? Do other Nordic Sea records show similar high SST variability during the Holocene? A short discussion of these data could be appropriate, in the context of determining whether the Pliocene SST swings (within a not dissimilar range of 4 to 6 degrees) likely represent major oceanographic/climate changes, or smaller regional shifts in currents or fronts that can have big impacts on SST at the given location.

Personally, I don't like the subdivision of the Pliocene study interval into seemingly random sub-intervals of time based on changes occurring in one proxy record. I think it would be more intuitive and easier to follow if you approached the discussion using a "one paragraph, one idea to get across" method. Then for each paragraph, you can describe the new evidence from your records and supporting evidence from the literature that support that idea. Are the 6 shorter time intervals used here the same as the "climate phases/transitions" defined for the same site and time period in Risebrobakken et al., 2016? Based on a quick comparison, the intervals seem to be different, which is going to lead to lots of confusion. If you insist on using sub-divisions (other than official Pliocene stages or MIS terminology), make sure the stated intervals of time and the terminology used are identical in both papers (if it doesn't make sense to use the subdivisions defined in the Risebrobakken paper, maybe this strengthens the case for not using them at all).

In my opinion the discussion of the new SST record and comparisons to all other available orbital-resolution SST records covering the same interval should form the backbone of the discussion. This will naturally lead on to discussion of what is forcing the various records. For example, if CO2 changes on orbital timescales (21 kyr through to 400 kyr) drive your SST record then the same orbital-scale cooling and warming should be seen in all SST records (because this would constitute a top-down forcing everywhere). Note that orbital forcing and CO2 forcing can't really be treated separately until a reliable orbital-resolution CO2 record exists for the Pliocene, because in the Pleistocene, CO2 changes are modulated by orbital parameters. On the other hand, if circulation/northern heat transport/AMOC strength changes drove this orbital-scale variability then one might expect opposing SST patterns in different key regions on these timescales, as seen for the Pleistocene. For example, Lisiecki et al. (2008) showed that during the Pleistocene at certain orbital periods, reduced overturning as determined by benthic d13C gradients was associated with cooling at high northern latitudes and warming at low latitudes, consistent with a decrease in meridional heat transport. Similarly, if all SST records show the same patterns on long secular (»100 kyr) timescales then that would be consistent with a tectonic-driven CO2 forcing of SSTs. Or, if as you suggest tectonic changes in the CAS influenced AMOC, northern heat transport, and your SST record (as well as other high northern latitude records and other sites on the path of the NAC?), then you should see opposing SST trends at high northern latitudes versus the Caribbean on such timescales. You could certainly look for these types of patterns during the Pliocene (e.g. use the Caribbean Mg/Ca SST records presented by Steph et al., 2010), and this should hopefully lead you into a clearer discussion of mechanisms driving SST variability at your site on orbital and tectonic timescales.

p6, line 22: “notable temperature transitions” – I think you can be bolder/more specific with your language here. The SST shifts that your new record documents are large (up to 6 degrees) and well-defined.

“extended cooling phases and relatively fast warming phases”. Please quantify this statement.

p7 line 13: This sentence reads as if the Site 907 SST data come from all 3 references. Is that correct?

p7 line 28: give an order of magnitude for “far smaller” (ideally comparing with the same/a nearby site)

p8 line 28: which changes?

When discussing specific glacial events, it would be clearer to use their MIS names (rather than saying for example, the 4.9 Ma event).

I think the stand-alone CO2 paragraph of the discussion is not useful, and should be incorporated into the discussion as mentioned above.

Given that no significant variance in the obliquity band in the SST record is identified in spectral analysis, I find all the interpretations related to changes in obliquity/seasonality very speculative. Perhaps if the arguments in the rest of the discussion can be strengthened, these statements will be superfluous.

Please add a reference to support the idea that there was a threshold in the closure of the CAS at 4 Ma.

Additional References (not cited in the manuscript):

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