Clim. Past Discuss.,
doi:10.5194/cp-2016-131-AC2, 2017

Climate of the Past Discussions
Open Access

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

Paul E. Bachem et al.

paul.bachem@uni.no

Received and published: 5 May 2017

Comments by Clara Bolton

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.

Response:

We agree that a more Pliocene-specific reference is useful here and will cite Lisiecki and Raymo (2005) here, in which the LR04 benthic d18O stack shows the overall global climate stage of the Pliocene in contrast to the Pleistocene. We will also provide

Printer-friendly version

Discussion paper

Pliocene modelling synthesis papers as a reference (e.g. Haywood et al. 2013), as well as the global Pliocene SST trend overviews by Fedorov et al. (2013) and Herbert et al. (2016).

—

Comments 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 âĹij4.6 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.

Printer-friendly version

Discussion paper

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 Bell et 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. Quote: "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 (in addition to Raymo et al., 1996 already cited) that discusses evidence for an enhanced AMOC based on spatial water-mass structures such as Ravelo and Andreasen (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).

Response:

We thank Clara Bolton for the very constructive and useful comments to our manuscript. It is true that the proxy evidence for AMOC changes during the Pliocene stems mainly from the Caribbean, and hence possibly represents local changes instead of a large-scale AMOC changes. The decrease in Pacific-Atlantic water mass flow has been shown in models to increase AMOC and thus northward heat transport in the North Atlantic (e.g. Brierley and Fedorov, 2016; Maier-Reimer et al., 1990; Zhang et al., 2012). Because of this hypothetical connection, we consider there to be a possible link between Caribbean proxy responses and increased northward heat transport. However, we acknowledge that the potential for AMOC variability has been constrained by more recent publications (Bell et al., 2015b). What we note are changes in surface water currents, which are not necessarily fully coupled to deep water changes, especially if Nordic Seas deep water formation is less pronounced than today (e.g. Jansen et al., 2000).

We agree that there is a difference between AMOC strength variability and water mass distribution changes. It is indeed, a challenge to separate changes in AMOC from changes in key water mass properties. In order to keep the introduction streamlined and clear, we will put less weight on the vaguely defined topic of AMOC intensity and rather provide the presently available evidence for the extend of specific water masses.

In a revised introduction (and mainly in the discussion) we will make the established arguments against Pliocene AMOC variability clearer and more concise. We will take into account the suggested papers by Bell et al. (2015a), Bradtmiller et al. (2014), and Böhm et al. (2015) in order to clarify the arguments that exist for a relatively stable AMOC throughout warm and cold global climate states. Here we will add the knowledge of Early Pliocene deep water formation, and how it may be linked to surface circulation and climate of the Nordic Seas. This will set up a contrast to the interpretation that changes in the Caribbean (e.g. those published by Haug and Tiedemann, 1998) argue for a CAS influence on the thermohaline circulation, and that a gradual decrease of Pacific-Atlantic water mass flow had an effect on Atlantic climate and circulation during the mid-Pliocene.

We will also include these arguments in the discussion, which will reflect less certainty in AMOC variability as a cause for high-latitude climate variability and give more space and emphasis to other potential explanations, such as shifts in the Arctic Front or the effect of the Greenland Scotland Ridge on the North Atlantic heat transport into the Nordic Seas (e.g. as modelled by Hill (2015) and by Robinson et al. (2011).

We will replace the Sarnthein et al. (2009) reference in the manuscript with the relevant references containing original data, e.g. the suggested Raymo et al. (1996) and Ravelo and Andreasen (2000).

—

Comments on 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?).

Response:

We agree and will clarify that we are discussing site-specific averages, not regional averages. Because the respective sites are bathed in the same water masses, we assume that the comparison is nevertheless valid. We will also add a sentence stating that core-top (or any Holocene) measurements from Hole 642B were not available for this study, hence nearby sites had to be chosen as best analogues.

—

-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

Printer-friendly version

Discussion paper

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)?

Response:

We agree with the comment and will move the discussion of alkenone SST interpretations from their current later place in the manuscript into the methods section. The section will highlight that a seasonal bias is likely in the high northern latitudes because of very strictly limited growth seasons as published by Andruleit (1997) and Baumann et al. (2000). This has already been taken into account when interpreting the absolute SST values, since there is a significant summer-annual difference in the Nordic Seas of $\sim$3°C (at Sites 642 and 907) in the modern setting Locarnini et al. (2013).

The note about 'preservation of seasonality signals' refers to a number of studies which show that despite seasonal variations in alkenone production in the surface ocean, core-top studies usually demonstrate a best-fit relationship between UK'37 and mean annual SSTs. These issues have been reviewed by Rosell-Mele and Prahl (2013).

We will clarify the section mentioning the doubts regarding the summer bias in alkenone data that were noted by Rosell-Melé and Prahl (2013). In order to do this, we will expand upon the above-quoted statement regarding these doubts, and that they are mainly linked to alkenone flux. Our comparison of Pliocene SSTs to Holocene mean SSTs, generated by the same proxy at a proximal site, was also underpinned by a desire to investigate Pliocene-modern differences using the same technique and so with the same uncertainties (rather than comparing absolute values to instrumental data).

We also note that Holocene SST data points derived from UK'37 in the Nordic Seas (Iceland Sea core GS15-198-62MUC-A, this paper; Norwegian Sea, Calvo et al. 2002) and the Barents Sea (Risebrobakken et al., 2010) correspond most closely to summer temperatures.

—

-p6 line 13: number missing

Response: We will insert the missing grains/g number of the 4.2 Ma IRD peak.

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

Response:

In the updated manuscript we will move any deliberations about seasonal signals in the alkenone data into the method section, where this will be clarified. The seasonal imprint on any alkenone data should be similar in the Iceland Sea and the Norwegian Sea, since the growth period of alkenone producing coccolithophores is the (late) summer in both regions (Baumann et al., 2000). Hence, while there is most likely a general summer bias in the (absolute) data, the bias should be very similar between the Site 642 and the Site 907 data. Both SST data sets are most likely to reflect summer (JAS) SSTs than annual SSTs. We will clarify this in the updated methods section.

—

-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.

Response:

We appreciate this advice and will consider how best to apply it. We will include a comparison of our data with that of Lawrence et al (2009) from Site 982, as well as the extension of this data that was published in Herbert et al. (2016). We fully agree that a comparison of the available regional SST datasets is nevertheless important, and will do so in an updated discussion section. However, we consider it an important point of our study that the type of SST variability seen at Site 642 is of a different quality than the variability seen published for Site 982, by Lawrence et al (2009). While both records are of high resolution, the Lawrence data shows dominant orbital variability of several degrees C, while our new record shows variability on longer timescales that do not match a clear orbital periodicity. We fully agree that a comparison of the available regional SST datasets is nevertheless important, and will do so in an updated discussion section. The question why there is a much stronger obliquity signal at site 982 than in the Nordic Seas is an interesting one, and will receive further attention in the updated manuscript.

—

Comments on 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).

Response:

We will include SST shading gradient on an updated map.

-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?

Response:

We will display the two IRD data sets more clearly. The older IRD dataset by Jansen and Sjøholm (1991) has been updated to the new age model used in this study.

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

Response:

We will change this graph to a solid line.

-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.

Response: We will add the LR04 stack to these graphs as suggested.

Comments on Discussion:

-Holocene data: Relatively high variability (âĹij4 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).

Response:

We will include more information regarding the Holocene datasets and their internal variability. This variability is indeed in the 2-4°C range for the available Holocene alkenone SST datasets from the Nordic Seas (Calvo et al., 2002; Risebrobakken et al., 2010), and hence is not unusual in the new Holocene Iceland Sea dataset. We will expand upon this at an appropriate place early in the discussion. We will also make the full range of Holocene SSTs in the Iceland Sea and Norwegian Sea datasets clearer. New information on the age model of the Iceland Sea core will also be added.

—

-The subdivision used through the discussion follows the same framework as in Risebrobakken et al. (2016). Here, we do however not specifically include the transition at 4.65 Ma (considered to be the most uncertain in their study) and the one at 3.4 Ma. The one at 3.4 is not specifically mentioned due to the focus of this paper. However, in the new version, all of the transitions defined by Risebrobakken et al. (2016) is indicated in figure 4. The subdivision used should not be considered as a global or even more regional division, but it's a division that makes sense to use for the records from Site 642B, where clear long term changes occur in the records, at time scales that are much longer than between individual marine isotope stages. The low-frequency SST variability we see would necessarily bundle many MIS stogether and would place notable transitions in our SST record into several MIS. For example the (relatively) steep and steady SST increase from 4.05 to 3.95 Ma in our record falls into the MIS Gi22 to MIS G19 range. Hence, we do not believe that using MIS in our discussion would add much clarity to the discussion.

We agree that organizing the discussion by themes, and giving a stronger focus to the comparison of SST records, can enhance our manuscript. We will restructure the discussion to give more weight to the SST record and SST comparisons.

—

Continued comment:

-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

Printer-friendly version

Discussion paper

the various records. For example, if CO_2 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 CO_2 forcing can't really be treated separately until a reliable orbital-resolution CO_2 record exists for the Pliocene, because in the Pleistocene, CO_2 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 (Âż100 kyr) timescales then that would be consistent with a tectonic-driven CO_2 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.

Continued response:

We appreciate these suggestions and will provide a clearer discussion that is more thematically organized. We consider the CO_2 subchapter useful since it focuses on the problem of lacking high-resolution CO_2 records for the Pliocene. We will investigate whether a link with other low-latitude Northern Hemisphere records can be made. So far comparisons with records (not shown in the manuscript) have not shown a clear

Printer-friendly version

Discussion paper

correlation, which could be an argument against large-scale changes in AMOC-related heat transport. We also note that high-resolution SST records like the one we present are still not available from many sites, but acknowledge that new insights may be still be gained from presenting our record in different contexts and in comparison with other available records.

—

-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.

Response:

We agree and will emphasize the significance of these transitions.

-"extended cooling phases and relatively fast warming phases". Please quantify this statement.

Response:

We will add SST and time ranges to this statement.

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

Response:

This should only refer to Herbert et al. (2016), since that is the SST record we compare ours to. The other two papers do provide SST records of lower resolution, and also do not make assumptions regarding seasonality of SST data, but their datasets are not directly referenced. We will adjust the reference to only mention Herbert et al. 2016.

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

Response:

We will add here that our IRD counts are 1 to 2 orders of magnitude smaller than North Atlantic records mentioned by Heinrich (1988).

-p8 line 28: which changes?

Response:

We will clarify this sentence, based on the updated discussion section and a re-evaluation of our interpretations. The changes referred to here are all of the ones mentioned in the sentences above: gateway transitions, uplift events, and the impact of orbital parameters on insolation and seasonality. The overall updates made to the discussion section based on the reviewers comments will determine how this paragraph will be changed.

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

Response:

We agree that using MIS names for very short and time-specific events can add value, and we will add this to the discussion where appropriate.

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

Response:

We appreciate the advice and will incorporate it in the updated discussion section.

-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.

Response:

We will investigate this in a revised discussion section. We consider it important that there is such a weak apparent influence of obliquity at our study site in the Norwegian Sea (ODP Site 642B), when compared to the strong obliquity-scale UK'37 SST variability at Site 982. Hence we consider a discussion of the varying impact of obliquity, albeit speculative, to be of interest.

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

Response:

We add more descriptive references in support of this idea Steph et al. (2010) and Karas et al. (2017). The important links are the timing of regional climate changes linked to the CAS, where 4.0 Ma appears as an important inflection point in Early Pliocene data (e.g. the trend of Pacific thermocline shoaling, which is proposed to be linked to CAS development). Taking into account all reviewer comments, will generally give less weight to the CAS and AMOC interpretation and more space for other interpretations, such as regional water mass shifts and other gateway changes (e.g. Greenland-Scotland Ridge), which likely also affected the Nordic Seas SSTs.

—

References cited in these responses:

Andruleit, H.A., 1997. Coccolithophore fluxes in the Norwegian-Greenland Sea: Seasonality and assemblage alterations. Mar. Micropaleontol. 31, 45–64. doi:10.1016/S0377-8398(96)00055-2 Baumann, K.-H., Andruleit, H.A., Samtleben, C., 2000. Coccolithophores in the Nordic Seas: Comparison of living communities with surface sediment assemblages. Deep. Res. Part II Top. Stud. Oceanogr. 47, 1743–1772. doi:10.1016/S0967-0645(00)00005-9

Bell, D.B., Jung, S.J.A., Kroon, D., 2015a. The Plio-Pleistocene development of Atlantic deep-water circulation and its influence on climate trends. Quat. Sci. Rev. 123, 265–282. doi:10.1016/j.quascirev.2015.06.026

Bell, D.B., Jung, S.J.A., Kroon, D., Hodell, D.A., Lourens, L.J., Raymo, M.E., 2015b. Atlantic Deep-water Response to the Early Pliocene Shoaling of the Central American Seaway. Sci. Rep. 5, 12252. doi:10.1038/srep12252

Böhm, E., Lippold, J., Gutjahr, M., Frank, M., Blaser, P., Antz, B., Fohlmeister, J., Frank, N., Andersen, M.B., Deininger, M., 2015. Strong and deep Atlantic meridional overturning circulation during the last glacial cycle. Nature 517, 73–76. doi:10.1038/nature14059

Bradtmiller, L.I., McManus, J.F., Robinson, L.F., 2014. 231Pa/230Th evidence for a weakened but persistent Atlantic meridional overturning circulation during Heinrich Stadial 1. Nat. Commun. 5, 5817. doi:10.1038/ncomms6817

Brierley, C.M., Fedorov, A. V., 2016. Comparing the impacts of Miocene–Pliocene changes in inter-ocean gateways on climate: Central American Seaway, Bering Strait, and Indonesia. Earth Planet. Sci. Lett. 444, 116–130. doi:10.1016/j.epsl.2016.03.010

Calvo, E., Grimalt, J.O., Jansen, E., 2002. High resolution UK37 sea surface temperature reconstruction in the Norwegian Sea during the Holocene. Quat. Sci. Rev. 21, 1385–1394. doi:10.1016/S0277-3791(01)00096-8

Haywood, A.M., Valdes, P.J., 2004. Modelling Pliocene warmth: contribution of atmosphere, oceans and cryosphere. Earth Planet. Sci. Lett. 218, 363–377. doi:10.1016/S0012-821X(03)00685-X

Haywood, A. M., Hill, D. J., Dolan, A. M., Otto-Bliesner, B. L., Bragg, F., Chan, W. L., Chandler, M. A., Contoux, C., Dowsett, H. J., Jost, A., Kamae, Y., Lohmann, G., Lunt, D. J., Abe-Ouchi, A., Pickering, S. J., Ramstein, G., Rosenbloom, N. A., Salzmann, U., Sohl, L., Stepanek, C., Ueda, H., Yan, Q. and Zhang, Z.: Large-scale features of Pliocene climate: Results from the Pliocene Model Intercomparison Project, Clim.

Past, 9(1), 191–209, doi:10.5194/cp-9-191-2013, 2013.

Heinrich, H., 1988. Origin and consequences of cyclic ice rafting in the northeast Atlantic Ocean during the past 130,000 years. Quat. Res. 28152, 142–152. Hill, D.J., 2015. The non-analogue nature of Pliocene temperature gradients. Earth Planet. Sci. Lett. 425, 232–241. doi:10.1016/j.epsl.2015.05.044

Jansen, E., Fronval, T., Rack, F., Channell, J.E.T., 2000. Pliocene-Pleistocene ice rafting history and cyclicity in the Nordic Seas during the last 3.5 Myr. Paleoceanography 15, 709–721. doi:10.1029/1999PA000435

Karas, C., Nürnberg, D., Bahr, A., Groeneveld, J., Herrle, J.O., Tiedemann, R., DeMenocal, P.B., 2017. Pliocene oceanic seaways and global climate. Sci. Rep. 7, 39842. doi:10.1038/srep39842

Locarnini, R.A., Mishonov, A. V., Antonov, J.I., Boyer, T.P., Garcia, H.E., Baranova, O.K., Zweng, M.M., Paver, C.R., Reagan, J.R., Johnson, D.R., Hamilton, M., Seidov, D., 2013. World Ocean Atlas 2013. Vol. 1: Temperature, in: S. Levitus, Ed.; A. Mishonov, Technical Ed.; NOAA Atlas NESDIS. p. 40 pp. doi:10.1182/blood-2011-06-357442

Maier-Reimer, E., Mikolajewicz, U., Crowley, T.J., 1990. Ocean general circulation model sensitivity experiment with an open Central American isthmus. Paleoceanography 5, 349–366.

Ravelo, A.C., Andreasen, D.H., 2000. Enhanced circulation during a warm period. Geophys. Res. Lett. 27, 1001–1004. doi:10.1029/1999GL007000

Raymo, M.E., Grant, B., Horowitz, M., Rau, G.H., 1996. Mid-Pliocene warmth: stronger greenhouse and stronger conveyor. Mar. Micropaleontol. 27, 313–326.

Risebrobakken, B., Moros, M., Ivanova, E. V., Chistyakova, N., Rosenberg, R., 2010. Climate and oceanographic variability in the SW Barents Sea during the Holocene. The Holocene 20, 609–621. doi:10.1177/0959683609356586

Robinson, M.M., Valdes, P.J., Haywood, A.M., Dowsett, H.J., Hill, D.J., Jones, S.M., 2011. Bathymetric controls on Pliocene North Atlantic and Arctic sea surface temperature and deepwater production. Palaeogeogr. Palaeoclimatol. Palaeoecol. 309, 92–97. doi:10.1016/j.palaeo.2011.01.004

Rosell-Melé, A., Prahl, F.G., 2013. Seasonality of UK'37 temperature estimates as inferred from sediment trap data. Quat. Sci. Rev. 72, 128–136. doi:10.1016/j.quascirev.2013.04.017

Steph, S., Tiedemann, R., Prange, M., Groeneveld, J., Schulz, M., Timmermann, A., Nürnberg, D., Rühlemann, C., Saukel, C., Haug, G.H., 2010. Early Pliocene increase in thermohaline overturning: A precondition for the development of the modern equatorial Pacific cold tongue. Paleoceanography 25. doi:10.1029/2008PA001645

Zhang, X., Prange, M., Steph, S., Butzin, M., Krebs, U., Lunt, D.J., Nisancioglu, K.H., Park, W., Schmittner, A., Schneider, B., Schulz, M., 2012. Changes in equatorial Pacific thermocline depth in response to Panamanian seaway closure: Insights from a multi-model study. Earth Planet. Sci. Lett. 317–318, 76–84. doi:10.1016/j.epsl.2011.11.028

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-131, 2016.

Printer-friendly version

Discussion paper