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Climate of the Past Discussions

Interactive comment on "Land–sea coupling of Early Pleistocene glacial cycles in the southern North Sea exhibit dominant Northern Hemisphere forcing" *by* Timme Donders et al.

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The authors set out to test the phase relation between forcing and climate response around the Plio-Pleistocene transition. They use a composite record from the North Sea Basin and present new palynological and geochemical records to document the marine and terrestrial climate evolution. The site in a shallow marine sea is ideally suited to couple the climate signal from both realms. The authors identify variations in their data between G and IG, and conclude that NH obliquity forcing is the main driver for G-IG cycles in the Early Pleistocene.

I have some concerns about the age model (pmag, bioevents), the identification of

leads/lags, use of cutting samples in paleoclimate studies and the environmental interpretation of the dinocysts.

Age model.

Discussing phase relations between land, ocean and ice sheets hinges crucially on the age model. While the presented work is underpinned by previously published papers and insights into depositional environment (papers by Kuhlmann and co-authors), aspects of the age model can be questioned.

The authors rely here on the G/M reversal and the X-event for constraining the age of their studied interval (L152–155). Kuhlman and Wong (2008) discuss in fact 4 different possible interpretations of the pmag. It seems very questionable to me that the very short-lived X-event (2.420–2.441 Ma, Cande and Kent, 1995) can be detected in the sedimentary record of a shallow sea by measuring the magnetic signal of discrete samples (Kuhlman and Wong, 2008). This event does not show up in u-channeled, high-resolution pmag records of the North Atlantic (e.g. Hoddell and Channell 2016; Channell et al 2016), neither has it been tied to the LR04 Marine Isotope Stratigraphy. The dinocyst bioevents generally point to the Plio-Pleistocene, but the events are not well-recognised (e.g. Barssidinium, M. choanophorum) or not calibrated (e.g. I. multiplexum) outside the North Sea Basin. This questions the age assigned to these events and thus also the age model. Using additional/different tiepoints that have been calibrated outside the North Sea Basin could provide more credibility to the age model used (see below). Based on these concerns about the age model, it remains uncertain 1) whether the cycles visible in the gamma-ray reflect the interval MIS102–96 and 2) whether these are truly, consecutive (i.e. no erosional events in between) G-IG cycles.

Leads/lags.

The leads/lags between climate proxies and sea level are not so clearly visible as the authors claim in the abstract and conclusions. The leads/lags are not clearly demonstrated on a figure, or more importantly using statistical techniques. In fact, they are

only mentioned on L495–496 in the discussion. Furthermore, in L491-493, the authors write that "...regional NH climate on land and sea surface varies in concert with local relative sea level...". That, to me, suggests there are no leads/lags.

Detailed comments on the bioevents.

Several of the events used in Kuhlmann et al. 2006 (Palaeo-3) are ecologically controlled events (e.g. acmes, peaks in O. israelianum) that have not been detected outside the North Sea Basin. Even the LOD of I. multiplexum has not been calibrated to an independent time scale. Including these points as tiepoints thus make the age model questionable (Fig. S3). Generally, the assemblage does point to the Late Pliocene / Early Pleistocene. For example, the increase in H. tectata is a well-established event in the eastern North Atlantic at the Plio-Pleistocene transition ($\sim$2.6 Ma, De Schepper and Head 2009; Hennissen et al. 2015). But the choice of other bioevents used can be questioned: 1. LOD Barssidinium spp.: Barssidinium pliocenicum ranges up to 2.0 Ma in Iceland (Verhoeven et al. 2011) and 1.95 Ma in St. Erth Beds (Head 1993). In the southern North Sea (Belgium) it extends into the Merksem Sands ($\sim$2.6 Ma), but Early Pleistocene deposits are not present there. 2. LOD M. choanophorum. The HO is strongly diachronous in the Atlantic region, making this a bad species to use for biostratigraphy. It has a HO in Norwegian Sea $\sim$3.3 Ma, De Schepper et al. 2017); HCO around 3.0 Ma, but frequent records up to 2.75 Ma and rare occurrences in the Pleistocene of DSDP Hole 610 (De Schepper & Head 2009); HO in southern North Sea $\sim$3.2–2.7 Ma (De Schepper et al. 2009; Louwye et al. 2004, Louwye and De Schepper. 2010). In fact, this species is still around in the modern Gulf of Mexico (Limoges et al. 2013). Suggestions to improve the age model include using bioevents listed in De Schepper and Head 2008 (Stratigraphy): I. lacrymosa has a well-established and relatively synchronous HO in the eastern North Atlantic and Mediterranean in or immediately prior to MIS G4–G6. O.? eirikianum has a HO in $\sim$2.6 Ma (MIS 104–103) in the eastern North Atlantic, while in the subtropical North Atlantic it extends to 2.34 Ma ($\sim$MIS 94) (De Schepper et al. 2008).

Cuttings.

What is the effect of using cutting samples (caving, reworking) on your interpretations? This should be discussed in more detail. Cuttings can introduce caved material and together with reworked material is extremely difficult to extract meaningful paleoclimatic and biostratigraphic signals. A detailed account sample type (cutting, SWC, core) for each proxy would be useful to assess the effect of cuttings on the records. Figure 2 does show the use of different sample types, but the effect on the proxies is not discussed adequately. For example, was the FOD of I. multiplexum identified in a cutting sample? Could it be an artefact of the drilling procedure (i.e. younger material introduced into older material due to caving)?

Environmental signal from dinocysts.

The 4 species used to indicate a warm water signal are all coastal, shallow water species (L225–227). Their distribution in the shallow North Sea Basin could be strongly affected by SL fluctuations at the beginning of the Early Pliocene. Versteegh (1994) therefore does not include these taxa in a warm-cool index. Furthermore, L. machaerophorum is often used to indicate river input and sea level fluctuations (Holzwarth et al. 2010). How do you disentangle the effect of sea level and temperature for these 4 species, when their distribution could be affected by both? The T/M ratio is interpreted as a relative SL indicator. While this intuitively seems correct, I wonder if the relation is that simple? Terrestrial palynomorphs are affected by transport patterns (wind, position of rivers) and could thereby influence the sea level interpretation?

Minor comments.

L45 There is hardly proxy data to say something about MIS 100.

L47 Freshwater flux is not really supported by the fresh water algae.

L50-51 Confusing. Please rephrase.

L52 SST is not a good indicator of migration of a watermass. Microfossil assemblage

could help you with identifying such migration, but not SST alone.

L73 space missing before “and”

L105 rephrase “but which stratigraphic position”

L110 During the Neogene, there could have been a southerly connection between the North Sea and Atlantic (see reconstructions of e.g. Gibbard and Lewin 2003, 2016). It might be worth to use the more recent Gibbard and Lewin 2016 palaeogeographic reconstruction in stead of Zeigler 1990 (Figure 1).

L121 “different water types”: only reference to Atlantic water?

L123 blue, not black?

L126–128 Please provide a timeframe. Did this occur in the entire Neogene? Pliocene/Pleistocene only?

L157–162 This model would get more credibility if this has also been demonstrated for late Pleistocene glacial/interglacial cycles. Would the SL drop of up to 60 m in these glacials (e.g. Miller et al. 2005; Bintanja et al. 2005) not provide a stronger control on the sedimentation (rather than hydrography)?

L165 How was the age model transferred to LR04 MIS? This is not clear. Fig. S3 shows an age model, not the link with the LR04 stratigraphy.

L174–175 Please check also De Schepper et al. 2017.

L177–L186 Does this paragraph belong in the age model section?

L190 C. teretis in italics.

L202 How was recrystallization and dissolution determined? SEM analyses would be necessary. See the need for SEM in e.g. Risebrobakken et al. 2016 (Paleoceanography).

L211 de Vernal (no capital D)

L270 delete “, dinocysts”

L273 Why were there only relative abundances calculated? Typically, concentrations (and accumulation rates) provide support for your relative abundance based interpretations.

L304 Delete “For TOC determination”

L359 Not convincing when looking at Fig. 3, because the data is mainly restricted to MIS 97 and 95/94. There are very few data points in MIS 96 and 98, and none in MIS 100 – the three intervals where gamma-ray values are highest. L363 vary, not very

L372 diverse

L377 Are herb and heath pollen dominant? Pinus remains the dominant species. Please make clear that you are discussing the pollen record, excluding pine pollen.

L383 Which fresh water algae did you find?

L401-402 What does the n-C23 Sphagnum biomarker indicate in terms of the climate system/environment?

L413 MIS 96/95 (space missing)

L420-421 It needs to be better documented how LR04 MIS transitions are recognized (see earlier).

L422 Tables S and 2?

Fig. 3 The Lingulodinium machaerophorum record should be presented separately – difficult to see now.

L428–. . . Chapter 6.1 is confusing and does not really deal with paleoenvironment. It is not clear which MIS is discussed, and the switching between proxies (e.g. L429–433) and time intervals (all glacial/interglacials, MIS 98/97, 94 and 92) makes this difficult to follow.

L432 depend (not depends)

L434 Effect of SL on pollen is addressed here, but the effect of SL on the dinocyst record is not discussed in the MS.

L485, L535 Onset/intensification have been used intermixed. I think it is commonly accepted that the onset of NHG occurs at 3.6 Ma (e.g. Mudelsee and Raymo 2005) and that for the period around the Plio-Pleistocene transition the term intensification should be used. L492, 496 Please make a reference back to the proxy that is used to infer local relative sea level. If you are referring to the T/M ratio as your sea level proxy, do take into account that T is affected by different transport mechanisms (L251-258).

L495–496 What is small – please specify? Please indicate which figure shows the small lead.

L510 Speculation.

L518 Severe cooling. Subjective comment, certainly if you know that L. machaerophorum does not occur in regions with summer SST below 15ËŻC. This species is present in all glacials.

Fig. S3 Please provide a list with the tiepoints (event, type, age, age scale used).

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