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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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In this manuscript Donders et al. provide new proxy records from an early Pleistocene (MIS 103-92) marine sediment core from the North Sea. They use an impressive array of inorganic and organic proxies (e.g. pollen, dinoflagellates, brGDGTs) to reconstruct changes in paleoenvironment (temperature and vegetation) in the hinterland and regional surface water conditions (sea surface temperature). Based on this data they argue that low land and sea temperatures characterized glacial stages, while higher temperatures and influx of riverine freshwater characterized interglacials. The main conclusion is that these climatic changes appears to be obliquity paced and not

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Discussion paper

precession-drives as has been suggested before (Raymo et al., 2006).

I am not an expert in pollen or dinoflagellates and will therefore focus my review on the organic geochemistry and overall conclusions.

The main focus of the manuscript is the “proposed disputed on the phase relation between forcing and climatic response in the early Pleistocene”. I agree with the authors that such a dispute exists and answering it is of importance for our understanding of the climate system. However, the discussion on this specific topic in this manuscript is rather limited and is missing a discussion of crucial prior work on this topic. This is important as without such detailed discussion on the global implications of the findings, the paper provides not much more than a regional climatic story, making it of much less interest to the diverse readership of climate of the past. I think this work should be published in climate of the past, but I suggest that the authors provide a much more thorough discussion and include key references on this topic such as (Shakun et al., 2016), (Naafs et al., 2012), and (Patterson et al., 2014) in the revised manuscript.

In addition, I wonder whether the age model is robust enough. The low-resolution benthic d18O record of this site does not always look like the LR04 stack. In particular, MIS 94 is characterized by very negative d18O values in the core, while this is not obvious in the LR04 stack. I urge the authors to explore the uncertainty of the age model more. In terms of looking at forcing of climate during the early Pleistocene, it does not really matter what MIS we look at, but for comparison with other proxy records it does.

Minor comments in order of appearance: Line 51-55: this is a bit of a weird ending of the abstract, especially in the context of the main focus of the paper that is stated at the beginning of the abstract. The authors should end the abstract with a clear conclusion of what, according to their work, the phase relation is between forcing and climatic response.

Line 66: a full review paper on IRD in the North Atlantic during the Plio/Pleistocene is

given in (Naafs et al., 2013)

Line 73-78: somewhere make reference to mechanism proposed in (Haug et al., 2005)

Line 82-88; here other recent publications that refute or support Raymo's hypothesis (e.g., Naafs et al., 2012; Patterson et al., 2014; Shakun et al., 2016) should be introduced to provide a clear context for the rest of the paper and main focus of the paper.

Figure 1: the asterisk that marks the core location is hard to see when printed in black and white. Modify.

Line 202: what statistical basis was used to reject samples? What is the distinction between poor and not poorly preserved?

Line 280: cite (Eglinton and Hamilton, 1967) for odd over even predominance of n-alkanes.

Line 289: change sentence to ". . .. . .(brGDGTs), produced by bacteria and that are abundant in soils, versus that. . ..."

Line 290: add reference to (Sinninghe Damsté et al., 2002) for crenarchaeol

Figure 3: this figure is really cramped and it is very hard to see trends and what axis belongs to which curve.This figure should be split into several individual figures, I propose to have one focusing on the palynology, one on the inorganic and another on the organic geochemistry. Also, up-side-down axis and labels are hard to read.

Line 467: is there any other supporting information for the input of acidic peat input? For example, modern-day acidic peats are characterized by the dominance of the C31 ab-hopane (Dehmer, 1995; Pancost et al., 2002), which is normally only present in mature sediments. Is this seen in this immature marine sediment core as well? This could provide some key-supporting evidence for this statement of peat input.

Line 473-477: The authors should provide a ternary plot of the brGDGT distribution like used by (Sinninghe Damsté, 2016) and compare their brGDGT data with published

mineral soil (De Jonge et al., 2014) brGDGT distributions to rule out a significant non-terrestrial contribution.

Section 6.2: this section should be more extensive with a detailed discussion of the results from this sediment core in the context of previously published results and our understanding of Quaternary climate.

For the supplementary information, can the authors provide the abundances of the individual brGDGTs (and crenarchaeol) so that if the indices used for the soil-calibrations change in the future, the data can be easily recalculated and still be used in future studies.

David Naafs

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