

Interactive comment on “Mid-Pliocene shifts in ocean overturning circulation and the onset of Quaternary-style climates” by M. Sarnthein et al.

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1 Summary

This paper is primarily a literature review of (some) key hypotheses for explaining Late Pliocene Greenland glaciation, focusing on the Panama Hypothesis. In the final section, there is some new modelling presented which provides a refinement to the Panama Hypothesis in which the warming generally seen in previous modelling studies over Greenland following closure of the Panama Seaway (which would tend to inhibit inception) is counteracted by an intensification of the East Greenland Current (EGC) which brings cold polar waters southwards to eastern Greenland. It is suggested that this thermal isolation, in conjunction with increased precipitation over Greenland would

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favour the expansion of the Greenland Ice Sheet (GIS). This is an interesting hypothesis.

I would like to highlight that I carried out this review prior to reading any online comments from other reviewers, so my comments are independent.

2 General Comments

My main problem with this paper is that the two key pieces of evidence for this new hypothesis (1) the data supporting a long-term cooling of the EGC and (2) the modelling of the more intense EGC, are both unpublished, and not explained in enough detail here, by a long way. The key data (Bartoli et al, 2009) is submitted to Geology but I have no way of properly assessing it. The modelling (as summarised in Figure 15), cited as unpublished, needs much more explanation. What was the resolution of the model? What were the boundary conditions? Was a cooling/snow accumulation found over Greenland in support of the hypothesis? What was the heat transport of the EGC etc. etc. etc.

3 Specific Comments

A few comments on the discussion of the CO₂ hypothesis - (1) The change in CO₂ does change precipitation over Greenland, but it is in the wrong direction for inception, and of a small magnitude (in terms of impact on icesheets) relative to the temperature change. (2) An error of 100ppmv is not "much larger" than a shift from 400 to 280. (3) The CO₂ record cited (pagani et al, 2005). I don't think that deConto should be cited for this record. he may have used it but did not develop it) does not extend into the period in question but is from 45Ma to 5Ma. (4) The current CO₂ records are not necessarily

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of high enough resolution to detect a 400kyr signal. Also, I don't see the lack of this signal is a necessary weakness, seeing as we don't even know if it should be in the CO₂ record or not. (5) The fact that the CO₂ hypothesis does not give a reason for the CO₂ change is not a weakness; it is true that we need to find the underlying cause, but this is not a reason to question the hypothesis itself. (6) If the record of Foster is cited, then it could also be said that the record indicates a clear drop in pCO₂ at 2.75 Ma from about 400ppmv to about 280ppmv! However, this is just an AGU abstract so it should not really be cited at all unless it is said to be preliminary and/or unpublished.

The abstract needs to more clearly differentiate which sentences are accepted fact (e.g. onset of large glaciation in late Pliocene), what is supported by almost all studies (e.g. changes in ocean circulation and climate following seaway closure), what has previously been proposed but is not necessarily universally accepted (e.g. changes in ocean circulation could have led to increased arctic sea-ice due to modified river runoff), and what new is presented in this study (importance of Bering Strait and EGC).

Many figures appear to be directly lifted from previous papers. What copyright implications does this have? E.g. figures 5 and 9 appear to be more or less directly copied from my own papers. Whereas I am personally OK with this in principle, I guess that the copyright is held by the journal (certainly for figure 5), and if they are to be copied then permission should be sought from the original publishers?

Can you be more specific about "we wonder, whether sea ice-based albedo effects were sufficiently considered in the ice model of Lunt et al. (2008)" on P258. The albedo (and thickness) of the surface certainly varies as sea ice area grows and shrinks in our model in response to ocean and atmospheric forcings.

P258. Can you be more specific about the objections of Molnar?

P262, line 18. what model predictions are you referring to which are not in agreement with this oxygen isotope shift? As far as I am aware, no model of Panama closure has included δ¹⁸O tracers (although this would be a very nice study to carry out!).

P262 why should freshening of the EGC indicate an increase in seaice. If anything, more seaice would result in a more saline ocean? Or have I mis-understood?

The Conclusions are framed in a rather definite style. for me this is more a hypothesis that requires further testing.

4 Minor Comments

Word missing from first sentence in abstract.

References needed throughout abstract.

I prefer large or very large or large magnitude to massive.

I prefer variations or variability to ups and downs.

Figure 4 needs much more explanation in the caption. E.g. what are the arrows?

P258, line 6: "to" should be "too".

Interactive comment on Clim. Past Discuss., 5, 251, 2009.

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