Clim. Past Discuss., 5, S225–S238, 2009
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Climate of the Past Discussions

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Interactive Comment

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

M. Sarnthein et al.

Received and published: 13 May 2009

RESPONSE to comments of Daniel LUNT

1 Summary

As summarized by D. Lunt, our paper is indeed intended as synthesis to review and discuss recent published evidence (whereto most authors have contributed them–selves a lot over the last years) and various conflicting hypotheses on the origin of major Northern Hemisphere Glaciation (NHG). Moreover, the paper contributes unpublished modelling results on (1) the evolving salinity and temperature contrasts between the S.W. Caribbean Sea and the eastern equatorial Pacific and (2) the increased Arctic Throughflow as result of the final closure of the Panama Seaway and its consequence for the incursion of cold polar waters with the East Greenland Current (EGC) to the

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southern tip of Greenland, which have supported the Pliocene onset of major glaciation on Greenland ('Panama Hypothesis').

2 General Comments

(Complaints about insufficient access to unpubl. key pieces of evidence):

2.1 *The raw Mg/Ca-based SST data* supporting a long-term cooling of the EGC between 3.5 and 2.7 Ma ago (Bartoli et al., 2009; subm. to EPSL) are fully displayed vs age in Fig. 14, an information we regard as sufficient to the reader. Hopefully the publication will be accepted soon. The underlying age models (p. 260, line 23ff) are now discussed with more detail in the context of Fig. 13_{new}.

2.2 *Questions of the reviewer concerning some major boundary conditions* of the model which shows the increased Arctic Throughflow and EGC, are now answered in the caption of Fig. 15:

“Increased (doubled) flow rate of low-saline surface water through the Bering and Fram straits (“Arctic Throughflow”) as a result of closing the CAS as simulated by the NCAR Community Climate System Model, version CCSM2/T31x3a (Prange, 2008). Shown is the difference between two equilibrium runs (one with closed CAS, the other with open CAS arbitrarily applying a sill depth of 800 m and a width of ca. 200 km) in which the model was forced with present-day boundary conditions (i.e. a sensitivity study rather than a 'true' simulation of Pliocene climates). The ocean component has a mean resolution of 3.6° x 1.6° with 25 levels, while the atmosphere has a resolution of 3.75° x 3.75° with 26 levels. For a detailed description of the experimental set-up we refer to Steph et al. (2006).“

Moreover at p. 263, line 11ff:

...A recent model study (Steph et al., 2006) now suggests a suitable mechanism not yet considered (Fig. 15). Using the NCAR Community Climate System Model, version CCSM2/T31x3a (Prange, 2008), it shows that the full closing of CAS led to an increased steric height in the North Pacific (+15 cm as compared to the open CAS scenario), a decreased steric height in the North Atlantic (-20 cm) and, as a consequence, a doubling of the low-saline Arctic Throughflow ($\sim$1 Sv today) from the North Pacific through the Bering Strait up to a strongly intensified EGC, when assuming a Holocene sea level stand...

and at p. 264, line 5:

...Since this time the EGC formed a barrier important to promote the growth of continental ice. However, this feature is difficult to assess by low-resolution climate models. Nevertheless, while almost the entire Northern Hemisphere becomes warmer in response to CAS closure, the simulations with CCSM2/T31x3a exhibit a year-round regional cooling over Central and South Greenland (up to 2°C) along with enhanced annual snowfall (up to 50 kg/m2 in South Greenland). Further details of these model results will be presented elsewhere (Prange and Schulz, in prep.).

The Greenland continental ice sheet, once...

In contrast, the question of D. Lunt "*what was the heat transport of the EGC" ?* can not be answered because any calculation of this transport would require a hardly substantiated assumption of a reference temperature.

3 Specific Comments

(*Six remarks on our discussion of Lunt's CO_2 hypothesis for the onset of NHG*)

(1) (*Minor precipitation drop over Greenland found as result of CO_2 decrease*)

The reviewer is right that the change in CO_2 results in a very minor, almost zero precipitation change in Greenland. However, there are slight misunderstandings of the reviewer, that we shall try to straighten out by more specific wording: In our text (p. 262, last paragraph, and p. 263, line 2-9) we did not discuss the precipitation over Greenland but that over the *total* northern Hemisphere as factor that induces a freshening of the Arctic Sea and in turn, will induce the onset of NHG.

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In contrast to the view of the reviewer, a minor to major precipitation rise did indeed result from his models testing both the 'Panama hypothesis' (e.g., over East Greenland, the northern North Atlantic, and Scandinavia; as reproduced in our Fig. 9) and the CO_2 decrease (although here confined to the east of Greenland and Scandinavia) (Lunt et al., 2007, 2008). On the other hand, we ourselves arrived at the conclusion (p. 263, line 8) that this precipitation rise still appears insufficient to generate the observed SST decrease and freshening of the EGC.

(2) (*Error of $\pm$100 ppmv CO_2 is not much larger then a shift from 400- to 280 ppmv*)

Kürschner's et al. stomatal-density-based atm CO_2 values have an uncertainty of 60-120 ppmv (their Fig. 2), strongly oscillate with single points between 260 and 370 ppmv (their Fig. 6), but do not show any long-term trend toward lower values in the Pliocene, 5-2 Ma ago. Likewise, Raymo et al. (1996; their Fig. 5) show a suite of atm CO_2 variations between 370 and 470 ppmv from 3.3 to 2.9 Ma ago, with two minima at 3.06 and 3.00 Ma ago and a subsequent maximum and an uncertainty range of $\pm$120, but no long-term trend at all. Thus we don't understand how to infer a general atm CO_2shift from 400 to 280 ppmv from these records as claimed by Lunt et al. (2008).

To clarify our statement, we now state at p. 255, lines 28-29, ..."a range of uncertainty of $\sim$120 ppmv, that is as large as the shifts investigated, and..."

(3) (*Proper citation and extent of Neogene CO_2 record*)

We follow the reviewer and replace the citation of deConto et al. (with Pagani as co-author) at p. 256, line 2, by Pagani et al. (2005), moreover by Zachos et al. (Nature, 2008), who also depict a boron-based CO_2 record for 5–0 Ma, although poorly resolved. Both the alkenone- and boron-based records clearly document a very uniform and persistent CO_2 level over the last 20 m.y., with an upper uncertainty limit near 280-330 ppmv, however, without any general decrease around 3 Ma.

(4) (*Implications of a potential 400-kyr signal in CO_2 record*)

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In harmony with the reviewer we have only discussed at p.256, line 4-5, a "potential" 400-ky cycle, recently also described in more detail by Wang et al. (2009; now added to our list of references).

We agree that this excentricity signal is not really established yet for any existing CO_2 record. However, it definitely presents a major potential caveat to an assumption of any long-term unidirectional shift in the mid-Pliocene CO_2 level, a caveat that we regard as legitimate to mention in our discussion.

(5) (*Unknown underlying cause for a potential long-term CO_2 change is not a weakness for the CO_2 hypothesis)*

As claimed in our text on p. 256, lines 7-12, we fully agree with the reviewer that his CO_2 hypothesis will play an important role for the onset of major NHG. However, we still consider it as legitimate (as confirmed by D. Lunt) to ask for the primary forcing of the CO_2 drop and not to regard this CO_2 drop *per se* as the primary cause but just as (almost trivial) amplifyer mechanism for the onset of NHG.

To specify this intent we now add the implication "amplifyer" to our text.

(6) (*Proper citation of the new boron-based results of G. Foster, 2008)*

We now follow the advise of the reviewer and cite the new boron record more precisely as (Foster et al., 2008; preliminary data presented as AGU poster) (p. 256, line 16). Also, we see no problem with modifying our text at p.256, line 18, as follows:

..."", but finally, **around 2.75 Ma ago**, summed up to a long-term decline of **100-120** ppmv."

(*Better differentiate well-established, less-well established, and newly contributed evidence for the origin of Quaternary-style Climates in our abstract)*

We agree with the reviewer and modify the wording of the abstract, now separating different categories of evidence quality (page 252, line 2-20).

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(*Copyright implications*)

We have properly cited sources and formally requested (paid and obtained in 02-2009) the copyright from journal publishers for Figs. 1, 8 and 9, that is for figures not modified for presentation in our paper. Requests for Figs. 3 and 5 (modified) are still in the pipeline.

(*Specify the doubts in the consideration of sea ice-based albedo effects in the ice model of Lunt et al.)* p. 258, line 8-10ff (now rewritten):

...We wonder, however, to which degree the HadCM3 global climate model is able to capture the full possible range of regional sea-ice variations in the Arctic and the corresponding changes in surface albedo. The model used in the study of Lunt et al. (2007, 2008) does not include a realistic sea-ice rheology (Bryan, 1969) and uses simplistic parameterizations for sea-ice albedo (Gordon et al., 2000). Therefore, climate feedbacks that involve changes in sea-ice cover and the associated surface albedo are subject to large uncertainties in these model simulations. Moreover, we note that ice albedo is commonly used as a tuning parameter in coupled climate models, which makes the models less reliable with respect to predictions of future or past Arctic sea-ice changes (Eisenman et al., 2007). Recently...

(*Specification of the objections to the 'Panama hypothesis' of Molnar, 2008*)

On the one hand the "libel" of Molnar covers such a broad field of critiques that would require a separate full paper to be replied convincingly. On the other hand, he did not even cite evidence of papers that may contradict his views. Thus we cite Molnar only shortly in our section 3 "previous models and concepts" (p. 258, line 10-12), but refer more specifically to his theses in the text, wherever pertinent pros and cons are displayed (e.g., at p. 259, line 7-10; p. 260, line 15-16). As suggested by D. Lunt, we now add some further (counter-) citations.

(*Which model predictions are referred to at p. 262, line 18, that include δ^{18}O tracers?)*

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To clarify this question, we realize the need to explain at line 17 the term $\delta^{18}O_w$ (i.e. not: $\delta^{18}O$) as "$\delta^{18}O_{water}$" which closely resembles $\delta^{18}O_{salinity}$ (except for the fact that $\delta^{18}O_w$ has not been corrected yet for changes in the global ice effect). Moreover, we now specify that we consider model predictions of enhanced poleward atmospheric moisture transport (citing Klocker et al. and Lunt et al.) which in turn necessarily results in a reduced sea surface salinity of the EGC.

(*Why does a freshening of the EGC indicate an increase in Arctic sea ice ?)*

Here we don't understand the reasoning of the reviewer. Salinity changes in the EGC are directly controlled by salinity changes in its source region, that are changes in Arctic surface waters, as expressed in our text. Here any drop in salinity will necessarily induce more sea ice formation. This process certainly leads to brine formation and a salinity increase of deep waters, but not to more s.w. salinity of the EGC.

(*Rather definite style of conclusions*)

In some way our definite style follows the example of the title and abstract of Lunt's (2008) nature paper. Nevertheless, we now somewhat caution our wording wherever appropriate to meet at least some of the reviewer's objections.

4 Minor Comments

All suggestions are incorporated into the text.

RESPONSE to comments of Reviewer #2

(*General Comments*)

We acknowledge with thanks the detailed assessment of our major lines of evidence displayed and the overall processes discussed in our paper.

1 Introduction *(Motivation of this paper*)

As the reviewer we clearly arrive at the conclusion that the link between mid Pliocene

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warming of the northern Atlantic and the coeval increase of glaciation on Greenland cannot be regarded as analogue for the modern trend of global warming. However, we may assume that a number of colleagues silently may still ponder this question, e.g., because the widely used satellite-based evidence for Greenland ice melt is not that well substantiated yet. Just recently, a poster of Hvidegaard & Sandberg (DTU Space, DK-2100) at EGU Vienna, 2009, showed air plane-based records which reveal a widespread slight gain in ice thickness in contrast to the vast ice reduction inferred from satellite records. Also, the contrast between Pliocene and modern trends may be of interest to a wider community, which this synthesis paper tries also to address, colleagues who are interested in modern climate warming but not that familiar with the details of past climate evolution.

2 Conceptual models (*Proper citation and display of Pliocene CO_2 record; more consistent display of our view of the CO_2 hypothesis*)

As stated under 3 (3) in our response to Reviewer D. Lunt, we now replace our reference by Pagani et al. (2005) and in particular, by Zachos et al. (2008) who have extended the Neogene CO_2 record until today. Unfortunately we can not include yet the highly interesting but obviously still premature CO_2 record of G. Foster into our Fig. 12, a record which has been only published as AGU poster in Dec. 2008,.

Also, we now aim to display a more consistent view of the CO_2 hypothesis, including a more detailed display of the links between changes in the (Panama-driven) AMOC-controlled carbon cycle that acts as amplifyer mechanism instead of primary forcing.

3 Models and concepts

3.1 (*Explain changes in NADW production as shown in Fig. 7*)

Fig. 7 shows that increased NADW production started near a CAS sill depth of 550 m, where it was as low as at 2000 m water depth (i.e., reflecting *almost zero rise*), whereas two thirds of the increase only occurred at less than 250 m depth. The %

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sub-SSS contrasts were inferred from Fig. 7 in conjunction with the evidence displayed in Fig. 6. This derivation is now explained more specificly in the text (p. 257, line15 ff).

3.2 (*Explain the pronounced glaciations around 3.3 (M2), 3.15 (KM2), and 3.05 Ma (G20), that parallel low SSS gradients beween Caribbean and E Pacific*)

In addition to the long-term effects of the "closure of Panama" and resulting changes in AMOC and Arctic Throughflow (fully effective only after $<$3.08 Ma) the amplitudes of each particular glaciation depend on differential orbital forcing. In contrast to the amplitudes of MIS KM2 and G20, which hardly differ from those of a series of other cold stages between 3.4 and 2.85 Ma, the amplitude of MIS M2 at 3.3 Ma is far more pronounced and indeed coeval with a major re-opening of Panamaian Seaways.

For the build-up of continental ice volume and ice breakouts at all three cold stages we need to consider significant memory effects (controlled by overlapping effects of ice isostasy, potential thermal isolation of Greenland, pre-existing ice sheets and fjords, changing amplitudes of orbital cycles, etc.) inbuilt into the complex climate system, effects which imply significant lags (compare p. 260, line17). Accordingly, the time lag amounts to no more than $\sim$40 kyr between MIS M2 and a long period, when the Panamaian Seaways have been very shallow and almost closed (during MIS MG7-MG11), when glacial stages had gradually deepened. Similar lags also apply to MIS KM2 and G20 vs immediately preceding extremes of seaway closure.

3.3 (*Check consideration of sea-ice based albedo effect in model of Lunt et al., 2008*)

See our response to Reviewer D. Lunt, item 3, paragraph 4.

4 *Inappropriate citation of Huybrechts et al. (2004) at p.259, line 15*

The citation of Huybrechts et al. (2004) at p. 259 is now deleted as superfluous.

5 North Atlantic sediment records

5.1 (*Document and explain the temporal relationships between episodes of IRD in-*

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crease and the three episodes of increasing Caribbean-Pacific SSS gradients around 3 Ma; p. 260, line 5ff, and Fig. 12)

We object to the view of Reviewer 2 who is requesting an overly strict coherence between changing Panamaian SSS gradients and IRD increase. In contrast, this relationship is necessarily complex because of inbuilt memory effects, as outlined in detail under response 3.2. These reasonings are now supplemented to p.260.

In contrast to the 'simple' first S. 907 IRD rise the second and oscillating S. 907 IRD increase after 3.0 Ma is starting for a first time almost coeval with the SSS gradient increase and a second time with a ~40 ky lag. Finally the rise continues over 50 to 100 kyr subsequent to end of the increase in the SSS gradient at 2.92 Ma. The third episode of increasing IRD around 2.7 Ma is only weak, already starting from a high IRD level, and shows no more lag to the Panamaian SSS gradient, obviously because most of the Greenland ice sheet was already established after 2.9 / 2.82 Ma (MIS G10). Trend arrows in Fig. 12 are now better specified.

5.2 (*Explain problems with Site 1307 IRD record*)

As already outlined at p. 259-260, lines 25-27 and 1-4, the S.1307 IRD record displays a number of interesting local aspects, because it is fed to a large degree by icebergs originating from the deep fjords in mountainous South Greenland. As today, these icebergs do not necessarily reflect the extent of the continental ice sheet but rather the activity of local mountain glaciers. On the other hand, there may be indeed problems with mismatching stratigraphy (see below, Figure 13_{new}).

We now tackle this problem with more detail at p. 260, line 18-23. If using "age model 1", the IRD increase near 3.25 Ma is precisely coeval with the increase of the SSS contrast at Panama, although interrupted later-on by a major hiatus.

5.3 (*Add separate figure showing the newly tuned $\delta^{18}O$ records of Site 1307*)

In harmony with Reviewer #2 we regard it as helpful to add a separate Figure 13_{new}

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showing two alternative approaches for tuning the magnetic and δ^{18}O records of Site 1307 vs the reference record LR04 of Lisiecki and Raymo, 2005 (as previously mentioned at p. 260, line 18).

6 (*Explain mismatch of distinct S. 609 SST maximum at 3.25 Ma and a SST minimum at 2.65 Ma with Panamaian SSS gradient*)

As shown in Bartoli et al. (2005), the δ^{18}O stratigraphy at S. 609 is fairly well established. Similar to MIS M2 (see item 3.2), the distinct S. 609 SST maximum at 3.28-3.25 Ma probably reflects a 'heritage' of the long preceding period, when the Panamaian seaways have been very shallow over more than 200 kyr. On the other hand, the SST minimum at 2.65 Ma already is in part a result of full glacial stages dominating after MIS G6, when interposed short-term interglacial warmings were poorly recovered, e.g. at MIS 102.

These details of the record are now be displayed with more detail at p. 261, line 6ff.

7 (*Explain the lag between a maximum Panamaian SSS gradient, ending 3.18-3.16 Ma, and the 6°C cooling and 1-psu freshening of EGC, only reached ~3.02 Ma*)

Both age models 1 and 2 for S.1307 (see Figure 13_{new} and response items 5.2 and 5.3) result in a major hiatus unfortunately covering most of the critical transition prior to 3.02 Ma. Accordingly, the extremely cold, low-salinity EGC regime may have started as early as 3.17 Ma when using age model 1 and around 3.08 Ma with age model 2. Therefore, age model 1 appears better suited than age model 2 for producing a close coherence between the freshening of the Arctic and the closings of Panama, similar to our experience with the S.1307 IRD record (comment 5.2). Accordingly, the EGC records do not object but partly hide the postulated increase in steric height during the first episode of full closure of the Panamaian Seaways.

To avoid lengthy discussions in the text, we now replace our age model-2 records for the EGC by age model-1 records and display the age model-2 records only by thin,

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dotted contours for Figs. 12 and 14 (old numbering).

FIGURES

Fig. 1 does not appear obsolete to us based on the reasonings displayed under item 1 Introduction. Also, this paper is not considered as mere review of Molnar (2008).

Fig. 10. For more clearness, Rev. #2 suggest to supplement the LR04 stack to Fig. 10. However, the age scales for this figure have been generated independently of the LR04 record (now specified in the figure caption), although differences from the LR04 scale amount to less than five up to ten kyr (e.g., Bartoli et al., 2006), amounts that can be neglected in the context of the large memory effects discussed elsewhere in this study.

Fig. 12 and 14 are indeed partly redundant and thus now combined into one figure.

Figure 15. As responded to Reviewer D. Lunt (item 2.2), details of the model are now specified in the caption of Fig. 15, moreover at p. 263, line 11ff. However, we see no chance to redo the model within a short time span using a higher mid-Pliocene sea level and a pertinent shift of shorelines.

<u>RESPONSE to remarks of G. Bartoli</u>

Many questions raised by Bartoli have already been handled by answering to comments of Reviewers # 1 and 2, thus can be treated shortly.

1 *Age models*

S. 1307: Fig. 13_{new} now shows the derivation of two equally justifiable age models originally contained in an earlier version of Bartoli et al. (2009; where now age model 2 has been suppressed for reasons unknown). Fig. 12_{new} depicts the IRD and paleoceanographic records of S. 1307 for both age models.

Various age models for all remaining sites discussed in our study are now specified in the figure captions. Although these age models are generated independently of LR04,

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ages differences vs LR04 generally amount to less than five ky and only rarely reach up to ten kyr (tested in Bartoli et al., 2006), amounts to be neglected in the context of much larger memory effects like ice isostasy, etc. discussed in this study.

For questions regarding MIS M2 see our response to Reviewer 2, items 3.2 and 6.

2 *Discussion of Pliocene CO_2 records*

See our response to questions 3.1 - 3.6 of Reviewer D. Lunt.

3 *Closure of Panamaian Seaways*

Objections of Molnar (2008): See our response to Reviewer D. Lunt.

Uncertainties of SSS anomalies between Caribbean and East Pacific:

For details of uncertainty estimates our synthesis paper needs to rely on common equations (e.g., for δ^{18}O vs salinity) and publ. sources, where the records employed have been generated, e.g., on Bartoli et al. (2005) and the PhD theses of J. Groenveld (2005) and S. Steph (2005). The calcification depth of *G. sacculifer* has been specified in the caption of Fig. 10.

Finally, accumulated uncertainty values of Rohling (2008) for $\delta^{18}O_w$ ($\pm$0.2 permil) apply to single salinity estimates, but not to average values (running means) of 10 and more single values. According to textbooks this averaging will reduce the error by the square root of 10 values, i.e., rougly by 3. Hence an error of 0.07 is negligible for our considerations. On the other hand, the modelled salinity differences between Caribbean and E Pacific roughly match the calculated increase in NADW flow and were deduced from Fig. 6 (generated using the UVic-ESCM model of Schneider and Schmittner, 2006; as cited in our figure caption). Obviously Bartoli has not realized that Fig. 7 shows between 550 and 130 m sill depth a 2/3 increase in NADW-driven heat export to high lats, that is precisely as much as for 250 to 0 m depth. Thus we see no contradiction to earlier conclusions of Schneider and Schmittner, 2006.

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4 *The freshening of the EGC* shown in Fig. 14 (now Fig. $12_{new})$ is based on the cited $\delta^{18}O_w$ record of Bartoli et al. (2009, Fig. 4c), though modified by using – instead of age model 1 – age model 2 which was employed in the earlier version of their manuscript. For further details of phase relationships see our response item 7 to Reviewer #2. Finally, we cannot discover any bias of the enveloping arrows for the salinity trend in Fig. 14. Obviously the resulting trend is not in harmony with conclusions of Bartoli et al. (2009).

5 *Model of Bering Strait throughflow*.

See our response to Reviewer #2 (comment Fig. 15) and to Reviewer #1 (general comments, item #2).

With regard to the question on sea level changes, LR04 shows that the sea level was as high as today and up to 40 m (-0.35 permil δ^{18}O) higher over most of the time span prior to MIS G17, in particular so 3.25 – 3.15 Ma, during the first event of full CAS closing. Accordingly, the model assumption of a sea level as high as during the Holocene (Fig. 15) is fairly conservative. Later-on, the consequences of increased Arctic Throughflow had already been established and resulted in the self-enhancing thermal isolation of Greenland as threshold mechnism to keep the low-salinity EGC also over episodes with low sea level and dried-up Bering Strait (similar to the LGM).

For long-term memory effects and effect of age models see our response to Rev. #2, items 3.2 and 7 as well as the response regarding Fig. 12_{new} and Fig. 13_{new}.

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