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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.

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Major omissions and inconsistencies

I would like to make a number of comments on the age models referred to in this study, the Pliocene atmospheric CO2 drop, the relationships between the closure of the CAS and the North Atlantic oceanic circulation, the freshening of the EGC, and the Bering Strait through-flow during the Pliocene Epoch.

1) Age models The authors published a short comment to correct their references to the various age model used in this study. However I must correct the reference p. 260-line 23-24: Bartoli et al. (2009, paper subm.) did actually build an age model for Site 1307, by tuning a planktic foraminiferal d18O record, obtained for Site 1307, to

the benthic foraminiferal d18O stack LR04 (Lisiecki & Raymo, 2005) using the magnetostratigraphy and biostratigraphy produced on board by Expedition 303 Scientists (2006) for determining the position of the marine isotopic stages between 2.6 and 3.6 Ma. As Sarnthein et al. wrote, it sounds like the age model for Site 1307 used in this paper and based on the oxygen isotope stratigraphy was actually produced onboard by the Expedition 303 Scientists, which is not true. It is not clear to me why the age model used in this paper (age model 2?) would be superior to the age model established by Bartoli et al. (2009). Maybe Sarnthein et al. could explain in this paper what are their views.

The lag between the closure of the CAS and the IRD deposition at Site 907 described p. 260 as persistent could arise from the various age models used in these various records. Jansen et al. (2000) produced an oxygen isotope stratigraphy for the IRD record at Site 907 only from 0-1 Ma. For the period 1-3.5 Ma, the IRD record was dated by linear interpolation between paleomagnetic boundaries. Later, Lacasse & van den Bogaard (2002) produced four Ar/Ar dates to increase the reliability of the Pliocene age model. The closure of the CAS is based on the d18Os.w. records from Sites 999 and 1241. The age model at Site 999 was established by tuning the benthic d18O record from Site 999 to benthic d18O records from Site 846 and 659 as described in Haug & Tiedemann (1998). Tiedemann et al. (2006) established the age model for Site 1241 by tuning the GRA density, the percent sand of the carbonate fraction and the benthic d13C record to the orbital solution of Laskar et al. (1993). I would first suggest that the authors properly reference the age models used in their paper and second, in light of the description of the age models above, question their conclusion on the memory effect in the climate system (p. 260).

In their chapter 5, the authors search for evidences between the closure of Panama and the onset of NHG. However they do not consider benthic d18O records, in particular the benthic d18O stack LR04 displayed in Fig. 14. It appears clear from this figure that times when Panama was fully closed or open (less than 130 m) do not correlate

CPD
5, S104–S108, 2009

with major increase in global ice volume. On the contrary, major -transient- increase in ice volume occurred when Panama was open, i.e. MIS M2 and the glacial ice volume increase between MIS K2 and G20.

2) Pliocene CO_2 p. 255: The atmospheric CO_2 record by Kürschner et al. (1996) does show a drop between 2.8-2.3 Ma (although it is just two data points). The error-bars are lower than +/-50 ppm – and not +/-100 ppm as written by the authors. DeConto et al. (2008) do not show any data covering the last 5 Myr.

I do not understand the reference to the 400-kyr cycle (p. 256). Does it mean that the authors do not believe in the current CO_2 reconstructions because these records do not display a 400-kyr cycle? Or does it mean that we should integrate the idea that perhaps the atmospheric CO_2 has always been controlled by a 400-kyr cycle that may have played a role in the onset of northern hemisphere glaciations?

The authors state that there are no records showing a mid-Pliocene drop in atmospheric CO_2 but they also refer to the data by Foster et al. (2008, AGU conference) presenting a CO_2 drop by 100 ppm. Such drop would be sufficient to trigger the northern hemisphere glaciation (Lunt et al., 2008; Li et al., 1998). This paragraph p.255-256 is very confusing.

3) The closure of CAS The authors do not describe the objections of Molnar (2008) to the Panama Hypothesis (p. 259). It would actually make sense to take the arguments of Molnar (2008) and show the contradictory evidences.

The authors based their discussion of the timing of the closure of CAS on d18Os.w. records from Sites 999 and 1241 like in Bartoli et al. (2005) and the model results from Schneider & Schmittner (2006). Rohling (2008) calculated a minimum uncertainty for d18Os.w. reconstruction of at least 0.2permil taking into account a 1°C-uncertainty on the Mg/Ca-based SST. How do the authors take an uncertainty of +/-0.2permil into account for their interpretations in this paper? Moreover other uncertainties have to be discussed such as the changes in the calcification depth of the planktic foraminifer G.

sacculifer (on both sides of the Panama Isthmus) and the validity of the relationship 0.1permil = 2 psu for Pliocene times. Also, Sarnthein et al. uses a 0.6permil -salinity offset between Sites 999 and 1241 to decipher a full closure of Panama and 0.3permil -salinity offset to deduce a CAS open (less than 130m) but if one consider a +/-0.2 permil-uncertainty in the salinity records, one cannot actually truly distinguish between 0.3+/-0.2permil and 0.6+/-0.2permil.

In Bartoli et al. (2005), we just considered that when the salinity offset increased the CAS were closing. Here, Sarnthein et al. try to translate the sill depth of the Panama Isthmus into a % of the salinity gradient between Sites 999 and 1241. For this purpose, they present new data in Fig. 7 but do not explained how they calculated the % of the salinity gradient based on the strength of NADW formation (in Sv). And Fig. 6 is not taken from Schneider & Schmittner (2006).

Moreover, in Fig. 6 (right side) for the situation at 130 m, there is actually no salinity difference between Site 1241 (5°N, 86°W) and Site 999 (12°N, 78°W), their locations being different from the transect. Therefore one can wonder whether these sites are actually the best representatives for monitoring the closure of the CAS before the sill depth reached 130 m. Furthermore as described by Steph et al. (2006) and Groeneveld et al. (2008), salinity rises in the Caribbean Sea much earlier (4.2 Ma) than during the last steps of the CAS described here.

On top of that, it is written in Schneider & Schmittner (2006) that the maximum export of warmth to the high-latitude is when the Panamanian sill rises between 700 m and 130 m. The shoaling of the last 130 m did not significantly increase the North Atlantic sea surface temperatures. So why are the authors always referring to the full closure of Panamaian seaways? Finally, during the extreme glaciations of MIS M2 and G6, it seems that CAS are open (Fig. 14) although the sea-level drop was about -70 m compared to today during MIS M2 (Dwyer & Chandler, 2008) and maybe more during G6.

CPD

5, S104–S108, 2009

Interactive Comment

4) Freshening of the EGC Fig. 14 is not taken from Bartoli et al. (2009) but provided by myself and is unpublished. Using the age model preferred by the authors (age model 2?), the arrows in Fig. 14 are misleading and are actually masking the data. The EGC displayed cold (modern) temperatures after 3.05 Ma and it is assumed that the cooling took place during the data gap between 3.10-3.05 Ma. It is also clear that between 3.10-3.05 Ma, the CAS are open and therefore the closure of CAS cannot provide the forcing for the onset of a cold and fresh EGC.

5) Modeling of the Bering Strait through-flow The results presented in Fig. 15 are clearly new but are cited as Prange, unpub. This is confusing since M. Prange is one of the co-authors of the present paper, which could really beneficiate from some method description. In particular, the sea-level stand used by M. Prange is the Holocene sea-level stand, and in my opinion, this does not make sense at all. As the Bering Strait has a shallow sill of 50 m, changes in sea level will obligatory constrain the intensity of the through-flow. During the Pliocene Epoch, eustatic sea-level variations could have been up to 110 m according to Naish & Wilson (2008). Dwyer & Chandler (2008) calculated a seal-level stand of -40 m, compared to today, for glacial stages KM2 (3.15 Ma) and G20 (3.05 Ma) during the onset of the cooling of the EGC. Therefore it may be that the Bering Strait sill was only 10 m deep during the time when the increase through-flow is supposed to occur.

More importantly, according to their Fig. 14 the CAS are open at that time, and could not have forced a doubled flow-rate through the Bering Strait between 3.16-3.05 Ma contrary to what is stated by Sarnthein et al. Also see Bartoli et al. 2009, subm. Therefore this is in contradiction with their abstract.

I do not understand the last sentence p. 263 about siliceous plankton. Do the authors refer to some king of evidence they know of or some evidence we may find if we look up in the literature?

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