Dear Luc Beaufort,

We would like to thank the anonymous referee and F. Bassinot for taking time to review our manuscript. Their comments were insightful and constructive. We have answered all questions raised by the referees and made changes according to their suggestions. A revised version of the manuscript is now ready. The major changes are:

- We added Heiko Pälike as a co-author. His involvement in the manuscript and the science was already very substantial and his further help during the rewrite justifies his place on the author list.
- CPD figure 3 has been split into CP figures 3 and 4. And CPD Figure 6 has been split into CP figures 6 and 7.
- The differences between the sites are shown in CP Fig 4 and the local oceanography is discussed in the results and discussion section.
- CP figure 7 is added to further clarify the major findings
- CPD Fig. 5 has been removed because we think the added value of this graphs was limited and it was already partially covered by CPD Fig. 4 (CP Fig. 5).
- A more critical evaluation of the modeling results is added to the manuscript.

For further changes in the manuscript and the detailed comments on each point raised by the referees we would like to refer to the rebuttal (below).

We would like to thank you for editing this manuscript.

Kindest regards,

Diederik Liebrand and co-authors.

Appendix 1: Rebuttal on interactive comment by an anonymous referee
Appendix 2: Rebuttal on interactive comment by F. Bassinot.

Interactive comment on **"Dynamics of ~100-kyr glacial cycles during the early Miocene" by D. Liebrand et al.**

Anonymous Referee #1

Received and published: 9 February 2011

Rebuttal by Liebrand et al. 30 June 2011

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In the manuscript submitted to "Climate of the Past Discussions", D. Liebrand et al. present an impressive high-resolution benthic foraminiferal isotope record from ODP Site 1264 (Walvis Ridge, SE Atlantic Ocean), which closely tracks climate evolution over the latest Oligocene to early Miocene (23.7-18.9 Ma). The authors use a set of 1-D ice sheet models to deconvolve the temperature and ice volume components in the $\delta^{18}O$ signal, and conclude that Antarctic ice build-ups occurred during short episodes of low eccentricity forcing. The authors further argue that long-term ice sheet expansion was controlled by a non-linear mechanism (such as merging of discrete ice-sheets), whereas ice-sheet dynamics became highly sensitive to the 100-kyr eccentricity forcing during termination phases. This is an interesting and challenging paper, which will potentially provide a valuable contribution for understanding the main processes controlling climate evolution across the late Oligocene/early Miocene. However, I would suggest that the authors critically re-evaluate some of the results and interpretations and revise their manuscript before it can be accepted for publication in "Climate of the Past Discussions". My main criticisms are twofold:

1) some sections of the text are rather cryptic and lack relevant information (see details

below). These shortcomings could be easily remedied during revision.

We have made changes according to the suggestions below. Please read below for more detailed comments on what has changed.

2) I am not entirely convinced by the interpretations derived from the modelling results in relation to ice sheet expansion. Firstly, I wonder how applicable the modelling technique employed is for the late Oligocene-early Miocene, when climate boundary conditions were quite different from today's (gateway configuration, water mass distribution, unknown composition for Antarctic ice $\delta^{18}O$, etc..). Although most of these parameters remain poorly known, the authors appear quite uncritical about various alternative scenarios.

A more careful interpretation of our modelling results is now discussed. Although $\delta^{18}O$ changes of the ice sheets are taken into account and have been part of a thorough discussion in a previous paper (De Boer et al., P-3, 2011). Ocean volume changes, gateway configuration and water mass distribution are not taken into account. Combining the several sensitivity tests that have been performed we have an error range to the modelling results of ~ 10 %. This discussion is added to the 1D model Section.

Secondly, I am puzzled by the fact that local oceanography is not really discussed. The water depth of Site 1264 is relatively shallow (2505 m) in contrast to the deeper records shown in Fig. 3. Could the variability exhibited by the benthic signals in Site 1264 relate also to local water masses and not just to global climate?

A paragraph on local oceanography has been added to the isotope result section and to the discussion. Furthermore CPD Fig 3 has been split into CP Fig 3 and Fig 4 to better show the changes between the equatorial sites and the southern Atlantic sites. In the manuscript we discuss the influence of intermediate waters on the benthic (mainly $\delta^{13}C$) signal at Site 1264. However, the match with Site 1090 is very convincing and the similarities between the two $\delta^{18}O$ records make us believe Site 1264 is influenced by the same deep-water mass. The differences with Sites 929 and 926 are clear and these sites were bathing in different deep-water masses during the early Miocene.

For instance, the Site 1090 $\delta^{18}O$ signal does not show the prominent 100 kyr cyclicity after 23 Ma displayed in Site 1264 (Figure 3), although the resolution of the two data set appears quite similar.

It is true that Site 1090 does not show the 100 kyr cyclicity as prominent as Site 1264, neither do Site 926 and 929. Although we regard these cycles as changes in global climate and Antarctic ice volume, different sites have recorded these changes differently depending on their calcium carbonate content and the water-mass present at the time. Site 1264 records these changes the best of all. The resolution of Site 1090 is high enough to record these changes, however they are less prominently present due to poor preservation and poor isotope signal at this site. This has been added to the discussion.

As the model cannot resolve individual water masses and/or oceans (see Page 2747, Lines 22-24), the significance of the modelling results needs careful evaluation at the intermediate water depth of Site 1264.

We have clarified in the text how we interpret the modelling results. We explain that we regard Site 1264 as a global average even though no correction has been applied to match it with the Zachos (2008) composite.

Please find below detailed comments regarding various aspects of the manuscript

Abstract Concise and clearly written. Page 2742, Line 14: use lower cap for "Supports“.

“Supports” has been changed into “supports”

Introduction Page 2743, Lines 4-23: future and past verb tenses mixed in this section, please make consistent.

This has been rewritten.

Section 2 Page 2744, Lines 3-12: too sketchy and general, please provide relevant information concerning site, cores and samples:

More information about Site 1264, the Walvis Ridge cruise and the use of shipboard information/splices has been added. This section is now incorporated in the introduction.

1. Was a splice available for sampling?

We refer to Zachos 2004 for more details about Site 1264

2. What drilling tool was used?

We refer to Zachos 2004 for more details about Site 1264

3. How complete was core recovery?

We refer to Zachos 2004 for more details about Site 1264

4. Explain why Site 1264 is uniquely situated to record major oceanographic changes (Lines 9-11)

The relationship between the sites being situated above the CCD and lysocline and there unique situation to record changes in climate and oceanography has been made clearer in the text by combining these sentences.

Lines 11-12: last sentence is out of place, as you address this topic in subsection 2.2.

This sentence has been removed.

Section 3 Page, 2745, Lines 4-6: please explain why samples were sieved into >37, >65 and >125 μm fractions for foraminiferal analysis, and specify from which size fractions benthic foraminifers were picked for isotope analysis.

We corrected a typo in the methodology section: "…the larger than 37, 65 and **125** µm fractions…" **has been changed into** "…the larger than 37, 65 and **150** µm fractions…". **We changed the next sentence to indicate that we only picked and analysed *C. mundulus* from the largest fraction.**

It is quite unusual to use benthic foraminifers from small size fractions (<250 μm) for isotope analysis. Correct identification of Cibicidoides species is difficult in these smaller size fractions. Correct identification is quite critical because Cibicidoides species show different isotope signals. Additionally, isotope values may differ in juvenile tests, which may be common in smaller size fractions.

See above. Is there a possibility that the complete benthic fauna is smaller compared to other Sites? Sieving at larger than 250 μm would probably result in an almost completely planktonic foraminifera-dominated fraction we suppose. Although this has not been tested. Sizes of tests analysed were chosen as constant as possible and any particularly small or large tests were not used. Very large or small tests were not used. Species recognition in a <250 μm size fraction was no problem.

Page, 2745, Lines 24-25: the reproducibility quoted for duplicate measurements is quite low. Could this be due to erroneous identification of Cibicidoides species and/or picking of juvenile tests?

See above. Erroneous identification is always possible, however we do not think this is the case, or the cause for the offset in δ^{18}O found between the two labs. Our best explanation is that the perhaps small set of samples used to compare isotope signatures between the labs is not representative. This has been clarified in the text.

Please also specify cleaning methods: for instance were tests cracked and sonicated prior to analysis? Any infill may bias the isotope signals (for instance presence of coccoliths in chambers would impart a "surface“ signal to the benthic measurement). Pages 2745- 746, Lines 25 and below: could this unexplained offset be due to misidentification of Cibicidoides mundulus?

We don’t think so. The cleaning method has been added to the methodology section.

Were all samples picked by the same person?

Yes, they were.

This large offset is extremely puzzling, especially because the lower resolution record measured at UF shows no offset!

We don’t think it is extremely puzzling, but we do think it is worth mentioning and therefore it was/is shown in the CPD/CP supplements. Currently this is also better emphasized in the manuscript itself. Please keep in mind that we only needed to define 20 outliers on a ~1800 point data set. Almost all measurements fall within the 2 sigma boundary.

Section 5 The discussion should be expanded to include a more critical evaluation of modelling results: please discuss the applicability of the modelling technique for a far distant time interval with markedly different climate boundary conditions and the possible influence of local oceanography on the isotope signals.

The oceanography and model characteristics are added to the manuscript. For more details we refer to the Boer et al. 2011. In terms of the far distant time interval, the model reconstructs ice volume and temperature which are consistent with the benthic d18O observations, and we have a constrain on ice volume increase during

the E-O transition.

Figure 7 I do not fully understand the relevance of Figure 7. In Section 5 (Page 2749, Lines 6-8), Figure 7 is briefly mentioned to support age estimates for Mi events. However, the position of Mi events is quite debatable, as these were originally determined in low resolution isotope data sets. Figure 7 shows that this terminology is rather confusing, as the placement of events appears quite arbitrary in the various isotope records shown.

The point of CPD Figure 7 (CP 8) is indeed to show that the position of 'named events/zones/episodes' is arbitrary and based upon low resolution records from the 1980's which since then have sometimes been erroneously transposed to higher resolution records generated in the late 1990's and early 2000's. This has been clarified in the figure itself, in the figure caption and in the text of the manuscript.

Please note misspelling of Kerguelen Plateau in Figure caption.

Kuergellen has been changed to Kerguelen

Interactive comment on **"Dynamics of ~100-kyr glacial cycles during the early Miocene" by D. Liebrand et al.**

F. Bassinot (Referee)

Franck.Bassinot@lsce.ipsl.fr

Received and published: 27 May 2011

Rebuttal by Liebrand et al. 30 June 2011

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The paper submitted by Liebrand et al presents an interesting new set of high-resolution benthic $\delta^{18}O$ and $\delta^{13}C$ data from the South Atlantic ODP Site 1264, spanning the early Miocene. The careful study of their spectral content, their comparison with other records available over this time interval and the decomposition of the δ18O record into ice volume and temperature signals using an inverse modeling approach make it possible to bring new insights into the episodes of expansion of Antarctic (and Greenland for M1) ice sheets in response to orbital forcing. The paper is short and focused, well illustrated. It will clearly deserve publication in Climate of the Past after a few corrections or improvements have been performed.

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Here are a few specific problems, questions or suggestions that the authors should address to improve the manuscript.

1/ The time resolution of the Site 1264 record is not indicated in the manuscript. From the amount of samples studied (1754) and the time interval covered by the record (4.8 Myr, spanning from 23.7 to 18.9 Ma), this resolution appears to be around 3 kyr in average.

We have added it to the abstract and the stable isotope results.

It is important that the readers have this number in mind since it is above the Niqyst limit for the precession cyclicity. This strengthen the idea that the lack of a strong precession signature in the Site 1264 record is likely due to a poor precession imprint on the deepwater δ18O and temperature, and not a problem associated to a too low resolution.

This is further explained in the isotope results

2/ Until Figure 7 and the sentence from line 4- page 2749 (« .. on which the latter two periods are close within the age estimates of the Mi-1a and Mi-1aa episodes »), it was not perfectly clear to me whether the four major ice sheet growth episodes discussed in the manuscript had been solely determined based on the high-resolution, Site 1264 $\delta^{18}O$ record, or if the authors focused on specific ice building episodes that seemed to fit stratigraphically with those already recognized and labeled in previous works (the so-called « Mi » episodes). Maybe a sentence should be added somewhere at the start of the discussion to make it unambiguously clear that the study is fully self-supported by the analysis of Site 1264 data.

This has been added to the last section of the stable isotope results.

3/ Page 2746 (stable isotope stratigraphy).

Over the entire time interval discussed, Site 1264 (and 1090) $\delta^{18}O$ record is about ~ 0.5% heavier than the benthic oxygen records at the two Ceara Rise sites (figure 3). Yet, Zachos et al (2001) pointed out the existence of a ~ 0.4‰ difference in the average $\delta^{18}O$ values before and after Mi-1 at the Ceara Rise sites ; a shift which – according to the authors - is not recorded at Site 1264. Reading these elements, I was puzzled by the fact that the general $\delta^{18}O$ offset between Site 1264 and the two Ceara Rise sites could remain apparently unchanged before and after Mi-1…

This was indeed confusing and difficult to see in the CPD figure 3. The plot with all isotope records on top of each other has been moved to a new figure (fig 4) to show the 0.4‰-ish decrease in Ceara Rise $\delta^{18}O$ values

Looking at the Ceara Rise records, it appears actually that the « 0.4‰ shift » indicated by Zachos et al does not correspond to an overall shift in the $\delta^{18}O$ values, but to the occurrence – after the Mi-1 event - of more « glacial » episodes with heavier δ18O values, while the peak « interglacial » episodes retain δ18O values that are relatively similar to the pre-Mi-1, $\delta^{18}O$ base line. In other words, the average shift in δ18O at Ceara Rise is actually indicative of an increased variability with enhanced glacial conditions after Mi-1 … an evolution which - from what I can see in the figures presented in the manuscript- is also apparent from the Site ODP 1264 record.

The new figure 4 shows this difference in average $\delta^{18}O$ values much better.

Thus, to me, as far as this ~ 0.4‰ $\delta^{18}O$ shift prior/after Mi-1 is concerned, there might not be a striking difference between the evolution of the $\delta^{18}O$ records at Ceara Rise and in the Southern Ocean. This implies that the authors shouldn't need to suggest potential explanations to deal with it (i.e. changes in abyssal circulation, flow reversal through the

Panamanian Seaway).

See above. We think the step in equatorial $\delta^{18}O$ is better represented in the new Fig. 4, which justifies the discussion about changing water circulation across the Oligocene/Miocene boundary. Furthermore, the added oceanography section incorporates other lines of evidence that hint towards major oceanographic reorganisations in the early Miocene (equatorial) Atlantic

4/ Page 2747 (and suppl. material). I must admit that I've always been rather skeptical about inverse modeling of $\delta^{18}O$ for old periods where there is not much quantitative control possible. Having said that, I recognize that the bulk ($\delta^{18}O$, $\delta^{13}C$) data presented in the paper convey enough temporal and spectral pieces of information to support the major conclusions reached by the authors, even if flaws can exist in the inverse modeling. Recent studies dealing with carefully dated benthic $\delta^{18}O$ records covering the last deglaciation have revealed that there exist important diachronisms (up to several thousand years) in the temporal evolution recorded at remote sites (or located at different water depths), due to oceanic circulation and the complex interplay of deep sea temperature and $\delta^{18}O$ effects (i.e. Skinner and Shackleton, 2006). Given these recent developments, I wonder to which extent the fact that the modeled temperature component represents a global value for all oceans (instead of representing the true temperature signal of the water mass at Site ODP 1264) can have an impact on the conclusions reached by the authors. In particular, it is puzzling to notice that inverse modeling of Site 1264 data suggests that ice-sheet growth precedes (~7 kyr) northern hemisphere polar cooling, a result which is in contrast to previous findings using the same modeling approach (Bintanja and Van de Wal, 2008).

This point is now addressed in the paper. We explain that our underlying assumption is that Site 1264 represents a global average $\delta^{18}O$ signal even though a 0.53 per mil offset with Zachos 2001/2008 is present. This is indeed an uncertainty, however we interpret our results in terms of relative sealevel change and in terms of relative ice-sheet expansions to present-day. The absolute numbers are indeed more arbitrary. With respect to the phase relation between NH-temp cooling and SH-ice sheet expansions: we looked into this more detailed and recognised that the previously (CPD) described phase relation could be true, but falls within the 95% uncertainty band of the Blackman-Tukey cross spectral analyses. This has been clarified in the text.

5/ The interval of high benthic δ18O that takes place in 400kyr cycle 52 (around 20.7 Ma) is not far from showing the same characteristics than the four major ice sheet growth episodes discussed in the manuscript. The inverse modeling suggests an as important (and long-lasting) Southern Hemisphere ice sheet extension over this interval, and – although it is not as pronounced as for the other four episodes – the wavelet analysis also suggests that this ice building event is followed by an interval of increased (near) ~100 kyr spectral power (i.e. Figure 4).

We agree with this comment made by the reviewer and have explained this better in the text.

If this interval could be interpreted as an additional « major ice sheet growth episode », then all the major ice building episodes recognized from Site 1264 record would be two 400kyr cycles apart, suggesting that the sequence of events is even more regular than concluded by the authors.

In the manuscript we now talk about 4 to 6 intervals of ~100 kyr dominated climate cycles instead of 4. We keep the "two to four (or multiple) 400 kyr" term as a description of the time interval between "100 kyr worlds" since every other "100 kyr world" has a much better expression (those at 400 kyr cycle number 49, 53 and 57) compared to those in between (at 400 kyr cycle number 51 and 55). Also the equatorial Atlantic bottom waters cool every four 400 kyr cycles to southern Atlantic $\delta^{18}O$ values (see new Fig 4). Almost all Figs have an additional suggestion- "100 kyr world"-gray bars added to them.

6/ As clearly seen from the data, in the sequence of events to and from a major ice expansion event, the increase of ice volume during a low amplitude eccentricity interval is directly followed by an episode of high amplitude, ~ 100kyr variability.

- Could this enhanced 100 kyr variability reflect an increased instability associated to the size reached by the ice sheet during the preceding growth episode?

This could very well be. We incorporated this in the discussion.

- Obviously, during these intervals of high amplitude, ~100 kyr variability, major ice retreats take place every 100 kyr (!). Couldn't the occurrence of these large amplitude retreats help to explain why the ice sheets are not adequately preconditioned to enter a major growth episode at the next node of the 400 kyr cycle? (It's like seeing the succession of events under a different perspective. The authors tend to put the emphasis on how can a large ice sheet finally build-up (i.e. merging of several icesheets), whereas I'd rather put the emphasis on why the large ice sheets cannot build up at every 400kyr node..).

We think this is an interesting idea and have presented this option as an alternative to the 'merging East Antarctic ice sheets' hypothesis.