

Interactive comment on “Late Pliocene age control and composite depths at ODP Site 982, revisited” by N. Khélifi et al.

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Below you will find our response to the referees' comments, which were very helpful indeed to improve the quality of our manuscript.

1. Response to comments of Referee #1

– *Issue #1 (To mention the paper of Venz and Hodell, 2002)*

A reference to Venz and Hodell (2002) was already made in the caption of Fig. 1 (p. 14 in Khélifi et al., Clim. Past Discuss., v. 7) and in Section 4 of our text (p. 4, line 19). We now also add it to the caption of Fig. 2 (p. 15).

– *Issue #2 (To consider the update of the shipboard magnetic data using u-channel*

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samples by Channell and Guyodo, 2004)

Following Ref. 1, our text and figures now cite the M/G boundary in Hole A at 57.28 m c.d. according to Channell and Guyodo (2004). However, we had to take care of the discrepancy between the inclination records of holes A and B by means of our new $\delta^{18}\text{O}$ record which requires a match of the lower MIS 104 with the M/G boundary 80 cm further below the boundary suggested by CH & G (2004), in the lowermost section of Core B6, which unfortunately was only analyzed by the shipboard magnetic data, not by u-channel. Thus we had to stick to the only magnetic record available for this interval, *i.e.*, that of shipboard data.

– *Issue #3 (To delete any statement about the Mammoth Subchron in the Conclusions; Section 7)*

Accordingly, we changed our statement as follows “Here, it might be possible to identify the Mammoth magnetic subchron. . .”.

– *Issue #4 (To improve on the English wording)*

- We now replace “. . . better comparable to” by “. . . more consistent with” (p. 2, line 14).

- Former p. 2, line 20; we clarified our slightly confusing text by writing “as basis to establish the LR04 stacked record. . .”

– *Issue #5 (To delete the term “Excursions” considered to be linked to the “Matuyama Chron”)*

In harmony with Ref. 1, we now replaced the misleading term “excursions” by “oscillations toward less negative magnetic polarity” (Section 3, line 16). Moreover, we added a new Table 1 which compares the different levels of uncertainty of the M/G boundary in holes 982A, B, and C.

– *Issue #6 (Where to capitalize the H in “Hole”?)*

We now consistently use the capitalized “Hole”, when referring to a single hole, and

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spell “holes” where citing several holes (for example: holes A, B, C).

2. Response to comments of Referee #2

2.1. Response to “General Comments”

– *Issue #1 (To expand on Section 6 “Implications”)*

We prefer to stick in this manuscript to our main goal, that is the new age model for the Pliocene section at ODP Site 982, which is truly crucial for many questions of paleoclimate, and to outline some climatic implications here just serving as examples. We don't like to act as “missionaries” commenting in detail on all issues ever previously published from this site. In turn, we trust that our study will be of primary interest to the *Climate of the Past* community, since it will help to understand more properly the various Pliocene climate trends published on the basis of S. 982 data.

– *Issue #2 (To show the original SST record of S. 982 on both the revised and old age scales)*

This suggestion had already been followed in Fig. 3 (p. 16).

– *Issue #3 (To display other SST records from elsewhere in the North Atlantic for comparison)*

As outlined above (issue #1 in section 2.1.) we renounce for various reasons to inflate the paper by presenting any SST records from other sites. (1) We do like to focus on the actual evidence for the bias in paleoclimatic records, induced by errors in age control. (2) We cannot refer to all sorts of different SST trends and settings in the North Atlantic that may arise for different Atlantic sites, for example, from changing current positions (*e.g.*, Naafs et al., 2010), a problem that goes beyond the scope of this paper.

– *Issue #4 (To comment on the validity of the conclusions on the SST record originally published by Lawrence, 2009, from Site 982) (former p. 8, line 21, and p. 9, line 2)*

We are now more specific in our comments on the conclusions of Lawrence et al.

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(2009). Accordingly, we point out that in contrast to their view “the first major long-term cooling occurred on the basis of the new age scale only at $\sim 3.24 - 3.02$ Ma”.

– *Issue #5 (To show the CO₂ record of Pagani et al. on both age scales)*

Unfortunately, Pagani et al. (2010) did not provide original data points in their paper but only a rough envelop plot the trends of which one can't reproduce but only describe in the text.

2.2. Response to “Specific Comments”

– *Issue #1 (To improve the structure of the paper by a more comprehensive description of the resampling)*

We now inserted a new “Method” section after the “Introduction” (p. 3), which shortly summarizes all technical questions. Moreover, we added a new Fig. 1S in the Supplement, which shows the supplementary $\delta^{18}\text{O}$ data measured for this study.

In the new “Methods” section we mention the use of the “*Analyseries*” program for correlating the marine isotope stages in the S. 982 $\delta^{18}\text{O}$ record to LR04.

– *Issue #2 (To improve on the clarity in the distinction of “new” and “old” age models)*

Down to 68.36 m c.d. the composite depth values remained unchanged by our study, thus their wording was left unchanged. Further downcore, we now consistently use for distinction the terms m c.d. (old) and m c.d. (new).

Figure 1 which compares the results of the previous and new assignments of marine isotope stages is now displayed less overcrowded, with less excessive and improved labeling, and expanded horizontally to provide an enlarged view of the new splice of composite depths and the new tuning results. In contrast to Ref. 2, we still see a strong need to display the magnetic inclination record because it forms a crucial piece of evidence for our questions of age control.

We agree with Ref. 2 that Fig. 2 serves to show the superior match of LR04 with the

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$\delta^{18}\text{O}$ record using the new age scale, while Fig. 1 highlights the superior core splice.

In harmony with Ref. 2, we now removed the $\delta^{13}\text{C}$ record from Fig. 2, because it is not discussed in the text.

2.3. Response to “Comments on the construction of a major hiatus”

Unfortunately, an examination of the core photographs did not reveal any structure indicative of a hiatus.

A new text Table 2 now provides a short summary of the various lines of evidence in support of the conclusion to assume a major hiatus between MIS G22 and KM5.

2.4. Response to “Technical Corrections”

All suggestions for “technical corrections” were properly considered in the new version of our manuscript. Also, we tried to specify the general perspective outlined in the last sentence in the “Abstract” and to improve on the final sentence of the “Conclusions”.

IMPORTANT NOTE TO THE EDITOR:

Following an international vote (2010), we insist to use in our paper “ka” for “1000 yr ago” and “ky” for an interval lasting 1000 yr.

With sincere thanks for all help and valuable advise of the referees,

N. Khélifi et al.

Interactive comment on Clim. Past Discuss., 7, 1631, 2011.