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Interactive comment on "Re-evaluation of the age model for North Atlantic Ocean Site 982 – arguments for a return to the original chronology" *by* K. T. Lawrence et al.

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Khelifi et al. (2012) proposed a modification to the spliced composite section of ODP Site 982 and the correlation of its oxygen isotope record to LR04. Lawrence et al. argue the modifications were not necessary and the stratigraphy should revert to the original splice and age model. Although one may question why readers of Climate of the Past should be interested by technical arguments about a specific ODP site, stratigraphy underpins all of paleoceanography and there are several general lessons to be learned from the two papers.

There are three technical points of disagreement:

The 982 Splice Shipboard splices are often constructed hurriedly using preliminary core logging data and, as a result, they must be checked again post-cruise. However, rarely are the composite depth scales revisited following an ODP cruise, which can lead to problems with time series data that propagate for years (or decades) to come. The "Splicer" program used aboardship to construct composite sections employs a single tie point to create a constant depth offset for each core. Cores are correlated between holes only at the splice tie point and no stretching or squeezing of the record is accounted for. The composite is created by splicing segments from the different holes to create a complete sequence. The composite should be complete but also be kept simple by minimizing the number of jumps between holes to facilitate sampling and avoid potential errors when sampling.

In the interval in question, holes were correlated using GRAPE and colour as magnetic susceptibility drops to low levels below 55 mcd. The original shipboard splice used only the A and B holes because no colour reflectance measurements were made shipboard on the C hole for cores 7, 8 and 9.

Khelifi et al claim that: "Between 55 and 72 (old) mcd, all core fits amongst holes A, B, and C are poorly established as they cannot rely on specific oscillations and structures in the color reflectance and magnetic susceptibility records, here dropping to background levels"

Lawrence et al. counter that the data used to generate the original composite and mcd is entirely adequate for splice reconstruction (Fig. 3).

Whereas many of the splice tie points in Figure 3 look solid, a few are weak. Please include the core breaks and numbers on Figure 3 to make it easier to follow. It would be very useful to show the GRAPE data from Hole C in Figure 3 even though it was not used for the shipboard (A-B) composite. Similarly, the colour data should be measured on the archive halves of Hole C, cores 8 and 9. This could be easily done in less than a day by the IODP Bremen repository. A check of the JANUS database indicates the archive halves have not been sampled, so it should be possible. Close scrutiny of GRAPE and colour data from Holes AB and C should settle the argument whether the original AB splice is complete or not.

“Reflectance” as labeled in Figure 3 has no meaning. Which parameter of reflectance is plotted (L,a,b, etc.)? The authors should consult and cite the processed colour data of Site 982 by Ortiz et al. (1999: doi:10.2973/odp.proc.sr.162.029.1999). In fact, a variety of colour parameters could be compared among holes A and B to help verify the splice. Similarly, “GRAPE data” as a label will not be meaningful to many readers – change it to “GRAPE density” and add the units of g cm-3.

I agree with Lawrence et al. that the 194 tie points suggested by Khelifi et al. is excessive and, even if correct, is unmanageable from a sampling perspective.

The bottom line is shipboard splices should be carefully scrutinized postcruise and additional data collected if there are questions or discrepancies.

The Gauss/Matuyama boundary

It would appear that a fundamental assertion of the Khelifi paper is incorrect:

"Channell and Guyodo (2004) updated their shipboard magnetic data using u-channel samples, unfortunately without measuring the lowermost 100 cm in Core B6 (lowermost sections 6 and 7), and concluded that the M/G boundary in Hole B is located at the same composite depth as in Hole A."

Lawrence et al. states:

"the precise depth of the G/M chronozone boundary in Hole B has been identified at 57.29 mcd (51.77 mb.s.f., 982B-6H-6 at 77 cm, Channell and Guyodo, 2004; J. Channell, personal communication, 2012), which compares favorably (within 5 cm) with the shipboard-derived depth of the G/M chronozone reversal boundary in Hole A (57.24 mcd)."

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The statement by Lawrence et al. implies that u-channels from section 6 of Core B6 were measured by Channell and Guyodo (2004) and the G/M boundary placed at 982B-6H-6 at 77 cm instead of the shipboard placement in 982B 6H-7, 5 cm. This is critical to the argument and should be double checked with Channell – indeed, it would be useful to show the shipboard versus u-channel inclination data for Hole B to demonstrate the revised position of the G/M boundary (as in Figure 1A of Khelifi et al). The placement of the G/M at 57.24 mcd in Hole 57.29 mcd in Hole B is entirely consistent with the original oxygen isotope stratigraphy and puts the G/M in MIS 103 (as it occurs at other sites).

The lesson is shipboard paleomagnetic data should be considered preliminary as they only involve a single demagnetization step.

Kaena Hiatus (MIS K1 to KM4)

As noted by Lawrence et al., the revised mcd of Khelifi et al. (2012) doesn't differ greatly from the original one. Where the two papers disagree the most is how the benthic δ18O record of Site 982 is correlated to the LR04 stack:

"To generate a new age model for Site 982, Khelifi et al. (2012) compressed three meter-scale benthic _18O cycles that in the stratigraphy of the original age model were attributed to MIS K2, G4 and G2 into a much shorter interval which they now assign, respectively, to MIS G20, G2 and 104. They then proposed the existence of a hiatus, $\sim$130 kyr in duration, that spans most the mid Pliocene Warm period (MPWP, 3.19–3.06 Ma, MIS KM4-K1)."

This is a more difficult problem to resolve because the subchrons of the Gauss (Kaena and Mammoth) could not be identified in either the shipboard or u-channell data. I don't think biostratigraphy can help in this interval but it's worth considering.

Lawrence et al. show that the insertion of a hiatus by Khelifi et al. produces large spikes in sedimentation rates, up to 5x greater than average rates at Site 982. A

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guiding principle of constructing age modes is to avoid correlations that produce large changes in sedimentation rate. However, the assumption of constancy of sedimentation rate is highly dependent on the time scale being considered. One would expect sedimentation rates to become progressively more variable at shorter timescales (e.g., Sadler et al., 1999, GeoResearch Forum, 5:15-40, Trans Tech Publications, Switzerland). Whereas the assumption of constancy of sedimentation rate is reasonable on long time scales, it becomes progressively unreasonable at shorter time scales. Indeed, it would be worthwhile to apply dynamic programming (Match) to correlate the oxygen isotope record of Site 982 to LR04 given varying penalty functions for sedimentation rate changes (Lisiecki and Lisiecki, 2002). This could be done using the G/M boundary and M2 as tie points and let Match determine the optimal alignment in between.

Summary This paper should be published as a response to the technical comment of Khelifi et al. It demonstrates the vital role stratigraphy plays in paleoceanographic interpretation: e.g., one interpretation includes the Mid Pliocene warm period (3.19–3.06 Ma, MIS KM4-K1), the other includes a hiatus during this period.

The paper would be of greater interest to a broader audience (e.g., those not interested in Site 982) if the discussion could be expanded to include some general comments on the reliability of shipboard data and splices, principles of correlation, assumptions of constancy of sedimentation rates, recognition of hiatuses in deep-sea sequences, etc.

Lastly, I think Khelifi et al. should be given the opportunity to respond as it is a direct comment on their paper.

Interactive comment on Clim. Past Discuss., 9, 2217, 2013.