

Interactive comment on “The Eocene–Oligocene transition at ODP Site 1263, Atlantic Ocean: decreases in nannoplankton size and abundance and correlation with benthic foraminiferal assemblages” by M. Bordiga et al.

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Received and published: 5 August 2015

Reply to referee T. Dunkley Jones

We would like to thank the referee Tom Dunkley Jones for the constructive and helpful comments which were very helpful in improving our manuscript. Below we answer to all his points (here as condensed quotes, please refer to full comments in original review).

Reviewer: On this basis I would support its publication in *Climates of the Past*, subject to the authors addressing one key issue, as well as some more minor comments

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outlined below.

1) My primary concern is the placement of the Eocene / Oligocene boundary within this section (see also the comment of Pearson and other reviewers). [. . .] In fact, the pattern they observe is actually consistent with the calcareous nannofossil assemblage record from Tanzania, with major assemblage changes actually preceding the first oxygen isotope step and the major planktonic foraminifera extinctions (Dunkley Jones et al. 2008).

Reply: We agree that we cannot place the boundary only on the basis of the T of Hantkenina. We are performing new analyses on planktonic foraminifer assemblages to identify the other bioevents documented in Pearson et al. (2008). The $\delta^{18}\text{O}$ curve allows to clearly identify just Step 2 (or Oi-1) but not Step 1 (although Peck et al. 2010 defined it, it is still unclear). We could tentatively place the boundary in between Step 2 and the T of Hantkenina, but we prefer to be more precise by additional planktonic foraminifer analyses. See also our reply #2 to P. Pearson. Nevertheless, even without further biostratigraphic constraints, it is indeed clear that the nannoplankton assemblage changes preceded the oxygen isotope steps.

2) I would reinforce the comment of Paul Pearson - the authors need to be careful about their use of “Oi-1”.

Reply: We agree with the referee and have given more attention to the use of the proper terminology in the text and also in the figures. See our response #1 to P. Pearson and #2 to G. Villa.

3) I'm intrigued as to why two independent samples sets were worked on by two different nannofossil workers. Was this really to do a duplicate sampling test, or just that two groups started working on the same section at the same time? If the later, I think this shows a positive willingness to collaborate that shouldn't be “covered up” or reengineered into an a priori experimental test. It has proved to be a very informative test in its own right, and I strongly support its publication, however it came about. To me it

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demonstrates that, although there are some minor differences, the primary signals are consistent and recovered. This is reassuring.

Reply: The two groups/researchers collected the two datasets independently. The collaboration came about when all the analyses were performed. We agree that it is very interesting to show how, independently from the operator or the sample preparation or the (taxonomic) criteria adopted during the counts, the primary signals were recorded by both researchers. We will clarify this in the revised text, as suggested by the referee.

Detailed Comments:

4) P1619 – increase in $\delta^{13}\text{C}$ benthic as a change in storage of organic carbon in the lithosphere through increased organic carbon burial – maybe, but check other mechanisms of Merico et al (2008). Simple driver of this shift by carbon burial alone appears hard to reconcile with carbon cycle box models.

Reply: Correct. We now also mention, in particular, the effects of the glacioeustatic sea-level fall and sea water carbonate chemistry dynamics during this time, cf. Merico et al. (2008).

5) P1627; line 25 – the explanation of H diversity could be clearer: really a combination of evenness and diversity rather than “taking into account the relative abundances”.

Reply: We changed the text following the suggestion of the referee.

6) P1628 – first paragraph – again the placement of Oi-1; as noted above the base of this should be placed at the maximum $\delta^{18}\text{O}$ value in the basal Oligocene. This seems clear in the Riesselman et al. 2007 paper, but my impression is that the current authors are sliding into a usage for Oi-1 that includes the isotope shift itself.

Reply: The text in the previous version it is now edited for clarity and accuracy. See also reply#2 to G. Villa. We changed the text as follow: “Riesselman et al. (2007) placed Oi-1 at 93.14 mcd in correspondence of the maximum value recorded by the benthic $\delta^{18}\text{O}$.”

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7) Why are the authors using Okada and Bukry nannofossil zonations? Given that they are citing the new Agnini et al. 2014 zonations, and this zonation scheme seems to give better resolution around the E/OB, I would suggest either they use this scheme or justify why it is better not to. (Or at least show both).

Reply: We followed the suggestion of the referee and we adopted the zonation of Agnini et al. (2014). In the text we referred mainly to this zonation, with some references to the previous zonations of Okada and Bukry. In the figures (see revised Fig. 2) we used the scheme of Agnini et al. (2014).

8) P1628 – line 18: does the softness of the sediment really control the presence and / or preservation of palaeomagnetic signals?

Reply: In the Shipboard report (Zachos et al., 2004) this issue with the paleomagnetic data was mentioned. The softness of sediments may affect the magnetic signal because the grains tend to re-orient with the polarity shift. Moreover, the high carbonate content (thus, low % of clay material) led to poor paleomagnetic resolution. Unfortunately, no other papers or studies on paleomagnetism are available for this site.

9) Lines 25-26: I don't like these references to the calibrated ages. I would much rather the authors use the properly compiled calcareous nannofossil bioevents and calibrations given in Agnini et al. (2014). The authors would then need to make it explicitly clear which timescale they are using and why, and insure that all nannofossil datums are consistently calibrated with the chosen timescale.

Reply: We adopted now the zonations of Agnini et al 2014, and so we changed the calibrated ages for the ones available in that paper. We also clearly state that the timescale used is the one in Agnini et al 2014 (which refers to Pälike et al., 2006).

10) Table 1 – typo in “Massignano”.

Reply: Modified in the table.

11) Page 1629 – use of abbreviations “B” and “T” for base and top within the text. I

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am happy with the use of Base and Top, and I can understand “LO”, “HO” and similar appearing in text as abbreviations (lowest occurrence / highest occurrence). The words “base” and “top” are fairly concise and I would use them within the text. B and T are fine on diagrams and in tables, but can be ugly within sentences, for example: “commonly compromising the identification at the species level and thus possibly, its B.”

Reply: We agree with the referee and changed the text accordingly. We also conform the use of Base and Top for all the bioevents, so no confusion with HO and LO can occur.

12) Page 1629 – using top *D. saipanensis* to approximate the EOB, when this is clearly some way below the EOB (Dunkley Jones et al. 2008; Agnini et al. 2014). And in the figures, (e.g. Fig. 2) they clearly haven’t used this event to approximate the EOB, but place the EOB 6 meters above it! If they haven’t used this (wrongly) to approximate the EOB, why say they have? I would also like the authors to note the strong latitudinal diachroneity in the extinction of the multi-rayed discoasters (from ~40Ma to ~34.5 Ma; Agnini et al. 2014 and references therein). This may be depressing the level of this bioevent at ODP 1263 (compared to its new calibration at ODP 1218).

Reply: The reviewer is right and we have now made the necessary corrections in the text.

13) Page 1629 – identification of *Sph. tribulosus* – the figured specimen in the supplementary information (Fig. S1, 8) is not *Sph. tribulosus*, but looks like *Sph. predistentus* with somewhat overgrown upper spines. *Sph. tribulosus* has a very characteristic broadening in the basal part of the spine, I can’t see any evidence of this in the specimen figured.

Reply: We agree that the photograph in itself in the supplement is not enough to justify the classification as *S. tribulosus*. Thus, we added in the Supplement a photo to show the same specimens oriented parallel to the crossed nicols. With the two orientations of the specimens, the identity should now be indisputable.

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14) I also agree with Guiliiana Villa – Fig. S1, Fig15 isn't a dissolved Dictyococcites but a (slightly overgrown?) grill-bearing reticulofenestrid.

Reply: Correct. We removed the misleading photo from the supplements. See reply #6 to G. Villa.

15) use of *Clausicoccus obrutus*. I would like a little more detail on the species concept here and on the differentiation (if any) between this species and *Cl. subdistichus* and *Cl. obrutus*. Do the authors differentiate between these two species at a size of 5.7 μm ? Or by number of plates visible in the central area? Based on their distinction, what is the difference between the acme events in *Cl. obrutus* and *Cl. subdistichus*? At ODP 1263, is this increase in abundance more marked in the larger forms, for example?

Reply: When counting we divided *C. obrutus* and *C. subdistichus* on the base of size (*C. obrutus* >5.7 μm). There is a difference between the acmes of the two species: in Fig. S2 it is clear that *S. subdistichus* has an acme slightly before *C. obrutus*. The specimens of *C. subdistichus* are usually very small, so its abundance can be also affected by dissolution. In the end, we used the definition of Agnini et al. (2014) for *Cl. subdistichus*, by combining the two species in one group. It is also important to notice that the % of *C. subdistichus* is always very low (see Fig. S2) – average 1% - with a maximum peak of 6%.

16) Also be careful with previous zonal schemes – Okada & Bukry (1980) (based on Bukry 1975) – the base of the zone is defined by *Cl. subdistichus* not *Cl. obrutus*. Subsequent work may have compared abundance patterns in “*Cl. obrutus*” with the *Cl. acme* but the Okada and Bukry (1980) zonal scheme makes no mention of *Cl. obrutus*. In the new zonal scheme of Agnini et al. 2014 they regard *Cl. obrutus* as a junior synonym of *Cl. subdistichus*.

Reply: We use the definition of Agnini et al. (2014) for *Cl. subdistichus*, by combining the two species in one group.

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17) p1631. consistent presence of hantkeninid spines below 96.41 mcd. Linked to discussions above - were these in observed in absolutely every sample studied below the last occurrence in this section? This is important, and if there are samples without spines below this level, they should also be plotted in Figure 2 along with the crosses identifying the presence of spines. Unless of course all samples truly did show spines, in which case I'd like clear confirmation of this from the authors in the text.

Reply: Spines were present also in all the samples observed below the last occurrence. We now made it clearer in the text and also in the figure 2 by plotting all the observed samples for the hantkeninids occurrence.

18) p1632 – line 26 – “dissolution may be intense”; I think this is over-estimating the dissolution; with “intense” dissolution, I'd expect to see nothing but some robust placolith rims and heavily calcified nannoliths. I think this has slipped over from a description of the “more intense” dissolution interval?

Reply: Correct. We modified in the text by using “intervals of higher dissolution may have affected the nannofossil assemblage”.

19) p1633 – I have significant concerns about the discussion of nannofossil abundance (and assemblage) changes relative to the EOB. This links to my primary concern about the placement of the EOB some 2m below the plateau interval in the oxygen isotope shift, as discussed above. Placing the EOB before the isotope shift spuriously correlates important events in their nannofossil record with the EOB Hantkenina extinctions. In fact, these nannofossil assemblage changes are before the first isotope shift and significantly precede the tropical Hantkenina extinction. For example, the increase in abundance of *C. obrutus*, the decline in total coccolith abundance, drop in *D. bisectus* / *D. stavensis* abundance, major changes in PC1 & 2 and size changes all precede the isotope shift and should not be correlated with the EOB event.

Reply: Correct. The major events recorded by the nannofossil assemblage preceded the EOB and also the Step 1 (although Step 1 is not very clearly defined at Site 1263).

The discussion on nannofossil abundance and variations also of PC1 and PC2 were revised to make clear that the changes are pre-dating the EOB and the cooling (Step 2 and possibly also Step 1).

20) P1642 – Section 5.3. As above the placement of nannofossil assemblages changes in association with the EOB. For the reasons outlined above, I think the nannofossil assemblage changes significantly precede the EOB, as evidence by their relationship to the oxygen isotope stratigraphy in this section.

Reply: Section 5.3 has been revised by considering the new evidences for the placement of the boundary. Indeed, the referee is right in pointing out that the nannofossil assemblage changes preceded the EOB, and we did state that in the original text, albeit possibly somewhat confusing with regard to “at the EOB”. We changed this in the revised manuscript, because the nanoplankton response is preceding the EOB and the isotope oxygen signal.

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Interactive comment on *Clim. Past Discuss.*, 11, 1615, 2015.

CPD

11, C1269–C1278, 2015

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Fig. 2

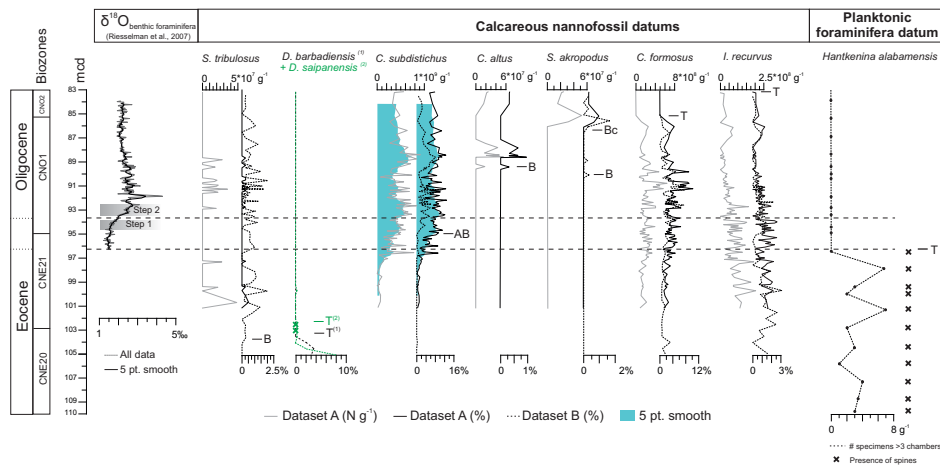


Fig. 1. Fig. 2 modified

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