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Climate of the Past
Discussions

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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This is an important and detailed study of coccolithophore community change across the Eocene – Oligocene transition (E/OT) in the mid-latitude South Atlantic. It presents detailed calcareous nannofossil assemblage data, from two independent researchers, combined with new placolith morphometric analyses and benthic foraminifera assemblage data. Together, these provide an important new record of the planktonic ecosystem, marine primary production and climate-biosphere connections across the E/OT. 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 outlined below.

My primary concern is the placement of the Eocene / Oligocene boundary within this section (see also the comment of Pearson and other reviewers). The authors do this on the presence / absence of Hantkenina spines found within foraminiferal residues, correlating the last occurrence of these spines in ODP1263, with the level of the extinction horizon of the genus Hantkenina at the E/O GSSP stratotype section at Massignano. In this section, the top of Hantkenina corresponds to the level GSSP defined E/O boundary. This is where care is needed, because the E/O boundary is not "defined" (p1618-19) by the Hantkenina extinction but, at this particular site, they are coincident. The Hantkenina extinction is thus the primary means of correlating from the GSSP to other sections. If, however, the extinction event is preceded by a significant range contraction or there is poor preservation or sporadic occurrence of Hantkenina, there is the potential for a stratigraphic offset in the position of this bioevent between sites. As the authors note, this correlation is not easy, given the often poor preservation of this genus and potential latitudinal diachroneity. Under the assumption that the positive oxygen isotope shift across the E/O transition is globally synchronous, then there is clearly an offset in the position of the top of Hantkenina spp. between ODP 1263 (below the first δ18O step) and Tanzania (between the first and second δ18O steps).

Regardless of whether the top of Hantkenina spp. at ODP 1263 or Tanzania is closest to the level recorded at Massignano, given the tropical location, excellent planktonic foraminifera preservation in the Tanzania record and synchronous extinction of multiple Hantkenina species within the "plateau" interval between the two δ18O steps at this location, it should be clear that this is the better record of the timing of the actual final Hantkenina extinction event (Pearson et al 2008; Wade & Pearson 2008). The authors assume that this extinction actually leads the first isotope shift, and thus falsely correlate the major calcareous nannofossil assemblage changes, which do appear to lead the first oxygen isotope shift at this location, with the Hantkenina extinction. 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).

I would reinforce the comment of Paul Pearson - the authors need to be careful about their use of "Oi-1". The base of Oi-1, as defined by Miller et al. (1991), is at DSDP Site 522 at "the maximum δ180 value in Stilostomella spp. at this level which is slightly below the increase reported in Cibicidoides spp." I would recommend a re-read the section "Terminology, correlation and calibration" in Coxall & Pearson (2007) – which addresses the definitions of E/OB and Oi-1.

The authors incorrectly use the term Oi-1, for example:

"Pearson et al. (2008), however, recorded the extinction of Hantkeninidae, thus by definition the EOB, in the plateau between the two main steps in the isotope records (i.e. within Oi-1) at Tanzania Drilling Project (TDP) Sites 11, 12 and 17."

The extinction level is within the plateau of the "E-O shift" (in oxygen isotopes), in the terminology of Coxall & Pearson (2007), and precedes the base of Oi-1.

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

Detailed Comments:

P1619 – increase in ∂13C benthic as a change in storage of organic carbon in the litho-

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sphere 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.

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".

P1628 – first paragraph – again the placement of Oi-1; as noted above the base of this should be placed at the maximum δ18O 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.

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).

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

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.

Table 1 – typo in "Massignano"

Page 1629 – use of abbreviations "B" and "T" for base and top within the text. I 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

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compromising the identification at the species level and thus possibly, its B."

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).

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.

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

– 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? 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

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new zonal scheme of Agnini et al. 2014 they regard Cl. obrutus as a junior synonym of Cl. subdistichus.

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.

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?

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.

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.

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