

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 anonymous reviewer

We thank the anonymous reviewer for the helpful comments on our manuscript. The reviewer also provided several editorial suggestions, which we will all consider in our revision of the text. Below we focus our discussion on the more topical points raised by the reviewer. Note: the pages and lines are updated in this file, because the pages number given by the referee did not match with the statement(s) or idea(s) referred to.

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Reviewer: I have a number of comments that can be synthesized in three main general groups:

A) The Oi1/EOB issues I totally agree with Paul Person (reviewer #1) on the Oi1/EOB issue. The position of EOB at Site 1263 is quite suspicious and the Top of Hankkenina and Cribroantkenina, the authors use to mark the boundary, is more likely anticipated because of dissolution and/or ecological factors. In addition, the use of different nomenclatures makes the reading very difficult and confusing.

Reply: This was clearly an issue picked up by all reviewers. We are currently undertaking additional analyses on planktonic foraminifer assemblages to better denote the position of the EOB at Site 1263 (see reply #2 to P. Pearson). We will also clarify the nomenclature (Oi-1, EOB, EOT ...) in the revised text (see reply #2 to G. Villa).

B) The biostratigraphic issues There are many comments on taxonomy, reliability and positioning of biohorizons, misleading use of biostratigraphic concept, mistaken use of biozone definition, age model, etc

Reply: We followed the suggestions of the reviewer, paying attention to the different concepts used for the biostratigraphy.

C) The paleoceanographic/paleoecological interpretation issues I have made some comments on the interpretation the authors did of their calcareous nannofossil and benthic foraminifera data. In particular, the authors will find observations on calcareous nannofossil absolute abundance data, role of dissolution, paleoproductivity proxies, statistical analyses, etc. ...

Reply: We understand the points raised on paleoceanographic and paleoecological issues and respond to individual comments below.

In the following the authors can find a list of minor to major issues ordered as they appeared in the text:

We edited the text accordingly, for the comments #4-5-7-12-13-15-17-19-21-23-24-25-

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26-28-30-35-38-39-40-42-43-46-47-48-50-53-55-59-60-66-70.

1) Pag. 1617, line 14. Do you have evidences for that?

Reply: It is possible that smaller, less calcified taxa have lower PIC/POC ratios on a cellular level than larger, heavily calcified taxa (compare for example modern species *Gephyrocapsa oceanica* vs. *Coccolithus pelagicus*, e.g. Zondervan et al. 2001; Langer et al., 2006; Rickaby et al. 2010; Müller et al. 2010). Smaller individual nannofossils/coccoliths relates to smaller carbonate mass (per individual) and may also relate to lower carbonate production per cell, but the net amount of carbonate produced is not only linked to the coccolith's size but also to the overall productivity of coccolithophores. Since we don't undertake any modelling in this paper to deepen this aspect, we will erase this statement here to avoid any over-interpretation.

2) Pag. 1617, lines 16-23 E/I is sensitive to carbonate saturation and O₂ not just food supply. How can you disentangle the role played by these three parameters?

Reply: We should not address this question in the abstract, but in the text. For this reason, we have now expanded the discussion in section 4.3 of the text as follow: "After Oi-1 (starting at 33.4Ma; 90.41 mcd), the abundance of *N. umbonifera*, an indicator of carbonate corrosive bottom waters, increased. Due to this evidence for dissolution, benthic foraminiferal accumulation rates can not be used to estimate food supply quantitatively and reliably. Evidence for effects of dissolution on benthic foraminiferal assemblages is seen in the interval where *N. umbonifera* is common, not in the interval with peak abundance of phytodetritus species (thin-walled, solution-sensitive species), and the interval where buliminids peak. These intervals also are not recognized as influenced by carbonate corrosivity (Riesselman et al., 2007). We thus do not consider that the increased percentage of infaunal taxa is, in this studied section, due to dissolution, although such an effect is seen in section with much more severe dissolution effects, such as the Paleocene-Eocene interval of ocean acidification (Foster et al., 2013). Increased abundance of buliminid taxa (though not of phytodetritus taxa)

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could possibly result from lowered oxygen conditions in bottom or pore waters (e.g. Jorissen et al., 2007). However, there are no indications in the sediment of low oxygen conditions (e.g. lamination), and the overall benthic assemblage does not indicate low oxygen conditions (e.g. the diversity is too high)."

3) Pag. 1617, line24.planktonic calcareous nannofossil. Too much general, in principal includes planktonic forams, but the authors do not present any new planktonic foram assemblage result, the only exception is the determination of the position of the Top of Hantkenina.

Reply: We will have more data on planktonic assemblages (in progress), especially to unravel the issue on the position of the EO boundary. Thus, the text will be changed according to the new data when available.

6) Pag. 1619, lines 5-8. I would agree with Paul Person. Though the formal definition of Rupelian GGSP includes the Top of Hantkenina and Cribrohankenina. These biohorizons better play the role of primary markers, which denote rather define the boundary. The recognition of the Top of Hantkenina and Cribrohankenina could be problematic in some cases but the use of alternative markers could serve to better constrain the position of the EOB.

Reply: We agree that by using only the T of Hantkenina we cannot accurately assess the position of the boundary, especially if the specimens are not well preserved. We added in the text also the other species used in Pearson et al. (2008) and we are performing extra analyses on planktonic foraminifera to identify the Pseudohastigerina size reduction and the T. cerroazulensis group extinction (see also reply #2 to P. Pearson).

8) Pag. 1619, lines 15-16. This concept is non-intuitive and should be explained by the authors. The increase of C org/Cinorg ratio can surely be the result of enhanced export productivity but can also be related to the increase in C org preservation. Additionally, if one considers long periods, this is the case, seasonal enhanced productivity (biological pump) usually works on a short time scale and constrained areas as a response to

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environmental changes but its efficiency as a buffer dramatically decreases if long time intervals and a global perspective are considered.

Reply: Coxall and Wilson (2011) wanted to “test the hypothesis that the onset of widespread Antarctic glacial activity in the earliest Oligocene was associated with a change in primary productivity and export production”. They documented how the increase in C org/C inorg at the EOB is related to increased surface biological production and/or increased efficiency of organic carbon export by comparing the $\delta^{13}\text{C}$ signal and the BFAR results which “appears to correlate predictably with export production in a variety of modern locations [Herguera, 1992; Loubere, 1994; Jorissen et al., 2007] and the method has been used widely as a qualitative/ semiquantitative proxy of paleoproductivity in the Pleistocene [e.g., Schmiedl and Mackensen, 1997; Gooday and Rathburn, 1999] and early Cenozoic [Smart, 2008; Alegret and Thomas, 2009]”.

9) Pag. 1619, line 20. The CCD deepening is a consequence more than a cause, as inferred by the authors. The sentence should be probably re-write in order to make this clear or the authors should explain better their point.

Reply: We re-phrased the sentence.

10) Pag. 1619, line 21. The use of term response sounds strange.

Reply: The word “response” is commonly used to indicate how a biotic community is behaving/changing to environmental/climatic variations. So the term is correct in this sentence. See also the use of “response” in e.g. Flores et al. (1995), Villa et al. (2008; 2014), Dunkley Jones et al., (2008).

11) Pag. 1619, lines 22-23. Extinctions always occur in the geological time. What the authors might mean is that rate of this extinctions either increases in its absolute number or increase if compared the speciation rate of the same interval.

Reply: Correct. The extinctions across the EOT increased if compared to the speciation rate. Now reflected in the text.

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14) Pag. 1620, lines 9-14. I'm not aware of any comprehensive species diversity study. This would include a measure of both species number and 'equitability' (or 'evenness') (e.g., Simpson Index, Fisher's alpha).

Reply: Several studies on nannofossil assemblages displayed the Shannon Weaver Index – i.e., H index (e.g., Persico and Villa, 2004; Dunkley Jones et al., 2008; Pearson et al., 2008), so we report here what is available in the literature for this time interval. It is possible to show also other indices, but they do not add different/ or new information to the H index.

16) Pag. 1620, line 15. I'm not sure they were more abundant but for sure they dominated the marine phytoplankton and show a higher species diversity with a maximum recorded in the early-middle Eocene (Bown et al., 2004).

Reply: The reference Bown et al. (2004) is added in the text and the higher diversity recorded during the early-middle Eocene (as summarized over 3 Myr intervals) is also added in the text.

18) Pag. 1620, line 17. The increase in abundance and species richness of diatoms started well before the EOB and coincide with the general decline displayed by calcareous nannoplankton since the early/middle Eocene (e.g Bown et al., 2004; Spencer-Cervato, 1998).

Reply: Correct; now this is better specified in the text.

20) Pag. 1620, line 26. What do you mean with “driven”? A macroevolutionary trend observed in group can not be “driven” by a part of the that group. Rather, it could be the result of something affecting selectively a part of the group. That is a complete different concept. Not sure what the authors mean.

Reply: In this context “driven” means that the trend is linked or mainly due to the decrease in abundance of large reticulofenestrids. We changed “driven” to “resulting from” in the text.

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22) Pag. 1621, line 1. The authors should strengthen their point using modern ocean analogues that are easily findable in literature.

Reply: We will add in the text references to modern analogues, in particular referring to the relationships between pCO₂ and cell size variations in modern phytoplankton/coccolithophores. We will take into account literature such as: Iglesias-Rodriguez et al. (2008); Finkel et al. (2009); Barcelos e Ramos et al. (2010).

27) Pag. 1621, lines 1 6-17. This is crucial. The number of forms per gram does not provide an estimate of fluxes, it is rather an evaluation of absolute abundance. The definition of paleofluxes are very different since it implies to put absolute abundance in a time tuned series (g10-6mm-2 y-1).

Reply: The number of nannofossils per gram does not provide an estimate of fluxes, and in the text we always refer to these data as absolute abundances, not as fluxes. The calculation of paleo-fluxes in this context is not possible in our opinion, because it requires a very detailed and well-constrained age model. Unfortunately, this is not the case at Site 1263. We identified several bioevents, but the magnetostratigraphy is not available (Zachos et al., 2004). Thus, by adding an estimated sedimentation rate that is below the resolution of the data, we would introduce a substantial error in the evaluation of paleo-fluxes. For these reasons we decided to present only absolute abundances.

29) Pag. 1622, lines 6-15. The authors should explain why they follow this approach, which is the bonus of having two datasets of “virtually” the same material? This is non-intuitive.

Reply: We now explained better in the text why we compared two different datasets. Two groups worked independently at the same time interval for this site, although the amount of samples and the temporal resolution are different. The decision to collaborate arrived after the analyses were performed. We think that this approach shows how, independently from the sample preparation and operator, the primary signals are

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documented and consistent, as displayed also in the results and the statistical analysis (PCA) sections (see also reply #3 to T. Dunkley Jones).

31) Pag. 1622, line 22. How the authors avoid the selective settling effect? This step should be described in much more detail.

Reply: For sample preparation we followed the protocol described in Bordiga et al. (2015), where the technique was accurately tested. Any further description is not required. Here, we would like to specify that the settling is not an issue in this technique as we start from bulk sediment and all the suspension (sediment and buffered water, well mixed by short sonification intervals) is evenly placed on the cover slip.

32) Pag. 1623, lines 3-4. It is quite significative considering the relative abundance changes observed for most of the taxa (see Fig. 3). The author should comment on this issue. CV => Please in full (coefficient of variation), at least the first time.

Reply: We will define “CV” as coefficient of variation in the text. The CV of 11% refers to the absolute abundances, not to relative abundances which are highly comparable on replicates and when compared with different techniques (see Bordiga et al., 2015). The variation among abs. ab. replicates of 11% is comparable to the results obtained in previous studies (see Bollmann et al., 1999; Geisen et al., 1999, and summary table in Bordiga et al., 2015).

33) Pag. 1623, lines 4-6. This is not correct. The number of specimens per gram counted in a prefixed area is an absolute abundance but this index does not take into account the time. What I mean it is that we know the number of forms per gram (absolute abundance) but we do not know the flux. i.e. number of forms \times g10-6mm-2 y-1. This value could have changed substantially if the mass accumulation rates change. In other word, if the authors has not a good highly-resolved age model for their study profile then they can say very little about paleofluxes /paleoproductivity. This a quite important point and should be convincingly discussed by the authors.

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Reply: It is correct that the number per gram does not take into account the time. We did not use the absolute abundances with this connotation in the paper, and we cannot calculate paleofluxes for the low resolution of the age model (see the reply #27). On the other hand, we were able to discuss paleoproductivity using the assemblage data (treated with the PCA) because we compared them with the independent proxies - the isotopes ($\Delta\delta^{13}\text{Cp-b}$) and benthic assemblage data - to reinforce our observations.

34) Pag. 1623, lines 6-10. I would agree with the authors but this issue is rather more complicated than explained here. See for instance (discussion paper of Gibbs et al. 2012 <http://www.biogeosciencesdiscuss.net/9/C618/2012/bgd-9-C618-2012.pdf>). In the following the authors eventually decide to use relative abundance data quite heavily (though with some transformations), so why to destroy this kind of data. This seems incoherent. Please comment.

Reply: We made this point more clear. The reviewer is right that the wording we used before could be construed as to “destroy” the relative abundances. Here we used both absolute and relative abundances and by comparing them we can investigate the influence of dilution and sedimentation rate. Indeed, as pointed by the reviewer, Gibbs et al. (2012) correctly argue that “Relative abundance provides ‘biological’ information, i.e. relative abundances of taxa within the populations, which is independent of modifying sedimentological effects, such as varying accumulation rate.” But we disagree that “changing the relative abundances into a more ‘absolute’ abundance form, such as numbers per gram does not provide any additional information and is actually adding a level of degradation. For example, to convert to numbers per gram would mean we introduce a dilution signal to our data that isn’t necessarily anything to do with a biotic response.” It is true that absolute abundances (N g^{-1}) are affected by dilution and sedimentation rates, but by comparing the absolute with the relative abundances (see results section and supplement Fig. S2) we documented that their trends are very similar. This suggests that the dilution/sed. rates at this particular Site were constant over time, thus we can refer to the absolute abundances as linked to bio-

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logical processes. (The constant sedimentation rate is also documented by the age model which, although not highly-resolved, provides a relatively constant sed. rate). In our case, the absolute abundances provide additional information about the total coccolith absolute abundance, which it is not possible to derive from other type of data (such as the relative abundances), and it is an important part to define the calcareous nannoplankton productivity. Indeed, if the trends of absolute and relative abundances of single species are similar, we can infer that the total absolute abundance is linked to biological processes.

36) Pag. 1623, lines 22-23. I would suggest that this information (number of fields of view (FOV) observed) would be added to the supplementary material.

Reply: The number of FOVs and its relative mm² area observed are now added in the text, not in the supplementary material.

37) Pag. 1623, lines 22-23. This is not clear to me. The authors first claimed that relative abundance data are problematic and now they decide to use these data to describe the composition of nannofossil assemblages. This is awkward. Why they do not use their absolute abundance data? This point should be better explained and justified.

Reply: We did not want to claim that relative abundances are problematic, but we agree that the sentence might have led to this misunderstanding. Now we rephrased it in the paragraph above. See reply to #34 above for more details on the use of relative and absolute abundances. We used the relative abundances mainly for PCA: that was necessary because we wanted to compare two different datasets, and the absolute abundances were available only for one of these datasets.

41) Pag. 1625, lines 2-6. I think this is a very nice approach but then, again, the authors should rethink about their statements on the poor validity of relative abundance data. Either they are a good proxy of what is going on or they possibly lead to loss of information and misinterpretation of the results (as stated above). You cannot have

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your cake and eat it, you have to reformulated your sentence. . .

Reply: See comment #34 and 37, we do support the validity of relative abundances. Considering the explanation now given in paragraph 2.2.1, the sentence does not need to be reformulated.

44) Pag. 1628, lines 11-13. I do not get the point here, which kind of bias do you mean? In addition, Set A and Set B area quite different one to each other and even if consistent results finally popped out, these should be discussed properly. Just as a note, if you think something can bias your data, as it is generally written in your sentence, then, in principal, you have to be worried about the possible misleading alteration due to “the two operators effect”. Again, I do not see the real point in following this approach (duplicating datasets).

Reply: We made the sentence clearer. We meant that we wanted to compare the occurrence (detection) of marker species, especially the rare ones. The use of the two datasets is also important to compare the results of PCA, and to observe if the primary signals are documented by both datasets. About the use of two datasets see reply #29.

45) Pag. 1628, lines 20-26. The age model as constructed by the authors to compare the two dataset followed a quite circular reasoning, without any independent correlation tool (e.g., magnetostratigraphy, isotope stratigraphy, . . .) in support of their chronological framework. Now, it is quite clear that this will not going to affect the correlation between set A and set B too much because the two series are recovered by different holes of the same Site and they hopefully recorded the same geohistory, but what could instead happen if these datasets (without any independent age constrains, but derivated nanno biochronology) will be correlated using the same rationale? I would suggest the authors to add a sentence about this issue.

Reply: The correlation with the oxygen isotope curve (Oi-1, or step 2) is inserted in the text and figures (figs. 2-3-4-5-6). We are improving the age model with more analyses

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on planktonic foraminifers to better define the age model (see reply #6).

49) Pag. 1629, lines 8-9. The stratigraphic range is related to a species not to a bioevent. The position of bioevent (e.g., B. S. trilobosus) is the consequence of the stratigraphic range of S. trilobosus. In other word, a biohorizon has not a stratigraphic range is rather a stratigraphic level, in this case the stratigraphic level where S. trilobosus first occurred.

Reply: Correct. Modified in the text.

52) Pag. 1629, lines 12-14. abundant and it's the poor preservation of the study material is commonly compromising the identification at the species level and thus possibly, its B. Again, this sentence sounds strange. Abundant? This is an euphemism. I would say that this species is rare to very rare and sporadic.

Reply: Modified in the text. Yes, the species is rare and sporadic.

54) Pag. 1629, lines 18-19. Looking at the abundance pattern, I would say that the Top of D. saipanensis should be positioned at ca. 104 mcd, where this species goes to 0. Above that level only sporadic occurrence of the species is detected. This choice would guarantee for a higher reproducibility of the event, but this might depend on a different philosophy, but the authors never explain their rationale.

Reply: The T of D. saipanensis had been placed at 102.27 mcd because specimens of D. saipanensis had been continuously found until 102.52 mcd, although outside the count of 300 specimens. We added the presence of D. saipanensis between 104 and 102.27 mcd (green crosses) also in Fig. 2 to be clearer.

56) Pag. 1629, line 24. Please consider to use C. subdistichus in place of C. obrutus. See taxonomic note on the pivotal work of Backman (1987), www.nannotax.org and other recent papers.

Reply: Modified in the text: according to Agnini et al. (2014) we combined C. obrutus and C. subdistichus because Clausiococcus obrutus is considered to be a junior

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synonym of *C. subdistichus* (www.nannotax.org).

57) Pag. 1630, lines 2-3. This is not correct. The base of Zone CP16b is defined by the T of *C. obrutus*. Recently, Agnini et al (2014) proposed to use the B of *C. subdistichus* (whose definition include also *C. obrutus*) to define their Zone CNO1. Backman (1987) never emended the original definition of the base of CP16b. In his key paper, he emphasized the potential of the Bacme of *C. subdistichus* and suggested that this bio-horizon could be used to subdivide Zone NP21. He did not mentioned about the base of CP16b, whose application is for sure difficult since the Tacme of *C. subdistichus*. All this issue should be managed.

Reply: We modified the text and Fig. 2 according to the definition of Agnini et al., (2014) and we specify now better the adopted scheme for biozonation (see Fig. 2 re-submitted).

58) Pag. 1630, line 10. The B of *C. altus* can be is tentatively placed with certainty at 89.4 mcd. Based on what the authors wrote in the previous sentence, I would say that the use of “with certainty” should be avoided.

Reply: Correct. Modified in the text.

61) Pag. 1630, lines 16-17. B and Bc were identifiable I would agree for Bc of *Sphenolithus akropodus* but B of *Sphenolithus akropodus* is very very tentative. Do you really think this is a reproducibile event?

Reply: Just few and sporadic species of *S. akropodus* are present below its acme. We think that it is important to also report the first occurrence of this species, but we specified in the text that this is tentative due to the rarity of this species.

62) Pag. 1630, lines 20-22. This is tricky. The abundance plot stops exactly where *E. formosa* goes to 0. My point is how can you be sure that 85.15 mcd actually corresponds to the Top of the species. Is this just because of the shipboard data. I can not see any other independent evidence for this statement. Please comment.

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Reply: Considering both datasets A and B, six samples above 85.15 (up to 83 mcd) where analysed to detect the disappearance of *C. formosus*. We consider this good evidence for the position of this datum. Moreover, this depth is very close to the one detected by the Shipboard Party (at 86 mcd). We modified Fig. 2 plotting also the six samples where *C. formosus* is not present.

63) Pag. 1630, lines 23-27. See comment above.

Reply: For the case of *I. recurvus* we cannot be sure that the depth of 83.19 mcd truly corresponds to its disappearance because just one sample above the last observed specimen was analysed. This is why we stated in the text that this depth is “an approximation”.

64) Pag. 1631, lines 11 -13. I totally agree with Paul Pearson. The Top of Cribohan-kenina and Hantkenina are in fact a marker of the EOB but they should be used with extreme caution and, if possible, integrated with other additional biohorizons that would strengthen the datum.

Reply: We agree with this observation. Unfortunately, at the moment, we cannot say anything until the additional planktonic assemblage will be completed. For these reasons, the placement of the EOB cannot be completely solved with the available data. We could have placed the EOB in between the T of Hantkenina and the step 2 (or Oi-1) (dashed lines in Fig. 2 re-submitted), but we decided to add more analyses on planktonic foraminifer markers (in progress). See also reply #2 to P. Pearson and #6 here.

65) Pag. 1632, lines 5-7. This inconsistency could be related to a change in carbonate source (more forams) but this is not the only possible explanation. The authors should take into account the different amount of carbonate produced by different taxa. Small taxa produce less carbonate so that the same number of specimens could in fact have produced a smaller amount of carbonate and viceversa. Hence, the absolute number of specimens per gram does not give a straightforward indication of what is going on.

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To obtain this information you should have the total amount of carbonate produced by calcareous nannoplankton at that time. Please discuss.

Reply: We agree with the reviewer. We modified the text specifying that we don't know the amount of carbonate produced by foraminifers and calcareous nannoplankton, so that the real contribution of each group cannot be unravelled in this study.

67) Pag. 1632, lines 17-18. How can you say that? During the late Eocene - early Oligocene, small placoliths are by far the dominant taxa in the ocean, the total absence of small placoliths would have an (important) impact on the paleoecological interpretation of CN data. This is the endless debate pristine signal vs dissolution. Please comment.

Reply: The small placoliths were not abundant in the studied interval, particularly during the late Eocene. It is true that the absence of small placoliths might be indicative of dissolution, and clearly dissolution is present at this site. For comparison we calculated also the coccolith dissolution index which shows intervals of increased levels of dissolution (although overall still "moderate to good preservation" Zachos et al., 2004), but not always corresponding to lower absolute abundances. Of course such hypothetical/possible signal from small placoliths is not detectable anymore, but by calculating the coccolith dissolution we provide an alternative and valid index for comparison between samples and with other data.

68) Pag. 1632, line 20. (Fig. 3) => I would add the isotope curve. . .

Reply: We added the isotope curve in Fig. 3.

69) Pag. 1632, lines 24-26. The absolute abundance of CN is not preserved. As you stated just few lines above, many small placoliths were dissolved. What you can try is to support the idea that the relative abundance of the different taxa remained the same, but this is obviously not true because, as you said, dissolution is selective, which in turn implies it does not affect different taxa in the same way.

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Reply: It is correct that the absolute abundance may have been affected by dissolution, because we cannot reconstruct the story of small placoliths. But we can say that moments of intensified dissolution do not correspond to moments of lower abundances: thus, we can still discuss about the main features of the assemblage. See also comment #18 from T. Dunkley Jones: the dissolution was not so intense, otherwise “we would expect to find just some robust placoliths and heavily calcified nannoliths”. Instead, we find abundant small-medium sized *Cyclicargolithus* and also *holococcoliths* (which are recognized to be very prone to dissolution; Blaj et al., 2009; Bown et al., 2008; Young et al., 2005) in our samples.

71) Pag. 1632, lines 25-28. This is actually not clear to me. At the EOB, the large placoliths increase, this is crystalline, but if I look at the total absolute abundance the decrease is much less marked, may be because of the increase of *C. pelagicus* that, at least in part, counterbalances the trend of large placoliths. It is likely, however, that the export carbonate productivity decreases because larger coccoliths produce more carbonate. So again, coccolith absolute abundance and carbonate export productivity are different concept.

Reply: It is correct that *C. pelagicus* slightly increase when the large *reticulofenestrads* decrease, and this is why the decrease of total abundance is less marked than the abundance curves of large *reticulofenestrads*. We better explained this point in the text, being careful to refer to the decrease of large “*reticulofenestrads*” and not decrease of large species in general. We added a more detailed explanation about the export carbonate productivity concept in the discussion.

72) Pag. 1636, lines 8-9. I would say that *Cycligarlolithus* mean cell size drives the high correspondence between in V:SA and PC1.

Reply: We do not show “*Cyclicargolithus* mean cell size” as a separate curve, but we agree with the reviewer that the abundance and medium size of *Cyclicargolithus* influences the red curve most (since it is the most abundant species in the mix). However,

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in similar fashion one could argue that the contribution (abundance) of large-sized reticulofenestrids (which, as shown in the green dashed curve in Fig. 5, do not drastically vary in size, but rather, decrease in ABUNDANCE as shown in Fig. 3) is “driving” the curve.

The comment prompted us to clarify the point that the mean V:SA values are derived from the relative abundances of differently sized morphotypes, so that both changes in relative abundance and potential size shifts within morphotypes may influence the results (although the latter could also mean jumping into a different size class and thus be seen as a relative abundance shift). For example, the large retics actually do not “decrease in size” across the EOT, but they contribute less to the mean V:SA value across the EOT.

73) Pag. 1636, lines 15-18. I would reiterate my point. You do not have any information of dominant taxa, just because they are not in the assemblage anymore. It might be the case that smaller placoliths show a particular trend. For instance, if they would be very abundant, where larger coccolith are very rare then your hypothesis is collapsing like a house of cards.

Reply: The information about the very small placoliths is not available, either because they indeed were rare or, more probably, because of dissolution. But we think that it is encouraging that the independent proxy for dissolution, i.e. the coccolith dissolution index, does not correspond to increases in mean size (which could be seen as the result of dissolving away the smaller sizes of the assemblage).

Moreover, the loss of large reticulofenestrids cannot be linked to a dissolution bias – since they are more robust and resistant to dissolution than smaller (medium) forms such as *Cyclicargolithus* spp. The trend of mean V:SA under discussion is not affected by the presence or absence of small placoliths, that are irrelevant in this consideration of mean size variation within the medium to large reticulofenestrids. See also our response above, #72.

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Other authors did not either document a substantial or consistent presence of small ($<3\mu\text{m}$) specimens in the same time interval (late Eocene-early Oligocene) at various latitudes (Persico and Villa (2004) and Villa et al. (2008) in the Southern Ocean, Fioroni et al. (2015) in the equatorial Indian Ocean).

74) Pag. 1636, line 9. This paragraph is a long dissertation on what is going on in the placolith world where CO₂ values are decreasing. This is really fascinating but I would like that the authors look at the entire assemblage. Are the changes observed in non-placolith taxa (e.g, *Sphenolithus*, *Discoaster*, *Z. bijugatus*) confirming their interpretation? This is would be really interesting to see. The authors may claimed that these taxa represent a minor component but they are ca. 20% (on average) of entire assemblage and, even more importantly, they produced much more carbonate than a small placolith. Finally... What about *C. pelagicus*? It is a placolith (a major component of the assemblage) but it does not seem to follow the same trend observed for reticulofenestrids (Fig.3). How can you explain that? Why V:SA ratio of *C. pelagicus* increase when reticulofenestrids decrease in their cell size?

Reply: Other species, such as *Sphenolithus* spp. (all species grouped together), do not show any particular abundance variations at the same depth as the large reticulofenestrids. *Z. bijugatus* shows a slight decrease before the decrease of large reticulofenestrids and in correspondence of the decrease in abundance of *Discoaster* spp. (Fig. S3). Considering the paleoecological significance of *Sphenolithus* and *Discoaster*, as reported in the literature, their higher abundances may relate to the more oligotrophic conditions during the late Eocene. However, a relationship with the reconstructed trends in paleo-CO₂ cannot be seen. Still, we added in the text that also these two species decreased before the EOB and the oxygen isotope maximum.

We re-iterate that mean V:SA values are derived from the relative abundances of differently sized morphotypes – whether for (groups of) reticulofenestrids or *C. pelagicus*. *Coccolithus pelagicus* shows little variation in abundance and “size composition” (differently sized morphotypes), and thus not either in mean V:SA estimates. Why this is

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the case, from a paleo-biological perspective, is of course a very interesting question. We don't know why this taxon would be less affected across the EOT, but it seems evident that Coccolithus vs reticulofenestrids tend to respond in different ways, likely linked to differences in physiology between the two families (e.g. Rickaby et al. 2010; Krug et al. 2011; Lohbeck et al. 2012; Gibbs et al., 2013).

75) Pag. 1637, lines 10-12. I would stress this point because this really supports the authors' scenario. Larger forms are proved to be less prone to dissolution. A general decrease in size would not be caused by dissolution, which works the other way around.

Reply: Correct. We modified the text as follows, making this point clearer: "The distinct variation in nannoplankton abundance and average size of medium to large placoliths below the EOB at Site 1263 cannot be explained by dissolution – which would affect smaller coccoliths preferentially and lead to an increase of the mean size of the whole assemblage, opposite to what we observed – nor by a change in species diversity, but is mainly linked to changes in community structure (Fig. 3)".

76) Pag. 1640, lines 8-13. a) The positive loading of PC2 is bizarre. How can you explain the fact that the major component of the assemblage (even considering that clr is applied to the dataset), the reticulofenestrids, show very little load capacity? b) How can you explain that Sphenolithus and Discoaster, two warm oligotrophic taxa, have a positive component loading in PC2. If your interpretation is correct, PC2 represents paleoproductivity, then I would expect the opposite behavior. This result points for a strong eutrophic affinity for sphenoliths and discoasters. c) How can you explain that D. stavensis and D. bisectus show an opposite behaviour if compare with that of R. scrippsae? The reason why I ask this question is that if you accept the taxonomic validity of genus Dictyococcites, you should consequently ascribed scrippsae to Dictyococcites not to Reticulofenestra. As a note, D./R. scrippsae (Fig. 4) is possibly considered a junior synonymous of D. hesslandii, so please consider to revise its taxonomy. d) Are you sure that PC2 (by the way, PC2 could account just for the 14% of

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the variance of the entire assemblage) could be correlated with paleoproductivity so straightfully?

Reply:

a) The reticulofenestrids show little loadings with PC2 because two components retained through the PCA are, by definition, uncorrelated variables (Quinn and Keough, 2002 – Data analysis for biologists). Thus, the species strongly loading the PC1 will not necessarily load also the other components (as it is the case here). It is not always the case that the most abundant species have to load both (or several) components of the PCA. Moreover, we cannot really say that PC2 is loaded by “not abundant” species, because *C. pelagicus*, *L. minutus*, *Sphenolithus* together represent the 20-40% of the assemblage.

b) It is true that the positive loading of *Sphenolithus* can be problematic for the interpretation of the PC2, because it has always been considered a warm-oligotrophic species. We can infer that probably *Sphenolithus* was an opportunistic taxon, as also documented by Wade and Bown (2006) for the Messinian: “*Sphenolithus* appears to have been an opportunistic taxon that was capable of occupying a wide range of environments including restricted mesotrophic waters”. But the interpretation of the positive loading of *Sphenolithus* at Site 1263 still remains controversial, and we made it clearer in the text. The positive loading of *Discoaster* is not strong enough to be meaningful for explaining the PC2.

c) We agree that from a taxonomical point of view it is better to ascribe *R. scrippsae* as *Dictyococcites scrippsae*, and we changed this in the text. We cannot fully define the different behaviours of the different loadings in PC2, especially because the loading of *D. scrippsae* is very small, thus it does not have to be taken into account for the interpretation of PC2. We will maintain the name species “*scrippsae*” because our specimens can be described as “coccoliths with a solid central plug” (nannotax3 website), while *D. hesslandi* is defined as “Coccoliths small to medium sized (3-7 μ m)

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with central area nearly closed by inner tube” cycle (nannotax3 website). We did not observe a nearly closed inner tube but a solid central plug.

d) We were able to correlate PC2 to paleoproductivity not only on the basis of the species loadings, but also from the comparison with carbon isotope data and data assemblage from the benthic foraminifers. Indeed, PC2 is not the primary factor, so it is not mainly driving the assemblage.

77) Pag. 1640, lines 19-20. ...Or just because this correlation doesn't work. I do not mean that the final interpretation is incorrect but it shouldn't be based on so weak an argument. The authors have a stronger potential defense for their interpretation.

Reply: We agree that we need to strengthen our interpretation in the text. We can argue that the low correlation between the two curves is due to the low number of samples in common between PC2 and carbon isotopic gradient (indeed the use of the same samples is required to make a correct correlation analysis). But from a visual comparison between the two curves we can see that both recorded an evident decrease around 92 mcd. Moreover, we have a good comparison with the benthic foraminifer assemblage, which confirms the interpretation of PC2 as paleoproductivity or nutrient supply. Indeed, the increase of phytodetritus species corresponds very well to the positive peak of the PC2. So, we have two independent proxies to strengthen the interpretation of PC2 derived from the nannofossil data.

78) Pag. 1640, lines 24-25. This is counter-intuitive. Looking at the Pacific record (Coxall et al., 2005) as many others, I would expect an increase in productivity. If the authors claimed for the opposite, they should provide an explanation for this inconsistency. Is this a local effect? And, if this is the case, Can they provide a global paleoproductivity model in which their dataset could be included? Is there any chance that their results could be interpreted in a different way?

Reply: We reported in the text several previous studies where the paleoproductivity has a similar trend as we recorded (Page 1642 lines 1-14). This confirms that similar

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conditions occurred also at other tropical and equatorial latitudes. Of course we cannot completely exclude the influence of some local effects. The text had been changed to make this point more clear. About providing a global paleoproductivity model: this would imply another level of analysis and it is not the main aim of this paper. We think that our interpretation is well supported given the two fossil groups and the isotope data.

79) Pag. 1641, line 3. This data set could nicely account for what is going on in bottom waters not in sea surface waters. It often happens that these two domains are “disconnected”, especially during dramatic changes in paleoenvironmental conditions, as the EOB.

Reply: Although the two domains might be disconnected, the benthic foraminifers have been used as indicator of paleoproductivity at the surface also at the EOB (e.g. Coxall and Wilson, 2011). We previously used “nutrient supply” which suggests supply in N and P to photosynthesizers, but here we are talking about food supply to the benthos, which is not ‘nutrient supply’. To make this point clearer we changed p. 1641 lines 3-4 to: “The benthic foraminifer assemblage confirms the interpretation of the PC2, adding information on the nature of supply of organic matter to the seafloor (Fig. 6).” We will also specify better and separate nutrient supply to the nanoplankton from food supply to the benthos (for example in p. 1621 line 12).

80) Pag. 1641, lines 12-14. If I have understood correctly, the decrease in coccolith size is driven by decreasing CO₂ values, Am I wrong? Which is the driving forcing for this change? The CO₂ or the paleoproductivity? And, in case they are both responsible for this change, which is the factor commanding the decrease in coccolith size?

Reply: We postulate that the decrease in mean cell size of placolith-bearing coccolithophores could have been driven both by varying levels of CO₂ and/or paleoproductivity (or, rather, nutrient supply). We divided the discussion of the two factors in two paragraphs, but it is probably better to state again the influence of pCO₂ also in this

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paragraph. In the previous paragraph (Page 1639 lines 5-11) we discuss that other abiotic factors might have affected the decrease in cell size, and that they are deemed to be dominant. For clarity, we changed the text as follow: “The variations in nutrient supply, as reflected in both nannofossil and benthic foraminifer assemblages, is a factor that could possibly have driven – together with the pCO₂ decrease – the mean coccolith size decrease across the EOB.”

81) Pag. 1641, lines 16-20. I do not get the point here. The decrease in coccolith size occurred at ca. 96 mcd and coincides with high seasonal productivity in benthic communities (Fig. 6 and Fig.7=> phytodetritus abundance). Is there a possibility that buliminids show a relative decrease in abundance because they are temporary over-competed by phytodetritus species, in a different but still high productivity regime? Did I miss something?

Reply: As explained in the reply to comment #79, benthic forams can not be directly linked to nutrients (N, P) since they are not autotrophs. To make this point clearer, we changed the text (p. 1641 lines 3-20) as follow: “The benthic foraminifer assemblage confirms the interpretation of the PC2, adding information on the nature of the nutrient supply (Fig. 6). The increase across the EOB of the phytodetritus-using species indicates an increase in seasonal delivery of food to the seafloor, correlated to the interval with positive scores in PC2 (Fig. 6), though interrupted by a short interval of increased productivity across the EOB (as shown by the peak in the buliminid species curve at 96.27 mcd; Fig. 6). This peak indicates a high, less seasonally interrupted food supply, and reflects the highest food supply to the sea floor of the studied interval. Strongly seasonal food supply returned, suggesting both increased seasonality and somewhat lower food supply during the peak-buliminid interval. Seafloor conditions changed after Oi-1, and the increase in abundance of *N. umbonifera* and the decrease of phytodetritus and buliminid species indicate more corrosive bottom waters, possibly combined with less food arriving at the sea floor and a less pronounced seasonality (Fig. 6). This overall decline in productivity, and export productivity from the EOB through Oi-1

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as reflected in nannofossil and benthic foraminiferal assemblages, could possibly have driven the mean coccolith size decrease starting at the EOB (~96 mcd). In fact, the transient higher availability of nutrients at the onset of Oi-1 (~ 92 mcd), may have made it possibly for small opportunistic species above the EOB to outcompete large specialist species. The decrease of mean cell size (less biomass per individual) and, also, of total nannofossil abundance could have led to less available organic matter and, thus, less food for the benthic foraminifers, and smaller nannoplankton could have caused a decrease in delivery of organic matter to the seafloor (and/or higher remineralization)."

82) Pag. 1641, lines 21 -24. What about the missing part of the story, the smaller placoliths. If I follow your reasoning, I would imagine that smaller placoliths (3-4 μm), which are absent from the fossil record, should have been very abundant at that time, may be increasing in number as their larger counterpart (4-7 μm) did.

Reply: With "small placoliths" we meant specimens smaller than 3 μm . Specimens with a size of 3-4 μm are present and well-represented in the assemblage. It is possible that during moments of higher productivity the small nannoplankton could have flourished or increased its abundance, and it is true that we don't have this information at the Site 1263. But, also other sites, with a good preservation, high abundances of small placoliths have not been recorded during the same time interval. See reply #73.

83) Pag. 1642, lines 0-13. As I commented above, you need to synthesize all these data in more global perspective.

Reply: In the text we will add a general synthesis of the data available also from other sites and already presented in the discussion. For a synthesis, a more detailed study involving also modelling (as suggested in comment #78) is needed, but it is outside of the scope of this paper.

84) Pag. 1644, lines 12-15. See comments above on the same issues.

Reply: See reply #83. We will add a more detailed comparison with previous studies

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here as well (partially already done in paragraph 5.1).

85) Pag. 1645, line 13. I commented above on each of the main results reported in the conclusions. Some should be revised.

Reply: The conclusions will be revised/clarified following all discussions in this open online forum. In particular, we will clarify the position of the biotic changes relative to the EOB (updated with new planktonic foramin data) and Oi-1, as discussed the paleoproductivity issues raised by this referee.

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Fig. 2

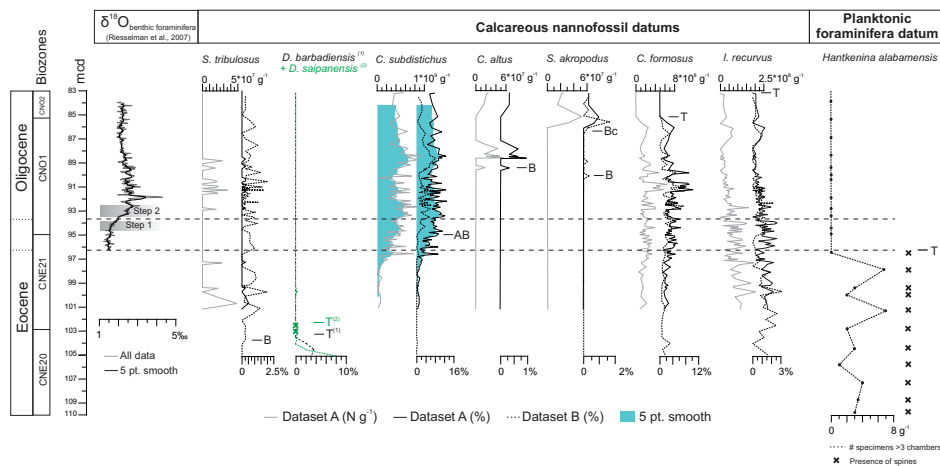


Fig. 1. Fig. 2 modified

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