Review of the manuscript entitled "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, J. Henderiks, F. Tori, S. Monechi, R. Fenero, and E. Thomas

This is an interesting contribution focused on the response of calcareous nannofossils and benthic foraminifera to the EOT as recorded at Site 1263 (Walvis Ridge, SE Atlantic). The authors provide a very nice and highly-resolved integrated dataset(s), which they use to provide the occurrence of paleoclimatic/paleoenviromental changes around EOB.

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

-*The Oi1/EOB issues*

I totally agree with Paul Person (reviwerer #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.

-*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 ….

-*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….

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

Legend=> ~~Strikethrough~~=text deleted, *Italic= text added*

Pag. 1616, line 14. Do you have evidences for that?

Pag. 1616, lines 16-23 E/I is sensitive to carbonate saturation and O_2 not just food supply. How can you disentangle the role played by these three parameters?

Pag. 1616, line14.~~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*.

Pag. 1617, lines 26-28: The Eocene–Oligocene boundary (EOB; ~ 33.89 Ma, Gradstein et al., 2012) is *formally* defined by the extinction of *two* planktonic *foraminiferal genera*~~s~~ (specifically, ~~the genus~~ *Hantkenina* *and Cribrohantkenina; Premoli Silva and Jenkins, 1993*),

Pag. 1617, line 1. by a *positive* peak

Pag. 1617, 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.

Pag. 1617, lines 5-7. *A possible explanation of this inconsistency might be that* t~~The~~ highest occurrence of *Hantkenina* spp. *and Cribrohantkenina spp.* may be~~ influenced~~ *affected* by

Pag. 1617, lines 15-16. This concept in non-intuitive and should be explained by the authors. The increase of C_{org}/C_{inorg} 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 environmental changes but its efficiency as a buffer dramatically decreases if long time intervals and a global perspective are considered.

Pag. 1617, 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.

Pag. 1617, line 21. The use of term response sounds strange.

Pag. 1617, 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.

Pag. 1617, lines 21-23. There is a strong link between climate ~~change~~ and ~~response~~ *change observed in* ~~of the~~ marine and land biota during the late Eocene-early Oligocene. This ~~was~~ *has been considered a* time *of deterioration* ~~of~~ *with high* extinction rates and ecological reorganization ~~in~~ *affecting* many biological groups

Pag. 1619, lines 9-14. ~~The~~ *C*~~c~~alcareous nannoplankton ~~community~~underwent significant changes at the EOB. Although the group did not suffer extinctions right at the boundary, as the planktonic foraminifers *did*, the structure of *their* assemblages underwent *a significant* global reorganization. Species ~~diversity~~ *richness* decreased ~~through the loss~~ *at the expenses* of K-~~selective,~~ specialist*s* ~~taxa~~ and ~~the abundance~~ *favoring the increase* of opportunistic species, more adapted to the new climate/environment*al*~~,increased~~ *conditions*

Pag. 1619, 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).

Pag. 1619, line 15. Flourished *and diversified.*

Pag. 1619, 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)

Pag. 1619, line 17. Diatoms ~~become more abundant(~~ *eventually overcompeted the group*

Pag. 1619, 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).

Pag. 1619, line 24. ~~presumed.~~ I would delete "presumed", there are several lines of evidences which support this view.

Pag. 1619, 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.

Pag. 1620, line 1. ~~Become~~ *are*

Pag. 1620, line 1. The authors should strengthen their point using modern ocean analogues that are easily findable in literature.

Pag. 1620, lines 6-7. These studies have highlighted distinct ~~compositional~~ shifts *in the composition of the assemblages* and changes in species diversity. The term changes does not clarify the direction of the variation.

Pag. 1620, lines 10-13.We report on calcareous nannofossil and foraminiferal biotic events between 34.76–32.7*0* Ma. *In particular, we* ~~to~~ refine the shipboard biostratigraphy published in Zachos et al. (2004) and describe the ecological response *of calcareous nannoplankton and benthic foraminifers* to environmental change*s during EOB*.

Pag. 1620, lines 13-14. fluctuations in *their* total abundance and ~~species~~ *taxonomic* composition *of the assemblages*. I image you mean total abundance of coccoliths and taxonomic variations within the assemblage. "Species composition" does not make any sense to me.

Pag. 1620, line 15. And to benthic foraminiferal assemblage data *available for Site 1263* ~~from the same site~~

Pag. 1620, lines 16-17. 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 ($g10^{-6}mm^{-2}\ y^{-1}$).

Pag. 1620, line 20. across the EOB ~~in greater detail~~.

Pag. 1621, 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.

Pag. 1621, lines 13-16. ~~An~~ *A*~~a~~dditional 76 samples were analysed in set B (83.59–105.02 mcd, sampling resolution of 10–15 50 cm). The two sample sets were independently analysed by *two* different researchers ~~and we combine these data~~ *and eventually combined*.

Pag. 1621, line 22. How the authors avoid the selective settling effect? This step should be described in much more detail.

Pag. 1622, 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.

Pag. 1622, 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 x $g10^{-6}mm^{-2}\ y^{-1}$. This value could have could 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.

Pag. 1622, 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.biogeosciences-discuss.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.

Pag. 1622, lines 11. examined under *a* ~~crossed~~ polarized light microscopy. Hope the authors examined their samples both under crossed and parallel nicols.

Pag. 1622, lines 22-23. I would suggest that this information (number of fields of view (FOV) observed) would be added to the supplementary material.

Pag. 1622, 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.

Pag. 1623, line 10. Sample set A was ~~also~~ used to characterize

Pag. 1623, line 22. 2.3.2 *Calcareous* Nannofossil~~s~~ proxies

Pag. 1623, line 23. The distribution of coccolithophores in *sea* surface water*s*

Pag. 1624, 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 your cake and eat it, you have to reformulated your sentence…

Pag. 1625, line 1. indicates water*s*

Pag. 1625, lines 11-12. during the middle Eocene climat~~e~~*ic* ~~maximum~~ *optimum*

Pag. 1627, 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).

Pag. 1627, 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.

Pag. 1627, lines 20-26. For bioevents which are diachronous or not reported in Gradstein et al. (2012), the most recent literature was selected, onsidering the datums recorded at latitudes as close as possible to the studied site.

Pag. 1627, lines 22-24. I would say more reliable. The fact that the calibration used for a specific biohorizon is new or just geographiclly close does not guarantee for their quality.

Pag. 1627, lines 24-25.from 32.7 Ma (~~HO~~ *Top* of *Isthmolithus recurvus*, Lyle et al., 2002) to 34.76 Ma (~~HO~~ *Top* of *Discoaster barbadiensis*, Gradstein et al., 2012).

Pag. 1628, lines 8-9. The range ~~for~~ *of* this ~~bioevent~~ *index species* (Bown and Dunkley Jones, 2006) is from …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. trilobusus.* 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.

Pag. 1628, line 10. corresponding to CP16-18 Zones *(Okada and Bukry, 1980).*

Pag. 1628, line 11. This is an awkward sentence. Please re-phrase.

Pag. 1628, lines 12-14. abundant and ~~it's the~~ poor preservation *of the study material* ~~is~~ commonly compromise~~ing~~ 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.

Pag. 1628, lines 17-19. The T of *D. barbadiensis* was not identified *reported in* ~~by~~ the Shipboard Scientific Party (Zachos et al., 2004), and we placed *this bioevent* ~~it~~ one meter below

Pag. 1628, 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.

Pag. 1628, line 22. show that they are ~~not coeval~~ *shortly spaced*.

Pag. 1628, 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.

Pag. 1629, 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. subdisticus* (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 biohorizon 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.

Pag. 1629, 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.

Pag. 1629, line 11. the youngest *representative* of the genus

Pag. 1629, lines 15-17., B and Bc of *Sphenolithus akropodus*. The rare *sporadic* occurrence and poor preservation affect the recognition of this species, but B and Bc were identifiable (Fig. 2; Table 1). The Bc is ~~well related~~ *consistent* with the ~~first occurrence as~~ identified *datum reported*

Pag. 1629, 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 reproducibible event?

Pag. 1629, 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.

Pag. 1629, lines 23-27. See comment above.

Pag. 1630, lines 11-13. I totally agree with Paul Pearson. The Top of *Cribohantkenina* 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.

Pag. 1631, 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. To obtain this information you should have the total amount of carbonate produced by calcareous nannoplankton at that time. Please discuss.

Pag. 1631, line 14. and planktonic foraminifer ~~communities~~ *assemblages* (The term communities usually refers to living organisms.

Pag. 1631, 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.

Pag. 1631, line 20. (Fig. 3) => I would add the isotope curve…

Pag. 1631, 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.

Pag. 1632, line 8. Became ~~more~~ dominant

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.

Pag. 1635, lines 8-9. I would say that Cycligarlorithus mean cell size drives the high correspondence between in V:SA and PC1

Pag. 1635, 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.

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?

Pag. 1636, 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.

Pag. 1639, lines 8-13. 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? How can you explain that *Sphenolithus* and *Discoaster*, two warm oligostrophic 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. How can you explain that *D. stavensis* and *D. bisectus* show an opposite behavior 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. Are you sure that PC2 (by the way,PC2 could account just for the 14% of the variance of the entire assemblage) could be correlated with paleoproductivity so straightfully?

Pag. 1639, lines 19-20. …Or just because this correlation doesn't work. I do not mean that the final interpretation is incorrect but it shoudn't be based on so weak an argument. The authors have a stronger potential defense for their interpretation.

Pag. 1639, 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?

Pag. 1640, 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 paleoenviromental conditions, as the EOB.

Pag. 1640, lines 12-14. If I have understood correctly, the decrease in coccolith size is driven by decreasing CO_2 values, Am I wrong? Which is the driving forcing for this change? The CO_2 or the paleoproductivity? And, in case they are both responsible for this change, which is the factor commanding the decrease in coccolith size?

Pag. 1640, 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 overcompeted by phytodetritus species, in a different but still high productivity regime? Did I miss something?

Pag. 1640, 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.

Pag. 1641, lines 0-13. As I commented above, you need to synthesize all these data in more global perspective

Pag. 1643, lines 12-15. See comments above on the same issues.

Pag. 1644, line 13. I commented above on each of the main results reported in the conclusions. Some should be revised.