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Climate of the Past Discussions

Interactive comment on "Productivity response of calcareous nannoplankton in the South Atlantic to the Eocene Thermal Maximum 2 (ETM2)" *by* M. Dedert et al.

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We thank Ellen Thomas for her comments and time she has put into this manuscript.

We address each comment separately below.

ELLEN THOMAS: However, the authors do in my opinion in the present manuscript not really document that their two main conclusions as described in abstract and conclusions are justified by the data. These two main conclusions are: 1. an increase in Sr/Ca (in single species of nannoplankton and to some extent in bulk carbonate) suggests that there was 'slightly elevated' productivity during ETM2 at Site 1265 on Walvis Ridge, and 2. the paleoproductivity signal is dominantly governed by orbital forcing.

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Interactive Discussion

Discussion Paper

The authors themselves appear not to be fully convinced by their first conclusion (increased productivity during ETM2), in view of their statement on p. 2104 (25-27): 'The amplitude of Sr/Ca measured in the dominant genera Coccolithus and Toweius both prior and during the ETM2 suggests that productivity in response to ETM2 did not change significantly'. If the change is not significant, then one of the two main conclusion of the manuscript cannot be that productivity was slightly elevated. And if the increase in productivity during ETM2 was not significant, then it is very hard to make a convincing case that there is such as thing as a significant, long-term, orbitally controlled productivity signal – if the largest peak is not significant, then the smaller wiggles can not be either.

ANSWER: We have clarified that the productivity change as inferred from the ion probe data across ETM2 largely implies continued productivity, with the main conclusion being a sustained/ continued productivity. Ion probe Sr/Ca suggests that productivity was slightly higher during ETM2 in comparison with Sr/Ca below and above the event. However, the increased productivity during ETM2 as measured in the bulk carbonates is predominantly the result of pronounced dissolution during ETM2, and consequently the result of reduced diagenetic overgrowth. More significantly in the bulk record are the productivity changes during later cycles, where diagenetic conditions are more uniform, dissolution has been less pronounced and secondary overprinting is less variable.

ELLEN THOMAS: In addition, the authors argue on p. 2100 'Because the ion probe geochemical data appear minimally affected by diagenesis, we infer the overall pattern of productivity and ecological change from the Sr/Ca variations in the dominant genera of the sediments, Coccolithus and Toweius.', and thus think these taxon-specific data more reliable than the bulk data, because of more severe diagenesis in bulk data. Then why do they think that the long-term bulk data signal productivity rather than a dominantly diagenetic signal?

ANSWER: Based on dissolution intensity in the bulk Sr/Ca record, we distinguish trends in Sr/Ca on longer timescales, which have been strongly enhanced by reduced diagenetic overgrowth during dissolution (notably within the Elmo horizon), from the other cyclic increases in intervals of high and less variable CaCO3 intervals that likely represent an actual primary increase in Sr/Ca.

ELLEN THOMAS:'Because a large increase in Sr/Ca of Toweius slightly precedes the CIE, it is unclear if this response is part of the environmental changes accompanying the PETM'. Now in this record, it is Toweius (and Coccolithus) that show the increased Sr/Ca interpreted as productivity increase: why is it that Sr/Ca in Toweius during the PETM at 690 showed no increase, while Sr/Ca in other genera does and is interpreted as productivity increase? Why does that record show an increase in Sr/Ca during the PETM at 1265? What does that say about the ecology of that genus? I agree with Gibbs et al 2010 say '. . . there are no absolute calibrations for how much production change is represented by a given Sr/Ca change in Paleogene genera'.. In short – I do not really see that the case for 'increased productivity during ETM2 at Sitec1265' has been made convincingly, as also seems to be clear to the authors as shown by internally inconsistent sentences as noted above.

ANSWER: We agree that the clearest productivity signal inferred from Sr/Ca is that productivity was sustained. In our revisions we have now clarified that the main message of our research is that no productivity crisis in coccolithophores occurred during ETM2.

ELLEN THOMAS: The authors appear to suggest that increased primary productivity during ETM2 could have worked to take carbon out of the ocean-atmosphere system. But in order for that hypothesis to make sense, one must argue not just for increased primary productivity in large parts of the oceans, but for increased export productivity followed by storage of organic carbon in the lithosphere – increased productivity if followed by increased mineralization would not take carbon out of the system. The authors present no evidence (e.g., high TOC in sediment) for such a process to have been at work. And since the Sr/Ca records show no increase in productivity during the

PETM at Pacific Site 1209, there are no good arguments for globally increased, open-ocean productivity during the PETM (although increased productivity and storage of organic matter in shallow marginal basins may have functioned as negative feedback).

ANSWER: The biological pump provided by export production, in and of itself, effectively regulates atmospheric CO2 independent of burial (as argued recently by Sexton et al 2011 for more recent hyperthermals). Export production is closely regulated by the primary production of large biomineralizing eukaryotic phytoplankton (like diatoms and coccolithophorids today, and by coccolithophorids in the Paleogene) because small picoplankton is not effectively exported (see thorough discussion in Sweeney et al., 2003). Increased export production may lead to increased burial in the sediment record (ie removal from both atmosphere and ocean), but such burial is not always recorded in enhanced TOC content, particularly after a ∼53Ma time lapse (as is the case for the Walvis Ridge sites). A recent study by Diester-Haass et al. (2011) has shown strong evidence for higher organic carbon burial rates during the M/O transition, when benthic foraminiferal accumulation rates increase significantly, and carbon isotopes in carbonates suggest enhanced organic burial. Nonetheless, in these same core sediments there is no appreciable TOC increase across this interval today (presumably due to posterior remineralization in the sediment column).

ELLEN THOMAS:I do not necessarily agree that we can use ‘nannoplankton productivity’ as a proxy for ‘overall primary productivity’. It may be true that calcareous nannoplankton was the most important eukaryote primary producer, although there are also non-calcifying haptophytes, but in oligotrophic parts of the oceans productivity by prokaryotes has been estimated to contribute 30-80% of primary production.

ANSWER: As Barber and Hiscock (2006 “A rising tide lifts all phytoplankton:. . .”) point out, contrary to conventional wisdom, in modern bloom events all phytoplankton, diatom and non-diatom, increase in growth rates and abundance because all are stimulated by increased nutrients and light. Thus the general paradigm in the modern ocean, and likely applicable in the Paleogene as well, is that productivity of algal groups in a particular location is frequently correlated.

More importantly, coccolithophorid productivity is well-suited as a proxy for overall export production, because small prokaryotic plankton are not efficiently exported, whereas the larger eukaryotic plankton, especially biomineralizing phytoplankton whose mineral shells serve as ballast, are much more effectively exported (see review by Sweeney et al., 2003). We have clarified this in the introduction.

ELLEN THOMAS: The authors deal with rather complex information and they do not always clearly explain the steps in which they collect evidence and what that means for the interpretation of data. I think that I understand correctly that the authors use of a value Sr/Ca in abiogenic calcite as measured at one Pacific site in PETM sediments to deconvolve the relative amounts of biogenic and abiogenic calcite in ETM2 sediments on Walvis Ridge. I would like to see this more clearly described, and described in more detail, e.g. by showing a mxin ratio plot. I also wonder why the authors did not measure Sr/Ca in 'abiogenic calcite' from the sediments at Site 1265 deposited during ETM2? Not all 'abiogenic calcite' is the same, as clearly shown in the stable isotope data in Minoletti et al. And why would Sr/Ca in crystals formed at Site 1209 during the PETM reflect sea water values? Are we not looking at pore water values formed during a time of increased dissolution – precipitation?

ANSWER: We have clarified in a new figure (Fig. 8) how the relative amounts of abiogenic and biogenic calcite were calculated.

Regarding the characterization of the abiogenic end member, slowly precipitated abiogenic calcites all obtain the same equilibrium partitioning coefficient so it doesn't matter where in the ocean they are forming, because the seawater Sr/Ca ratio is uniform. The abiogenic blades from 1209 are the most readily isolatable material to analyze and are very representative of the nature of secondary overgrowth forming during the earliest Eocene. Both the changes in amounts of secondary overgrowth on nannofossils, and the appearance and disappearance of abiogenic needles at the PETM at 1209, occur

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over very short length scales in the sediments of the hyperthermals. This indicates that in both cases much of the overgrowth, and the precipitation of the abiogenic needles, must have occurred at or very near the sediment water interface. If overgrowths were forming deeper in the sediment column, diffusion would dramatically smooth the gradients in porewater chemistry and generate much more gradual changes in sediment character. So the abiogenic needles in 1209 are representative of the abiogenic overgrowths forming elsewhere in the ocean in the earliest Eocene.

ELLEN THOMAS: The authors do not show errors/intervals of uncertainty. They should do so in the Sr/Ca measurements, since they state that they used 15–âĂŘ20 specimens (2093 line 17). If the variability lies within the size of the marker in the plots, that should be stated explicitly.

ANSWER: The picked populations analysed by ion probe give an average value of Sr/Ca in the probed taxa. Given the time-consuming analytical procedure, no individual specimens have been probed for Sr/Ca.

We now clarify in the text that analyses of replicate populations of Eocene coccoliths from the same sample picked and analyzed several months apart on the IMS3f yield Sr/Ca ratios which differ by 1% to 8%. On the IMS1280, due to different nature of the sample-beam interaction, replicate populations yield Sr/Ca which differs by +/-9%. The scale of this variation is now indicated in the figures.

ELLEN THOMAS: The author should also show uncertainty within their estimates of percentage of abiogenic calcite: All steps in the process must induce uncertainty in the finally obtained values.

ANSWER: The error for the abiogenic carbonate as inferred from SEM images falls within the error in calculations by Young and Ziveri, 2000. This is now stated in the text in lines 326-327.

ELLEN THOMAS: In general, I found the long section on diagenesis, biogenic calcite and dissolution (pages 2098–âĂŘ2102) confused and in places repetitive. In my opinion this whole section should be carefully rewritten, so that the line of evidence is easier to follow for the reader. (see notes by page). It is quite possible that I just do not understand the arguments and that they are valid, but they should be more clearly described.

ANSWER: We have revised parts of the sections dealing with the effects of diagenetic processes. In particular we have clarified that avoiding analyses of diagenetic calcite is vital for Sr/Ca productivity reconstructions, the implications of overgrowth patterns found during ETM2 interval with respect to the bulk Sr/Ca record, and which intervals in the nannofossil record can be interpreted as representative of actual productivity response

ELLEN THOMAS: Of less importance, it seems to me that the manuscript appears to be written too much as if intended for a journal dedicated to specialists in hyperthermal events –it does not explain clearly and concisely what they are to a more general audience. A short description of what a hyperthermal is should be added to the abstract (high temperature, negative carbon isotope excursion, dissolution), and the description should be improved in the introduction.

ANSWER:We have added a short description of hyperthermals including the relevance for the present atmospheric CO2 increase. In the introduction a more detailed description of hyperthermals has been added.

ELLEN THOMAS: Line 5: my usual gripe: Sr/Ca IS a ratio, thus it makes no sense to say ‘Sr/Ca ratio’. Sr/Ca value, or Sr–âĂŘCa ratio, or just Sr/Ca would all do. Mention here how Sr/Ca was measured (ion probe).

ANSWER: Sr/Ca ‘ratio’ has been changed to Sr/Ca. We indicated now if the measurements were done by either ICP or ion probe.

ELLEN THOMAS: Line 7: measuring what in ‘elected nannofossil populations’? Sr/Ca?

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ANSWER: The selected populations were indeed analysed for Sr/Ca.

ELLEN THOMAS: Lines 11–13: I would like to read first what the variability is during back ground fluctuations: how much, as compared to the 13–âĂŘ21% during the ‘event’? and the 13–âĂŘ 20% is in single species, right, as compared to bulk for the long–âĂŘterm record? Is it not possible that the bulk record reflects dissolution/lysocline movement rather than productivity? I make more notes on this later –maybe I just misunderstand later arguments.

ANSWER: This background variation refers to ion probe data variability which gives the best representation of Sr/Ca variation, compared to the bulk and size fraction data that is biased by diagenesis. Therefore, the actual background forcing on Sr/Ca is best defined by the ion probe data.

ELLEN THOMAS: Line 25 and into 2091: 2090–âĂŘ2091: A better definition of a hyperthermal event should be written. Here there should be a succinct definition of a hyperthermal, which word means an unusually warm interval during an overall warm period of Earth History.

ANSWER: A clearer description of hyperthermals has been added.

ELLEN THOMAS: Line 2: temperature increase is not an example of a geochemical or biotic characteristic (it is a physical property).

ANSWER: This section has been revised.

ELLEN THOMAS: Line 6–âĂŘ7: the authors should define where on what that CIE was measured: bulk carbonate? Benthic foraminifera? Planktic foraminifera? Organic carbon? Walvis Ridge? What depth? Shoaling of lysocline or CCD? Where, and from what to what depth? It is not clear to the reader that you are referring to data from the same sites, and for ETM2 there is not by far such as global database as for the PETM. It seems to me from fig. 2 that the CCD never reached Site 1265 since $CaCO_3$ does not fall below 50%.

ANSWER: That the isotope data referred to here was obtained from bulk carbonates from the Walvis Ridge has been added to the text. CCD has been changed into lysocline.

ELLEN THOMAS: Lines 9–âĂŘ11: It is not increase phytoplankton productivity by itself that can act as a feedback to high pCO2: that occurs ONLY if the produced organic matter is also taken out of long–âĂŘterm contact with the atmosphere (e.g. by deposition in the deep ocean). And note that calcification in surface waters puts one mole of CO2 in the atmosphere For each mole of CaCO3 formed.

ANSWER: We added the significance of export production for CO2 drawdown. As shown in a series of recent papers, the net effect of calcifying plankton is still as a sink of C in the biological pump, because coccoliths are the most effective biomineral for ballasting (e.g. Klaas and Archer, 2002; Francois et al., 2002; Ziveri et al„ 2007; Balch et al., 2010).

ELLEN THOMAS: Line 17: Sexton et al. 2011 looked at fairly small hyperthermal events, which in this paper might have been included in background variability rather than seen as hyperthermals – hence the need for a definition. Also, the authors appear to argue for release of isotopically light carbon into the ocean–âĂŘatmosphere to cause hyperthermals in disagreement with Sexton et al.

ANSWER: Sexton et al. describe events separated by 100-400 ky in time. This is much longer than the timespan between the pre-ETM2 peak in the ion probe data and the ETM2 itself. The total estimated duration of the section analyzed for bulk carbonate is 200 ky, implying that some but not all of the local maxima in d13C could be of the timescale of the hyperthermals described by Sexton et al. However, not all of them are accompanied by d13C minima or warming – so in the bulk carbonate there is variability, which is of higher frequency than the hyperthermals described by Sexton et al (2011), but which may nonetheless be linked to orbital cycles.

This is clarified briefly in the text in section 1.

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ELLEN THOMAS: Lines 1-2: need at least one reference

ANSWER: We now refer to Gibbs et al., 2006 and Rigdwell & Schmidt, 2010

ELLEN THOMAS: Line 5-6: the data n Stoll and Bains 2003 are no longer supported by the Ba accumulation rates' maybe cite Stoll et al. 2007 if on wants to argue for this disagreement – but see also Gibbs et al. 2010.

ANSWER: This reference has been eliminated since it is superceded by subsequent studies with more precise methodology (e.g. ion probe analyses) at 690.

ELLEN THOMAS: Lines 6-9: how is it possible to use 'well preserved intervals' across ETM2 which is characterized by strong carbonate dissolution?

ANSWER: The well-preserved interval refers to an interval prior to the Elmo horizon of higher carbonate content. This has been clarified.

ELLEN THOMAS: Line 17: NOT CCD, since CaCO3 did not go to zero.

ANSWER: This has been corrected into lysocline.

ELLEN THOMAS: Lines 15-20: A discussion of the CIE and release of various carbon compounds should have been included in the introduction. In order to discuss amounts of released carbon one must include the magnitude of the CIE. Are the authors talking about the CIE during ETM2? Or during the PETM? Ridgewell 2007 is not a good reference here; see e.g. Pagani et al. 2006, Science. The discussion of amount of dissolution and such should also refer to Stap et al 2009, not only to Lourens et al 2005.

ANSWER: We now cite Zachos et al., 2010 We have included Stap et al., 2009 as a reference.

ELLEN THOMAS: Line 27: further one the authors discuss that bulk Sr/Ca can not simply be seen as reflecting productivity changes.

ANSWER: We altered this sentence, clarifying that we analysed bulk Sr/Ca (<20 micron) sediments in order to try to obtain a long term context in which to interpret the detailed ion probe Sr/Ca productivity record at the ETM2.

ELLEN THOMAS: Lines 3-4: explain exactly how this is done, or delete here and expand on information on page 2095, lines 4-6. Information is needed on why the value of 0.13 can be seen as representative for all 'abiogenic carbonate'.

ANSWER: We explained in more detail the calculations and added a figure for clarification. For the value of abiogenic calcite, the formation of these abiogenic crystals is restricted at the water-sediment interface as described earlier; the 1209 crystals formed basically at the seawater interface or very near it because they are confined to a very short interval and reflect a fast response to change in ocean saturation state. Diffusion into pore waters blurs abrupt changes and generates much broader changes in sediment chemistry, so if they were formed deeper in the sediment column there would not be such a sharp boundary in their abundance. Slowly precipitated abiogenic calcites all obtain the same equilibrium partitioning coefficient so it doesn't matter where in the ocean they are forming, because the seawater Sr/Ca ratio is uniform. These abiogenic needles are thus representative of the composition of diagenetic overgrowth in the earliest Eocene.

ELLEN THOMAS: Lines 24-26: please describe clearly that 'bad preservation' can be both overgrowth (CaCO3 deposition) as well as dissolution. Why is it that some species of placolith are not overgrown while others (presumably in the same sample) are? Are placolith species that are not significantly overgrown so in all samples, independent of CaCO3 percentage?

ANSWER: Revised and now reads; "SEM analyses revealed significant contrasts in nannofossil preservation in intervals of high CaCO3 compared to intervals with strong dissolution and low CaCO3. In sediments of high CaCO3, severe overgrowths cover the nannoliths Discoaster and Tribrachiatus, and the holococcolith Zygrhablithus under high CaCO3, whereas the placoliths of Coccolithus, Chiasmolithus and Toweius are not significantly overgrown, and generally not strongly etched or fragmented (PLATE nannofossils). In the sediments of low CaCO3 of the Elmo horizon, the degree of overgrowth on Discoaster and Tribrachiatus, and Zygrhablithus decreases. With decreasing carbonate content, the liths of placolith taxa are increasingly etched and fragmented".

ELLEN THOMAS: In general, I think that section 3.2 should be rewritten with less statements about increases/decreases in parts of the section: in some sentences we are talking about so few data points that such a discussion is not valid. The authors should make sure that all plots show the dark grey bar (maximum dissolution, Elmo) as well as the light grey bars above and below (extent of CIE). There is some confusion between the use of these two intervals (Elmo and CIE) in lines 17-19 as compared to the figures. E. g., the text says 'After the CIE, the Sr/Ca ratios in Coccolithus remain stable until the C- isotope signal has returned to pre- ETM2 values.', but by definition the C-isotope signal returns to pre- ETM2 values exactly at the end of the CIE – that is how it is defined. The 'minimum at 277.65 mcd' is defined on 1 data point. The text also says 'The Sr/Ca measured in individual specimens of Discoaster is higher in the two specimens present in the Elmo horizon, compared to the Sr/Ca measured in individual specimen below and above the ETM2 (Fig. 2).', but I see only 1 data point above, none below.

ANSWER: Figures have been improved and symbols for probed Discoaster values made larger, so that all four datapoints are better visible. Section 3.2 is corrected and rewritten:

"At Site 1265, the Sr/Ca in picked populations of both Coccolithus and Toweius displays an initial decrease prior to the ETM2 interval (Fig. 3a), which is followed by a return to former Sr/Ca in Coccolithus a few centimeters below the onset of the CIE (Fig. 3b; as measured in the fine bulk fraction (<20 μm)). The Sr/Ca in Toweius increases at the onset of the CIE (Fig. 3a). Within the Elmo horizon, the Sr/Ca in Coccolithus and Toweius yield higher values by $\sim$13 and 21%, respectively, compared to maximum

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values before and after the event. During the recovery period, the Sr/Ca in Coccolithus remains stable until the C- isotope signal has returned to pre- ETM2 values. The Sr/Ca in Toweius drops to a second minimum comparable to the low Sr/Ca in Toweius preceding the event, followed by a return to the initial pre-excursion values during the recovery period (Fig. 3a). Chiasmolithus does not show a significant response in the ETM2 interval, except for a small increase prior to the Elmo horizon that coincides with the increase in Coccolithus and Toweius. High Sr/Ca in Coccolithus, Toweius and Chiasmolithus corresponds with light δ13C-values measured below and in the Elmo horizon (Fig. 3a). The Sr/Ca values measured by ion probe show no correlation with changes in carbonate content across the ETM2 interval (Fig. 3c)."

ELLEN THOMAS: Lines 4-10: what is the size fraction with Discoaster ? same as for Zyghrablitus? sentence should be rephrased since it is not clear to me.

ANSWER: Using several sediment separation techniques, we were able to obtain discoaster enriched samples. This resulted in a fraction to which Discoaster dominantly contributes to the carbonate. We added three representative SEM images of an original nannofossil assemblage, a separated discoaster fraction and a 5-8 μm size fraction to the PLATE # 1 as to visually make the obtained results more clear.

ELLEN THOMAS: Section 3.3: the authors should explain here how they calculated the percentage biogenic/abiogenic calcite. I guesss that it was by using Sr/Ca in both types of calcite; the authors should show a plot with end-members and mixing values. It is very confusing that later on in the manuscript (2099) the authors talk much more on primary versus diagenetic signal, but there use a different line of evidence, i. e., SEM evidence, which I understand, will be submitted in a separate paper. I think that the discussion on diagenetic versus primary carbonate, as based on Sr/Ca, and SEM analysis, is not well organized and hard to follow. If that discussion is in essence presented elsewhere, the authors should severely cut the discussion in this manuscript, and present all the evidence on biogenic versus abiogenic calcite in one section, not spread out over sections 3.3, 3.4 and 4.1. This section is very confusingly written, and there is no clear indication of the exact amount of disagreement between the SEM and Sr/Ca methods (see below, p. 2099).

ANSWER: We have revised these sections.

ELLEN THOMAS: Lines 1-10: in my opinion it should be tested statistically whether there is a significant orbital periodicity in the record; the figure is to me not very convincing, and I do nto really see that 'clearest cyclic-driven increase in Sr/Ca'

ANSWER: We now clarify that there is some cyclicity to the variations in bulk Sr/Ca which may be related to some orbital cycles (spectral analysis revealed power at precession and obliquity, but not statistically relevant), but in absence of a precise age model the link to orbital forcing cannot be rigorously evaluated and is therefore included simply as a possibility.

ELLEN THOMAS: 3.5, nannofossil abundance: why not show relative abundances of all taxa analyzed for Sr/Ca, i.e., Chiasmolithus abundance in figure 5?

ANSWER: We have added Chiasmolithus abundances to the figure.

ELLEN THOMAS: Section 4.1.1 Lines 8-10: have SEM studies indeed been done on all samples analyzed for Sr/Ca? if so, please say so.

ANSWER: In the text we now note that SEM analyses were done on all analysed intervals.

ELLEN THOMAS: Lines 12-14: what is meant by 'dissolution, which is common to all sediment components': I thought that different species had different sensitivity to dissolution?

ANSWER: In this section we discuss the geochemical evidence for the robustness of the ion probe data. One argument for measurements being done on primary calcite is that across the ETM2, with its significant changes in carbonate preservation, and consequently potential differences in degree of overgrowths on the analzed liths, the

Sr/Ca in the taxa shows no correlation with CaCO3 content. This would have been the case if ion probe Sr/Ca analysis would have included abiogenic calcite.

ELLEN THOMAS: Line 18: ‘as a result of reduced presence of overgrowth’: do you mean to say that more dissolution results in specimens which are better for analysis because dissolution does not distort the signal as much as overgrowth? But if abiogenic calcite has lower Sr/Ca than biogenic calcite, does that not mean that there is differential solution, with Sr/Ca depleted in the non-dissolved fraction because of ‘loss of Sr to pore waters during diagenetic recyrstallization’? or is the Sr remaining in the pore water during the recrystallization? Please explain, because I do not exactly get the meaning.

ANSWER: We now clarify that overgrowth results from bottom waters and shallow pore waters which are oversaturated with respect to CaCO3. Dissolution below the lysocline is a symptom of bottom waters which are much less saturated (in some cases undersaturated) with respect to CaCO3 and therefore much less prone to overgrowth on nannofossils. For this reason, concerning the geochemistry of the coccoliths, dissolution intervals have better preservation of primary calcite signal due to the reduced importance of secondary calcite. Note that coccoliths, unlike foraminifera, do not feature strong heterogeneity in composition and are not prone to changes in chemistry during partial dissolution (in fact coccoliths because of their thin shell, when “partially dissolved” generally fragment rather than thinning and losing mass at the same (intact) morphology.

ELLEN THOMAS: line 4: I do not think placoliths or nannoliths are commonly called ‘shell’.

ANSWER: Shell has been corrected into lith.

ELLEN THOMAS: Line 15: is it possible that some of the non-nanno calcite could be other things such as foram fragments ? see Minoletti et al.

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ANSWER: Although the fractions do contain foram fragments, the actual contribution of their calcite to the total carbonate is minor. Most of the non-nanno calcite is contributed by ('micarb') abiogenic calcite. In Paleogene sediments foraminifera are much less abundant than in Neogene sediments and rarely exceed 5% of the $CaCO_3$.

ELLEN THOMAS: Lines 20-25: I do not follow this sentence – do you mean that the % abiogenic versus biogenic estimates of SEM studies do not agree between the Sr/Ca method and the SEM method? If so, this should be explicitly mentioned, and the extent of discrepancy shown in figure 3c. and how do d18O data compare? How large are all these uncertainties in estimates? Is there a signal left or not? How does this influence the estimate of the diagenetic versus productivity signal in the bulk record?

ANSWER: We have reorganized the section. This section deals with quantifying the abiogenic calcite contribution by the overgrown taxa based on SEM and single specimen Sr/Ca analyses using the ion probe, and is further supplemented by the ion probe calculations to show the extent to which overgrown taxa contribute to the abiogenic carbonate, and what portion is contributed by other sources.

ELLEN THOMAS: lines 5-7: please explain the d18O data: you mean that there has been overgrowth formed in colder water, after the coccoliths arrived on the bottom of the ocean? Then please say so.

ANSWER: This indeed refers to overgrowth formation in colder deep-waters, as we now clarify in this section.

ELLEN THOMAS: Section 4.1.2: the discussion of background variability versus signal is not clear. Spell out the exact range of variability deemed to be background (average –minimum-maximum).

ANSWER: The background variability as reconstructed from ion probe data for Coccolithus and Toweius is ∼20 to 30%, and has been added to the text.

ELLEN THOMAS: line 5: 'less salient' in my opinion is an understatement – I just do not think there is anything significant in the Chiasmolithus record. If the authors think there is and that one datum point is significant, they should include a statistical analysis to document this significance.

ANSWER: We agree that this datapoint is not very significant, particularly in view of the datapoint preceding this small increase. However, since such a response in Chiasmolithus has also been recorded at other sites (PETM Sites 690 and 1263), we regard it as noteworthy.

ELLEN THOMAS: Line 10: since picked Zygrhablites was not analyzed for Sr/Ca, this information is irrelevant.

ANSWER: We have removed this information.

ELLEN THOMAS: Lines 23-24: these two discoaster data points appear to me not to be very significant, since there is only one background data point.

ANSWER: The plot has been revised with Discoaster datapoints better visible.

ELLEN THOMAS: Lines 22–âĂŘ28; 2104, Lines 1–âĂŘ6): but note that for some of the sites dissolution during the PETM was not very severe, e.g. 1209. And Sr/Ca data for Site 1209 do not show an increased productivity either.

ANSWER: We have now included a Sr/Ca record from Site 1209 covering the ETM2 interval, which shows a similar response as to the PETM. The fundamental difference between Site 1209 and other sites for which Sr/Ca productivity records were obtained (and indicating sustained productivity), is that conditions relating to nutrient supply are profoundly different. Apart from the Sr/Ca productivity record, we show that high CaCO3 does not necessarily imply a good preservation (increased abiogenic calcite contribution to Discoaster fraction prior to Elmo horizon). Moreover, the bulk Sr/Ca obtained for Site 1209 suggests that too at this site, a certain degree of abiogenic calcite contributes to the bulk. This is also demonstrated by SEM analyses to be presented in another paper.

ELLEN THOMAS: Lines 17–âĂŘ20: why is a warming signal in oxygen isotopes supposed to indicate upwelling? Is upwelling water not usually cold, so that cooling indicates upwelling?

ANSWER: Upwelling at both sites (1265 and 690) makes a plausible candidate for nutrient supply to surface waters. At site 1265, the temperature gradient between depth waters and surface waters, as inferred from benthic and planktic foraminifera isotope data ∼2-4ËŽC (Stap et al., 2010), which is relatively small. However, as they point out, upwelling at this site could have subdued the actual warming trend.

ELLEN THOMAS: Lines 20–âĂŘ24: why does it imply that there was high productivity during ETM2 when There was upwelling before the event?

ANSWER: The increased upwelling within the CIE (prior to the Elmo horizon) coincides with an orbital forcing identified by Stap et al. (2009). The intensity of upwelling may have varied with orbital forcing, as is well-known from modern upwelling systems, e.g. in the Arabian Sea (e.g. Clement et al. & Prell et al.).

ELLEN THOMAS:Lines 25–âĂŘ27: so why is the productivity change now not significant? Is that not in direct contradiction to line 23?

ANSWER: We have revised the text so that it is coherent with the discussion.

ELLEN THOMAS:Lines 1–âĂŘ2: I do not understand the reasoning here.

ANSWER: Revised

ELLEN THOMAS: Lines 6–âĂŘ10: if the authors want to argue for a change in wind intensity, then should they not argue that it may be possible that the zone of highest wind intensity shifted latitude, thus moved from away from Site 1265 to over Site 1265? In view of observations over the recent oceans (Sarmiento), that seems more probable than overall more or less wind intensity. Lines 8–âĂŘ15: I personally do not think that wind patterns and upwelling patterns for the Quaternary are relevant for the Paleogene. Probably, Drake Passage and the Tasman gateway were both closed to deep circulation, making the overall wind and current pattern very different (various papers by Sarmiento, Sijp), and changing such things as the Agulhas current.

ANSWER: We now cite Sloan and Huber 2001, in which the intensity of upwelling patterns during the Early Eocene is discussed. Upwelling patterns would have remained largely stable in response to precessional forcing, whereas continental run-off slightly increased for the South-Atlantic. Both mechanisms are proposed in the text as a possible mechanism for productivity variability across ETM2.

ELLEN THOMAS: Lines 20–âĂŘ22: is there evidence in the CaCO3 record for carbonate 'overcompensation' after ETM2?

ANSWER: Not an obvious one. We removed this sentence.

ELLEN THOMAS: 2105–âĂŘ28 through 2106–âĂŘ1–âĂŘ4: this is over interpretation, in my opinion. I do not think the evidence for productivity changes is that convincing, and now it is asserted that these changes were nutrient–âĂŘstimulated? In my opinion the long discussion on what caused the higher productivity is much too long and convoluted: the authors have no Evidence for increased upwelling nor for increased weathering rates (and it is not clear That such increased weathering would work on the proposed timescales for the 'lesser Magnitude events', so they can just state that either mechanism might have worked.

ANSWER: We have revised the sections of possible mechanisms.

ELLEN THOMAS: Line 7–âĂŘ10: for the PETM, constant or possibly increased varied by locality (Stoll et al. 2007). 10–âĂŘ12: in my opinion the uathors should here clearly distinguish between organic productivity and carbonate productivity – they do not have to go together (e,g., Doney et al./'s evaluation of ocean acidification processes).

ANSWER: A summary of potential effects has been added.

ELLEN THOMAS: Line 23: the nannofossil signal is not really a 'climate' signal, more climate and productivity or something like that.

ANSWER: This section has been revised.

ELLEN THOMAS: Lines 27–âĂŘ28: as stated above, I do not agree at al that 'coccolithophore productivity likely represents the overall marine primary productivity during the Paleogene'; there must have been prokaryotes (and the dinoflagellate people would not agree either).

ANSWER: As detailed in our response to earlier comments, and now clarified in the text, export production and productivity by coccolithophorids, a main primary producer, would likely have been tightly coupled.

ELLEN THOMAS:lines 1-5: foraminiferal calcite would also have contributed. How do we know coccoliths were the main ballasting and not forams? As Schiebel (2002) says for the recent oceans: 'The total planktic foraminiferal contribution of $CaCO_3$ to global surface sediments amounts to 0.36–0.88 Gt yr^{-1}, âĹij32–80% of the total deep-marine calcite budget. (doi:10.1029/2001GB001459).

ANSWER: In late Holocene deep-sea sediments, coccoliths contribute 40 to 60% of the $CaCO_3$ in tropical sediments, although this percentage is higher in more northern latitude sediments and sediments adjacent to continental margins (Broecker and Clark, 2009). In the modern ocean foraminifera play a negligible role in ballasting because they are in a completely different size spectrum and sink more or less independently from the organic aggregates. This was one of the most significant conclusions of the 2005 Chapman Conference on the Role of Marine Organic Carbon and Calcite Fluxes in Driving Global Climate Change, Past and Future(http://www.whoi.edu/page.do?pid =11798): ("The Foraminifera have their own flux story since they have an unusual means of reproduction. Adult Forams subdivide their protoplasm into tens of thousands of gametes that swim off, abandoning the shell which settles rapidly to the ocean floor. Thus, we have carbonate 'bombs' delivered to the depths containing relatively little organic carbon. The gametes develop into little foraminifera, most of which die before maturity with their shells sinking slowly as debris, or being incorporated in some way with other fine material. Much of this gets dissolved in the upper 700m of the water column. Large foraminiferal shells could dominate the calcite flux to the deep sea but might have nothing to do with the organic carbon flux!")

Furthermore, in the Paleogene forams contribute a much, much smaller fraction of deep ocean carbonate (typically 5%) than they do in the modern ocean, for evolutionary reasons which are not yet resolved.

Interactive comment on Clim. Past Discuss., 7, 2089, 2011.

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Interactive Comment