I labeled the 'scientific significance' as 'good', but I think the manuscript has the potential to become excellent, with rewriting. I labeled the Scientific Quality and the Presentation Quality as 'Fair', but these also can become much better with rewriting.
In my opinion this manuscript contains new and very interesting information and should be accepted for publication, but it needs serious rewriting before it can be accepted. The authors present nannoplankton Sr/Ca and assemblage information for a hyperthermal event (ETM2) for which much additional information is available, and an evaluation of the trace element and nannofloral assemblage data can add significantly to our understanding of hyperthermals. 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. 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. 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? On p. 2102 (lines 17-26) the authors appear to mak apossible argument for such a case during ETM2. In my opinion figure 4a is not very convincing, because there is no analysis of the significance of any signal at orbital frequencies (record too short?). And the authors say (2102) 'Overall, long-term Sr/Ca productivity trends measured in the bulk fine sediments do not show a clear correlation with the precession cycles in the proximity of the ETM2.' And the authors also say that 'Bulk fine Sr/Ca shows no direct correlation with the carbonate dissolution as calculated by Stap et al. (2009) (Fig. 4a).'. How is this possible if both Sr/Ca and dissolution are supposedly occurring at orbital frequencies? Should we not see both high dissolution and high Sr/Ca during hyperthermals, i.e. a negative correlation between CaCO3 (dissolved) and Sr/Ca? This is what the authors argue for in lines 2102-26through 2107-2? Such a correlation does not appear to me to be there in figure 4a; if the authors think there is such a correlation they should show this statistically.

I personally have problems (despite the cited publication on lab and modern ocean observations) in accepting Sr/Ca values in a few species of calcareous nannoplankton as a proven indicator of overall primary or export productivity, and I do not think this topic is addressed sufficiently in this manuscript. Stoll & Bains 2003 seemed promising, at least in part because there appeared to be a correlation between Sr/Ca and Ba accumulation rate during the PETM at Site 690. But Torfstein et al Clim. Past, 6, 265–272, 2011 http://www.clim-past.net/6/265/2010/ show that there is no such increase in Ba accumulation rate if one applies a more recent age model to Site 690, so there is no independent support for productivity increase during the PETM at Site 690. The Sr/Ca record for 690 tuned out to be more complex in Stoll et al. 2007 (EPSL) with improved methodology, i.e. single species analysis rather than size-fraction analysis. Sr/Ca in *Toweius* and *D multiradiatus* show little effect across the PETM, *Chiasmolithus* shows a short extreme spike, *Fasciculithus* shows a longer spike offset from that of *Chiasmolithus*. What does that mean for overall productivity? Stoll et al say 'Because in this site no species exhibit decreased Sr/Ca indicative of lower productivity, while several exhibit increased Sr/Ca indicative of higher productivity we suggest that this site experienced a net increase in coccolithophore productivity during the PETM', but to me the record looks as if we really do not know about overall productivity averaged over all species, as also indicated by the next sentence of the authors '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 doesand 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.

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).

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.

Gibbs et al 2010 EPSL combine *Toweius* counts with taxon-specific Sr/Ca data, and say 'Reconstructed nannoplankton production at Ocean Drilling Program (ODP) Sites 690 (Southern Ocean), 1209 (Pacific Ocean) and Bass River (New Jersey) did not appear to vary significantly across the PETM indicating that on geological timescales there is no evidence for interruption of phytoplankton carbonate production, despite the major assemblage shifts associated with this interval', which thus appears to be in disagreement with Stoll et al 2007. It seems possible for the authors of this paper to do the same combination of *Toweius* relative abundance and *Toweius* Sr/Ca as a measure of the offset between original and preserved abundance of carbonate as Gibbs et al 2010, then compare that to estimates of dissolved carbonate in Stap et al?

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?

The authors do not show errors/intervals of uncertainty. They should do so in the Sr/Ca measurements, since they state that they used 15-20 specimens (2093 line 17). If the variability lies within the size of the marker in the plots, that should be stated explicitly. 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.

In general, I found the long section on diagenesis, biogenic calcite and dissolution (pages 2098-2102) 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.

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.

Remarks by page/line:

2090,
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-Ca ratio, or just Sr/Ca would all do. Mention here how Sr/Ca was measured (ion probe).

Line 7: measuring what in 'elected nannofossil populations'? Sr/Ca?
Lines 11-13: I would like to read first what the variability is during back ground fluctuations: how much, as compared to the 13-21% during the 'event'? and the 13-20% is in single species, right, as compared to bulk for the long-term 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.

2090- Line 25 and into 2091: 2090-2091: 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. The idea that high temperatures occurred is derived directly from proxies (d18O or Mg/Ca or Tex86). The idea that there was a disturbance of the carbon cycle and high atmospheric pCO2 comes from the observation that there was a negative carbon isotope excursion through the ocean atmosphere system, and dissolution in deep-sea carbonates. The existing text introduces the definition of a hyperthermal over several sentences which makes the text hard to follow for a non-specialist – e.g., it is not at all clear to a non-specialist reader why you want to have feedback for high pCO2. For instance, during a hyperthermal, 'pronounced increases in pCO2' did not just 'take place', but the general ideas is that the high pCO2 CAUSED the hyperthermal. We THINK that there was increased pCO2 during hyperthermals, but we have no direct proxy data on pCO2 during most hyperthermals – we deduce it from warming as combined with the d13C-negative excursion and dissolution. Maybe you should cite one of the papers discussing hyperthermals in more general, such as Cramer et al 2003, Paleoceanography, or Zachos et al 2010 EPSL (or Hilgens et al 2010 EPSL).

2091:
Line 2: temperature increase is not an example of a geochemical or biotic characteristic (it is a physical property).
Line 6-7: 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 CaCO3 does not fall below 50%.
Lines 9-11: 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-term contact with the atmosphere (e.g. by deposition in the deep ocean). And note that calcification in surface waters puts one mole of CO_2 in the atmosphere for each mole of $CaCO_3$ formed.
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-atmosphere to cause hyperthermals – in disagreement with Sexton et al.

2092
Lines 1-2: need at least one reference.
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.
Lines 6-9: how is it possible to use 'well preserved intervals' across ETM2 which is characterized by strong carbonate dissolution?
Line 17: NOT CCD, since $CaCO_3$ did not go to zero.
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.
Line 27: further one the authors discuss that bulk Sr/Ca can not simply be seen as reflecting productivity changes.

2093:
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'.
24-26: please describe clearly that 'bad preservation' can be both overgrowth ($CaCO_3$ 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 $CaCO_3$ percentage?

2095:
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.

2096, 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.
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).

2097:
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'.

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

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.
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?
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.

2099, line 4: I do not think placoliths or nannoliths are commonly called 'shell'.
Line 15: is it possible that some of the non-nanno calcite could be other things such as foram fragments ? see Minoletti et al.
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?

2100:
lones 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.

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).

2101:
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.
Line 10: since picked Zygrhablites was not analyzed for Sr/Ca, this information is irrelevant.
Lines 23-24: these two discoaster data points appear to me not to be very significant, since there is only one background data point.

2103, lines 22-28; 2104, lines 1-6): 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.
Lines 17-20: why is a warming signal in oxygen isotopes supposed to indicate upwelling? Is upwelling water not usually cold, so that cooling indicates upwelling?
Lines 20-24: why does it imply that there was high productivity during ETM2 when there was upwelling before the event?
Lines 25-27: so why is the productivity change now not significant? Is that not in direct contradiction to line 23?

2105:
Lines 1-2: I do not understand the reasoning here.
Lines 6-10: 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 8-15: 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.
20-22: is there evidence in the CaCO3 record for carbonate 'overcompensation' after ETM2?

2105-28 through 2106-1-4: this is over interpretation, in my opinion. I do not think the evidence for productivity changes is that convincing, and now it is asserted tha these changes were nutrient-stimulated? 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 time scales for the 'lesser magnitude events', so they can just state that either mechanism might have worked.

2106, line 7-10: for the PETM, constant or possibly increased varied by locality (Stoll et al. 2007).
10-12: 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).
Line 23: the nannofossil signal is not really a 'climate' signal, more climate and productivity or something like that.
Lines 27-28: 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).

2107
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}, ~32–80% of the total deep-marine calcite budget. (doi:10.1029/2001GB001459).
Also – if the authors want to discuss the model of Sexton et al. 2011, they should discuss his model against the causation of hyperthermals by other CO2 sources.'