

Ferrara, February 16, 2016

Dear Climate of the Past Editors,

Perhaps the biggest headache in producing this paper lies in detailed stratigraphy and the proper correlation of multiple records at multiple sites across both depth and time. Through 2015, detailed stratigraphy for the middle to late early Eocene has remained a gigantic mess. Most of the comments pertaining to the initial version as well as to the revised version, whether directly stated or not, ultimately arise from this issue. We think that many of us are now fully realizing the true extent of the mess, from different time scales and definitions, to incorrectly aligned cores and paleomagnetic reversals, to a general appreciation of a dynamic early Eocene world where high-resolution sampling is absolutely necessary because Earth surface processes were changing rapidly in the time domain.

We are the first to state that this paper is not ideal in terms of planning and final structure, as about half the effort (or more) was spent cleaning up stratigraphic mess. Here, it should be emphasized, though, that this does not reflect past work by colleagues; the issues only become obvious when confronted with multiple good records at multiple sites, much generated by others.

All the above stated, we are now very proud with current manuscript. We think that we have solved numerous stratigraphic issues, presented a cogent understanding of carbon isotope records and planktic foraminiferal evolution within current knowledge, and offered avenues for future research. We also think that we have satisfactorily addressed all referee comments, but also expressly thank them, as they have made the end product much better.

Following below are responses to the very thorough commentary by Robert Speijer and Paul Pearson.

Sincerely,

Valeria Luciani on behalf of co-authors

Notes

- Marked-up file: in blue are the corrections suggested by Paul Pearson, in orange the corrections suggested by Robert Speijer, in grey our minor adds/corrections.

Figures:

- Even though not requested by referees we have deleted titles on figure 1, since there is explanation in the figure caption. In this figure we have also corrected the E6/E7a Zones (previously - erroneously - combined).
- We have added a line between E and Zonation in figures 4 to 7, 10 and S1 to be coherent with "NP-Zonation" and text.

Review of 'Major perturbations in the global carbon cycle and photosymbiont-bearing planktic foraminifera during the early Eocene' by Luciani et al. (2nd version) [Robert Speijer]

First, I must congratulate the authors with their great effort to improve the quality of the paper. In my view, the shift in focus, the structure, the extensive considerations of concerns expressed earlier and the addition of data from other localities have strongly enhanced the value of this paper.

We appreciate this comment, as we indeed put considerable new effort into the manuscript, including new data.

The planktic foraminifera of Possagno are now described as being recrystallized and infilled with calcite – a feature that is very common in foraminifera from many early Paleogene (and older) outcrops. Although this is unfortunate as it will not allow geochemical analysis of the foraminifera, the good news is that the infillings make the planktic foraminifera much more robust to various ways of processing so that for instance %F will not be significantly affected through this and may thus only reflect settling and/or early diagenetic processes. With this new information one concern is tackled.

The authors now provide a generally well-balanced discussion on diagenesis and dissolution influencing the faunal and isotopic patterns. However, I'm still missing consideration of a few issues:

A short discussion to the (potential) effects on the isotopic records of meteoric water penetrating (as groundwater) the rocks in the subsurface. This also may or may not have played a role in the red coloration of the Scaglia Rossa (refs?).

We added a short paragraph.

In connection with the previous: as mentioned in the first review, I'd like to see the author's viewpoint to the often observed positive correlation between $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ records in outcrop sequences like the Scaglia rossa in the Gubbio area, that were largely or in part interpreted as resulting from diagenetic overprint (e.g. Corfield et al. 1991 –Terra Nova). Obviously, this correlation is also expected for hyperthermals, so a short discussion of how the two causes can be distinguished would be very useful.

We have made no changes here. Part of the problem is that earlier work had no understanding of the dynamic early Eocene world (to no fault of the authors). The very fact that a series of $\delta^{13}\text{C}$ excursions can be correlated across many sites, from those exposed on land to those still in the ocean, entirely invalidates this argumentation. We think that most in the community have moved beyond this issue, and since the manuscript already has numerous side tangents, we are reluctant to add another.

512-513 This holds only true in organic-lean rocks. If originally there were levels with increased Corg content in this sequence this ^{12}C -enriched material may also have decomposed and be incorporated during the recrystallization process, leading to negative excursions. Please provide

some reflection on this.

Have now stressed this point.

538-541 Show this in a detailed correlation graph, even if the details of correlation are still ambiguous, it would be more convincing if this is shown in a graph and that further research can focus on evaluating the patterns and correlations proposed here.

We are not sure what is being asked for here, as Figure 9 pretty much already shows this. However, we did add the word “seemingly”, as we are the first to admit (and hopefully have conveyed) that the correlations are somewhat tenuous.

756-758, 766-769, 816-817 Assemblages may contain large numbers of Acarinina for ecologic reasons and without taphonomic overprint. This is also well documented for the PETM in Spain and the Middle East. The Acarinina dominance over Morozovella and the link to eutrophication of marginal basins during the PETM has also been documented for the southern Tethyan basins either through upwelling or through enhanced discharge (e.g. Schulte et al. 2011 – Chemical Geology and references therein). It occurs to me as if only in open ocean settings Morozovella may be doing well during the PETM. Along most(?) continental margins Acarinina seems to flourish, notably at the expense of Morozovella, that nearly disappears from the record at least in the Middle East (Guasti & Speijer, 2007 – GSA SP) and southern Spain (papers by Molina & Arenillas).

We added the references suggested.

Minor issues (queries, typo's, style, formalities, additions..)

53 an archetypical..

Fixed.

55 two other significant Eocene warming...

Fixed.

59 the term ELMO should be reserved only for the ETM-2 clay bed at Walvis Ridge

Modified to “the ELMO event”. Technically, Robert is correct, as the ELMO refers only to the clay bed at Walvis Ridge. However, the clay bed itself has been correlated to similar horizons elsewhere, and so is often also called “the ELMO event” (as stated).

60 why not stick to ‘K’ in the remainder of the text (connecting the terminology to the use of J)?

No change. This is because in many papers, including recent ones, it is also called X. Indeed, this has led to confusion, where some people have thought they are different events. Ultimately, ideally in the near future, when multiple records such as at Possagno become published, there undoubtedly will be a singular terminology.

85-88 and turnovers in nannos, ostracods, platform organisms (larger foraminifera, corals,..), ..

Added this along with a few extra references.

105 Fraass

Fixed.

107 morozovellids and acarininids are informal terms for the two genera. I see no advantage for using these terms over Morozovella and Acarinina. Why not just call it the Morozovella crisis further in the text?

We prefer to use morozovellids and acarininids, which is not wrong.

111 delete 'and'

Fixed.

111 leave out reference to fig. 1 here, as it rather would suggest that the decrease in Morozovella takes place near the top of the EECO.

Fixed.

115-116 "These foraminiferal assemblages presumably reflect relationships between climate and carbon cycling across the EECO." Rather cryptic phrasing. Intended are the overall patterns of abundance changes.

Rewrote.

136 Why circa?

Removed.

143 Important issue! perhaps give some examples of the overall range in previous estimates.

Rewrote this and gave examples.

189 Why not add a standard biozonation (NP or CNE)? If not, please briefly explain why this is not done. Biostratigraphic inconsistencies?

Agree. We added NP-Zonations according to Agnini et al. (2006) in figures 4 and 6.

203-204 Confusing: "an important decrease in sedimentation rate (up to ca. 1.4 m/Myr)". The maximum was 1.4 and then it goes down?

Agree (silly writing mistake) and have rewritten.

272 a total is collected?

Fixed.

273 various spacing of 20 and 50 cm?

Rewrote.

286-287 Unclear: "We collected new samples spanning their effort for stable isotopes"

Agree! Rewrote.

304 Mass percent? (unusual in geosciences, but perhaps some geophysicists can be made happy)

Okay, as correct but funny, but we have changed accordingly.

325 and onwards the delta notation varies

Fixed.

402 Fig. 8!

Agree.

416 show or occur?

Fixed.

464 unclear: is the increase on average 7% or is this a typo?

Rewrote.

519: lower and higher temperature instead of colder and warmer T?

Changed.

610-612: the same observations were already made and briefly discussed some 20 years ago for the PETM (e.g. Schmitz et al. 1996 – Geology)

This is actually a fun paper to cite. Even we admittedly forgot about some of the data and insights. We think (and suspect even that the authors would agree), that some of the large-scale interpretations are wrong, but the data and insights are really good. Hard to fault a good paper written 20 years ago when the community had little clue about how to understand this time interval! We now reference.

677 'Morozovella crisis'?

Reworded

678 also at 577?

Yes, and added.

686 this correlation should be highlighted with some colored bands like for ETM2 etc. Otherwise it is not really clear.

We think that the figure and text are already clear. The problem here is that adding more lines to an already complex figure makes things distracting.

689-690 'nearby' instead of 'proximal'

Changed.

732 it is more likely that variations of the depth of the lysocline, rather than the CCD, played a direct role in the taphonomy of foraminifera in bathyal to upper abyssal sequences.

Agree and changed.

750-751 note that in Petrizzo et al. 2008 the dissolution susceptibility planktic genera includes Eocene records up to zone E8. The results are in line with the experimental results of Nguyen et al.

Agree and have rewritten.

824-825 show this more clearly in the figures (as for 686)

As noted above. No change.

1420 The captions are very long and can to some extent be shortened

Agree and now have shortened considerably.

1491 typo A. cuneicamerata

Removed.

1508 hexagons?

Agree. Fixed.

1552-1553 information is not quite clear in figure

Fixed.

1556 in what way is 'slightly modified' meant? Do you mean as indicated in the next sentence?

Fixed.

1559 what is exactly meant with 'this group'?

Reworded.

1595 Coxall not in italics

Fixed

1599 Gümbel

Fixed

1600 Gohrbandt not in italics

Fixed

1612 why refer to Grassé and not Deflandre (note spelling of Deflandre)

1614 what is meant with (Stradner 1958) Bukry 1972? In Bukry 1972?

1615 idem for (Bramlette and Riedel 1954) Shamrai 1963

Explanation for 1612-1614-1615:

Calcareous nannofossil nomenclature follows ICN (International Code of nomenclature for algae, fungi and plant).

1612: Deflandre was misspelled and it is now correct. We have added the "l" missed. The ICN roles prescribe that we have to cite the author that described the species and then, if it is the case, refer to the paper, volume, book which include the description. This is exactly the case. Deflandre described the species in a volume authored by Grassé.

1614: Tribrahiatus contortus (Stradner, 1958) Bukry, 1972: Bukry, 1972, transfers the species contortus in the genus tribrahiatus.

Some references:

BUKRY, D., (1972). Further comments on coccolith stratigraphy, leg 12 deep sea drilling project. initial reports of the deep sea drilling project, 12: 1071-1083. PERCH-NIELSEN, K., (1985). Cenozoic calcareous nannofossils. in: Bolli, H.M., Ssaunders, J.B. and Perch-nielsen, K. (editors), Plankton stratigraphy. Cambridge University Press, Cambridge, pp. 427-555. STRADNER, H., (1958). Die Fossilen Discoasteriden Osterreichs. J. Erdöl-zeitschrift, 74: 178-188.

Review: Luciani et al. [Paul Pearson]

[-- snip most of a very good and kind Introduction --]

Nevertheless, I think the paper will make a useful addition to the literature on a neglected but climatically interesting interval.

My main concern is potential over-interpretation of 'carbon isotope excursions' (CIEs). A number of these are claimed for the EECO, and they are numbered in the figures, but for me a bona fide 'excursion' ought to be defined by a series of points, not a single one.

We agree, but we cannot resample the original Possagno section! This is precisely why we expanded our work to measure stable isotopes at DSDP Site 577. At this location, which has very good stratigraphy and reasonably well preserved planktic foraminifera most of the purported CIEs given in this work are defined by more than one point, although even at Site 577, the resolution could improve. At Site 577, though, the limitation is the very slow sedimentation rates. One place to really nail the overarching issue, at least for carbon isotopes, is New Zealand, where thick sections of early Eocene limestone are now exposed, and yes, we are indeed chasing this (see for example, Slotnick et al., NZJGG, 2015, which shows the $d^{13}C$ record surrounding the K/X event). The problem with many of the NZ sections, from the perspective of the present manuscript, is that the planktic foraminifera are poorly preserved! I think here that we have to go with our guts and suggest that our correlations of multiple CIEs are reasonably close to being correct but indeed somewhat speculative. We think the manuscript is written with view.

The Possagno section is affected by foram shell infilling which could well add noise to the record and give spurious so-called excursions. Allocating to these points the same numbers in different sites implies correlation that I find highly doubtful. It may turn out that they really are short-lived global excursions, which might be the most exciting new finding here, but that has not been demonstrated. My recommendation is that the those parts of the manuscript are rewritten with much more caution, or higher resolution isotope data are obtained to show that these are well defined excursions and not 'noise'.

We agree very much; however, see above response.

Some minor and additional points:

19-22. This claim appears to be 'global' but it is based on just three locations. What about the high latitudes?

We think the observation is more general than three sites (especially including work by Pearson and his students!), but have rewritten appropriately.

35. 'Demise of the morozovellids' implies the genus went extinct but only some species went

extinct. The phrase is over-used in other places too.

Agree and have amended throughout.

47-48 Was the EECO warmer than the PETM?

This is actually a wonderfully and fundamentally interesting question, although beyond the scope of the manuscript. As hopefully clear in the manuscript, there is very good reason to suspect that temperatures fluctuated across the EECO. It is certainly possible that short intervals during the EECO, such as K/X, were actually warmer than the PETM. However, with available data, this is really difficult to decipher. In any case, though, we appropriately modified the text by stressing "long-term".

95 Note we tested specifically for environmental versus biotic influence on evolution and found both to be significant (Ezard et al., 2011, Science).

We are fully aware of this nice paper but inadvertently omitted; we have amended.

108 meaning not clear. Muricae are essentially layered pustules

Fixed

114-115. Also 398. What is the meaning of well-preserved? Generally they are recrystallized except in clays.

Agree that this is somewhat subjective, so have amended.

137 Point (i) - I don't understand. Who says the temperature defines time?

Well, it is called the EECO, and this usage is entirely defined by proxies for temperature! We have slightly reworded things, but this very much remains a major problem.

154-155 exactly what is meant by the onset of low d18O values? I think I know what you mean but the trend towards lower values starts before the petm.

Have amended.

182-183. Comment: this seems a reasonable working definition of the EECO to me.

Entirely agree. (Progress! At least one person beyond those affiliated with this paper is now on board -- smiles).

325, 350, 354 and elsewhere: delta not d

Fixed.

330, 336-338 What is a carbon isotope excursion. Is a single extreme number and excursion? I think an excursion should be defined by multiple points so as to rule out the possibility of noise. Hence I am not convinced these are real excursions although they may be. 371-372 equates lone negative values with excursions.

We agree, but are not sure how else to express things with the currently available data. (See comments above).

350 below the petm, not prior

Fixed.

430-431 morozovellid abundances (not morozovellids)

Fixed.

449-452 How much of this mirroring of morozovellid decline and acarininid increase is due to autocorrelation. What other genera are present other than Subbotina?

The other genera are shown in supplementary figure S1.

584 font size

Not sure what this refers to.

586-7 check grammar

Fixed.

631-633 For more on d18O and temperature on Shatsky Rise see Dutton et al., Pearson et al. (2007, Geology) and comment and reply by Dutton, which deals with species issues as well. We

proposed the then-controversial notion that temps were underestimated by over 10 degrees because of sea floor dissolution.

We are well aware of this work, which remains interesting but perplexing. The problem with this particular paper is that the interpretations for the early Eocene are entirely consistent with our views, but that those for the later Eocene are not (although we like to keep open minds on such matters). In any case, for completeness, we have added to the references, and we let potential readers sort things out.

677 Never reversed is better than irreversible

Agree and fixed.

795 Actually there is good evidence the hantkeninids moved from subsurface to shallower habitats in the Eocene (Coxall et al., 2000, Geology)

Agree and fixed.

812, also 862-3 as previously, not all morozovellids died out in the EECO - better demise of some morozovellids

Agree and fixed.

842 which others?

This is somewhat awkward, because several groups/papers have independently realized this in 2015. We have thus put we propose (knowing that it is generally consistent with current works).

1435. The phylogeny in Aze et al in this interval is taken pretty much from Pearson et al. (2006)

Added the reference.