

Interactive comment on “Benthic foraminifera at the Paleocene/Eocene thermal maximum in the western Tethys (Forada section): variability in climate and productivity” by L. Giusberti et al.

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Authors answers to referee #1 (R. Speijer)

MAIN ISSUES 4212, 13-15: point out to what extent these extraction methods may or may not interfere with foraminiferal preservation in these deposits, notably %F. In pyrite-lean sediments the use of H₂O₂ solution may not significantly alter foram residues; however, it will corrode pyrite-filled shells. Experimental studies demonstrate that “H₂O₂ is an unsuitable chemical for extracting delicate CaCO₃ and pyritized microfossils because of the damage and dissolution it causes” (Kennedy & Coe 2014 - Journal of Micropalaeontology). This is in agreement with earlier as well as own ex-

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perimental observations. Note that when all radiolarians are calcified this calcite must have come from a carbonate source, probably from within the rock unit and it is not unlikely that in a hemipelagic setting the most fragile foraminifera and/or nannos provided this calcite.

The authors are aware of the problems concerning the extraction methods of forams with pyrite-filled shells, but the tests in the Forada samples are not pyrite filled, and invariably filled by calcite. None of the examined samples yielded foraminifera preserved with pyrite infilling, or replaced by pyrite. Moreover, the samples with the lowest content of CaCO₃ (e.g., clays of basal Clay Marl Unit) were treated with diluted H₂O₂ (10%), in order to avoid possible additional breakage of tests. We will add a sentence in the paragraph ‘Material and Methods’, in which we will give details about the state of preservation of foraminifera at Forada. We do not fully respond to the comment on %F (fragmentation index) in this paper, because these data (and preparations) have been discussed in Luciani et al. (2007), and we just cite them here.

Part 2.2.1. It is not quite clear how the ecological overview is assembled. It seems as if the present data are already included in this overview, as “We allocated the taxa to the categories recolonizers (R1,R2) and opportunistics (O1, O2), based on their abundance pattern in the studied and other PETM sections”. Does this mean that the observed patterns of Forada are include to allocate the taxa to the various ecologic groups? If so, then this is not a correct procedure as it may lead to circular reasoning once the data are being interpreted. It would be helpful to add a table with included PETM localities from which these patterns are derived. (also note that ‘opportunist’ is the correct noun to the adjective ‘opportunistic’).

Having re-read the manuscript in light of comments by referee #1 and #2 on paragraph 2.2.1 (the benthic ecology overview), we appreciated their concern, and also agree with the reviewers that the paragraph is too long. We thus propose to eliminate the entire paragraph 2.2.1 and with it any reference to taxa ecology as inferred from the Forada record (e.g., the clustering into the categories R1, R2, O1, O2). We propose

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to discuss part of these aspects in the Discussion paragraph. In place of paragraph 2.2.1, following referee (1 and 2) suggestions, we will provide a new table summarizing the ecology of the most common taxa at Forada, based on published literature.

4216, 11: *Osangularia* - I'd like to caution for ecologic interpretations at generic level, especially when the reference taxa are many millions years younger or older, such as the OAE2 data referred to here. Mid-Cretaceous benthic foram communities were quite different from those of the early Paleogene and ecologic affinities at generic level (in as far as these can be generalized at all) are likely to have been different. For instance modern *Lenticulina* is a deep water taxon and *Oridorsalis* a cold water taxon (Murray, 2006). Both were, however, common inhabitants of early Paleogene (sub)tropical shelves (numerous data from Egypt, Tunisia, Tanzania). Extrapolations from the Recent or Mid-Cretaceous to the Paleogene (as for microhabitats pointed out in 4218, 11-18) should be avoided, especially if there is no information included from the early Paleogene itself, like for *Osangularia*.

We never meant - and in fact did not - draw any ecological interpretation for the genus *Osangularia* by quoting those OAEs papers. We just reported them as information. We just use the general information on test shape (disc-shaped, biconvex, trochospiral, keeled), to infer that fossil and extant *Osangularia* most probably were epifaunal, preferring stable, oligotrophic, well oxygenated environments (e.g. Murray, 2006; Alegret et al., 2003; Alegret, 2007). For this reason, we found it of some interest to see peaks in abundance of small *Osangularia*, coinciding with the doubtlessly stressed environment of the basal CIE in the Forada section, and therefore note that peaks of *Osangularia* occur within Cretaceous OAEs. We also note that Boscolo Galazzo et al. (2013) found small-size *Osangularia* (Plate II fig. 13 in Boscolo Galazzo et al., 2013) within organic-rich levels immediately following the Middle Eocene Climatic Optimum in the Alano section. A peak in abundance of small *Osangularia* has been observed in the basal PETM at Contessa Section, as highlighted for the first time in the present manuscript, and representatives of the genus *Osangularia* (*Osangularia* spp.) behaved

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opportunistically in the PETM of the Tethyan Alamedilla section (Alegret et al., 2009). We thus, in fact, are making the same point as the reviewer – that one can not always assume that environmental preferences of morpho-taxa were constant over time. We will explain this better in the text. A specific assignment of basal PETM osangulariids at Forada (and Contessa section) was not possible because of their very small size and sub-optimal state of preservation. We will add a sentence in the text to that effect.

4218, 11: the above point could be addressed here, as this problem does not only concern microhabitats, but ecologic traits in general.

The paragraph will be deleted and substituted with a Table (explained above). The specific case of *Osangularia* will be briefly discussed in the proper section of the paper (Discussion, paragraph 4.3.2.).

4219, 13: Results. I consider it a missed opportunity that the authors do not include a statistical evaluation on such a great data set. This would allow for an objective subdivision of the main patterns and by plotting the results (e.g. PCA, DCA) in cross plots this will almost certainly provide a graphic synthesis of the faunal evolution across the PETM. Now only frequency plots and a summary of highlights in a table are provided. These data deserve better than that.

We thank the reviewer for the suggestion, but we do not agree that a full statistical analysis would (in this case) actually add to the interpretation of the assemblages. Multivariate statistical analysis is highly suitable to document subtle patterns which are not clearly discerned in the raw data, especially when many taxa are involved. In this case, however, an objective subdivision of the faunal assemblages is possible using the raw data at Forada, because of the rapid stratigraphic succession of biotic events, and abrupt and pronounced changes in species abundance.

4221, 9: As an individual Zoophycos often spans several dm of sediment and bioturbates 1000s of years of sedimentation, it is worthwhile to add (from Giusberti et al. 2007?) to what extent these bioturbations may or may not have affected the

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foraminiferal sequence.

All micropaleontological (benthic and planktic foraminifera and calcareous nannofossils) and geochemical evidence (mineralogy, stable isotopes, etc.) published on the Forada PETM concur in indicating that the foraminiferal sequence was virtually unaffected by significant disturbance (we found no evidence of "mixing" of different biotic and abiotic signals or "anomalous" signals). We stress that samples collected for micropaleontological and geochemical analysis of the PA I interval of Giusberti et al. (2007) and samples from the rest of the section were carefully collected avoiding, where possible, the portions of rock containing clear evidence of the above-mentioned ichnofossils, and other traces as Planolites, Thalassinoides, etc. In the specific case of the Pa I interval ("Assemblage B" of the present manuscript), the entire block of the uppermost portion of Paleocene (ca. 20 cm) was removed during sampling in the field, and sliced in the laboratory into subsamples of ca. 3 cm thick, checking carefully the integrity of the sediment, and lack of (bio)disturbance. We are well aware of the fact that hemipelagic Scaglia sediments in "normal conditions" are always bioturbated (e.g., mottling). To what extent bioturbation affected Scaglia sediments in general is beyond the scope of present paper, because a proper and reliable evaluation will be possible only through an extensive ichnological analysis. We underline that, based on the comment of the reviewer, almost the 100% of published sections should undergo a ichnological revision/approach in order to test the reliability of micropaleontological and geochemical datasets. We will insert a sentence explaining that we, to the extent possible, excluded bioturbated materials, and have found no evidence for bioturbation-effects in data published on the Forada section.

4226, 19: "surface waters were oligotrophic" whereas 4227, 4 "indicates oligomesotrophic surface waters"

This was an oversight, and we will consistently use the term oligo-mesotrophic for surface waters.

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4222, 12: are these clays (probably shales; also in other places in the text), truly laminated, resulting from a lack of bioturbation (e.g., laminae with different composition, mostly caused by lack of oxygen) or are they just fissile (homogeneous composition) from reorientation of clay minerals through compaction as is a normal feature in shales? Some image support of the laminations (cf. Nicolo et al. 2010) could significantly strengthen the interpretation. Without this, skepticism will remain.

Clays in the lowermost decimeters of the CMU are not shales (as described in detail in Giusberti et al., 2007). The only shales occurring in the entire Scaglia sequence of the region are the black shales of the upper Cenomanian OAE2 Bonarelli level. Clays of basal PETM at Forada present primary lamination, but they are not "fissile", in strict analogy to what Rodriguez-Tovar et al. (2011) described for the basal siliciclastic unit of PETM at Zumaia section (Spain). We interpreted such lamination as consequence of the lack of bioturbators in the strongly stressed conditions of basal PETM, conform Nicolo et al. (2010). Besides high temperatures, changes in food availability and dysoxia of interstitial water pores, as well as low-pH sea-floor conditions, may have played a significant role in excluding macrobenthic fauna in this early phase of PETM. Deep-sea organisms are highly sensitive to even modest but rapid pH changes (Seibold and Walsh, 2001; Science, 294, 319–320) that have been shown to be harmful even for infaunal deep-sea communities (Barry et al., 2004; Journal of Oceanography, 60, 759–766). Usually, the Paleogene Scaglia Rossa is mottled (greenish flames in reddish-brownish sediments), indicative of activity of macrobenthics in the sediments. In the investigated section, mottling completely disappears only within the basal-most, laminated greenish clays of CMU, and gradually reappears (as thin reddish "flames") at the Assemblage D-E transition. We will ensure that this description of sediment is clear in the revised version.

4228, 20: It would perhaps be worthwhile here to add a section on extinction rate within this particular sequence. I have the impression that considering the inclusion of the fine size fraction here, leads to a (much?) lower extinction rate than the 40% or more that

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is usually mentioned.

The proportion of Paleocene cosmopolitan “extinction” species expressed relative to the total of benthic foraminifera at Forada is quite low (ca. 10%), clearly related to the huge number of Bolivinacea dominating the fine size fraction used for this study (>63 μm). Note that many extinction taxa are epifaunal morphotypes, commonly larger than 125 μm , as also noted elsewhere. Similarly low percentages (12-15%) of cosmopolitan extinction taxa have been recorded in Scaglia sediments of the Contessa section (Giusberti et al., 2009) and at ODP Site 690 by Thomas (2003), where infaunal morphotypes (buliminids and uniserial calcareous taxa) are abundant/dominant in the >63 μm fraction. We will add a sentence in the text to clarify this, and we can move Fig. S1 from Supplementary material to the text.

4228, 22-25: note that a very similar dead zone is observed at Dababiya (Ernst et al. 2006), but relating this to a rise of the CCD and lysocline up into the continental shelves of the Tethys seems quite unlikely. So the question that comes up is, up to what shallow depths could a rise of the CCD still account for the effects observed? Is that indeed up to 1000-1500 m or perhaps even shallower?

With this paper the paleobathymetry of the Forada section is refined, and our dataset points out a fully bathyal paleodepth. We thus argue that the CCD was raised up to 1000-1500 m in the Tethys (see also tables in Thomas, 1998); we cannot say anything about Tethyan shelves. This interpretation is consistent to what has been observed at Tethyan sites with similar paleodepths (Egger et al., 2005; 2009; Alegret et al., 2009; Giusberti et al., 2009), and with the 2 km CCD raise estimated in the south Atlantic (Zachos et al., 2005). As far as the black clay is concerned, the problem of its interpretation has been stressed in the text, as also previously in Giusberti et al., (2007). In much shallower, coastal/mid shelfal sites, carbonate may be not preserved due to local eutrophic conditions with oxidation of organic matter leading to waters corrosive to carbonate (similar to circumstances in present-day eutrophied shelves/estuaries), which may well have been the case in the Egyptian sections, but that is not relevant to

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this manuscript.

Minor issues:

This paper is about much more than the benthic foram record as it integrates data from earlier studies on Forada. In order to maximize readership the title could be improved accordingly, e.g. by adding a term like ‘integrated’ or alike.

We thank the reviewer for the suggestion and will follow his advice modifying the title of the paper.

The introduction is quite long and detailed. Some parts can certainly be preserved for the discussion instead of elaborating them in the introduction.

We agree in part with the reviewer and will try to modify the introduction accordingly, but we do not agree that all the introductory text should be included in the discussion, because it is introductory in nature and interferes with the line of discussion in the discussion section.

4208, 18: First records of anoxia related to the PETM along the Tethyan continental margins: Gavrilov et al. 1997 – Lithology and Mineral Resources; Speijer et al. 1997 –Geology).

Ok. We will add these references and Benjamini (1992).

4215, 22: Note that *G. subglobosa* is a common to abundant component (up to 20%) in Paleocene neritic deposits at Dababiya (and Aweina, . . .), Egypt. It returns within PETM DQB 3 (10-15%), together with various buliminids and *Tappanina selmensis* (Ernst et al. 2006), under improving, but probably fluctuating seafloor oxygenation. In this shelf setting (as elsewhere in Egypt) *G. subglobosa* it is not part of the first colonizers though (due to severe anoxia persisting after a (nearly) ‘dead zone’).

Ok. We will add such reference in the Table 1 summarizing the ecology of benthic foraminiferal taxa. It is well possible that *G. subglobosa*, like *T. selmensis*, originated

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at shallower depths and migrated into the deep sea after the BFEE, because the taxon is at most deep-water sites absent in the Paleocene, or present only rarely in the very latest Paleocene (Thomas & Shackleton, 1996).

4226, 3: indicate at what depth ranges the modern OMZ is observed. Under high productivity zones with a similar export production in warmer oceans the OMZ is likely to have been more expanded.

Ok- but this varies very strongly by oceans. We will refer to the reconstruction of OMZ depth during the PETM in Zhou et al., 2014.

4227, 5: add references for "seasonal to periodical increases in primary productivity" leading to "high faunal diversity" at middle bathyal depths.

Ok, we will add as references: Fontanier et al. (2006a) Seasonal variability of benthic foraminiferal faunas at 1000 m depth in the bay of Biscay. *Journal of Foraminiferal Research*, 36, 1, 61–76; Fontanier et al. (2006b) Stable oxygen and carbon isotopes of live benthic foraminifera from the Bay of Biscay: Microhabitat impact and seasonal variability. *Marine Micropaleontology* 58, 159– 183; Fontanier et al. (2014) Living (stained) deep-sea foraminifera off Hachinohe (NE JAPAN, Western Pacific): environmental interplay in oxygen-depleted ecosystems. *Journal of Foraminiferal Research*, 44, 3, p. 281–299. We will add also Gooday (2003).

4232, 20-22: is this similar to modern dust supply from the Sahara to the Atlantic? This proposal needs some referencing.

Ok. Relevant citations are reported in line 4232 7-9. We will report them also where indicated by the reviewer.

4234, 15: indicate that 800 ky after the end of CMU deposition is well beyond the top of the studied interval. This also reminds of the extended (650 kyr) humid period, starting at the onset of the PETM, observed at Site 401 (Bornemann et al., 2014).

Ok. We will add in the text a sentence including the suggested reference.

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Fig. 8: meaning of texts not quite clear: e.g. what is meant by "Coccolithus-Toweius dominated assemblage. No Coccolithus-No Toweius assemblage dominated by Zygr., Sphen. and Octol."? Similar in other boxes.

We will modify both text and figures as follows: Assemblage dominated by Coccolithus and Toweius. After the removal of Coccolithus and Toweius, the assemblage is dominated by Zygrolithus, Sphenolithus and Octolithus.

Table S1: note that Schulte et al. 2011 (Chem. Geol.) provided additional data and an update on fluvial discharge during the PETM at Dababiya, relative to the paper by Ernst et al. 2006.

The reference of Schulte et al. 2011 will be added in Table S1 (block 24).

Technical issues:

4208, 10: too much 'profound' (rather bombastic).

Ok. We will remove "profound" from line 10. We will also substitute at lines 6-7 "profound shallowing of the calcite compensation depth" with "severe shallowing of the calcite compensation depth" (already adopted in Giusberti et al., 2007).

4208, 20: spelling 'Pälike'

Ok

4209, 14: spelling 'Collinson'

Ok

4213, 13 and elsewhere: please use infaunal/epifaunal terminology consistently. Here and there the terms 'morphotype/morphogroup/morphology' are used. As for most taxa the preferred microhabitat is unknown, one of these terms should always be added to infaunal/epifaunal. Also note that 'epibenthic' and 'endobenthic' have been proposed as preferable terms to denote 'epifaunal' and 'infaunal' microhabitats (Walker & Miller

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1992 - *Palaeos*). One last terminological nitpicking: I'm not sure whether the term 'agglutinant' as noun or adjective is appropriate in English (as it is in Dutch). Native English speaking workers generally use 'agglutinated' or 'arenaceous' (taxa).

Ok. We will make the terminology uniform as suggested. As far as epifaunal versus epibenthic etc., we are aware of the Walker & Miller 1992, but we prefer the still widely used infaunal and epifaunal terms, and prefer the more extensive discussion of this topic in Jorissen et al., 1995, 2007. We will substitute, as requested, agglutinant with agglutinated.

4215, 21: spelling 'Giusberti'

Ok

4231,27: spelling of 'sibaiyaensis'. Also note that a very similar Acarinina acme (80-90%) is observed in most PETM sections in Egypt, where it was also linked to enhanced nutrient availability (Guasti & Speijer 2007, GSA SP424), in contrast to the – then - widely held (open ocean) view that the acme resulted from oligotrophy.

Ok, we will correct the specific name. The significance of Acarinina acme at Forada has been previously stressed (comparing the African record and citing Guasti, 2005 and Ernst et al., 2006) in the paper of Luciani et al. 2007. We will add the suggested reference in our text. Note however, that Acarinina acmes occur widely in open ocean (e.g. Site 690; various papers by Kelly et al.), and have also been linked to carbonate over-saturation.

4234, 27: add 'atmospheric' (CO₂)

Ok

Fig. 1: The Svalbard Archipelago includes the main island Spitsbergen. Iceland probably slipped into the text erroneously as it did not yet exist back then.

Ok

C2601

Fig. 3: CF has no unit and it's not clear what is meant here. Here and there in the text reference is made to wght%. Specify this at least clearly in the caption. Reference only to Hancock & Dickens (2005) is insufficient.

Ok. We will specify in the caption: "The weight percent of the >63 μ m size fraction relative to the weight of the bulk sample (coarse fraction, CF) was calculated for the Forada samples following Hancock and Dickens (2005)".

Fig. 4: Please clearly specify that N/g for the radiolarians refers to the amount of radiolarians relative to the weight of the washed residue >125 micron (N/g often refers to the number of microfossils relative to the weight of the dry sediment).

Thank you very much for this remark. We realized that there was a mistake in the original plot (X axis) of the original figure (Fig. 10) from Giusberti et al. (2007). We will modify the figure and we will add the sentence suggested in the caption. We will use "number" of radiolarians (instead of "amount of radiolarians"), the word 'amount' is incorrect use of English.

Fig. 5: "Bulimina spp." should not include other genera. Buliminids? Buliminacea? At any rate specify usage of the grouping chosen.

"We will substitute "Bulimina" with "other Buliminacea", explaining what is excluded.

References: In a separate upload various errors (probably not exhaustive) are highlighted in yellow in the reference list: misspellings, non-abbreviated journals, missing initials, Palaeo3 being consistently indicated as Palaeo2. Doi numbers are given for a minor part of Elsevier and Science papers I'd personally plea for consistency in the usage (all or nothing).

Ok. We will remove all the doi throughout the references and we will correct the errors/mistakes (see also answer to reviewer #2).