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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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The authors provide a well-written and -documented manuscript on a large and detailed benthic foraminiferal record spanning the PETM at Forada. The authors not only provide new insights into the foraminiferal sequence, but through linking this to diverse physical and chemical data published before, they propose a an integrated view on oceanic and ecologic changes before, during and after the PETM. Overall this is an excellent contribution to increasing the understanding of the PETM and its relationship with changes in the marine biosphere. Below various suggestions are made to further improve the quality and impact of the paper.

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Main issues to consider:

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 experimental 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.

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 included 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’).

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 Re-

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cent 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*.

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

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.

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 foraminiferal sequence.

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.

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 is usually mentioned.

4228, 22-25: note that a very similar dead zone is observed at Dababiya (Ernst et

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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?

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.

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

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)

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'

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.

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

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

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

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).

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.

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.

Technical issues:

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

4208, 20: spelling ‘Pälike’

4209, 14: spelling ‘Collinson’

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 1992 - Palaios). 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).

4215, 21: spelling ‘Giusberti’

4231,27: spelling of ‘sibaiyaensis’. Also note that a very similar Acarinina acme (80-

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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.

4234, 27: add 'atmospheric' (CO₂)

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.

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.

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).

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

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).

17/10/2015

Robert Speijer, KU Leuven

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/11/C2029/2015/cpd-11-C2029-2015-supplement.pdf>

Interactive comment on Clim. Past Discuss., 11, 4205, 2015.

C2034

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