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Climate of the Past Discussions

Interactive comment on "Onset of the Paleocene–Eocene Thermal Maximum in the southern Pacific Ocean (DSDP Site 277, Campbell Plateau)" *by* C. J. Hollis et al.

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A new multi-proxy record of the PETM from a deep sea core in high southern paleolatitudes is very welcome and adds to our understanding of this very large and still-enigmatic climate perturbation. The authors should be congratulated for finding the PETM in such an old drill-core. There can be little doubt that the data should be published and the study is suitable for Climate of the Past. However the data set is moderately complex and (I think) difficult to interpret unambiguously, hence the challenge is to distil the salient points as clearly as possible for the wider community interested in this event, and for comparing records. Hopefully the review process can help with that.

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Interactive Discussion
Discussion Paper

Some of my comments are relatively minor and can probably be dealt with quite easily but I have two more substantive issues to deal with first.

1. The relation between the bulk d13C record and the so-called 'onset' of the PETM, and what happens near the base of the event.

The PETM is by definition a warming event hence its onset (for me) is, perforce, the bit where it gets hotter, i.e. where the d18O goes negative (and is also seen in the Mg:Ca excursion in this study). This is the interval 457.25-457.29 m which corresponds to a 4 cm chunk of core. However even the 'intermediate' bulk d18O and whole foram values from within this chunk may be affected by differential diagenesis, as the paper rightly points out, and some bioturbation mixing is also likely given the burrows, so it is not clear that the real 'onset' of the thermal maximum is sampled at all in this drill core. The paper mentions almost in passing that the portion directly below the PE boundary (457.3-457.58) has sparse and poorly preserved foraminifera (p. 256 line 3-4) but offers no explanation. This suggests the possibility, even likelihood, of dissolution burn-down caused by a transient rise in the carbonate compensation depth during the real onset, also the time of the benthic extinction, and what the core records would therefore be renewed sedimentation onto a dissolution surface / semi-hiatus.

Most previous records of the PETM have the onset of warming coincident with a large and abrupt negative carbon isotope excursion (CIE), so that the onset of the CIE is pretty much coincident with the onset of the PETM - the coupled excursions mentioned in the introduction. But this record is quite different, basically un-coupled, and I have to admit to being puzzled by the run of intermediate bulk d13C values through most of what appears to be the body of the PETM, even while bulk d18O is becoming slightly more positive. This, for me, is the enigmatic aspect of the record and the discussion (Section 3.3) does not wrestle it down sufficiently.

Part of the problem (I contend) is that the authors refer to the gradually decreasing bulk d13C values as the 'onset of the PETM' (in title, abstract line 8; discussion in Section

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3.3) which only confuses matters. The d18O and Mg:Ca show that this interval is in fact the 'body' of the thermal maximum. It may be the onset of the CIE at this location, I grant, but I doubt whether it correlates with the onset of the CIE everywhere else. Why the bulk d13C values are a run of intermediates is difficult to explain, especially given the foram d13C values around 457.22 m appear to be more or less 'fully negative' and similar to those above from around 467.03 m.

I do not offer an explanation for this conundrum, all I can think of is some far-fetched ideas, but the authors need to tackle it head on, and perhaps take the opportunity of clarifying what they mean by onset of the PETM / CIE / event and making sure the text is clear throughout. Incidentally, the claim that the 'onset' lasts 50-100 kyr in other sites (p. 258, line 4) contrasts with what I think most people would think of as around 10-20 kyr or less, with the 'body' being about 150-200 kyr (the Mead Stream section is also peculiar).

Also incidentally, if the lower carbonate content is because of reduced carbonate flux then the PETM here would also represent more time than an interpolated or extrapolated age model, possible equivalent to much of the body of the PETM (see also my comment on cycles below).

2. Foram preservation and diagenesis

The authors indulge in quite an extended discussion about possible diagenetic effects on foram tests and d18O and Mg:Ca. They assert, reasonably I suspect, that the preservation is better in the PETM than below and above because of inhibited recrystallization related to the higher clay content. I have often wondered whether such an effect might be at play in some PETM records which go from chalk to clay (if not cheese) - and indeed other such events such as the Mediterranean sapropels. To demonstrate this effect would be very neat. However the reader cannot evaluate the likelihood of this because no SEM evidence is presented. This paper would benefit hugely from a comparative textural study of the foram walls in internal section under high powered

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SEM so that the extent of recrystallization can be visually assessed relative to published criteria (which we have produced for just this kind of material - Pearson, P.N., and Burgess, C.E. 2008. Foraminifer shell preservation and diagenesis: comparison of high latitude Eocene sites. Pp. 59-72 in Austin, W.E.N. and James, R. H. (eds.), Biogeochemical Controls on Palaeoceanographic Proxies. Geological Society Special Publication 303: 59-72. Doi: 10.1144/SP303.5). This work has been done ('stringent [SEM] screening criteria were applied to exclude zones within test walls that show evidence of diagenetic alteration' - p.252, l. 5-6) so it really should be shown. Actually I will challenge the authors: given that the carbonate content is fairly high even within the PETM, I predict that all forams at this site will show clear evidence of diagenetic alteration on a micron scale throughout their entire walls (although the effect maybe variable) - I do not believe you can exclude recrystallization at this site - so go prove me wrong by illustrating the typical biogenic microgranular texture of a well-preserved foram!

It would also be nice to see what is causing the reputed poorer preservation of the forams immediately below the PETM - are they dissolved?

In short, I think we should be moving away from near-meaningless phrases like 'carbonate microfossils are moderately well preserved' (p. 256, line 3) and 'relatively well preserved tests' (p. 258, line 23) to something much better documented, but for that we need the SEM images.

My other comments are:

p. 246, line 3. Reword? These references are compilations, not original studies, but in any case all early Paleogene pCO2 reconstructions are very uncertain - and yet the statement seems to take it as fact.

p. 250, line 9. Acarinina is spelt wrong. Were species combined in these analyses? We know Acarinina species can differ quite a bit in habitat, so it would be better to stick to species where possible as in the trace elements.

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p. 256, line 12. The missing NP7 and NP8 are not shown on the log on Figure 2, where the zones are combined. Hence the text says something important that is absent from the fig.

p. 256. Can you say something about planktonic forams in the PETM? Did you find any excursion taxa or influx of warm forms?

p. 257, line 9. Can you prove the Fe record is cyclical by power spectra? Are these cycles likely obliquity? If so you have virtually all the PETM here, not the 'onset'!

p. 257, line 18. This positive shift in d18O of ∼0.4 per mil is not obvious to me on Fig 2. I wonder if this is a burn-down dissolution effect, also the enigmatic peak in Fe and mag. susc. which you say the cause is unknown.

p. 260. "Benthic foraminifera tests are dense [true] and thus less prone to recrystallization [not true?]" This has been claimed many times, and I blame myself partly for it, as we all hoped it would be true, but whenever we have actually studied the tests they are in fact just as recrystallized as the plankton. Again, you could substantiate this with SEMs or omit the statement / claim.

p. 262. Is there any paleontological evidence for a change in fossil assemblage that perhaps might be related to the subtropical gyre?

In summary this is a fascinating new record from the high southern latitude but the text needs to be clearer regarding what is meant by the onset of the event; and images from the textural-diagenetic SEM study that has been done would improve the case for understanding what has been observed regarding foram test preservation and quantifying the temperature excursion.

Paul Pearson

Interactive comment on Clim. Past Discuss., 11, 243, 2015.