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Climate of the Past Discussions



# *Interactive comment on* "Mammal faunal response to the Paleogene hyperthermals ETM2 and H2" *by* A. E. Chew

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General Comments: This paper is clearly written and attempts to answer a very important question – what were the terrestrial biotic effects of the ETM2 and H2 hyperthermal events and how do they compare to changes observed at the PETM? Although on the surface, the results presented here look very compelling, there are some fundamental methodological issues that create significant, unrecognized uncertainties that seriously question the reliability of the conclusions. Because the connection between the climate events (ETM2 and H2) and the biotic events (Biohorizon B, B1 and B2) are not recorded in the same stratigraphic sequences, the precise correlation between these records is absolutely critical to the whole foundation of the study, yet the large uncertainties in the stratigraphic correlation between these records and more recent

results that significantly impact the stratigraphic level of one of the key tie points are not included in the analysis and thus their impact on the conclusions of the paper cannot be assessed. There are also important, albeit more easily resolved, issues with (1) the use of the term EECO (Early Eocene Climate Optimum) and (2) in the projection of results outlined here to biotic effects of future climate change.

Specific Comments: My biggest concern about this paper, and one that in some sense I think is an Achilles heel since it is fundamental to the thesis of the paper, is that neither the stratigraphic framework that ties the fossil localities together nor, more importantly, the stratigraphic correlations that tie the fossil framework to the isotope records is precise enough to confidently resolve the very short hyperthermal events being discussed. Stratigraphic framework of fossils - The Fifteenmile Creek *composite* section (~700 meters thick) used in this study ties together some 410 fossil localities in an area that is roughly 30 km x 40 km by correlating some 44 different local sections (Bown et al., 1994). Indeed, this is how the study achieves the high sample sizes that are the foundation if its result. Although this is certainly one of the most densely sampled regions of the world for fossil mammals, the stratigraphic uncertainty in the correlations between local sections and fossil localities must be on the order of at least +/- 10 meters (and probably more) given the difficulty of tracing beds through the low-lying outcrops in this area and the prevalence of "cut and fill" channel structures (Bown et al., 1994). This study, however, assigns a single meter level to each fossil locality with no error. The study needs to account in some way for the uncertainty that is associated with the stratigraphic correlations being made over such vast distances. ETM2 and H2 where they have actually been identified by isotopic data in the northern part of the basin are each only ~20 meters thick (and would likely be less than that in the southern part of the basin where sediment accumulation rates are lower) so the error in correlating between such distant fossil localities is very significant for the question at hand. Stratigraphic correlation between the Fifteen Mile Creek composite section and the isotope stratigraphies in the McCullough Peaks – This entire paper rests on the correlation between the Fifteen Mile Creek composite section (I will call this the



CPD
11, C400–C407, 2015

"fossil stratigraphic framework") and the Gilmore Hill and Deer Creek sections in the McCullough Peaks where ETM2 and H2 have been identified using isotopic methods (I will call this the "isotope stratigraphic framework"). Chew chooses to tie these two frameworks together using the C24r-C24n geomagnetic reversal and the Biohorizon B faunal event. However, there is no discussion of the large uncertainty associated with the correlation of these tie points except to say that "these are rough predictions" (line 14, p. 1376). Can "rough predictions" provide the kind of precise stratigraphic correlation necessary to support the conclusions (and title) of the paper? Unfortunately, I don't think so. In the case of Biohorizon B, the McCullough Peaks sections have an uncertainty of ∼20 meters based on Fig. 1 in Abels et al., 2012. More recent work in this area has shown clearly that the event labeled H2 in the Gilmore Hill section in Abels et al., 2012 is actually ETM2 (this was presented at the Ferrara 2014 CBEP meeting and is the basis for D'Ambrosia et al., 2014 and Snell et al., 2014 and the details will be part of an upcoming paper by D'Ambrosia based on her Ph.D. thesis work). This pushes the position of Biohorizon B down ∼25 meters relative to the hyperthermals and thus fundamentally alters the correlation of the faunal turnover events to the isotopic anomalies (the faunal peaks identified by Chew will now fall below the isotope peaks by ∼25 meters). In the case of the Chron 24r-24n reversal, the McCullough Peaks sections have an uncertainty of ∼60 meters (see Figure 1 in Abels et al., 2012). These tie points also have uncertainties in the Fifteen Mile Creek section (unknown for Biohorizon B because it is assigned a single meter level despite previous arguments that it lasted ∼300 ky [Chew 2009] and ∼13 meters for the C24r-24n reversal [Clyde et al., 2007]). When the ∼25 meter change to the stratigraphic position of Biohorizon B relative to ETM2/H2 in the McCullough Peaks sections is combined with the large uncertainties (relative to the short timescales of the hyperthermals) in the positions of the Chron C24r-24n reversal, it poses very fundamental challenges to the reliability of the stratigraphic correlations (and thus conclusions) in the paper. The easiest and most obvious solution to this problem is to isotopically sample the Fifteenmile Creek localities from which the fossils come so an isotope record showing the precise position



CPD
11, C400–C407, 2015





of ETM2 and H2 is directly tied to the fossils being analyzed.

Other, less significant concerns I have with the paper are (1) the use of the term EECO and (2) the discussion of future projections for biotic change under modern global warming. (1) In several places in the paper, Chew suggests that ETM2 and H2 occur during the beginning of the EECO (e.g. line 9 in Abstract, p. 1372) but these events (which are older than 53.5 Ma, Zachos et al. 2010) occur before the EECO (which is 53-51 according to Chew – line 10, p. 1373 - and are considered even younger by many others). This misstatement is repeated many times in the paper and I suggest rewording to align with Line 8 in Introduction (p. 1373) that says "in the approach to the Early Eocene Climate Optimum (EECO)". (2) In section 4.3 labeled "Implications for modern anthropogenic change", Chew suggests that the changes observed here and at the PETM "will probably occur" in response to current and future anthropogenic warming. I think this discussion needs to at least acknowledge the huge impact that other human activities (e.g. widespread habitat/landscape changes and human controlled dispersal [e.g. invasive species]) have already had on "natural" mammal populations and how those factors could very well swamp any response to climate change. Also, the 12°C temperature change in Wyoming (from 8°C – 20°C MAT) over the next 300 years quoted in this section (Line 15, p. 1388) is way beyond typical predictions. I believe this is due to confusion between Celsius and Fahrenheit temperature scales.

Technical Corrections:

Title – If published, I think the title needs to change to "Mammal faunal changes near Paleogene hyperthermals ETM2 and H2" or something much less definitive given the uncertainties in the correlation that I outline above.

P.1372 Abstract Line 9 – change to "following the onset of warming leading up to the EECO" Line 12 – Change to "relatively unknown" (e.g. D'Ambrosia et al, 2014) Line 19 – "Does not include immigration" seems too extreme – it is impossible to know for sure if an FAD is immigration or anagenesis (or cladogenesis). Line 23 – See comment



CPD
11, C400–C407, 2015

about EECO above

P. 1373 Line 1-2 – See comment above about more nuanced statement concerning future predictions for biotic change based on these results. Line 8-9 – This is a good way to refer to EECO – change to this throughout. Line 21 – Clyde and Gingerich, 1998 should definitely be cited here since it was the first paper to carry out this kind of analysis of mammalian assemblages across the PETM.

P. 1374 Line 20 – See comment above about EECO Line 25 – Change to "before EECO" instead of "at EECO"

P. 1375 Line 12 – Are the "distinctive red beds" mentioned here that mark the base of the FC composite section really at the beginning of the PETM? In other parts of the basin, the distinct red beds are in the second half of the PETM.

P. 1376 Line 4 – I would argue that the "biostratigraphic events at the beginning of Biohorizon B" are not "loosely tied" in the McCullough Peaks sections since the fossil localities there are tied directly (by bed tracing of usually less than 1 km) to the isotope sections. Delete "loosely". Line 8 – This section of the paper is the major problem area. Aside from the fundamental issues outlined in detail above, nowhere in this sentence does it state the exact meter levels used for Biohorizon B and the C24n-C24r reversal in the McCullough Peaks "isotope sections". Such levels are given for Fifteenmile Creek tie points so they need to be provided for the McCullough Peaks section(s) as well. Also, this is where (1) the recent change to the stratigraphic position of Biohorizon B relative to ETM2 and H2 needs to be incorporated and (2) a systematic analysis of the stratigraphic uncertainties on the tie points needs to be carried out. Line 15 – Actually the position of Biohorizon B is much better resolved in the McCullogh Peaks sections ($\sim$20 meters) compared to the position of the C24r-C24n reversal ($\sim$60 meters). The main issue is that both tie points are imprecise relative to the duration of the hyperthermals in question so the correlation of the faunal record from Fifteenmile Creek also carries these uncertainties. Line 21 – Bown et al, 1994 actually put the



CPD
11, C400–C407, 2015

Elk Creek and FC sections in the same composite sections. Clyde et al., 2007 were the ones to argue for separating them into separate composite sections so that paper should be cited here.

p. 1377 Line 3 – I agree that these are “outliers” and am happy to see acknowledgment of that. Line 20 – More explanation is needed to explain how this analysis of randomly overlapping time bins of different length artificially increases the temporal resolution of the data when the raw data are not sufficiently resolved to begin with. A simple simulation would be helpful to illustrate the point. In essence, it seems to be arguing that you can get better temporal resolution than your original data set by a moving window averaging method but that sounds like a free lunch :)

P. 1379 Line 1 – Replace “algorithmic” with “subsampled”

P. 1380 Line 23 – Why sum these metrics instead of just plotting them separately to see if they agree? It seems like summing them unnecessarily masks them (and same question on Line 9, P. 1381). Line 18 - It is absolutely not possible to say with confidence that this 40 meter interval is the same as the ETM2 and H2 interval without having isotope data tied directly to the fossil localities (see detailed discussion above).

P. 1382 Line 28 – What is meant by “aligned” here. I am assuming it means independently correlated but it sounds like the patterns were wiggle matched which of course would not be appropriate. Assuming independent correlation, there are still all of the issues mentioned above (especially with respect to the updated position of Biohorizon B relative to ETM2 in McCullough Peaks sections).

P. 1383 Line 3 – Some reference to D’Ambrosia et al., 2014 should be included here since she was the first to show mammal body size changes directly tied to the isotope sections in the McCullough Peaks (also in prep. as part of her Ph.D. thesis).

p. 1384 Line 1 – “similar” to each other? Line 4 – Except earlier you indicated that immigration was not important at ETM2 and H2 but it certainly was at PETM.



CPD
11, C400–C407, 2015

p. 1385 Line 1 – Again Clyde and Gingerich, 1998 should be cited here given it was the first to analyze this in detail. Line 15 – Snell et al., 2014 should be cited here as it is the first to give absolute temp estimates for these hyperthermals in the Bighorn Basin.

p. 1386 Line 2 – Same comment about EECO. Change to "warming leading to the EECO" not "onset". Line 7 – "there is no evidence of a CIE in the McCullough Peaks isotope sections of Abels et al. (2012) to suggest a hyperthermal mechanism". Not clear what this means given these are the sections where the CIEs are actually documented. Line 16 – "Biohorizon B, the largest faunal event in the FC record after the PETM, coincides with the onset of this change" Onset of what change? To be clear, Biohorizon B does not correlate to ETM2 or H2 just as Abels et al 2012 argued and is further supported in this analysis.

p. 1387 Line 10 – add "in this basin" after "evolutionary change". To my knowledge, Biohorizon B has not been reliably documented in any other place in the world (as opposed to PETM which has).

p. 1388 Line 16 – What? A 12°C change in Wyoming over the next 300 years is more than the projections I have seen but 12°F (Fahrenheit) is more like it.

p. 1389 Line 10 – Here there is some limited acknowledgment of the uncertainty in the correlation "most likely related". This should be included in title and throughout (assuming new correlations with new tie points still allow the same argument). Line 17 – Again, "does not include immigration" is too strong a statement given that we can never really know the cause of an FAD. Line 20 – Change "onset of the" to "lead up to the" Line 22 – Again, this is not the beginning of the EECO (even by definition used in this paper; 51-53Ma) since these events are older than 53.5.

p. 1398 Table caption – "represent segments of individual lineages" should be changed to "represent segments of what are thought to be individual lineages"

p. 1401 Figure 2 – Could the peaks in abundance be driving your peaks in turnover?



CPD
11, C400–C407, 2015

These should be shown side-by-side or correlated to make sure that sampling is still not influencing the turnover results.

p. 1403 Figure 4 – The relative spacing between ETM2 and H2 and the tie points shifts between the bottom bar (which I assume represents the McCullough Peaks record?) and the upper graphs. Why would that be if the McCullough Peaks spacing of these events is being used as the independent guide to interpreting the turnover curves? The caption needs a lot more detail to explain this as well as what all of the different color curves represent.

Appendix In the Supplementary Table. (1) Why are some specimens from Elk Creek included when the text says they were not? (2) Many specimens have a level that says "∼". What does this mean? How much stratigraphic error is on these specimens compared to the others?

References in review that are not in original paper: D'Ambrosia, A.R., Clyde W. C., Fricke, H.C., and Gingerich P. D., 2014, Repetitive mammalian dwarfism associated with early Eocene carbon cycle perturbations: Rend. Online Soc. Geol. It, v. 31, p. 53–53, doi: 10.3301/ROL.2014.41. Chew, A.E., 2009, Paleoecology of the early Eocene Willwood mammal fauna from the central Bighorn Basin, Wyoming: Paleobiology, v. 35, no. 1, p. 13–31. Clyde, W.C., and Gingerich, P.D., 1998, Mammalian community response to the latest Paleocene thermal maximum: An isotaphonomic study in the northern Bighorn Basin, Wyoming: Geology, v. 26, p. 1011–1014. Snell, K.E., Fricke, H.C., Clyde, W.C., and Eiler, J.M., 2014, Large temperature changes on land during Early Eocene hyperthermals: Rend. Online Soc. Geol. It., v. 31, p. 204–205, doi: 10.3301/ROL.2014.122. Zachos, J.C., McCarren, H., Murphy, B., Röhl, U., and Westerhold, T., 2010, Tempo and scale of late Paleocene and early Eocene carbon isotope cycles: Implications for the origin of hyperthermals: Earth and Planetary Science Letters, v. 299, no. 1-2, p. 242–249, doi: 10.1016/j.epsl.2010.09.004.

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