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Interactive comment on “Drilling disturbance and constraints on the onset of the Paleocene/Eocene boundary carbon isotope excursion in New Jersey” by P. N. Pearson and E. Thomas

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General Comments:

This is a very interesting and insightful manuscript about the timing and thus nature of the PETM and associated carbon perturbation. The PETM has received much attention and numerous publications as it has been suggested as a potential analogue for modern climate warming. This is particularly important as a recent publication (Wright and Schaller, 2014) from the Malboro Clay at Millville (ODP Leg 174X), suggests that the carbon perturbation was effectively instantaneous over geological time (through the interpretation of the presence of thirteen annual layers during the initiation of the carbon

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isotope excursion). In this manuscript, the authors present an alternative explanation for the observed layering in the Millville core, expanding on initial core observations by Pearson and Nicholas (2014) which indicate that the “annual” layering was a result of drilling mud injection and biscuiting. Further evidence in the form of foraminifer concentrations and re-interpretation of outcrop exposures is also presented in support of their argument. Given the significance of the topic, this manuscript is suitable for *Climate of the Past* and is well written and presented. The manuscript raises important concerns over the interpretation by Wright & Schaller (2013), something which I am surprised was not identified in that publication. The manuscript is of high quality, however, I feel that this manuscript also falls slightly short of the stated conclusions, particularly given the reply by Wright and Schaller (2014) to Pearson and Nicholas (2014), Stanssen et al (2014) and Zeebe et al (2014). Certain aspects could be expanded on and improved before publication.

Specific Comments:

1. Interpretation of core: drilling disturbance The evidence the authors present is compelling and the discussion well presented. The occurrence of drilling buscuiting is a common phenomenon and in my experience does not require borehole overpressures (although this would facilitate). The core photographic evidence (Figure 2) is clear; evaluation of rotary grooves on the contacts of the partings (Figure 4) suggestive of core spinning within the barrel is valid; which combined with reference to the operations difficulties and concerns all support drilling disturbance as a valid interpretation for both Millville and Wilson Lake cores. I have no issues over this aspect of the manuscript. However, the occurrence of drilling disturbance does not necessarily mean that a cyclic nature of the sedimentation is purely mechanically derived. I would suggest the authors have a more balanced discussion of the data and possible alternative scenarios so that the reader can make an informed decision of the nature of the layering in the core and outcrop. The current discussion and data presented does not definitely rule out a primary environmental signature to the layering.

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a. 'Regularity has a mechanical origin related to strength of formation and torque. . .' (p. 3310, lines 1-4). In addition, the rate of penetration (ROP) related to torque also has an impact and in many cases is also 'driller' specific, so is hard to explain similar regularity in several cores drilled by different parties. In this sentence, can the authors reference work on rock mechanics/physics to further clarify these relationships? In addition, if the drilling logs are available for the Millville core operations both torque and ROP should be documented and might provide further insight (in supplemental information).

b. More likely, I would agree with the statement on page 3308; line 22-24 as also presented by Wright and Schaller (2014) in the reply to Pearson and Nicholas (2014) that mud injection would follow 'pre-existing zones of weakness'. I would like to see an expansion of this discussion (as in points below).

2. Primary deposition versus injection of drilling mud

a. The manuscript would benefit with a wider discussion on this topic. The authors mention the possibility contamination of the d18O signature due to injected drilling mud (p. 3310; lines 12-14). I would have thought the drilling mud (water based; presumably bentonite additive from quarries of the Wyoming bentonite) would be devoid of calcite and prevent extraction of isotopic data for these intervals. However there seems a drift in values that follow the trend of the CIE (Figure 1) suggesting a genuine primary isotopic signal. The authors also mention in the text that maxima correspond with the thin smectite layers (page 3307, line 6), so perhaps this could genuine, or a mixture of drilling mud (if contaminated with additional calcite additive?) and sedimentary signal. However, reviewing Wright and Schaller (2013; Fig S2) I do not agree with the contention that the maxima in d18O correspond with the smectite layers.

b. Ideally, the authors would have stable isotope values of the drilling mud to determine contamination, or as mentioned in Pearson and Nicholas (2014) some geochemical fingerprinting of the mud to demonstrate the cyclic nature observed in the isotope data is indeed contamination. If this data is not available from the original drilling mud or re-

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sampled Wyoming bentonite, this should be at least mentioned and the authors expand on the discussion in Pearson and Nicholas (2014).

c. Of note, if bentonite water based mud was used in the operations, then by nature these would be smectite rich, which if injected into the core would be similar composition to the thin smectite layers described there. Bentonite drilling mud would also have clear elemental signature (high Thorium for instance), which unfortunately Thorium concentrations was not presented in the core XRF data of these thin smectite layers by Lombardi (2014).

d. I would like to see Figure 1 amended to include: d18O (as this is mentioned in the text); calcite (%) values (Wright and Schaller, 2013, supplemental data 04) alongside the core photograph, or symbology to show which samples come from the thin interbeds/injected mud and adjacent clay. This would allow evaluation of the data and enable this manuscript to stand-alone and be more robust.

3. Re-interpretation of field photograph. As rightly mentioned this photograph (Fig 3) was never intended as definitive evidence (page 3310; lines 17-20). I am not convinced that reproducing the photograph here is beneficial as in my opinion reinforces poor photograph of a poor outcrop. I would recommend just referring to Fig. 1 (Wright and Schaller, 2014) and keep the statements about intent and need for further investigation. As such I am also not convinced any interpretation of this photograph is very robust and can be easily disputed, particularly as the cutting tool marks are so prominent. The authors interpretations of joint surfaces are just as valid as sedimentary layers, but the uncertainty should be emphasized (unless I missed that either Pearson or Thomas visited the outcrop in person). I would also suggest that the co-ordinates of this outcrop be stated (can these be obtained from Wright and Schaller) as this outcrop would seem unique in identifying either cyclic layers or joints in the Malboro clay compared with other studies (page 3311; lines 8-13). If the outcrop cannot be located this should also be mentioned.

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4. Foraminifera accumulation rates. In this section the authors expand on previous arguments by Stassen et al (2014) and reply with Wright and Schaller (2014). The authors arguments regarding interpreted depositional environment, accumulation rates and calculated revised foraminiferal abundances are very interesting. Here, Pearson and Thomas calculate from foraminifera abundances that the CIE onset at Millville likely represents thousands of years. If this is the case, it would be good to expand on this discussion to include that if the layering observed is genuine, what would be the predicted cyclicity – if not annual then what? - Milankovitch cyclicity (obliquity, precession) or possibly sub-Milankovitch century scale variations (e.g. Bond, DeVries)? It is strange that annual layers were the preferred interpretation by Wright and Schaller (2013) when Milankovitch cyclicity on the CIE initiation has already been observed and independently dated with U-Pb geochronology (e.g. Charles et al. 2011; Harding et al. 2011). The manuscript would benefit from a wider discussion of potential alternative interpretations of periodicity of the layering (if not purely mechanical induced) and the resultant impact on foraminifera accumulation rates. In addition, I also note that expanded PETM sections with similar sediment accumulation rates are also observed elsewhere, such as the North Sea where Eldrett et al (2014) recorded significant changes in regional vegetation assemblages/biomes, which if the CIE initiation was thirteen years would be impossible for the vegetation dynamics and response observed in those cores.

Overall, this manuscript and data presented clearly demonstrates that the layering presented by Wright and Schaller (2013) are affected by drilling disturbance, but currently does not irrefutably demonstrate that the layers do not preserve some primary signal and could be cyclic (century to millennia).

I hope this review is useful to the authors. James Eldrett

Additional References:

Charles et al. 2011, Constraints on the numerical age of the Paleocene-Eocene bound-

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Harding et al. 2011. Sea-level and salinity fluctuations during the Paleocene–Eocene thermal maximum in Arctic Spitsbergen, Earth and Planetary Science Letters 303, 97–107

Eldrett et al. 2014. A seasonality trigger for carbon injection at the Paleocene–Eocene Thermal Maximum. Clim. Past, 10, 759–769, 2014

Lombardi C (2014) Lithostratigraphy and Clay Mineralogy of Paleocene-Eocene Thermal Maximum Sediments from Wilson Lake, NJ. MSc thesis (Rutgers Univ, New Jersey).

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