Clim. Past Discuss.,
https://doi.org/10.5194/cp-2018-121-AC1, 2019

Interactive comment on "Mercury anomalies across the Palaeocene-Eocene Thermal Maximum" *by* Morgan T. Jones et al.

Morgan T. Jones et al.

m.t.jones@geo.uio.no

Received and published: 19 January 2019

We thank the reviewer for this thorough and constructive review. We acknowledge that the manuscript can be improved in terms of clarity, particularly regarding the uncertainties introduced by post-depositional processes. To that end, we have made edits to the discussion and conclusion, further clarifying current limitations of the dataset and identify important unknowns to move the field of Hg as a volcanic proxy forward. Specific comments (marked by parentheses) are addressed below

[Abstract, Line 21: I assume the CIE you are referring to is the PETM, but that is not clearly stated.] We have added 'PETM' to make that clear in the text

[Page 3, Line 4-5: A "strong positive correlation" is a bit imprecise, I would rather

state that a close temporal coincidence between several LIPS and mass extinctions has been noted.] We have adapted the text based on the reviewer's recommendation. The sentence now reads: "There is a close temporal coincidence between the emplacement of LIPs and both rapid climate change events and mass extinctions in Earth history (Courtillot and Renne, 2003; Wignall, 2001), suggesting a possible causal connection."

[Section 3.4: I suggest that the description of the PETM in the ACEX hole be amended to reflect that the onset of the PETM is clearly missing, with the underlying 50cm (separated by a core gap) "disturbed by drilling and various proxies suggests that the sediment from this interval represents a mixture of uppermost Palaeocene and PETM material" (Sluijs et al, 2006).] We have amended the text accordingly. The sentences now read: "Core recovery of PETM strata was poor, with the onset of the PETM completely missing. Only 55 cm of disturbed core (302-31X) was recovered from anywhere between 388 meters composite depth (m.c.d., the top of core 302-32X) and 384.54 m.c.d. (the bottom of core 302-30X). The core material was disturbed during drilling and this interval represents a mixture of uppermost Paleocene and PETM material (Sluijs et al., 2006)."

[Page 8, Line 8: "and represents the most distal locality to the volcanic activity of the NAIP studied here." Is it more distal than Dababiya?] Dababiya is more distal than the Bass River locality, but Dababiya is not one of our sections. We have amended the text accordingly to draw the attention of the reader to the fact. The text now reads: "The site represents the most distal locality to the volcanic activity of the NAIP studied here, although the previously studied Dababiya section (Keller et al., 2018) is more distal."

[Page 12 Line 23: "It is conceivable, but unlikely, that the Hg/TOC anomaly at Dababiya could purely be a product of diagenetic and weathering processes, given the amount of dissolution and acidification observed at this site (Figure 9; Keller et al., 2018; Khozyem et al., 2015). However, the effects of such processes on Hg/TOC ratios are poorly understood." Well, this begs the question then, could not the Hg/TOC anomalies (or lack thereof) at all of the sites be influenced by the effects of dissolution and diagenesis?] The potential role of weathering is significantly different between the Dababiya section and the sites studied here. Four of the five localities are cored sections of marine sediments, which means it is very unlikely there has been any subaerial weathering. The Fur Island section is a rapidly eroding cliff face, retreating at a rate of approximately 0.5-1 m per year. Soil formation on top of the section is also limited and we argue the chances of significant surface weathering are very low. Although the degree of thermal maturation changes from immature (Fur, Bass River, ACEX) to somewhat mature (Svalbard, Grane) for all our localities, organic matter is well preserved. In contrast, the Dababiya section and other sites in Egypt are characterised by very degraded organic matter, signalling alteration of the OM through weathering and/or excessive heating (e.g. Speijer & Wagner, 2002). We further clarified this important difference in our revised text, adding a paragraph at the beginning of the section 5.2 – "Other influences on Hg/TOC values."

[All of the sites here have large changes in lithology across the PETM, which of course suggests variable susceptibility to diagenetic alteration, with Svalbard apparently the lone exception. Perhaps the authors should allow that the records at all other sites may be dominantly controlled by the changes in lithology, which preserve mercury and TOC to various degrees, while Svalbard (which actually has quite a convincing Hg anomaly coincident with the PETM), due to being fairly homogenous clay throughout, might be the most reliable record.] In terms of diagenesis, we cannot completely exclude the possibility of these processes affecting these sections, with possible knock-on effects to the resulting Hg and Hg/TOC signals. The theory the reviewer proposes seems entirely plausible for some of the signals we observe (such as the broad Hg/TOC increase during the PETM CIE at Svalbard), However, given recent evidence (Them et al., 2019), we would in fact expect an anomaly in the other direction. We also emphasize it is difficult to reconcile variable diagenetic effects and the sharp Hg/TOC anomalies within homogenous lithological layers, such as we observe at Fur and Svalbard. We have added a paragraph to the discussion (section 5.2 – "Other influences on Hg/TOC val-

ues") to discuss this further, and included the recent work by Them et al (2019), which considers these problems in more detail.

[Page 13, Line 22: "In Svalbard, palynological evidence indicates that there was a distinct transient shift towards marine-derived organic matter across the PETM (Harding et al., 2011). The organic matter before and after the CIE is dominated by terrestrially-derived phytoclasts of cuticle and wood, while the body of the PETM is largely comprised of amorphous organic matter and marine dinocysts (Harding et al., 2011)." Well, that gives me second doubts about Svalbard being the most reliable record - if the nature of the organic matter (that hosts the Hg signal) is changing dramatically, then it's entirely possible that the trends preserved in the record are a result not of original Hg deposition, but how that Hg survives the ravages of time and diagenesis. It seems to me that there's a potential problem here with the fidelity of the records. The effects of changes in the host organic material, subsequent dissolution, diagenesis and weathering on the preservation of Hg in sediments are all relatively unconstrained. It would be one thing if you had a set of sites of varying lithological changes, that all showed the same Hg/TOC trend. Then you could argue that the Hg/TOC trends are robust to diagenesis. But instead, I see a number of sites with varying lithologic changes, that all show quite different trends in Hg/TOC. Now, I'm not suggesting that there's nothing to be learned, but perhaps the authors should stress that constraining the effects of all of those secondary processes is crucial to the interpretation of these Hg/TOC records. Before we have that information, it's difficult to make conclusions about NAIP volcanism and the PETM. These complications are discussed in sections 5.2, but not afterwards - in section 5.3 and in the conclusions the records are interpreted as is they are known to be robust. I find this to be an omission, and suggest adding statements to the effect of: Hg/TOC anomalies at individual sites (or the lack thereof) may reflect changes in the way Hg is preserved in sediments, not related to NAIP volcanism.] As mentioned above, we have added to the discussion (5.2) that adds more clarity to the relative importance of various unknowns in the Hg cycle, and what this means in terms of limitations to the conclusions that can be drawn from this study. We have also added sentences to the Abstract, Section 5.4, and the Conclusion to address these uncertainties.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2018-121, 2018.