We thank the reviewer for their time and effort in reviewing our manuscript. Below are our detailed point-by-point replies and suggested manuscript improvements (blue) for each comment (black).

This study examines the record of the Carnian Pluvial Event (CPE) in a drillcore through continental facies in northern Ireland. The main goal is to evaluate the relationship of the CPE to volcanic activity (proxied by Hg concentrations), which is assumed to be associated with the Wrangellia LIP.

Summary: There are many problems, both large and small, with this study that preclude publication in its present form. It can either be rejected or assigned "major revision followed by re-review", depending on the editor's preference.

Response: We thank the referee for the report and the annotated manuscript. We accept that the documentation of the weathering-index calculation, the statement of the study goals, several statements in the Introduction, and the framing of the Hg data needed correction. The main changes made in response are: (1) the weathering indices are no longer used as a measure of weathering intensity and Figure 3 has been withdrawn; (2) the four Introduction statements at L33–L41 have been corrected; (3) the final paragraph of the Introduction now states the study questions; (4) the Hg data are presented as a small, reproducible anomaly whose source cannot be uniquely identified from concentration data alone, and the link to Wrangellia is presented as a possibility consistent with published records rather than as a finding; (5) the claim of a single, stepped excursion has been replaced by a quantitative description of the record, including a statistical test of the "stepped" structure (see also our reply to Referee 2); (6) the figures have been revised as listed below. Each comment and each margin annotation is answered in turn. Because Figure 3 has been withdrawn, former Figures 4 and 5 are now Figures 3 and 4; figure numbers in these replies refer to the preprint unless stated otherwise.

Weathering proxy calculations: In looking at Figure 3, I realized that the calculated CIA values are completely in error. CIA values begin around 40-50 for fresh (unweathered) igneous rocks and go to 100 for highly weathered sediments. All of the values here are <25, which probably means that carbonate CaO has not been subtracted out. CIA is a proxy for silicate weathering intensity, so CaO associated with carbonates and phosphates must first be removed, yielding a silicate CaO value (designated CaO*). The authors seem completely unaware of this (while the first two authors may be students, why didn't any of the more senior authors further down the authorship list catch such a serious problem??). The same thing applies to the CIW values.

Response: We thank the referee for this comment, which led us to re-examine the index calculations. The Ca-bearing indices were calculated with CaO* following McLennan (1993), in which CaO is corrected for apatite using P_2O_5 and, where the remaining CaO exceeds Na_2O, is set

equal to Na_2O; recalculating the indices from the oxide data reproduces the tabulated values. The referee is right that this procedure cannot remove Ca hosted in carbonate and sulfate, which in this halite- and anhydrite-rich succession is most of the Ca. The very low index values through most of the core (CIA ~13) therefore reflect Na hosted in halite and Ca hosted in sulfate and carbonate, as Section 5.1 of the preprint noted (L267–278), and they are not weathering intensity values. We also agree with the margin note on Figure 3 that the seven indices share the same oxides and are not functionally independent. The referee's reading that the values are almost all below 25 is correct for most of the core (76 of 94 samples; median 13). Six samples between 474 and 485 m, where Na_2O falls below 3 wt.%, have CIA values of 45–61, but that interval coincides with the sandstone beds, so the change records lithology as much as weathering. In the revised manuscript, the indices are no longer used as a measure of weathering intensity: Figure 3 and the paragraphs interpreting the indices in Sections 4.1 and 5.1 have been withdrawn; the Methods now state the CaO* procedure and its limitation and cite Algeo, Hong and Wang (2025) alongside McLennan (1993), and the Coolmaghra Skerry interval is described from its sedimentology and the siliciclastic input proxies Si/Al and Zr/Al (Fig. A1). The index values remain available in the data repository.

Significance of volcanic proxy evidence: The authors document modest increases in Hg concentrations in the study core, but the significance of these data is uncertain. Rather than volcanic inputs, higher Hg concentrations might have resulted from redox changes (e.g., a shift to more reducing conditions, e.g., Shen et al. 2019) or decreased sedimentation rates (e.g., Liu and Algeo, 2020). Demonstrating that the increased Hg concentrations were due to enhanced volcanic Hg fluxes generally requires paired Hg isotope data, with a shift to near-zero values of D199Hg reflecting volcanic inputs.

Shen, J., Algeo, T.J., Chen, J., Planavsky, N.J., Feng, Q., Yu, J. and Liu, J., 2019. Mercury in marine Ordovician/Silurian boundary sections of South China is sulfide-hosted and non-volcanic in origin. *Earth and Planetary Science Letters*, *511*, pp.130-140.

Liu, J. and Algeo, T.J., 2020. Beyond redox: Control of trace-metal enrichment in anoxic marine facies by watermass chemistry and sedimentation rate. *Geochimica et Cosmochimica Acta*, *287*, pp.296-317.

Also, even if the increased Hg concentrations represent volcanic inputs, there is no evidence showing that the volcanic Hg came from the Wrangellia LIP—this is merely a speculation. Speculation is okay but it must be acknowledged as such in the text.

Response: We agree that the Hg enrichment in the CRN-1 core is modest and that Hg concentration data alone cannot demonstrate a volcanic input. We have therefore reframed the Hg data throughout.

The Abstract now reads: ‘The high-resolution organic carbon isotopes document a negative excursion of ~2‰ between interval means (up to ~4‰ between individual samples), accompanied near its onset by a small, reproducible rise in Hg concentrations above background levels, consistent with external Hg loading during the CPE interval.’ The elevated values are now described with their positions and their reproducibility: 4.7 ng/g at 512.45 m, ~4 m below the onset of the excursion, and 6.2 ng/g in two adjacent samples at 503.62 and 503.97 m, ~4–5 m above it. All three samples were re-analysed and the repeat values (4.8, 6.4 and 6.3 ng/g) agree within 3 %; ten core samples were re-analysed in total, with a median difference of 3 %. This information, which was missing from the preprint, has been added to the Methods and to Section 4.1.

Response (continued): On the two papers cited by the referee: Shen et al. (2019) and Liu and Algeo (2020) concern anoxic marine settings in which Hg is sulfide-hosted or in which trace-metal enrichment is governed by watermass chemistry and sedimentation rate. The CRN-1 succession is an oxidised, evaporitic red-bed sequence, so a sulfide host is unlikely here, but the general lessons of both papers apply: the host phase must be identified, and dilution or changes in sedimentation rate must be considered. We had not addressed either point. Section 5.5 now states that the host phase of the Hg cannot be identified with the available data (no sulfur or mineralogical data), that a change in sedimentation rate or dilution by halite and anhydrite cannot be excluded, and that Hg isotopes were not measured. Hg isotope analysis has not been applied in most CPE studies, in which a volcanic contribution has been argued from Hg concentrations together with carbon isotope and stratigraphic evidence, and it is not required for the interpretation presented here. It is also costly, and at the Hg concentrations of the CRN-1 samples (mostly well below 1 ng/g) it would require impractically large sample masses; it was therefore not a reasonable undertaking within the scope of this PhD project. The Fe-based (pyrite) argument of the preprint and the Hg/Fe panel have been removed. Section 5.5 now reads: ‘These low correlation coefficients are interpreted cautiously because many Hg values are close to analytical uncertainty and because paired data exist for only part of the sample set. The association of the elevated, reproducible Hg values with the onset of the negative CIE (at 512.45 m, ~4 m below it, and at 503.62–503.97 m, ~4–5 m above it), as also observed in other CPE sections (Fig. 5), is consistent with a short-lived, modest enhancement of Hg delivery to this continental basin, possibly associated with increased magmatism (such as Wrangellia LIP emplacement) at that time. The host phase of the Hg cannot be identified with the available data (no sulfur or mineralogical data), a change in sedimentation rate or dilution by halite and anhydrite cannot be excluded, and Hg isotope analysis, which can help to discriminate volcanic from other Hg sources, was not attempted because the Hg concentrations in most samples are too low for routine isotope analysis; the volcanic attribution therefore rests on the stratigraphic association with the carbon isotope excursion and on comparison with published CPE records.’

The brevity and small size of the anomaly are not in themselves evidence against an external source. Because the atmospheric residence time of Hg is short (0.5–1 yr; Pyle and Mather, 2003; Selin, 2009) and large igneous province volcanism is pulsed, whether a sedimentary record captures a volcanic Hg signal depends on whether sediment was deposited and preserved during individual eruptive pulses (Kovács et al., 2020). In a distal, evaporitic red-bed succession such as CRN-1, a brief, modest anomaly is the expected expression of such a flux. We present this as a possibility, not as a demonstration.

On Wrangellia: we agree that the attribution is speculative and it is now labelled as such wherever it appears. The Abstract clause has been removed, and the Conclusions now read: 'The attribution of the CPE to Wrangellia volcanism rests on published Hg and $\delta^{13}C$ records from other localities; the CRN-1 Hg record is consistent with, but does not independently demonstrate, that attribution.' The sentences in the Conclusions that stated the magmatic forcing and a "short lag" as fact have been reworded accordingly.

The Introduction is full of factual misstatements—I identified four errors on lines 34-42.

Response: The four statements are at L33 (herbivores), L35 (coal gap), L39 (modern mammals), and L40–41 (ecosystem recovery). Each has been corrected; see the replies to the individual comments below and, for L40–41, the reply under "Margin annotations" at the end of this response. We have also corrected two further statements in the same paragraph (the marine groups and the first global appearance of amber).

Project goals--What are the study goals? They need to be articulated in the final paragraph of the Introduction. The text is just stating what the authors did, not what the project goals were.

Response: The final paragraph of the 'Introduction' has been rewritten to state the questions the study addresses rather than the datasets generated. It now reads:

'Here, we present new high-resolution geochemical data from the Knocksoghey Formation of the Triassic Mercia Mudstone Group in Northern Ireland, as studied in the Carnduff-1 (CRN-1) core of the Larne Basin. By integrating stable organic carbon isotope measurements, Hg concentrations, XRF-derived elemental concentrations, and detailed sedimentological observations, this multi-proxy approach aims to determine whether this continental evaporitic succession preserves evidence of a CPE-related carbon cycle perturbation, whether that perturbation is expressed as one prolonged excursion or as several discrete NCIEs, whether there is evidence of external Hg loading, and what the associated sedimentological and environmental responses were. We also present a new Hg record from the Luojiagou section (Hubei, South China) for comparison.'

Figure 1—the Wrangellia LIP is shown as a huge feature, far larger than any modern continent. The actual size might have been ~20% of what is shown here.

Response: We agree. Figure 1 has been redrawn. Wrangellia is no longer shown as a single large outline: the Wrangellia flood basalts are now shown as discrete units at a much smaller size, and the other Middle–Late Triassic volcanic provinces and complexes are numbered on the map and named in a legend. The caption states that the paleogeographic reconstruction is modified from Tomimatsu et al. (2021) and gives the sources for the locations of the volcanic complexes.

Other figures—nearly all of the figures need various improvements per the comments that I have added to them.

Response: The figure changes are: Figure 1, Wrangellia redrawn as discrete flood-basalt units at a much reduced size, volcanic complexes numbered on the map and named in a legend, source of the reconstruction stated (Tomimatsu et al., 2021), place names corrected; Figure 2, star moved to north-east Ireland in inset A, grey shading over the excursion interval, Fe and Hg/Fe panels removed, Hg axes in ng/g, font sizes increased; Figure 3, withdrawn; Figure 4 (now Figure 3), shaded intervals labelled CPE-CIE and T-CIE and defined in the caption; Figure 5 (now Figure 4), dashed correlation line at the onset of the negative excursion in each section; Figure A1, age column, CPE shading and depth label added, (Rb+Zr)/Sr cited; Figure A2, onset of the excursion marked in every section.

Language—there are numerous errors in verb tense, subject-verb agreement, word choice, grammar and other issues. I have pointed some of them out with comments, but one of the native-English-speaking coauthors needs to go over the manuscript carefully and correct all of the language errors.

Response: We thank the referee. The errors noted in the annotated manuscript have been corrected, and the revised manuscript has been edited throughout for verb tense, subject–verb agreement, word choice, and consistency of style; changes are shown in the tracked-changes version. The use of an AI tool for spelling and syntax checking is declared in the Acknowledgments.

Comment (L33): Wasn't it the primitive (crocodylian) archosaurs that were being replaced? They were carnivores, not herbivores.

Response: Bernardi et al. (2018), whom we cite, describe dinosaurs opportunistically replacing herbivorous groups (rhynchosaurs and most dicynodonts) after the CPE, while also noting the gradual displacement of crocodile-line archosaur faunas. The original sentence did not say which groups were meant, and “increase in dinosaur population” was loose. The sentence has been revised to name the groups, and Nesbitt et al. (2017), which concerns Middle Triassic bird-line archosaurs, has been removed from it:

‘Another striking change was the diversification and increased abundance of dinosaurs, alongside the decline and extinction of key herbivorous groups such as rhynchosaurs and most dicynodonts (Benton et al., 2018; Bernardi et al., 2018).’

Comment (L35): The Coal Gap ended in the Anisian Stage of the Middle Triassic, about 15 Myr earlier than the CPE.

Response: We agree: the coal gap ended in the Anisian (Retallack et al., 1996). The sentence has been revised so that the CPE is described as a period of expansion of peat-forming environments, not their re-establishment:

'Peat-forming environments, which had re-established in the Middle Triassic after the Early Triassic coal gap, expanded markedly in several regions during the Carnian (Retallack et al., 1996; Dal Corso et al., 2020; Zhang et al., 2023).'

Comment (L39): There were definitely no "modern mammals" in the Carnian; there were only cynodonts, which were ancestors to mammals.

Response: We agree. The clause referring to "modern mammals" has been removed from the Introduction, and the two references that supported only that clause (Datta, 2005; Preto et al., 2014) have been removed from the reference list.

Comment (Figure 1): This depiction of Wrangellia must be too large--it is shown larger than any modern continent, which is just not possible. Probably needs to be reduced to ~20% of the present size.

Response: See our reply to the Figure 1 comment above.

Comment (L71): What are your study goals? they need to be articulated in the final paragraph of the Introduction. You are just stating what you did, not what the project goals were.

Response: The final paragraph of the Introduction now states the study questions; the full text is given in our reply to the "Project goals" comment above.

Comment (Figure 2): This map shows NE Ireland, yet the yellow star in the inset map has been placed on NW Ireland. The star needs to be shifted rightward.

Need to highlight the interval of the CPE with background shading.

Response: Thank you for identifying this error. The star in inset A has been moved to north-east Ireland. The stratigraphic panels now carry grey shading over the interval of the negative carbon isotope excursion (508.4–449 m), the Fe and Hg/Fe panels have been removed, and the Hg axes are in ng/g; the caption defines the shaded interval. [Shading in the redrawn Figure 2 ends at ~457 m: extend it to 449 m or amend this reply.]

Comment (Figure 3): Essentially all of these weathering index values are incorrect; you have probably not correctly factored out carbonate CIA. You cannot use total CaO--it is essential to subtract out carbonate CaO to arrive at silicate CaO (which is designated CaO*).

Response: See our reply to the weathering-index comment above: Figure 3 has been withdrawn together with the index interpretation. The two margin notes on this figure, that the seven indices are not functionally independent and that a CIA value of 13 cannot be a weathering value, are both accepted and are now stated in Section 5.1.

Comment (Figure 4): Label the CPE and T-OAE intervals.

Response: The shaded intervals are now labelled “CPE-CIE” and “T-CIE” and are defined in the caption, which also states that the records are plotted against depth and that no duration is implied by the comparison.

Comment (Figure 5): It would help to add inferred correlation lines among these sections.

Response: Figure 5 (now Figure 4) has been redrawn with a dashed correlation line at the onset of the negative carbon isotope excursion in each section. The caption states that the records are plotted against depth and that the line is an inferred correlation, not an isochron.

Margin annotations in the annotated PDF not covered above

Abstract (“coincided”, “data demonstrate”) and L36/L56 (“perturbation”), L73 (“strata”): all changed as suggested; “perturbation” now also replaces “perturbance” in the title and in Section 5.5.

L40–41 (“peak of recovery … recovery was essentially complete by the Anisian”): We agree that recovery sensu stricto was largely complete during the Anisian. The sentence followed Callegaro et al. (2023), who use “recovery” for the radiation of modern clades. It now reads: ‘… a significant phase of ecosystem reorganization and radiation of modern clades, well after the initial recovery from the end-Permian mass extinction …’.

Figure 3 margin notes (indices not functionally independent; CIA of 13 in error): accepted; see the reply on the weathering-index calculations.

Figure 4 margin note (Remírez and Algeo, 2020): the humid character of the T-OAE is supported by several lines of evidence already cited in Section 5.4 (Os isotopes, Cohen et al., 2004; clay mineralogy and sedimentology, Xu et al., 2018; Izumi et al., 2018; Kemp et al., 2024). We have added Remírez and Algeo (2020) as salinity-proxy evidence alongside these. [M. Ruhl to confirm wording.]

Figure A1 margin notes: an age column, CPE shading and the label “Depth (m)” have been added. (Rb+Zr)/Sr is a published grain-size and provenance ratio (Dypvik and Harris, 2001), now cited, and its purpose is stated in the caption; Si/Al and Zr/Al carry the main argument.

Figure A2 margin note: the onset of the negative excursion is now marked in every section in Figures 4, 5 and A2.