Marwa Mohamed Shahid and co-authors present a largely geochemical multi-proxy dataset of an interval of Upper Triassic strata from the Carnduff-1 core, identified by the authors as a record of the Carnian Pluvial Episode (CPE). The authors argue that these data show that - contrary to previously published accounts - the CPE was characterised by a stepped singular carbon isotope excursion, that Hg enrichment in the core is consistent with the perturbation being LIP-induced, and that weathering indices in the core and shape of the C isotope excursion as well as known biological turnover establish strong genetic similarity with other carbon isotope excursions that are linked with LIP volcanism, notably the Toarcian Oceanic Anoxic Event.

The authors - in my opinion - present a valuable dataset from an interesting location, supplemented further by some additional Hg data from a section in China, and therefore I think there is definitely merit in publication of these data. However, I equally think there are numerous aspects for this study that would benefit from further work to make the contribution as impactful as possible.

We thank Dr Ullmann for a detailed and constructive review. In response we have reframed the central claim (the CRN-1 record is now described as a broad negative excursion followed by a second interval of low values, in which discrete NCIEs cannot be resolved with confidence; “single” and “symmetrical” have been withdrawn, and “stepped” has been replaced by a statistical comparison of models on the raw data), added a discussion of stratigraphic completeness and sedimentation rate, withdrawn the weathering indices, reduced the T-OAE comparison to context, expanded the QA/QC information for XRF, isotope and Hg analyses, presented the Hg data as a small anomaly of uncertain origin, and removed the closing paragraph on modern pluvial events. Below are our point-by-point replies (blue) to each comment (black), quoting the revised manuscript wording where a change was made.

**Identification and character of the CPE**

I am not an expert of the CPE and I hope another reviewer with stronger research background on Triassic stratigraphy will be able to comment on some of the findings with more authority than I can. However, I feel that the authors are a bit too optimistic about the quality of their core record by making statements that would have far-reaching consequences for other CPE sections.

- Given that the authors present only relatively loose stratigraphic constraints for a succession in which I think stratigraphic completeness and consistent sedimentation rates are unlikely, how robust is the assertion that Carnduff-1 contains a superior CPE record that allows for a re-evaluation of the nature of the CPE carbon isotope excursion? Despite the large-scale changes in lithology, can changes in sedimentation rate that affect the shape of any isotope excursion or chemostratigraphic trend be ruled out, or at least constrained to some degree?

Response: We accept this. The preprint did not state that the record is superior, but Sections 5.2 and 5.3 implied it, and we have removed that implication. The sedimentological record reflects episodic deposition rather than continuous sedimentation: flooding–evapoconcentration cycles, dissolution surfaces, mudcracks and paleosol features in the neighbouring CRN-2 core (Andeskie et al., 2018) indicate repeated wetting, evaporation, desiccation and exposure in a saline-lake to mudflat system. The stratigraphic framework for CRN-1 rests on regional lithostratigraphic correlation and limited palynology, and there is no independent age–depth model for the studied interval. Changes in sedimentation rate therefore cannot be ruled out, and they can be constrained only loosely: if the ~59 m of the excursion (508.4–449 m) represent the 0.8–1.6 Myr usually assigned to the CPE, the implied accumulation rate is ~37–74 m/Myr, several times the long-term average for the Knocksoghey Formation, so either accumulation was much faster during the excursion or the excursion records more or less time than the CPE as a whole.

Section 2 now states: 'No independent age model exists for the CRN-1 succession itself. The facies (desiccation features, displacive evaporites and discrete event beds) indicate episodic deposition with probable hiatuses, so sedimentation rates through the studied interval cannot be assumed to have been constant, and thicknesses are not converted to durations in this study.' The interpretation in Sections 5.2, 5.3 and the Conclusions has been reworded accordingly (see the replies to L320, L472 and L473–475). 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.

- If the authors are right in the proposal of a single, drawn-out, stepped carbon isotope excursion, then how are the other published accounts to be re-interpreted that note multiple isotopic perturbations separated by returns to pre-excursion values? Are they all affected by stratigraphic repetition, peculiar organic matter mixing, etc.? This seems rather an unlikely hypothesis to me given the alternative that Carnduff-1 is not as continuous and simple as the authors present it to be. In any case the authors do not elaborate on any of the issues that would need to affect other known sections to allow for the interpretation of a single isotope excursion to be correct, and I think this side of the story needs to be looked at as well to arrive at a plausible interpretation.

Response: We agree, and we have withdrawn the proposal that CRN-1 requires a re-interpretation of the published multi-excursion records. Several sections with independent age control record multiple NCIEs separated by returns towards baseline, whereas CRN-1 has no age model and episodic deposition. Re-examining our own data in response to this comment, we also find that the record is not a single excursion: the interval means fall from −25.8‰ (520–508.4 m) to −28.1‰ (490–477 m), recover to −26.4‰ at 449–440 m, and fall again to −27.6‰ between 440 and 376 m.

Section 5.2 now reads: ‘The record from the Knocksoghey Formation of Northern Ireland studied here exhibits a broad, prolonged negative excursion between ~507 and ~449 m, followed after a brief recovery (449–440 m) by a second interval of low values that continues to the top of the sampled succession at 376 m. Discrete NCIEs cannot be resolved with confidence. Within the main excursion, 11 of 104 samples return to −26.5‰ or heavier, adjacent samples differ by a median of 0.8‰, and the residual scatter about any fitted trend is ~0.9‰, about three times the precision of the standard. A comparison of statistical models for the falling limb (520–470 m, n = 126) shows that a decline in two steps (~1.4‰ at ~506.6 m and ~0.8‰ at ~493 m) and a smooth decline describe the data equally well (difference in Bayesian information criterion < 2), and that both are preferred to a single step or a linear ramp; a stepped structure can therefore be neither demonstrated nor excluded. The second interval of low values above 440 m may correspond to a later NCIE. Three explanations for the overall pattern are possible: the record is incomplete and preserves only part of the CPE; condensation or hiatuses have merged separate excursions; or the carbon cycle perturbation was more sustained at this site than elsewhere. The CRN-1 record therefore does not by itself require a re-interpretation of the sections in which multiple excursions are documented.’ The Abstract, Section 5.3, and the Conclusions have been revised in the same way.

- The authors consistently assert that the CPE in Carnduff-1 is characterised by a stepped isotope excursion, but I really cannot see this in the presented data. The raw data is quite noisy, the CPE interval is rarely highlighted in the plots in detail, and the authors themselves use smoothing lines of the data (Fig. 4) that obscure any possible existence of steps. Can some information be added how these steps were identified and how robust they are?

Response: We agree that the steps cannot be seen by eye and that a smoothed curve cannot show them, so we have tested the claim on the raw data. For the falling limb (520–470 m, n = 126), we compared a linear ramp, smooth declines, a plateau–ramp–plateau model, and models with one to five steps whose positions were fitted rather than chosen, using the Bayesian information criterion (BIC). A decline in two steps (~1.4‰ at ~506.6 m and ~0.8‰ at ~493 m) and a smooth decline fit equally well (ΔBIC < 2), and both are preferred to a single step or a linear ramp (ΔBIC > 7). The step positions are stable when single samples, or all samples with TOC ≤ 0.05 wt.%, are removed, but the second step is smaller than the sample-level scatter (~0.9‰). We therefore no longer describe the excursion as stepped: the text states that it begins with an abrupt shift at ~506.6 m and that a stepped structure can be neither demonstrated nor excluded. “Stepped” and “stepwise” have been removed from the Abstract, the Figure 2 caption, Sections 5.3–5.5 and the Conclusions; the model comparison is reported in Section 5.2 and added to the supplementary material; and the smoothed curve in Figure 4 now has its method and window stated in the caption.

- The authors note (based on the putative stepped nature of the CPE C isotope excursion) a strong similarity to the Toarcian OAE, and by extension other LIP-induced carbon isotope excursions. How similar to the T-OAE is this isotope excursion really, and how representative of LIP-induced carbon isotope excursions is the T-OAE itself? The authors make it sound as if multiple other such carbon isotope excursions could be taken for comparison, but even the discussion of the similarity to the T-OAE receives very little room. What other such stepped, symmetric negative CIEs could one compare to?

Response: We agree that the similarity was overstated. “Symmetrical” and “comparable stepped onset” have been removed: the CRN-1 recovery limb is shorter and incomplete, and duration cannot be compared without an age model. Section 5.3 now reads ‘which is comparable in magnitude to the CPE excursion; duration cannot be compared because no age model exists for the CRN-1 core’, and the comparison in Section 5.4 is presented as context (‘broad similarities, which are consistent with, but do not demonstrate, a similar forcing’). The T-OAE remains the one event discussed in detail because the Mochras record provides a comparable organic-carbon and Hg dataset; we have not added other LIP-associated CIEs as comparable “stepped, symmetric” excursions, because we no longer make that claim for CRN-1.

**Geochemical analysis**

The authors obtained valuable data, but these datasets have analytical issues and the lithologies of the core impose strong controls on what evaluations are possible based on these data.

- The authors note that analytical data will be made available upon publication of the study, which is great. However, I have been unable to access the dataset on Zenodo so cannot make a detailed assessment of the proxies myself for this review. In particular I would have liked to look at the Cl, S and trace element data in some more detail. I think that in a second round of reviews these data should be made available (to reviewers at least) so that potential issues not currently visible can be recognized and it can be judged if these data are formatted and presented in a way that is clear and useful to the user.

Response: We apologize for the access problem. A reviewer-access link was included in the note to the editor at submission, but it evidently did not reach the referees. Access to the Zenodo record has now been provided through the editorial office, and the record will be made public on acceptance. It contains the Cl, S, major- and trace-element, Hg, TOC, and isotope data, together with the quality-control tables (replicate analyses, reference-material results, and blanks) referred to in the Methods. The Data availability statement has been updated accordingly.

- Very little information about core condition and mineralogy is provided, so it is not clear if any alteration of the core has taken place after it was drilled. The authors refer to evaporite minerals such as halite and anhydrite, both of which would have been exposed

to potentially high humidity if the core was not stored in a climate-controlled environment. Might the mineralogy have been affected since coring? Did the authors assess the presence of pore fluids in the core? Other cores of Mercia Mudstone contain large amounts of saline pore fluid that bleed out and cause formation of salt crusts. This can obscure the true mineralogy of the strata. Was this assessed by the authors?

We agree that post-coring modification of evaporite minerals is a relevant concern in Mercia Mudstone Group cores. During sampling, the core was visually inspected, and samples were taken from freshly broken interior material where possible. Andeskie et al. (2018) also documented primary to early diagenetic evaporite textures, including chevron and cornet halite, swallowtail gypsum pseudomorphs, displacive halite, dissolution features, and mud drapes, supporting the interpretation that the evaporites record saline-lake and saline-mudflat processes rather than solely post-coring salt growth within the neighboring CRN-2 borehole. Nevertheless, no modern pore-fluid measurements or petrographic studies were undertaken in the present study, and some post-coring redistribution or reprecipitation of highly soluble salts, particularly halite, cannot be fully excluded.

The Methods now state: 'The core, drilled in 2013, has been stored unslabbed in the core store of the Geological Survey of Northern Ireland; it was broken with a rock hammer and chisel and samples were taken from freshly broken interior material. Efflorescent salt crusts were not observed at sampling. No pore-fluid or petrographic assessment of post-coring alteration was undertaken.' The Results note that the Na enrichment may include early diagenetic halite precipitated from saline pore waters. We note that pore brine alone cannot account for the Na contents of the mudstones: a dried pore brine at typical porosity would add roughly 1–5 wt.% NaCl, whereas the measured Cl contents correspond to a median of ~31 wt.% NaCl (interquartile range 23–39 wt.%), so most of the halite is sedimentary or early diagenetic. The concern is more relevant to the low-Na sandstones at 474–485 m, for which Cl contents and Na/Cl ratios are now given (see L272). We also report in the Methods that repeated XRF analysis of a single pressed pellet showed $Na_2O$ rising by about a third (8.9 to 11.8 wt.%), which indicates that halite is mobile on the timescale of the analyses; absolute $Na_2O$ and Cl contents are therefore uncertain by several weight per cent, although element ratios are unaffected.

- The authors present some methodological detail for their work, but some key information is missing or incomplete.

For XRF assessment, the authors give standard deviations for major element oxides obtained for NIST 2711a. While this is useful, it is not mentioned how many measurements of NIST 2711a this is based on (potentially about ten based on the number of samples analysed?), and it is unclear how these uncertainties relate to analytical uncertainty of the samples which largely are geochemically distinctly different to NIST 2711a. Are these uncertainties thought to be absolute

uncertainties independent of concentration or are they thought to scale with concentration (i.e., would it be better to report relative standard deviations)? What are the relevant detection limits for the elements, how were detection limits identified, and are all the presented data above the limit of detection? How can the authors demonstrate that the data are accurate despite widely variable geochemical matrices? The authors use Rb, Sr, and Zr data for further interpretation but do not give any quality information for these data. Were these elements also measured via XRF using the described technique or derived differently? What are accuracy, precision and detection limits?

Response: We thank the referee for this detailed comment and agree that the original description was incomplete and easy to misread: the quoted standard deviations were from replicate analyses of one CRN-1 sample, not of NIST 2711a. Rb, Sr, and Zr were measured by the same WD-XRF pressed-pellet method. The Methods now give the following. (i) NIST 2711a (n = 10): relative standard deviations of 0.3–4.8 % for the major oxides, and measured values that were 75% ($SiO_2$), 92% ($Al_2O_3$), 93% (MgO), 106% ($K_2O$ and $Fe_2O_3$), 107 % ($TiO_2$), 112 % (CaO and MnO), and 126 % ($P_2O_5$) of the certified values. (ii) The correction applied: all oxides except $Na_2O$ were multiplied by the ratio of certified to measured NIST 2711a. This is a single-point correction, and its accuracy for halite-rich matrices is not independently demonstrated, as the referee suspects; oxide totals including Cl range from 86 to 116% (median 102%). (iii) The replicate standard deviations (nine analyses of one pellet), given as absolute values; the larger values in the preprint included the original analysis of that sample as a tenth measurement. Through the replicate series, $Na_2O$ rose from 8.9 to 11.8 wt.%, and $SiO_2$ fell from 34.0 to 30.4 wt.% while Si/Al stayed at 3.1–3.2, which we attribute to migration of halite to the pellet surface [laboratory to confirm]. Absolute $Na_2O$ and Cl contents are therefore uncertain by several weight per cent, whereas element ratios are robust; this is a further reason for withdrawing the weathering indices. Si/Al and Zr/Al are used only to track relative changes in detrital input. [Lab to confirm the detection limits of each element, which were calculated for individual spectra, based on the signal-to-noise ratio].

For C-org and TOC analysis, the authors present some information about reference materials used for correction of their measurements, but there is no reference to certified TOC content of EMA-P2 and therefore any information how to interpret the uncertainty of the TOC data. I gather that a single point calibration was carried out to shift isotope data of unknown samples based on L-Alanine, but was stretching of the isotopic scale due to instrumental fractionation taken into consideration? The analysed samples are geochemically very different to the standards. The standards are pure chemicals with very high TOC, while the samples have very low TOC and required the analysis of very large samples. This might mean that additional issues such as incomplete combustion or retention of some CO2 inside the sample may have affected the samples which would not be visible by testing only pure organic substances as standards. Can these issues be ruled out? The presented uncertainty of the EMA-P2 standard is already very large

(0.7 permil 2 s.d.), and this seems to be an optimistic value regarding the core samples. Have any repeat analyses of core samples been carried out to see if they give a comparable repeatability? How should one translate the uncertainty of TOC in EMA-P2 to a similar uncertainty for the samples? Is this uncertainty largely driven by weighing uncertainty and therefore much lower for core samples than for the smaller standard samples, or is this related to instrumental instability? Are the reported TOC data for acid-treated material or for original rock samples? In the latter case, how was loss of material during acid treatment dealt with?

The information regarding the certified TOC content of EMA-P2 has been added (68.35%). We agree that employed standards are pure organic materials with much higher carbon content than the analyzed sediment samples and therefore do not fully reproduce the matrix effects associated with low TOC sediments. This limitation is now acknowledged in the Methods, and the reported uncertainty from EMA-P2 is treated as an estimate of instrument and standard reproducibility, not as a complete uncertainty estimate for the low-TOC core samples. On scale stretching: [lab to state the normalisation scheme; if a single-point normalisation to L-alanine was used, the samples lie within ~4.4‰ of the anchor, so a plausible scale compression would bias values by ~0.1–0.2‰ relative to the ~4‰ excursion, and the EMA-P2 recovery (measured −28.19‰ ± 0.33‰) tests it directly]. On the transfer of the EMA-P2 uncertainty to samples: it does not transfer directly; [lab to state whether the standards were weighed to match the carbon amounts of the samples (10–125 µg C per 50 mg load), in which case 0.33‰ is a realistic low-signal precision dominated by blank and calibration rather than weighing].

We did not perform repeat analyses of core samples. Therefore, we cannot fully quantify sample-specific reproducibility for the low-TOC sediment matrix. The manuscript has been revised to acknowledge this limitation and to emphasize that the interpretation is based on the stratigraphic coherence of the $\delta^{13}C_{TOC}$ trend rather than on individual low-TOC measurements.

We also clarify in the revised text that the TOC values are reported for the decarbonated residue. The TOC values were measured on the acid-treated residue and were not corrected for acid-treatment mass loss because dry residue yields were not systematically recorded. The Methods now state this: ‘TOC is reported relative to the mass of the acid-insoluble residue and is not corrected for mass loss during decarbonation, so it overstates whole-rock TOC by a lithology-dependent factor; Hg (measured on bulk powder) and TOC are therefore on different mass bases.’ No carbonate was detected in the studied interval beyond trace amounts.

For Hg analysis the authors again present some useful information, but it is not entirely clear how it relates in detail to precision, accuracy, and detection limit of the analysis. Some of it may relate to the lack of identification of statistical measures employed and unclear terminology. Please add information what the presented uncertainties for Apple Leaves and Ryegrass refer to. Also, please state what the internal standard is and why there seem to be differences in reproducibility of

samples, internal standard (what is it) and standard plant materials. From the figures and text it becomes clear that barring very few exceptions the Hg concentrations in the samples are very low. What is the detection limit of the technique and therefore what would approximate error bars for the samples be and are they actually above the limit of detection/quantification?

Response: The Methods have been revised to state what each figure refers to. The CRN-1 samples were analysed with the protocol that the same laboratory (ESRL, Trinity College Dublin) developed for organic-lean successions with very low Hg, following Hall and Pelchat (1997) and as described by Kovács et al. (2020). In that work, the standard 'isolation-mode' of the LECO AMA-254 was found to be inadequate at very low concentrations, often yielding negative values, so all samples were measured in 'blank-mode', which records the absolute quantity of Hg (in ng) that is then converted to a concentration offline. We used the same approach, and the Methods now cite it. The ± values quoted for NIST 1515 and ERM-CD 281 are the certified values and their certified uncertainties; analytical reproducibility is given separately.

For the CRN-1 campaign (eight run days, October to November 2024), ERM-CD 281 gave 15.8 ± 0.7 ng/g (1σ, n = 20; 96 % of certified) and NIST 1515 gave 43.6 ± 1.7 ng/g (1σ, n = 20; 101 %). The figures in the preprint were incorrect: the number of analyses was overstated, and the smaller standard deviations were the spread of daily means. Ten core samples were re-analysed, including the three with the highest Hg (512.45, 503.62 and 503.97 m); first and repeat analyses differ by a median of 3% (maximum 15%). This repeatability is comparable with the ~4% mean difference between duplicates reported for organic-lean carbonates measured with the same protocol in the same laboratory (Kovács et al., 2020). Sample readings range from 0.013 to 1.1 ng Hg (median 0.05 ng) against a nominal instrument detection limit of 0.01 ng (~0.06 ng/g for a 175 mg aliquot); 67 of 373 readings are below 0.03 ng (~0.17 ng/g). The ~0.2 ng/g background therefore lies close to the limit of quantification, as the referee suspected, whereas the three highest samples (0.76–1.1 ng) are 25 or more times above it. Following the laboratory protocol, blanks or test measurements are run between successive samples when a reading exceeds 5 ng, to exclude carry-over (Kovács et al., 2020); no CRN-1 reading exceeded 1.1 ng, so carry-over from high-Hg samples is not a concern for this dataset.

For the Hubei campaign (April 2021) ERM-CD 281 was analysed four times (18.3 ± 0.5 ng/g, 112% of certified), the internal standard, an in-house rock standard (K009650), twice (146.0 and 146.6 ng/g), four blanks gave −0.001 to 0.040 ng, and nine samples were analysed in duplicate (differences of 0.4–7%, mean 2%). The differences in standard deviation between materials that the referee notes reflect their different concentrations. The quantification limit and the reproducibility of the repeat analyses are now stated in the Methods (Section 3.3) and in Section 4.1.

**Presentation of the data and use of proxies**

I noted that none of the figures includes any error bars, not even a representative uncertainty. It is therefore not possible to judge if patterns in the data relate to analytical noise, or if they are interpretable as geological features. Please add visual representation of uncertainty and (where relevant such as likely Hg) detection limit into the geochemical plots.

Response: We thank the referee for this suggestion. Error bars are not routinely shown on stratigraphic $\delta^{13}C$ and Hg profiles in comparable CPE studies, and we have followed that practice. The analytical uncertainties (±0.33‰, 1σ, for $\delta^{13}C$ from EMA-P2, and the repeatability and quantification limit for Hg) are now stated in the Methods and in Section 4.1, so that readers can judge which variations exceed analytical noise, and the intervals of interest are identified by background shading in Figures 2, 4 and A1 and by labels in Figure 4 (now Figure 3).

A wide suite of calculated weathering indices is employed in the study, none of which is introduced in any detail. It is not clear from the text how these are calculated and references are not included. Besides this lack of introduction it is also later noted by the authors themselves that the majority of these weathering proxies is compromised, particularly by carriers of Na and Ca. Little is mentioned about mineralogy of the core beyond halite and anhydrite (is calcite present above trace quantities?), but these seem to be quantitatively important and therefore the chemical weathering indices cannot be meaningfully used. While the authors acknowledge the issues with the proxies, this happens very late in the text (L264 and later), after an already extended description that reads as if the authors treat these data a genuinely reflective of weathering patterns. Even for the least compromised part of the succession it remains unclear how meaningful the weathering proxies really are.

Similarly the authors use Hg normalisation via TOC, while (very late in the text once more) acknowledging that this is against recommendations and likely not reliable. The issue of potential post-depositional degradation of TOC is not considered here either. Are TOC values considered trustworthy and primary, or how much TOC may have been lost? The authors note the presence of reduction spots which indicates chemical interaction of iron with a reducing substance such as organic carbon?

Response: We agree that TOC in these oxidised red beds cannot be assumed primary; the reduction spots record local Fe reduction at the expense of organic matter, and the amount of TOC lost cannot be quantified. Hg/TOC is therefore retained only as a flagged curve in Figures 2 and 5, it is no longer used in the Conclusions, and the limitation is now stated where the data are first presented (Section 4.1) rather than only in Section 5.5.

I would encourage a re-write of the text entirely removing the use of weathering proxies. In my mind even if they were applicable for use in the more sandy intervals there would not be any other part of the core to compare these values against. It may be preferable to instead reflect on

mineralogy and maturity of the sands and how this relates to other known expressions of the CPE?

Response: We have followed this recommendation. Figure 3 and the paragraphs that defined and interpreted the weathering indices (Sections 4.1 and 5.1) have been withdrawn. The index values remain in the data repository, and Section 5.1 now contains a short statement of why they are not used: in this succession Na and Ca are hosted in halite, anhydrite, gypsum and carbonate, the CaO* correction cannot remove non-silicate Ca, the seven indices share the same oxides and are not independent, and the only interval with silicate-dominated Na coincides with the sandstone beds, so the indices record evaporite content and lithology rather than weathering intensity. The sandstone interval is now interpreted from its sedimentology and the siliciclastic input proxies Si/Al and Zr/Al (Fig. A1). Petrographic work on the sandstones has not been undertaken and is identified as future work. The XRF data are retained for lithological description ($Na_2O$, Cl, CaO, S; Si/Al and Zr/Al as grain-size indicators).

**Overall assessment**

To me, the presented sample look to provide a valuable yet somewhat compromised multiproxy dataset that allows to draw comparison to other CPE records, but I would not go as far as saying that it is of the exceptional quality required to revise entirely the nature of the CPE carbon isotope excursion(s). The conclusions the authors come to really are a little bit too bold for me to comfortably agree with. In my opinion it would be more appropriate to critically reflect on the limitations of the data more widely (nature of the lithology, breaks and variability in sedimentation) and put some additional work into a nuanced discussion with more comprehensive citation of relevant literature. In such a revised form I think the study would be quite suitable for Climate of the Past.

Response: We accept this assessment. The revised manuscript no longer claims that CRN-1 revises the nature of the CPE excursion; it presents the record as consistent with the CPE, discusses its limitations (lithology, episodic deposition, absence of an age model, low TOC and Hg) where the data are first presented, and broadens the comparison with published records (Sections 5.2 and 5.3).

**Detailed comments**

L22: How are coarse siliciclastics indicative of intensified weathering? I would say that they may indicate enhanced erosion, unless they simply relate to a shift in the sediment transport pathways and reflect a river channel. It may be true that added rainfall would also have the capacity to lead

to more weathering, but would this cause an increase in weathering intensity, or rather an increase in weathering flux via exporting a lot more detritus that is, however, less strongly weathered?

**Response:** The sentence has been revised to state that the coarse siliciclastic interval reflects enhanced runoff and sediment delivery, rather than directly indicating intensified weathering (Abstract: 'intensified runoff and sediment delivery'). The same change has been made in Section 5.1 and in the Conclusions ('enhanced runoff and sediment delivery'), where the earlier wording inferred weathering intensity from the sandstones.

L23: I do not think that "deposition" is the best word for this.

**Response:** The term 'deposition' has been removed, as suggested. The sentence has been changed to, *'…followed by a return to arid, restricted conditions,* ***marked by*** *stacked anhydrite nodules and beds.'*

L25: I do not think that this has been demonstrated convincingly. Please revise with more evidence, or rephrase.

**Response:** We agree. The Abstract sentence has been rewritten: 'In the Carnduff-1 core, the perturbation is expressed as a broad, stratigraphically prolonged negative carbon isotope excursion that begins with an abrupt shift and is followed, after a brief recovery, by a second interval of low values; discrete NCIEs cannot be resolved with confidence. This pattern may reflect incomplete preservation, condensation of several excursions, or a more sustained perturbation at this site, and the main excursion resembles the broad negative excursions associated with other LIP-driven events such as the Toarcian Oceanic Anoxic Event.' "Demonstrate", "single" and "stepped" have been removed, and the size of the excursion is now given as ~2‰ between interval means (up to ~4‰ between individual samples).

L26: Please include more examples than the T-OAE, even though it may be enough to discuss one in detail.

**Response:** The Abstract now cites the T-OAE as an example of a broad negative excursion rather than as one of several comparable events, and Section 5.4 discusses the T-OAE alone in detail. We have not added further events as comparable "stepped, symmetric" excursions, because that claim has been withdrawn for CRN-1 (see our reply to the general comment and to L333).

L30: Is "wet" much different to "humid"?

**Response:** The sentence has been revised to read 'a prolonged episode of drastic climatic transition from arid to more humid conditions'.

L32: I would rephrase this to tie better with the following sentences and explain more what exactly these biotic changes were.

**Response:** The text has been rephrased, and the biotic changes have been explained in more detail with new references, as below:

*'The Late Triassic Carnian Pluvial Episode (CPE, ~232 Ma) has been described as a prolonged episode of drastic climatic transition from arid to more humid conditions. This climatic transition coincided with major terrestrial and marine ecosystem reorganization, including faunal turnover, changes in floral composition, and restructuring of carbonate-producing communities (e.g., Ruffell et al., 2016; Bernardi et al., 2018; Dal Corso et al., 2020). In marine settings, the CPE has been associated with major extinction and turnover among crinoids, ammonoids, and conodonts (Simms and Ruffell, 1989; Rigo et al., 2007; Dal Corso et al., 2020), and a transition to coral-dominated reef systems (Dal Corso et al., 2015). On land, palynological records indicate a shift from xerophytic to hygrophytic floral assemblages (Dal Corso et al., 2020; Lu et al., 2021), accompanied by increased fern diversity (Dal Corso et al., 2020; Peng et al., 2022). 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). 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). The first global appearance of amber in the fossil record is also noted in the Carnian, with greater amber preservation typically occurring in the presence of environmental stressors and under wet conditions (cf. Fischer et al., 2017; Seyfullah et al., 2018). Together, these biotic and environmental changes have led to the interpretation of the CPE as a significant phase of ecosystem reorganization and radiation of modern clades, well after the initial recovery from the end-Permian mass extinction, contributing to the development of more modern-style ecosystems (Dal Corso et al., 2020; Callegaro et al., 2023).'*

L34: The sequence of references appears to be incorrect here.

**Response:** The reference sequence has been corrected.

L37: There is an extra space before "The" here that can be deleted.

**Response:** The formatting error has been fixed.

L45: What is the source of the palaeogeographic reconstruction? Please provide reference. Annotations on blue background are quite hard to read and seem a bit eclectic. Should it be "Pindos Volcanics" and "Orogenic Belt"?

**Response:** The source of the paleogeographic reconstruction (Tomimatsu et al., 2021) and the sources for the locations of the volcanic complexes (Walderhaug et al., 2005; Sun et al., 2016; Ozsvárt et al., 2019) are now given in the caption and in the reference list. Figure 1 has been redrawn: the volcanic complexes are now numbered on the map and named in a legend below it rather than labeled on the blue background, which resolves the legibility problem, and the place names have been corrected ("Pindos", "Qinling", "Kara Dere", and the Northern Calcareous Alps, which lie in Austria).

L73: "strata" rather than "stratigraphy"?

**Response:** The term 'stratigraphy' has been replaced with 'strata'.

L80: Is the Westbury Formation really consistently marine?

**Response:** We agree that describing the Westbury Formation as simply "marine" is an oversimplification. The wording has been revised to 'marine-influenced'.

L84-86: This may be useful information but I am not fully sure how it integrates into the flow of the text here.

**Response:** The sentence has been modified to read *'Of particular relevance to this study is the occurrence of sandstone units of up to 2.5 m thickness, locally referred to as "skerries," within the upper part of the MMG. In the Knocksoghey Formation, the Coolmaghra Skerry Member represents the main sandstone-rich interval and provides an important sedimentological marker for evaluating changes in siliciclastic input during the Carnian (Simms, 2009).'*

L100: In such an environment, how can one assume continuous sedimentation with mostly uniform sedimentation rates?

**Response:** One cannot, and we no longer do. Section 2 now states that the facies indicate episodic deposition with probable hiatuses, that no independent age model exists for CRN-1, and that thicknesses are not converted to durations; see our reply to the general comment on stratigraphic constraints.

L106: What about syn-sedimentary faulting and subsidence?

**Response:** A sentence has been added to Section 2: 'Accommodation in the basin was therefore controlled by fault-related subsidence as well as by climate and base level (Shelton, 1997), although the relative contribution of these controls during deposition of the studied Carnian interval remains uncertain.' The reference to "(eustatic)" sea-level rise at the end of the section has been changed to "regional sea level rise".

L111: Why are three points in time noted here? Is the top of the Mercia Mudstone that diachronous?

**Response:** The sentence has been revised to clarify that the Mercia Mudstone Group spans a broad Middle to Late Triassic interval, from the Anisian through the Carnian and into the Rhaetian (Howard et al., 2008), rather than three separate points in time.

L113: For what reasons are they considered equivalent, and why is this relevant here?

**Response:** Direct chronostratigraphic constraints from CRN-1 are limited and palynological data from the Mercia Mudstone Group in the Larne Basin are sparse, so the regional lithostratigraphic correlation is the basis on which the Carnian interval is identified in the studied succession; that is why the equivalence is stated. The basis of the correlation is lithostratigraphic (the position of the skerry-bearing interval within the Mercia Mudstone Group; Simms, 2009; Raine et al., 2021) [co-author R. Raine to confirm]. We have also corrected the ages quoted for the English units: Hounslow and Gallois (2023) assign the Dunscombe Mudstone a late Carnian to earliest Norian age, and the sentence now reads 'which range in age from Ladinian to early Norian based on limited palynology and magnetostratigraphy, with the Dunscombe Mudstone itself assigned a late Carnian to earliest Norian age (Hounslow and Gallois, 2023)'. The text now states explicitly that the correlation is lithostratigraphic and that the English constraints are themselves limited.

L128: "10-12" tonnes are a weight range, not a pressure. To derive the pressure one needs to normalise by the area this weight is applied to. Please update accordingly.

**Response:** The term pressure has been replaced by 'load'.

L135: Is carbonate known from this interval in Carnduff-1? How was the acid replaced? How much acid was used for how much rock powder?

L136: How was the rinsing carried out? How was sample loss avoided?

**Response:** The methodology text has been revised to include the sample (~5 g) and acid quantity (40 mL). It has also been noted that the samples were centrifuged between rinsing steps to separate the residue and minimize sample loss. Carbonate is present in the studied interval only in trace amounts, and the acid step was applied to all samples as a precaution. The acid was replaced after the first reflux by centrifuging and decanting, as now described; the original text omitted the centrifugation step.

L143: The notation is odd here - please use super- and subscript for the organic carbon isotope description.

**Response:** Notation has been fixed.

L148: In general it would be good to state precision of the weighing step (also for isotope work).

**Response:** The weighing precision, 0.001 mg for isotope analysis and 0.01 mg for Hg analysis, has been added to both sections.

L150: What does the stated uncertainty relate to? Please consider using “ng/g” instead of ppb. Parts per billion is an ambiguous unit that could represent e.g., a molar fraction. “ng/g” cannot be misinterpreted.

**Response:** Hg concentrations have been changed from ppb to ng/g throughout the text and on the axes of Figures 2 and 5 (now Figure 4). The ± values quoted for NIST 1515 and ERM-CD 281 refer to the certified reference values and their associated uncertainties as reported for the standards, which is also now specified more clearly in text. Analytical reproducibility in this study is represented separately by the standard deviation obtained from repeated ERM-CD 281 measurements.

L152: What concentration was found, and how many analyses were carried out for this certified reference material?

**Response:** This information has been added and corrected. ERM-CD 281 was analysed 20 times over eight run days in the CRN-1 campaign and yielded 15.8 ± 0.7 ng/g (1σ), 96 % of the certified value. The “25 times” and “± 0.37 ng/g” given earlier were incorrect; 0.37 ng/g is the spread of the eight daily means.

L155: It is great that duplicates were analysed, but what samples were duplicated? Most samples seem to have Hg concentrations much below 1 ng/g, so could not give a relative standard deviation of 2 % if the absolute standard deviation is 0.2 ng/g. Also, please consider the number of significant digits presented according to the relevant uncertainty. It appears odd that the typical difference between two duplicates is “roughly 2.16 %”. 2.16 % is a very precise value.

**Response:** The nine duplicates belong to the Hubei (Luojiagou) sample set, whose mean Hg concentration is ~11 ng/g, so an absolute standard deviation of 0.2 ng/g corresponds to ~2 %; the sentence has been moved so that this is clear, the statistic is now defined as the mean relative difference between duplicate pairs, and the value is given as ~2 % (range 0.4–7 % for the nine pairs). The duplicate pairs are listed in the repository QC table. Ten CRN-1 samples were also re-analysed, including the three with the highest Hg concentrations; at 0.5–6 ng/g the repeats differ by 0–15 % (median 3 %), as now stated in the Methods.

L158: I am wondering why materials give a standard deviation of 0.2 (L156), 0.4 (158), 0.6 (L152), and 0.8 (L158) ng/g. Are some of the materials heterogeneous? Does the standard deviation scale with concentration? Which uncertainty is most useful for the consideration of the samples? How was accuracy demonstrated?

**Response:** The different standard deviation values should not be interpreted as a single uncertainty term because they derive from different materials, matrices, and concentration levels. The duplicate powdered rock samples assess sample reproducibility, the ERM-CD 281 certified reference material assesses accuracy and external reproducibility, and the internal

standard monitors instrument stability. The two campaigns are now reported side by side in the Methods: ERM-CD 281 gave 15.8 ± 0.7 ng/g (1σ, n = 20) in the CRN-1 campaign and 18.3 ± 0.5 ng/g (1σ, n = 4) in the Hubei campaign, i.e. 96 % and 112 % of the certified 16.4 ± 2.2 ng/g, both within the certified uncertainty; NIST 1515 gave 43.6 ± 1.7 ng/g (n = 20) and the in-house standard 146.3 ng/g (n = 2). The standard deviations do scale broadly with concentration. None of these materials characterises precision at the 0.1–0.3 ng/g level of most CRN-1 samples; for those, the repeat analyses of core samples (median difference 3 % at 0.5–6 ng/g) and the quantification limit now given in the Methods.

For the Hubei data, the reproducibility of the duplicate rock samples is the most relevant measure; for the CRN-1 data, it is the repeat analyses and the quantification limit.

L181: Some of the used font is tiny - so small that even when zooming in to 200 % I still struggle reading it. Please re-size ideally to at least 6pt in print size so that the figure can be easily read. Also, the codes for lithology are not easily visible on the log.

**Response:** Figure 2 has been re-exported with all text at 6 pt or larger in print size, and the lithology codes on the log have been enlarged and placed beside the log.

L186: Please highlight the “steps” in some way that makes them visible. I cannot see any clear steps.

**Response:** We agree that no clear steps are visible, and the claim has been withdrawn; the caption now reads ‘a negative $\delta^{13}$Corg excursion’ (see our reply to the general comment).

L192: Why is this information presented again? It is already noted in L126.

**Response:** This sentence has been removed from the text to avoid repetition.

L197: The high Na needs some explaining. Is it related to sedimentary halite or at least partly to pore fluids? Is the Na/Cl ratio approximately 1 mol/mol? Why is Na expressed as Na2O?

**Response:** The elevated Na is interpreted as mainly halite/brine-related Na associated with the evaporitic depositional and early diagenetic setting of the Mercia Mudstone Group. This interpretation is supported by the covariance between Na and Cl in the CRN-1 dataset (with Na/Cl ratio = ~ 1 mol/mol) and by published work from the neighboring Carnduff-2 core, where bedded halite, displacive halite, gypsum/anhydrite, and halite-replaced evaporite textures are widely documented. The $Na_2O$ and Cl concentrations are in the data repository, and the Na/Cl relationship is now stated in Section 4.1 (‘Measured Cl contents covary with Na (R = 0.96, n = 95), with a median molar Na/Cl ratio of 1.1 (10th–90th percentile 1.0–1.3)’) with a cross-plot added to the Appendix. Part of the Na may be early diagenetic halite from saline pore waters, as now stated in Section 4.1; see also our reply on core condition.

Na is reported as $Na_2O$ because that is the conventional oxide format for XRF major-element data; the text now gives the measured range (median 18 wt.% $Na_2O$, interquartile range 13–23 wt.%) and the NaCl equivalent of the Cl contents (median ~31 wt.%).

L205: None of these indices and abbreviations has been introduced, and there is no justification why they can be used here. A value of 13 (L206) is quite clear evidence that these lithologies are unsuitable for use of the CIA.

**Response:** We agree. The indices have been withdrawn (see our reply to the general comment on weathering proxies). The value of 13 is now cited in Section 5.1 as evidence that the indices cannot be used in this succession.

L219: Can a C isotope value be reliably be measured for samples with 0.02 wt%?

**Response:** We agree that $\delta^{13}C_{TOC}$ values from very low TOC samples require caution, as mentioned in the manuscript text. Only one analyzed sample has a TOC value of 0.02 wt.%, and only ~8% of the measured samples have TOC values ≤0.05 wt.%. These very low TOC samples are not used as the primary basis for defining the carbon isotope excursion.

In the interval most relevant to the onset of the NCIE, TOC values are higher, between ~0.10 and 0.22 wt.%. In addition, the low TOC samples with values around 0.03–0.04 wt.% do not show systematic anomalous $\delta^{13}C_{TOC}$ values relative to adjacent samples, suggesting that they do not control the overall isotope trend. We therefore interpret the lowest TOC values cautiously and emphasize the stratigraphically coherent excursion rather than individual low TOC measurements. We note that the lowest TOC sample (474.42 m) lies within the Coolmaghra Skerry interval, where the most negative $\delta^{13}C_{TOC}$ values occur; excluding the 20 samples with TOC ≤0.05 wt.% changes no interval mean by more than 0.3‰ and leaves the position, magnitude and shape of the excursion unchanged. The single most negative value in the record (−29.5‰ at 485.96 m) is a sample with 0.05 wt.% TOC, and for that reason the size of the excursion is now stated from interval means (2.3‰) rather than from individual extremes.

L220: This information is redundant. It is already noted in L138.

**Response:** The repeated information regarding the number of samples analyzed and the analytical interval has been removed from the Results section.

L222: How is the average of these values important or meaningful for this study?

**Response:** The average value of the isotope measurements has been removed as per the comment, as the study interpretation requires only the stratigraphic trend, the onset of the NCIE, its magnitude, and the recovery pattern.

L226: Is this actually measurable? Based on the presented analytical quality, the detection limit of the technique is likely 0.4 ng/g or even higher.

**Response:** It is measurable, but close to the limit. The lowest sample reading is 0.013 ng Hg, just above the nominal instrument detection limit of 0.01 ng (~0.06 ng/g for a 175 mg aliquot), and 67 of 373 readings are below 0.03 ng (~0.17 ng/g). The Methods now give these figures, and Section 4.1 states that values below ~0.2 ng/g lie close to the limit of quantification. The referee's estimate of 0.4 ng/g was inferred from the scatter of standards at 16–146 ng/g; repeat analyses of core samples at 0.5–1.5 ng/g differ by 0–15 %. Blank-mode measurement was chosen for this reason: at such low concentrations the instrument's isolation mode is unreliable, whereas blank-mode reports the absolute mass of Hg released from each aliquot (Kovács et al., 2020).

L227: What is a "background Hg concentration" - Is that the average of all samples that are not part of the two brief spikes? Why is it presented so precisely?

**Response:** The background value was calculated statistically using the iterative 2σ method of Matschullat et al. (2000), rather than by manually averaging samples outside the two visible spikes. This method progressively excludes anomalously high values to estimate the baseline population, so that the obtained 'average' is not skewed by anomalous values.

The value has been rounded and is presented as an approximate baseline of ~0.2 ng/g, with the statement that it lies close to the detection limit.

L230: Consider the uncertainty of the measurements. Conservatively, the measurement uncertainty of Hg is +/- 0.4 ng/g and the uncertainty of the TOC data has to be integrated into Hg/TOC as well. At a relative uncertainty of 6-7 % a value of 69 +/- 4 would be derived.

**Response:** We agree and have changed the approach. Section 4.1 now gives the Hg-only uncertainty for the two peaks ('The Hg/TOC values of the three highest samples are ~47, ~56 and ~69 ng/g/wt.%; the uncertainty on these ratios from the Hg measurement alone is about ±2 ng/g/wt.%, before the unquantified contribution from TOC'), states that TOC and Hg are on different mass bases (residue versus bulk powder), rounds the ratios, and no longer uses Hg/TOC in the Results headline or the Conclusions. An uncertainty larger than the ratio is a reason to stop interpreting the ratio, not to omit the uncertainty, and the text now treats Hg/TOC accordingly. This follows the recommendation that TOC normalisation be avoided below 0.2 wt% TOC and treated with caution below 0.5 wt% (Grasby et al., 2019), and the practice adopted for other organic-lean Hg records, where absolute Hg concentrations are interpreted instead (Kovács et al., 2020).

L233: See above; 11 ng/g is probably precise enough. Please check through all of the text, also L234, 236 and other places for precision of reporting.

**Response:** The Hg and Hg/TOC values have been rounded throughout the manuscript to avoid overstating analytical precision. We have also replaced "ppb" with "ng/g" for Hg concentrations to avoid ambiguity, as suggested.

L239: "Lithological evolution"?

**Response:** The heading now reads 'Lithostratigraphic evolution of the Carnian succession in the Larne Basin (Northern Ireland)'.

L240: Does this refer to Northern Ireland? It was initially identified earlier.

**Response:** To avoid misinterpretation, the sentence has been reformulated as *'The presence of fluvial sandstone units in the North Curry Sandstone Member within the Mercia Mudstone Group, which interrupted the otherwise arid and semi-arid playa-lake sediments that characterized the Upper Triassic of the UK, provided key sedimentological evidence for interpreting the CPE as a phase of increased humidity'.*

L244: The sentence construction could be improved here (second which after L 243). It is not clear what "which" is now.

**Response:** The sentence has been split for clarity. The text now reads,

*'Comparable facies were observed in late Carnian units across the British Triassic, which correlated with the 35 m thick Schilfsandstein unit of the German Keuper succession that overlies playa-lake mudstones (Simms and Ruffell, 1989; 1990). Similar sedimentological expressions of the CPE have subsequently been documented in multiple sedimentary successions globally (e.g., Boulvain et al., 2001; Arche and López-Gómez, 2014; Simms and Ruffell, 2018; Jin et al., 2019).'*

L250: Why "indeed" and what follows from that?

**Response:** The wording has been fixed.

L252: I agree that there is a plausible climate interpretation, but are there no alternative viable hypotheses?

**Response:** We agree that alternatives exist and have added them. Section 5.1 now states: 'Although other factors, including local channel migration, sediment-routing changes, aeolian reworking, accommodation change, or tectonic influence, may have contributed to this variation, the coincidence of the sandstone interval with the siliciclastic input proxies and with the carbon isotope excursion is consistent with its interpretation as a CPE-related phase of enhanced runoff and sediment delivery, although these proxies are not independent of the lithological change itself.' The alternatives are presented as not excluded rather than dismissed, and the lithostratigraphic equivalence with other UK CPE successions is given as supporting context.

L254: See comments above - I do not think that the weathering index information is helpful here.

**Response:** We agree; the index information has been withdrawn from Section 5.1 (see our reply to the general comment on weathering proxies).

L260: It seems a bit late here to introduce this information, especially as the depositional environment does not allow for the use of such indices.

**Response:** We agree; the paragraph that introduced the indices at this point has been removed, and the remaining statement on why they cannot be used in this succession is now the only reference to them in the Discussion.

L271: Up to, or down to?

**Response:** Wording has been fixed.

L272: How reliable is this assumption? What are the Cl readings for this sample?

**Response:** The median molar Na/Cl ratio is 1.1 (10th–90th percentile 1.0–1.3), as now stated in Section 4.1 with a cross-plot in the Appendix. The assumption is less secure for the low-Na samples the referee asks about: between 474 and 485 m, six samples have 0.3–2.8 wt.% $Na_2O$ and 0.2–2.4 wt.% Cl, with Na/Cl of 1.3–2.0, so a quarter to a half of the Na in those samples is not balanced by Cl and is probably hosted in silicates, while the remainder is halite. The text now says so.

L286: It seems that some work on thin sections would be more powerful here than interpretations of complex chemical signals?

**Response:** We agree. No thin sections were prepared for this study. The claims that would require petrography (weathering intensity of the sandstones and post-depositional K addition) have been withdrawn, and Section 5.1 now identifies petrographic work on the sandstones as future work.

L287: As above coarse-grained sands do not really point at increased weathering in my mind. At least not directly.

**Response:** Agreed; the inference of increased silicate weathering from the sandstones has been removed from Section 5.1 and the Conclusions, which now refer to enhanced runoff and sediment delivery (see also L22).

L293: I am not sure how this conclusion was reached. I would expect that the CIA of the clay fraction in the lower and upper interval would be much higher than the sandy interval, instead suggesting that this material is more weathered. Whether it indicates stronger weathering, however, would also then be unclear because of the strong lithological contrast and the lack of equivalent clay material from soils from which the sands derive.

**Response:** We agree, and the paragraph that reached this conclusion has been withdrawn together with the indices. We accept the point that the clay fraction of the mudstones above and below is likely to be more weathered than the sands, and that the lithological contrast and the absence of equivalent soil-derived clay make any comparison of weathering intensity between the intervals unreliable. Section 5.1 now states that the apparent rise of the indices between 475 and 485 m 'coincides with the sandstone beds, so the change records lithology and the absence of evaporite as much as any change in weathering', and no interval is described as the most strongly weathered.

L296: There seems to be an element of circular reasoning here. The C isotope curve is at least one of the tools to identify the CPE, so one should not then treat it as independent.

**Response:** We agree. The CPE assignment of the interval rests on lithostratigraphy and sparse palynology, and the carbon isotope excursion is consistent with, not independent confirmation of, that placement. The sentence now reads 'Collectively, these observations are consistent with the interpretation that the sandstone beds record a more pluvial phase …', and the word "independent" has been removed from Section 5.1.

L297: Is "pluvial" not another word for "wetter"?

**Response:** The wording has been fixed to avoid redundancy.

L301: I do not follow the logic here. What exactly are Si/Al, Zr/Al and (Rb+Zr/Sr) taken to be proxies for here? If they simply relate to terrigenous material, then they cannot be used, because as the authors state the entire succession is terrestrial and thus only contains terrigenous material.

**Response:** In this succession, Si/Al, Zr/Al, and (Rb + Zr)/Sr are not used as proxies for terrigenous input in the marine versus continental sense. Instead, they are used to track relative changes in detrital siliciclastic supply and grain-size/mineralogical variation against the evaporitic and carbonate-rich background of the Mercia Mudstone Group.

Higher Si/Al values are interpreted as reflecting increased quartz-rich siliciclastic input relative to clay-rich aluminosilicate material, whereas higher Zr/Al values indicate enrichment in heavy-mineral phases, commonly associated with coarser detrital fractions. The (Rb + Zr)/Sr ratio is used to compare detrital aluminosilicate/heavy-mineral input against Sr-bearing carbonate or evaporitic components. Therefore, the decline in these ratios above the Coolmaghra Skerry Member is interpreted as a decrease in relative siliciclastic detrital input and a return to evaporitic mudflat/playa deposition, rather than as a decrease in terrigenous material.

To avoid confusion, the term 'terrigenous proxy' has been replaced with 'siliciclastic input proxy' throughout, and the meaning of each ratio is stated in the Figure A1 caption; (Rb+Zr)/Sr is now cited to Dypvik and Harris (2001).

L303: This may be true, but why and how is this relevant in the greater scheme of things? It seems a bit odd to just refer to one other core record.

**Response:** We agree that the relevance of the WP-1 comparison was not explained. WP-1 is included because it is one of the few continental Mercia Mudstone Group records in which the Carnian interval is constrained by palynology, biomarker, and organic carbon isotope data (Miller et al., 2017), and because the upper Glenstaghey and Knocksoghey formations are lithostratigraphically equivalent to parts of the southern England succession. We have removed the description of WP-1 as "geographically nearby"; it lies in the Wessex Basin, some 500 km from the Larne Basin.

We have also added the following sentence for clarity: *'The occurrence of sandstone intervals associated with the carbon isotope perturbation in both records supports the interpretation that the Coolmaghra Skerry Member records a part of a broader sedimentological response during the CPE recorded in the UK.'*

L320: Please provide evidence for the "stepped" nature and that there is only one excursion.

**Response:** We have tested this on the raw data (see our reply to the general comment). A two-step decline and a smooth decline fit the falling limb equally well, so "stepped" is no longer claimed. Nor does the text assert a single excursion: the interval means recovery to −26.4‰ at 449–440 m and a fall again to −27.6‰ between 440 and 376 m, and Section 5.2 now describes this second interval of low values and gives the possible explanations.

L326: I also cannot see a 3 permil preceding positive excursion. The smoothed line in Fig. 4 shows at best 2 permil.

**Response:** We agree. The ~3‰ figure compared individual extreme values. Interval means are −26.4‰ at 532–520 m and −25.8‰ at 520–508.4 m, a rise of 0.6‰, and a regression through the 64 samples of that interval gives 1.0‰ ($p = 0.02$). Sections 4.1 and 5.2 now give these values.

L330: "comprise multiple"

**Response:** The wording is fixed.

L331: It may be an expanded record, but is it more expanded than others, and does "expanded" necessarily equate to "complete"?

**Response:** The term 'expanded' has been replaced by 'high-resolution', and Section 5.3 now states that high sampling resolution does not demonstrate stratigraphic completeness. The sentence reads 'the CRN-1 record, in which these excursions are not individually resolved, raises the question of whether at some sites the perturbation is expressed as a prolonged negative CIE …'.

L333: Consider adding additional examples.

**Response: Other** LIP-associated carbon isotope excursions (for example the end-Permian and end-Triassic events) are now mentioned in Section 5.3 as context only, with the statement that their isotope structures differ from one another and that the T-OAE is the one event discussed in detail here. We have not presented them as comparable "stepped" excursions.

L344-7: Please provide references.

**Response:** The following references have been added, and the sentence on the Karoo–Ferrar LIP is now cited (Pálfy and Smith, 2000; Percival et al., 2015; Ruhl et al., 2022):

Jenkyns, H. C.: The Early Toarcian (Jurassic) Anoxic Event: stratigraphic, sedimentary, and geochemical evidence, Am. J. Sci., 288, 101–151, https://doi.org/10.2475/ajs.288.2.101, 1988.

Suan, G., van de Schootbrugge, B., Adatte, T., Fiebig, J., and Oschmann, W.: Calibrating the magnitude of the Toarcian carbon cycle perturbation, Paleoceanography, 30, 495–509, https://doi.org/10.1002/2014PA002758, 2015.

Xu, W., Ruhl, M., Jenkyns, H. C., Leng, M. J., Huggett, J. M., Minisini, D., Ullmann, C. V., Riding, J. B., Weijers, J. W. H., Storm, M. S., Percival, L. M. E., Tosca, N. J., Idiz, E. F., Tegelaar, E. W., and Hesselbo, S. P.: Evolution of the Toarcian (Early Jurassic) carbon-cycle and global climatic controls on local sedimentary processes, Cardigan Bay Basin, UK, Earth Planet. Sci. Lett., 484, 396–411, https://doi.org/10.1016/j.epsl.2017.12.037, 2018.

L349: A global temperature increase of 7-10 degrees is unrealistic in my opinion, and I do not think that the data presented in the cited papers really support such a magnitude.

**Response:** We agree that the original statement was too strong. The global figure has been removed. Ruebsam et al. (2020) and Ullmann et al. (2020) support an approximate doubling of $pCO_2$, and the temperature estimate is now given as regional warming of several degrees, with Bailey et al. (2003) and Ullmann et al. (2020) cited for that rather than for pCO2.

The modified text reads: 'Studies suggest that the associated volcanic emissions (Pálfy and Smith, 2000) and igneous intrusions (e.g., Svensen et al., 2007), along with the release of carbon from methane hydrates (Hesselbo et al., 2000), resulted in almost a doubling of atmospheric $pCO_2$ levels (Ruebsam et al., 2020; Ullmann et al., 2020) and in regional warming of several degrees (Bailey et al., 2003; Ullmann et al., 2020) during the T-OAE.'

L351: I think this paragraph is too short and unspecific to act as a detailed comparison to the T-OAE. Please elaborate further.

**Response:** We agree. Section 5.4 compares the two events in terms of carbon-isotope expression, hydrological response and sedimentological change; it does not compare their Hg records, and

the bridging sentence at the end of Section 5.3 now says so ('the Hg records of the two events are compared in Fig. 4 only'). The comparison itself has been reduced rather than expanded: the claims of symmetry, comparable stepped onset and comparable duration have been withdrawn, and the remaining similarities are presented as consistent with, but not demonstrating, a common forcing.

L362: CO2 does not directly lead to rainfall, though, I would think?

**Response:** The sentence now reads 'The inferred volcanogenic $CO_2$ injection likely promoted greenhouse warming, which in turn may have enhanced the hydrological cycle, intensifying weathering and erosion …', so that $CO_2$ is linked to runoff through warming and an enhanced hydrological cycle rather than directly to rainfall. The Conclusions have been changed in the same sense ('Elevated $CO_2$ from coeval magmatism may have driven wetter conditions and enhanced runoff').

L364: Are there "non-fluvial" lakes?

**Response:** We agree that 'fluvial lakes' was imprecise wording. The sentence has been revised to "fluvial and lacustrine systems" to more accurately describe the expansion of river-influenced and lake environments during the CPE.

L382: It may be worth also mentioning some of the complexity, however. There are also instances where the T-OAE has been linked with reduced TOC, and multiple marine epicontinental basins do not show clear evidence for anoxia.

**Response:** The original wording overgeneralized the expression of anoxia and organic carbon enrichment during the T-OAE. The paragraph has been revised to acknowledge that the T-OAE was spatially variable, with some basins recording organic-rich black shales and reducing conditions, whereas other epicontinental or marginal-marine settings show weak or absent evidence of persistent anoxia or strong TOC enrichment. The revised text now emphasizes that local controls such as basin restriction, circulation, ventilation, clastic dilution, and organic preservation potential influenced the expression of the event.

The following references have also been added:

Suan, G., Pittet, B., Bour, I., Mattioli, E., Duarte, L. V., and Mailliot, S.: Duration of the Early Toarcian carbon isotope excursion deduced from spectral analysis: Consequence for its possible causes, Earth Planet. Sci. Lett., 267, 666–679, https://doi.org/10.1016/j.epsl.2007.12.017, 2008.

Bodin, S., Mattioli, E., Fröhlich, S., Marshall, J. D., Boutib, L., Lahsini, S., and Redfern, J.: Toarcian carbon isotope shifts and nutrient changes from the Northern margin of Gondwana (High Atlas, Morocco, Jurassic): Palaeoenvironmental implications, Palaeogeogr. Palaeocl., 297, 377–390, https://doi.org/10.1016/j.palaeo.2010.08.018, 2010.

Ruvalcaba Baroni, I., Pohl, A., van Helmond, N. A. G. M., Papadomanolaki, N. M., Coe, A. L., Cohen, A. S., van de Schootbrugge, B., Donnadieu, Y., and Slomp, C. P.: Ocean circulation in the Toarcian (Early Jurassic): A key control on deoxygenation and carbon burial on the European shelf, Paleoceanogr. Paleoclimatol., 33, 994–1012, https://doi.org/10.1029/2018PA003394, 2018.

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L390: I think this figure would require some work to show that there are isotope steps, and how these compare to the T-OAE (and other CIEs). The logs are presented in the depth domain, but the arguments are in the time domain, so it appears the authors propose that no sedimentation rate changes occur in these successions? This appears quite unlikely.

**Response:** We agree. Figure 4 now plots both records against depth with the excursion intervals labeled (CPE-CIE, T-CIE), and the caption states that no duration is implied; the text no longer compares duration or symmetry (see our replies to the general comment and L400–401).

L396: Which “other events” could be pointed out here?

**Response:** The phrase has been changed to ‘similarities in $\delta^{13}C$ evolution between the two events’; no other events are invoked in that sentence.

L400: As noted above, I really cannot see a 3-4 positive excursion in the Carnduff-1 record.

**Response:** We agree and have removed the figure of 3–4‰. The sentence now describes ‘a preceding positive trend, interrupted by a decline to negative $\delta^{13}C$ values, followed by a recovery towards heavier values that in CRN-1 is shorter and incomplete’.

L401: Also, as noted above, how reliable is such an assessment of symmetry? Besides, the Carnduff-1 record does not look to be entirely symmetrical even in the depth domain. The increasing limb is shorter and values do not return to background entirely.

**Response:** We agree. The sentence ‘Both excursions appear to be symmetrical, with a comparable stepped onset’ has been deleted, and the shorter, incomplete recovery limb of CRN-1 is now stated explicitly (see L400).

L402: Please provide references for gravity flows in Mochras and ideally further afield.

**Response:** Xu et al. (2018) is cited for the Mochras gravity-flow deposits, and comparable sedimentological responses in other T-OAE sections are now cited to Izumi et al. (2018) and Kemp et al. (2024), both already in the reference list. The sentence reads: ‘The Mochras record is also marked by coarser gravity-flow deposits succeeding the excursion onset (Xu et al., 2018), similar to the sandstone beds observed in the Knocksoghey Formation, and comparable

sedimentological responses are reported from other T-OAE sections (Izumi et al., 2018; Kemp et al., 2024).'

L408: There seems to be an issue with the "13" in the delta value.

**Response:** The font has been fixed.

L414: It should read "Ruhl et al., 2020, 2022," I think?

**Response:** Referencing has been fixed.

L420: What is the meaning of "assessed Hg/TOC"

**Response:** The wording has been revised to 'calculated Hg/TOC ratios'.

L423: I would recommend stating this limitation of low TOC much earlier and treat the data accordingly from the beginning.

**Response:** The following write-up has been added to the 'Results' section to state the limitations of the data earlier on in the text:

*'Since TOC values are low throughout the studied interval, $\delta^{13}C_{TOC}$ values from the lowest TOC samples and calculated Hg/TOC ratios are interpreted with caution. The carbon isotope interpretation is therefore based primarily on the stratigraphic coherence of the $\delta^{13}C_{TOC}$ trend, and Hg interpretations emphasize absolute Hg concentrations alongside Hg/TOC.'*

L429: Can this really be firmly established? There is (very) little Hg even in these samples. Could any redistribution have occurred that juxtaposed a Hg spike at a lithological boundary?

**Response:** The wording has been softened to *'The modest Hg enrichment occurs stratigraphically below the main siliciclastic interval, making direct delivery by the later siliciclastic pulse a less likely explanation for the observed Hg signal.'*

L432: These R values are indeed low, but are only valid for interpretation if there is actual measurable variability that relates to primary concentrations. If there is TOC degradation, a primary TOC correlation may be obscured. Also, the error bars of the Hg measurements are likely on average larger than the values themselves; in such a scenario it would be very tricky to detect any significant correlation?

**Response:** We agree. The paragraph has been reordered so that the caution comes first, and it now reads: 'Hg shows only weak covariance with TOC ($R = 0.08$, $n = 255$) and Al ($R = -0.15$, $n = 95$), suggesting no obvious dominant control by organic matter or clay-rich detrital phases. 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

possibility that TOC degradation obscures a primary correlation is acknowledged in the reply to the general comment and in Section 4.1.

L434: Would this environment be conducive of pyrite formation? These are largely continental red beds and evaporites formed in an oxidising environment? Is there any covariation with the sediment colour?

**Response:** The Hg enrichment does not show a clear relationship with reduced sediment colour, and the overall facies indicate largely oxidizing conditions. Accordingly, Fe normalization has been removed from the manuscript and the Fe and Hg/Fe panels have been removed from Figure 2; the statement on sediment colour has been added to Section 5.5. The Hg interpretation has also been revised to avoid using Fe as a proxy for pyrite-hosted Hg, while acknowledging that minor local host-phase effects cannot be fully excluded.

L436: In red sediments, this seems to me to be a risky assumption. See also L440.

**Response:** We agree; see our reply to L434. Fe normalization, the pyrite inference and the Hg/Fe panel have been removed.

L445: It would be useful to annotate this figure with some information where the CPE is located in each chemostratigraphic profile.

**Response:** Figure 5 (now Figure 4) has been redrawn with a dashed line marking 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.

L451: Why was Luojiagou selected? Please explain.

**Response:** Luojiagou was selected for practical and scientific reasons: it is a published CPE section with organic carbon isotope data but no Hg data (Li et al., 2022); sample powders were available through co-authors and could be analysed in the same laboratory with the same protocol as the CRN-1 samples; and it provides a geographically distant record from a terrestrial–marine transitional setting with independent stratigraphic constraints placing the upper Badong Formation in the Carnian. This explanation has been added to Section 5.5. The description of its isotope record as “stepped” has been removed.

L453: I also cannot see any clear steps in this record.

**Response:** We agree. Section 5.5 now describes the Luojiagou record as ‘a negative CIE, expressed as one sharp decline with a smaller precursor shift’.

L457: The onset of which CIE? There is a negative CIE around 20 m which does not show Hg enrichment.

**Response:** The onset meant is that of the main excursion, where $\delta^{13}C$ falls from −21.6‰ at 23.0 m to −26.0‰ at 28.6 m; Hg reaches 58 ng/g at 26.8 m. Section 4.1 now says so, and Section 5.5 acknowledges the exceptions: within the excursion, Hg is mostly below 4 ng/g, the highest values (27–38 ng/g at 40.5–44 m and 64–89 ng/g at 90–91 m) lie above it, and the smaller negative shift at 19 m that the referee notes is not accompanied by Hg enrichment, so the coupling is not one-to-one. The earlier statement that high Hg/TOC values persist throughout the NCIE was wrong and has been removed: six samples exceed 500 ng/g/wt.%, only two of them within the excursion.

L462: Enhanced with respect to what?

**Response:** "Enhanced" referred to the higher Hg concentrations in the Hubei section relative to the Larne Basin. The sentence has been rewritten to lead with the most likely explanation and to keep the atmospheric-transport argument as speculation: 'The higher Hg concentrations in the Luojiagou section cannot be attributed to organic matter content, which is as low as in CRN-1 (median TOC 0.05 wt.%); they more likely reflect the different depositional setting and Hg host phases of this terrestrial–marine transitional succession. A contribution from palaeogeographic position is possible but speculative: modeled wind patterns for the Carnian (Zhao et al., 2025; Fig. 1) suggest that the Hubei region lay closer than the Larne Basin to the main pathways of Wrangellia-derived atmospheric transport.'

L472: I am not convinced that Carnduff-1 is the best record to show this. A more balanced discussion of other existing records would be needed as well, I think.

**Response:** We agree, and the Conclusions have been rewritten: 'The carbon isotope record of CRN-1 shows a broad negative excursion between ~507 and ~449 m that begins with an abrupt shift, followed after a brief recovery by a second interval of low values above 440 m. The discrete NCIEs documented elsewhere cannot be resolved with confidence, and a stepped structure can be neither demonstrated nor excluded. Whether this pattern reflects incomplete preservation, condensation of several excursions, or a more sustained perturbation at this site cannot be decided without an age model.' The sentence on the "absence of repeated returns to baseline" has been removed, since individual samples do return to pre-excursion values within the excursion, and Section 5.2 now discusses the published multi-excursion records (see our reply to the general comment).

L473-475: This sentence really rather points at major changes in sedimentation rate - a topic not taken up and considered in the text.

**Response:** We agree. The sentence has been replaced by: 'In the Carnduff-1 core the Hg anomaly lies some 27–35 m below the first sandstone bed; without an age model the duration of this lag between the carbon cycle perturbation and the clastic response is unconstrained.' The discrete sandstone beds total ~2 m of the ~59 m excursion, so event-bed deposition itself cannot reshape

the excursion, but erosion at bed bases could, and Section 2 now states that sedimentation rates cannot be assumed constant.

L483: This last paragraph seems to be shoehorned into the text a little. Is this added because of the title of the funding research grant?

**Response:** The paragraph has been removed.

L489: Is precipitation not one of the variables measured by "climate"?

**Response:** The text has been revised to avoid redundancy.

Figure A1: why relative to "carbonate"? Carbonate seems to be subordinate at best in these strata?

**Response:** The Figure A1 caption has been revised to clarify that (Rb + Zr)/Sr is used here to track detrital siliciclastic input relative to Sr-bearing chemical components, including evaporitic and subordinate carbonate phases