**Comment on: "Tracing North Atlantic volcanism and seaway connectivity across the Paleocene–Eocene Thermal Maximum (PETM)" in EGUsphere**

Following the recent public comments made by Jones et al. (in review) regarding our recent paper (Gernon et al., *Nat. Geosci.*, 2022; https://doi.org/10.1038/s41561-022-00967-6), we feel compelled to respond, as we have identified some key errors and misrepresentations of our work in their comments. Unfortunately, the authors did not communicate their specific concerns directly to us prior to posting or alert us to their public comments on our study. Our primary concern here is to ensure the accurate representation of our work. In this Comment, we address the specific text we take issue with, presented by those authors in response to a review by Marcus Gutjahr, whilst also providing some important clarifications.

Firstly, Jones et al. state *"There are critical geochronological and geochemical issues with this study (briefly outlined below) that impact the conclusions and viability of the proposed concept of extreme sub-crustal carbon release coincident with the PETM. Discussing these issues is outside the scope of our present manuscript and hence we prefer not to cite this work."*

[1] Jones et al. then assert that *"the cited sedimentation rates of 50 cm kyr$^{-1}$ that form the basis for the PETM duration in the Rockall section do not match the site report's estimate of 9.5 cm kyr$^{-1}$ with a maximum of 26 cm kyr$^{-1}$ in the section. As such, the PETM interval is based on biostratigraphic observations of a single dinoflagellate cyst marker species in a single sample (site report), so the duration of this critical interval of the succession at Site 555 cannot currently be constrained with confidence."*

This is incorrect: while the absolute age models have of course been updated, we urge the authors to cross-check the depths in DSDP Leg 81 Hole 555 referred to in Extended Data Fig. 1 of our paper. Here, they will see that the inferred position of the PETM boundary between the 600 to 700-metre range falls firmly within strata exhibiting sediment accumulation rates of approximately 50 cm/1000 yr, as evidenced in Figure 2 of the site report by Backman et al. (1984), referred to in our paper.

[2] The authors further comment: *"The presented ages of the East Greenland and Faroe Islands basalts have not been corrected to the most recent 28.201 Ma Fish Canyon Tuff calibration (Kuiper et al., 2008). Recalculating these ages gives 56.78 ± 0.25 Ma for the base of the Milne Land Fm in East Greenland (from 56.1 ± 0.4 Ma), which places the chemical heterogeneities observed in the basalts much earlier than the PETM interval. The ages of the Faroe Islands lavas are heavily debated, with studies arguing that the PETM interval could be above or below the hiatus. A recalibrated Ar/Ar age of 55.57 ± 0.35 Ma from the Middle basalt series in the Faroe Islands (Storey et al., 2007b) gives a post-PETM age for these lavas. This result suggests that the post-hiatus, high-Ti basalts observed in East Greenland and the Faroes may not be synchronous. This implies, with the current best estimates, that the analysed materials span at least 1 Myr, not ~200 kyr as proposed."*

It is unclear to us, (1) how Jones et al. arrived at a revised age of 56.78 ± 0.25 Ma for the base of the Milne Land Formation; (2) what is reported in the age uncertainty (i.e., analytical uncertainty or decay constant uncertainty?), and (3) what is the confidence level of the reported uncertainty. When comparing age data from multiple chronometers (e.g., Ar/Ar and astronomical techniques) it is critical that age uncertainties include all sources of uncertainty and are reported at the 2-sigma confidence interval for data comparison.

When we recalculate the Ar/Ar age of 56.1 ± 0.4 Ma (2 sigma, analytical uncertainty) (Storey et al., 2007) using the Kuiper et al. (2008) Fish Canyon Tuff sanidine calibration (28.201 Ma), which uses the decay constants of Min et al. (2000), we obtain an updated Ar/Ar age of 56.466

± 0.403/0.434 Ma (2 sigma, internal/external precision), which is 0.31 ± 0.47 Ma younger relative to the age presented by Jones et al. Importantly, due to the large analytical uncertainties associated with the Storey et al. (2007) measurements, this recalculated age is indistinguishable (0.36 ± 0.59 Ma) from the age determined by Storey et al. (2007), and considering all sources of uncertainty, is 0.45 ± 0.43 Ma older than the onset of the PETM (using the 56.01 ± 0.05 Ma astronomical age determination of the PETM from Zeebe and Lourens, 2019). Contrary to Jones et al., these revised ages allow the stratigraphically lower volcanism of the Middle Lava Formation to be closely related with the PETM onset, which is consistent with the model proposed by Gernon et al. (2022).

There are similar issues with regards to the updated age determination (55.57 ± 0.35 Ma) for the Middle lava series as reported by Jones et al, and as they have not provided the original age used as reported by Storey et al. (2007b), we cannot verify their result. On face value this age presented by Jones et al. is 0.44 ± 0.35 Ma younger than the PETM, which is again not inconsistent with Gernon et al.

The use of the astronomical tuned calibration of the Ar/Ar system (Kuiper et al. (2008) is most appropriate here as we are comparing Ar/Ar ages with an astronomical age for the PETM, but we need to consider the limitations of this calibration. The Kuiper et al. calibration only addresses inaccuracies in the age of the Ar/Ar standard and not inaccuracies in the decay constant of the Ar/Ar system and there remains an ongoing issue with regards to inter-chronometer comparisons due to the difficulties in assigning quantitative uncertainties to astronomical ages that underpin the calibration.

Taking into account these factors, the authors cannot discount the possibility that the lava sequence is synchronous with the PETM. Moreover, there is clearly a false dichotomy in their argument: we have already shown—using radioisotopic ages and critical regional geological evidence that may have been overlooked by the authors, e.g., magnetic polarity chrons—that many of the Middle Lavas in the Faeroes are likely post-PETM (refer to Fig. 2b-c in Gernon et al. (2022)) — it is a very thick sequence. Taken together, these lines of evidence do not preclude synchronicity with the PETM, nor (considering all sources of age uncertainty) do they support Jones et al.'s assertion that the relevant interval "spans 1 million years".

[3] Reviewer Gutjahr had merely suggested that the authors might consider the realistic possibility of a higher mantle-derived $CO_2$ release scenario (8% or more) from the subcontinental lithospheric mantle. Jones et al. respond that *"The authors choose a pre-eruptive $CO_2$ concentration of 2 wt% for flood basalt eruptions in their model, citing Self et al. (2005), despite this cited paper stating "…0.5 wt% [is] a reasonable but possibly high value for pre-eruptive $CO_2$ concentration [in flood basalt eruptions]". The Monte Carlo calculations assume concentrations ranging from 1–8 wt%, all in excess of this value. There is no convincing geochemical evidence from the northeast Atlantic margin that currently supports such elevated $CO_2$ concentrations."*

Unfortunately, the authors have made another key error here. The section on 'Quantifying background volcanic $CO_2$ fluxes' of Gernon et al. (2022) focuses on determining the *maximum* potential $CO_2$ release from ridges and Large Igneous Provinces (or LIPs) to assess their potential contribution to PETM warming. To estimate this value, we considered pre-eruptive $CO_2$ concentrations of 2 wt%, which is perfectly reasonable given our overarching motivation in this part of our study to assess the *maximum* likely outputs from LIPs under a 'business as usual' scenario.

We respectfully note that the main numerical model of Gernon et al. (2022) [refer to Fig. 3 in that paper] is fully described in the paper's Methods section, which may not have been fully considered in the authors' critique. Here we state that we use “a Beta distribution with a mean value of 0.5 wt%, and minimum and maximum values of 0.2 wt% and 2 wt%”, which as Jones et al. themselves acknowledge is a perfectly reasonable pre-eruptive $CO_2$ content for flood basalts (Self et al, 2005). Jones et al. are therefore incorrect in asserting that our “calculations assume concentrations ranging from 1–8 wt%, all in excess of this value [0.5 wt%]”. Unfortunately, the authors appear to be conflating this with a separate model run, in which we evaluate the probable carbon release from the sub-continental lithospheric mantle (SCLM) keel. In this model, our chosen values of 1–8 wt% are completely consistent with, and in fact more conservative than, the expected range for the metasomatized SCLM (see for example, Foley and Fisher, *Nat. Geosci.* 10, 897–902, 2017).

While it is of course their prerogative not to cite Gernon et al. (2022), which presents an alternative point of view, their arguments against citing this work seem to be based on incorrect interpretations rather than objective facts or sound reasoning.

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