

Clim. Past Discuss., referee comment RC2
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Comment on cp-2021-63

Eliza Cook (Referee)

Referee comment on "No evidence for tephra in Greenland from the historic eruption of Vesuvius in 79 CE: implications for geochronology and paleoclimatology" by Gill Plunkett et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-63-RC2>, 2021

General statement: The manuscript is well written and well-structured and outlines evidence that will be relevant to a wide community, specifically those reconstructing volcanism over the past 2 millennia and those attempting to synchronise records to Greenland's GICC05 timescale. Resolving regional climate responses to short-lived events (e.g. volcanic events) between records (such as ice and tree rings) at the annual requires reducing uncertainties of each record. Using sound geochemical evidence, the Vesuvius 79 CE tie-point has been refuted, and its removal will improve the matching of events and future ice-core chronologies. Those using the high-res ice core timescale should be aware of these dating biases when transferring ages.

Specific comments:

Line 73: I think you should also mention the work of Adolphi and Muscheler (2016, 2018), as they also published age adjustments the GICC05 timescale for the Holocene. I believe their recommended adjustment of GICC05 is by -11 years at 79 CE, which is consistent with the corrected age of 88 CE by Sigl (2015) and McConnell (2018).

Line 116: In a recent paper by Barker et al 2019 who modelled Taupo fallout, they simplified source parameters - i.e. did not: 'include the effects of atmospheric rainfall on ash removal and assumed that 100% of the erupted mass rose buoyantly into the atmosphere. Were there similar simplifications/assumptions in this model? Can they be mentioned somewhere or referenced?

Line 121: Is there a reference for the observational source data from the 3 different locations, or is it the same as the reference link from line 115?

Line 125: What were the shard concentrations like for these samples? Were they a few hundred or higher.

Line 131: I think it would be useful to have the depths or age ranges here.

Line 154: I found lines 153-160 slightly confusing to read. I haven't plotted the data, but from Fig 4 a-d, it looks like there are perhaps 4 or 5 grains from 1859 that resemble 1832/3 when major elements are compared. You mention there are just 2. I know there are only 2 shards that could be compared on their trace elements.

Line 210: this is an interesting finding. Past tephra searches in Greenland, which concentrated on tracing VEI >5 events probably found less tephra than are potentially there. If continuous sampling is not adopted, then events with an estimated VEI of 4 should be included in the search criteria.

Line 220: The VEI is not known, but tephra from 1 event from Kuriles has been traced in NEEM (Bourne et al., 2016), around 23 kyr BP.

Line 234: References needed here for observed data?? or, are you still referring to data from Global Volcanism Program, 2013 for Chikurachki and Siebe and Machias 2004 for these parameters? Would be good to make this clear.

Line 264: I would suggest to add that the Barbante et al 2013 analyses were by the SEM-EDS method. Maybe you can discuss why this method is not so robust, compared to WDS, with less analytical precision, higher detection limits on top of the added challenge of dealing especially when dealing with 6 small grains of about 5 microns. There were secondary standards available, did you also assess the quality of data from these analyses?

Line 267: Do you mean due to measurement error (instrument sensitivity, detection, small glass area for bombardment or assay?) in the analysis of such small grains?

Line 279: This seems to follow the trends of late Holocene cryptotephra deposits found in different North Hemisphere, high latitude records from Greenland, North America, Newfoundland and Svalbard. Are the distal deposits found in these records all > VEI 4? Would be good to mention if so, as it is consistent with the model results.

Line 293 –ref to Adolphi and Muscheler (2016) here?

Line 339: Were you also able to test this model on any recent events from the different locations – with known parameters such as eruption duration, plume height, grain size

and deposition pathway? e.g. eyjafjallajokull. If so, did you find consistency between model output and the observations? Perhaps this has been demonstrated in other studies using the same model?

Line 348: can you give details about this? Which records showed the response and is there a duration or temp inference?

Typing/Technical

Line 19 – suggestion: add ‘deposits’ or ‘layers’ here?

Line 28 Suggestion: ‘assigning’? this volcanic event to 79 CE.

Line 34_ suggestion to simplify.

Line 44 – refs needed here

Line 51 – ref needed here

Line 53 – suggestion: tephra ‘deposit’?

Line 62 – suggestion: ‘major element’ geochemistry

Line 112 – suggestion –use of ‘vanishingly small’? is not commonplace outside of native English speakers?

Line 137 - Masaya, ‘Nicaragua’.

Line 144 - Should the regions of origin accompany the names here? E.g. Kamchatka.

Line 154 – Suggestion: of ‘the previously published’ QUB-1859 from....etc.

Line 160 - Fig 4?

Line 165 – Full stop after Pink Pumice – long paragraph,

Line 179 –Lesser Antilles (Caribbean)

Line 179- The sentence starting with Although is a bit confusing. But it seems both points suggest that there is no correlation with Japanese sources?

Line 207: Suggested insert - at least two other VEI 4 eruptions have occurred ‘at Aniakchak’.

Line 919 – remove the repeat mention of ‘in this paper’ Fig 1a.

Fig 4 – can you add (a) – (d) to the biplots.