No evidence for tephra in Greenland from the historic eruption of Vesuvius in 79 CE: Implications for geochronology and paleoclimatology – Plunkett et al., 2021, *Climate of the Past*

RESPONSE TO REVIEWERS

We are very grateful to the two Reviewers, Lauren Davies and Eliza Cook, and to Larry Mastin for their positive and constructive comments on the paper. Our responses are presented below in red font.

Reviewer 1

General comments

This manuscript presents a novel methodological approach for studying past volcanic eruptions that is intersectional and of relevance for many research areas. Combining sulphate and cryptotephra records with modelling of volcanic ash, the authors present a convincing argument that is well-structured and well-supported. Given the existing constraints, e.g. limited or no reference data available for many eruptions or volcanoes, I think this presents a thorough summary that has tested the limits of their hypothesis appropriately and represents an important step forward for work in this area. I also agree strongly with the concluding comment of the abstract here regarding a need for formal acceptance of the revised ice-core chronology.

Specific comments

97: It'd be nice to have a comment here on your choice of VEI 4 as a limit, and whether VEI 3 eruptions could erupt ash into the stratosphere in the right conditions (e.g. with a seasonally low tropopause). I don't know if modern observations can speak to this?

We have inserted a comment on our selection of VEI 4 as a threshold. VEI 3 eruptions (n = 38 from 25 sources) were initially considered in our assessment. These include two Icelandic eruptions (Torfajökull and Katla), individual eruptions in New Zealand and Columbia, and otherwise eruptions in regions encompassed in our comparisons. As our database includes geochemical data from a large number of Icelandic cryptotephra and our models suggest a minimum VEI of 4 is needed to disperse tephra from the N. Pacific or Caribbean regions to Greenland, we are confident that we have not overlooked potential small to moderate sources.

Figure 4: Is there a reason why mean values for Mt. Spurr are plotted when single-point data are available, for some modern eruptions at least (e.g. Crater Peak 1992)? If the average data are used I think the uncertainties need to be plotted as well.

Crater Peak 1992 point data (Davies et al. 2016, *Quat. Sci. Rev.*) were excluded as they comprise glass with a more evolved geochemical composition outside the range of our tephra. We were unable to locate original datasets with individual point data for the less evolved end-member. Error bars on the 19 Crater Lake datapoints are omitted to minimise the noise on an already busy plot; the large number of mean points conveys a sufficient impression of the geochemical variability for illustrative purposes.

169: While the point data that you show here do seem broadly offset between OC1-5 and QUB 1832/33, there is still a good degree of overlap with the compositional field that is plotted from published data. If you're writing this off as a source it would be useful to have a comment explaining why the published data field isn't seen as reliable here.

We consider our co-analysis of the cryptotephra and proximal material on the same instrument to be a more reliable basis for discriminating the tephras. We lack the reference material data to evaluate the quality of the Hasegawa et al. (2011) dataset. An explanation is now included in the text.

193-194: As you have whole rock data for Mount Spurr plotted, I think this point should be clarified here (e.g. specifying glass, or single point data, etc).

This was an oversight and we are grateful to the reviewer for catching it! The clarification has now been made.

I'm not overly familiar with data for this volcano myself, but the AVO website's geochemistry search (https://avo.alaska.edu/geochem/search) shows whole rock data are available from three additional references by Nye et al. (listed below), which may be comparable to the data from George et al. (2003). Two samples listed as tephra fall pyroclasts from the 1953 eruption (85CNS16 & 17, the latter looks like it was included in George et al.).

Nye, C.J., Beget, J.E., Layer, P.W., Mangan, M.T., McConnell, V.S., McGimsey, R.G., Miller, T.P., Moore, R.B., and Stelling, P.L., 2018, Geochemistry of some quaternary lavas from the Aleutian Arc and Mt. Wrangell: Alaska Division of Geological & Geophysical Surveys Raw Data File 2018-1, 29 p. https://doi.org/10.14509/29843

Nye, C. J., Harbin, M. L., Miller, T. P., Swanson, S. E., and Neal, C. A., 1995, Whole-rock major- and trace-element chemistry of 1992 ejecta from Crater Peak, Mount Spurr volcano, Alaska: in Keith, T. E. C., (ed.), The 1992 eruptions of Crater Peak Vent, Mount Spurr volcano, Alaska, U.S. Geological Survey Bulletin 2139, p. 119-128.

Nye, C. J., and Turner, D. L., 1990, Petrology, geochemistry, and age of the Spurr volcanic complex, eastern Aleutian arc: Bulletin of Volcanology, v. 52, n. 3, p. 205-226.

We thank the reviewer for drawing our attention to some additional sources. Nye & Turner (1990) present a limited suite of trace element data and were excluded for this reason. Four additional whole-rock analyses (excluding Sr and Y) for Crater Lake air-fall units were included in Nye et al. (1995) that we now add to our plot (mean value recalculated for spider plot). No new datapoints for airfall units from Crater Lake were identified in the Nye et al. (2018) dataset.

209: I would appreciate some discussion here of the parameters used for modelling shard size and shape. Were the grain sizes reported here for the cryptotephra converted into the diameter of an equivalent sphere? What does the fine tephra measurement of ~30 µm relate to here? Cryptotephra data have been used to show that the transport distances of modelled particles are affected by sphericity (e.g. Saxby et al., 2020) so a sentence or two that comments on these details as they relate to your work would be of value here.

A description of the Wilson and Huang (1979) fall model and the parameters used has been added to the text.

208-209: Where do these eruption parameters come from? It's clarified for some of the following examples, but not here.

We have no grain size estimates for the smaller (non-climactic) eruptions of Aniakchak. We have therefore based our input parameters on other andesitic eruptions in Alaska. We have added a justification for the Aniakchak grain size distribution.

228-230: Related to the previous point, why is the grain size distribution detailed here for this source but not for the other two?

We have drawn information from previous eruptions of comparable magnitude to the events we simulate.

SI suggestion: related to my two previous comments on eruption source parameters, it might be useful to add a summary table that lists these (with references) for all three sources to the SI.

A summary of the grain size distributions has been added to the SI.

233: I think that your approach here, and what you describe at the start of this paragraph, is really important. Given that you're investigating an eruption that we don't currently have records of the assumption that it'll be like other eruptions that we do know about may not be valid. The individual initial runs are therefore likely too narrow in range and the testing with 1000 random events gives useful probabilistic bracketing data. I would suggest that this understanding could be emphasised earlier – that you're trying a best approximation from known data but it may fall short and it's more useful to test a range of values – because I think it's the only way we can really usefully study past eruptions. A sentence along these lines could be added to the methods, e.g. around lines 118-121.

A comment to this effect has been added to the text.

Technical corrections

56: The use of 'rather' here seems like an odd choice of a qualifying word. From what you go on to say, I think this should be stronger.

We have changed "rather" to "quite".

74: What exactly is meant by an 'unambiguous tephra'? Does the ambiguity (or lack of) relate to whether the grains are indeed volcanic glass, the number and size of shards, their geochemistry, or whether they generally constitute a useful or reliable marker 'horizon'?

The characteristics of the tephra make it unmistakable (visually) and the size of the shards clearly implies primary ashfall, with minimal time-lag since its eruption. To reduce ambiguity caused by our use of "unambiguous", we have changed the text to "distinct".

99: I think this should be '…none of which are historically dated…' instead of 'none of which is…'.

We have amended the text to avoid the singular/plural debate.

244: It might be worth stating that this reference is a data archive/record, as this wasn't apparent to me at first glance and it seemed odd that the data weren't included in this publication. I can now see why, having accessed it!

The text has been duly modified.

Figure 4: It's a little confusing that the Aniakchak points and the Spurr range are in similar orange colours - my first impression was that they were the compositional range and point data for the same source (as is the case for the other volcanoes). I assume you've chosen similar colours to show they're both from Alaska/Aleutian Arc, but there might be a clearer alternative.

The Spurr crosses have been made darker to contrast with the Aniakchak field.

Figure 6: What are the triangle points shown on this plot – Aniakchak? They're not included in the key.

Another oversight – thanks for catching it! The legend has been updated to include the triangles.

Figure 8: I'd find some extra labels on the plots useful here (e.g. all months, Nov-Feb). When I first looked at this, I was also a little confused because I assumed the four parts would be read left to right in two rows, not in columns. If you keep this layout, I suggest that you highlight the labels more clearly. Lastly, I don't think the blue line in the key matches the blue line on the plot.

Labels have been added and the order of the plots re-arranged. All line and fill colours have been checked for consistency.

Reviewer 2

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).

A reference to Adolphi and Muscheler (2016) has been added. A reference to Adolphi and Muscheler (2018) could not be found.

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?

We also assume that all the mass is distributed with the same grain size distribution throughout the representation of the column. We also neglect removal processes through hydrometeors and aggregation. We have added text to this effect.

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?

The reference link on line 115 is for meteorological data. Information regarding grain size distributions and model parameters is now summarised in the Supplementary file.

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

Ice sample mass is presented in the Supplementary file, and shard numbers in the paper. We have now added a shard quantification per mass to the Supplementary file, which amount to hundredths. We are unsure what units the reviewer would have liked us to use.

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

We presume this comment refers to the previous line. We now reiterate the depth ranges covered by the two samples and include the age of the combined samples.

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.

Apologies for the confusion! There are multiple point analyses on each shard for major element geochemistry, and this is now clarified in the text.

Line 210: this is an interesting finding. Past tephra searches in Greenland, which concentrated on tracing VEI >5 events probably found less tephras 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.

We agree!

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.

In addition to eruption plume height and mass eruptive rate, meteorological conditions play an important role in determining ash dispersal. We do not venture to consider how atmospheric conditions during the Late Glacial Maximum will have influenced the extent of ash dispersal.

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.

We have inserted a reference back to the relevant section where this is discussed (now made more explicit in that section).

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 prevision, 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?

We welcome the opportunity to expand a little more on this. The glass reference data provided by Barbante et al. (2013) demonstrate good accuracy but they were measured under different operating conditions (rastered area of 100 μm^2) to those used for the <5 μm unpolished tephra shards, and were not corrected for Na loss in the same way. These data do not therefore speak to the accuracy or precision of the GRIP tephra data. We have now elaborated on our concerns with the interpretation of the GRIP tephra.

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?

We assume the reviewer refers to our point about the validity of the small particles. In this instance, we consider the implications if the GRIP tephra is indeed of Vesuvius origin, as this implies that very small particles have a longer than expected residence in the atmosphere, undermining their value as isochrons. We have modified the text to make this point clearer.

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.

In response to Reviewer 1's comment about VEI, we have added information about VEI estimates for NW European cryptotephras to the Methods section. We have added a reference here also to the known ultra-distal cryptotephras.

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

Reference added.

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?

The Ash3d model undergoes verification and validation testing. The paper that introduced the model (Schwaiger et al., 2012) used the August 18, 1992 Crater Peak (Mt. Spurr) eruption for validation as this was a well documented event with good deposit granularity data. Periodically, the model is part of comparison tests with other models and datasets (Eyjafjallajökull and Raikoke). The use of Ash3d for cryptotephras is novel and has not been tested against specific events. However, the uncertainties involved with specific events (variations in the wind fields, transient nature of the mass eruption rate, percentage of fine material in the total grainsize distribution, etc.)

would make it difficult to make quantitative predictions of mass per unit area for cryptotephras. It may be possible to tease out these details in a future study, but for the purpose of this study, we used the modelling to inform on likelihood of one source over another.

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

We have reviewed two different proxy sources: (1) Multiproxy compilations produced by PAGES2k and (2) compilations from networks of climate-sensitive tree-ring records.

(1) contain a number of proxy records with dating uncertainties and biases smearing the record during these early times to an extent that does not allow to detect short-lived post-volcanic cooling effects following 88 CE (PAGES2k Consortium, 2017; 2019). The continental reconstruction of Arctic2k shows a marked multi-decadal cooling signal starting at c. 88 CE (PAGES2k Consortium 2013) but is subject to the same limitations.

(2) are exactly dated and are generally highly sensitive to major volcanic eruptions (e.g., Sigl et al., 2015; Büntgen et al., 2020); they are, however inconclusive for the 88 CE event. Whereas tree-ring records from Northern Siberia (Jamal; Briffa et al 2013) show cooling in 88–89 CE, the most recent compilation from 7 to 9 Northern Hemisphere tree-ring sites show only a moderate cooling in 90-91 CE (Büntgen et al., 2020), well within the range of natural variability at the time.

Annual ice-core records of $\delta^{18}O$ from Greenland (NGRIP, GRIP, Dye-3) transferred to the NS1-2011 chronology show a local minimum at 89 CE which is also well within the range of natural variability at the time. We conclude that the climate response (i.e., temperature reduction) following the c. 88 CE eruption is limited and highly uncertain.

We have revised the text accordingly.

Typing/Technical

Line 19 – suggestion: add 'deposits' or 'layers' here?

Strictly speaking, we analysed the geochemistry of the tephra, not the deposits/layers.

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

Text amended.

Line 34_ suggestion to simplify.

We are not sure to what this refers specifically. We have removed "potentially climatically effective" as the most expendable part of the sentence.

Line 44 – refs needed here

References added.

Line 51 – ref needed here

Reference added.

Line 53 – suggestion: tephra 'deposit'?

Added.

Line 62 – suggestion: 'major element' geochemistry

Added.

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

We feel that the expression is appropriate.

Line 137 - Masaya, 'Nicaragua'.

Added.

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

Regions or countries added at first mention of each source.

Line 154 – Suggestion: of 'the previously published' QUB-1859 from….etc.

Added.

Line 160 - Fig 4?

Yes – a good catch! Correction made.

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

Text amended.

Line 179 –Lesser Antilles (Caribbean)

Text amended.

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?

No, the "although" refers to the fact that we are not comparing like with like: Albert et al. report trace element data for trachy-dacitic, dacitic and rhyolitic tephras. Andesitic Japanese tephras do exist, but we do not have trace element data for their glasses. We have rephrased our point to make it clearer.

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

Text amended.

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

Removed.

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

Done.

Community Comment: Larry Mastin

This paper analyzes the glass geochemistry of tephra particles in a Greenland ice core, previously attributed to the 79 CE eruption of Vesuvius, and finds the attribution to be erroneous. The glass chemistry of the particles in the ice core differ significantly from 79 CE Vesuvius glass. Comparison with the geochemistry of volcanic glasses from several volcanoes in Central America, Kamchatka, western North America, and the Aleutians leads the authors to conclude that the tephra is most similar to that of Aleutian volcanoes and mostly likely came from a thus-far undiscovered Aleutian or Alaskan eruption around 88 CE. The change in time of this layer is significant because several ice-core chronologies are based on the 79 CE attribution. The study also uses several thousand model simulations to assess the likelihood that particles 32 or 64 microns in diameter would have reached Greenland from Central America, the Aleutians or Kamchatka under different assumed plume heights and locations of origin. It concludes that eruptions from the Aleutians or Kamchatka are more likely to reach Greenland than those from Central America, and that higher plumes are more likely to have delivered 32 or 64 um particles.

Overall I find this paper well written and its results highly significant in their implications for the ice core record. My criticisms minor. For example,

- In the section on glass chemistry, it is not always clear which glasses are being plotted (e.g. in Fig. 4) or which are being referred to in the text. Specifics are given below. Also the caption to Fig. 5 incorrectly describes the contents of the sub-plots.

  Corrected – see below.

- In the modeling section, the plume heights and volumes used as input are not always consistent with the description of the VEI of these events. For example, a modeled event having a DRE volume of 0.5 km3 is likely a VEI 5 rather than a VEI 4 (line 208).

  An erupted mass of 0.5 km^3 DRE with a density of roughly 2000 kg/m3 corresponds to a tephra volume of approximately 1.0 km^3 with a tephra density of 1000 km/m^3. This is a high VEI 4 or low VEI 5. Because we were in VEI 4 events with plume heights near the tropopause, we used a plume height of 12 km. We changed the text to describe this as a high VEI 4.

- The model simulation use plume heights that range up to 35 km asl; but the NOAA NCEP Reanalysis 2 model output that provide the wind field extend only to about 30 km elevation. In order to accommodate higher plumes, Ash3d extends the wind vectors from the highest pressure level in the met. model to higher elevation. You may want to remove the runs with higher plumes.

  These runs have now been removed (see below).

Other comments are even more minor. I think this will be a significant contribution to the literature on ice core chronologies. It offers some important insights into the eruption size and plume height required from volcanoes in the West to deposit tephra in Greenland.

Specific comments:

Line 18: Here, in the first two words of the abstract, you are stating the subject of the study. “volcanic signatures” is a little too vague to tell people what exactly you are studying. Trace deposits of past eruptions?

We have changed the term to “fallout”.

Line 41: consider changing “continuous flow processes” to more specific wording that might be understandable to non-specialists.

Text amended.

Lines 42-43: you note here that pinpointing the source of the eruption is a critical factor needed to determine the amount or aerosol emission and their lifetime. How so? Is finding the source critical because it would allow you to find other studies that estimate aerosol emission from that eruption? Do you assume that more distal eruptions were bigger and therefore had more climate impact?

The quantity of sulfate deposited in the polar ice cores is dependent on the location, including latitude, of the volcano. Text has been amended.

Line 45: change "grain size analysis" to "isopleth analysis".

Text amended.

Lines 80-82: approximately what time period do these three core samples represent?

A good point. The samples were taken at annual resolution. Their ages have been inserted into the text.

Line 84: add "which was placed" before "on a hotplate"

Text amended.

Line 89: add "electron microbe" after "6500F".

We have added electron microscope.

Lines 118-121: this description of the number of models run is a little confusing. You say that you ran 350 simulations for each volcano, but you randomly selected 1,000 start times between 1950 and 2011?

The final sentence in this paragraph (beginning "Additionally") indicates that a second set of simulations (n = 1,000) were run.

Line 121 (or thereabouts). Does Plunkett et al. (2021) include a table with model settings for these simulations? (e.g. model resolution, domain size etc.). If so, perhaps note that here.

The information is now summarised in Supplementary Information.

Line 124: change "Cryptotephra was" to "Cryptotephra particles were"

Text amended.

Line 127: although one shard was microlite rich, it appears from the photos that most of the shards were almost completely aphyric. It might be worthwhile noting this.

Text amended.

Line 143: Cite Fig. 3a, b at the end of this sentence.

Fig. 3a, b is already cited at the end of the sentence.

Lines 147-148: can you cite the source of Icelandic glass chemistry when stating that QUB 1832/33 glass chemistry doesn't correlate with any Icelandic glasses from this period? I see four Icelandic tephras plotted in Fig. 3. Are these the only ones you compared with?

No, the QUB database contains an extensive record of Icelandic cryptotephras as well as glass data compiled from published datasets. The tephras and sources are too numerous to list or provide citations for, so some sample references are included. The plotted examples are those events for which cryptotephras have been reported within the timeframe examined in the paper.

Lines 170-171: here you note the homogeneity of the Popo Pink and Lorenzo pumice compositions. On Figure 4 I see a light green field labeled "Popo matrix glass", and green hexagons labeled "Popo". Do these represent the same analyses? Are the Pink and Lorenzo glass analyses part of these? If you cite their homogeneity, it would nice to see which data you're talking about. You also describe the chemistry of the Smithsonian Popo samples but it's not shown on Fig. 4 which points these are.

As stated in the caption, the compositional fields denote the matrix glass data, but we appreciate the pointer to explain the graph better. To make the distinction clearer, we had added to the caption a note about points represented by symbols, including the differentiation of the three Popocatépetl samples.

Lines 158-159: Change “Point data” to “Microbeam data”.

Text amended.

Line 208: what do you mean by “a VEI 4 event erupting 0.5 km3 dense rock equivalent (DRE) volume of tephra”? Is the volume 0.5 km3 DRE, or 0.5 km3 tephra? An eruption with a DRE volume of 0.5 km3 would likely be likely produce 1-2 km3 tephra. It would be a small VEI 5, since VEI 4’s are defined as having tephra volumes of 0.1 to 1 km3. A plume height of 12 km asl is also a bit on the low side for a VEI 4, which is defined as having plume heights of 10-25 km. (https://en.wikipedia.org/wiki/Volcanic_Explosivity_Index) (Newhall & Self, 1982)

Ash3d uses DRE so we used 0.5 km^3 DRE. We have clarified this in the text.

Line 217: add a comma after “century”.

Done.

Line 220-221: “In none of the Ash3d simulations did tephra reach Greenland”. Remind us how many simulations were run. 350 for Chikurachki? As above, an 11 km plume height is rather low for a VEI 4 event.

Number of runs added. The modelled scenario is based on the 1986 Chikurachki eruption.

Line 227: an eruptive volume of 1 km3 DRE would translate into a bulk volume of about 2-3 km3, putting this eruption into the VEI 5 category (1-10 km3 bulk). Not a VEI 6.

We used the grain size parameters of Arana-Salinas et al. (2010) for the VEI 6 eruption, as no data for younger eruptions were available; we modified the eruption magnitude parameters to match the VEI 5 Lorenzo Pumice, which occurred closer in time to the event we are considering. We have clarified the text to this effect.

Line 235: change “1,000 events” to “1,000 start times”.

Text amended.

Line 236-237: delete “from a uniform distribution”. If the start times were randomly selected, then the distribution should be uniform.

Text amended.

Line 238-239: I think you can directly note the best-fit relationship between plume height and erupted volume that you used. If it’s the one I’m thinking of (Mastin, 2009, Eq. 2), it’s H=25.9+6.64*log10(V), where V is erupted volume in DRE, and H is height above the vent (km). Also, add “empirical” before “best-fit”

Relevant equation added.

Line 240: cite “Mastin et al., 2009, eq. 1)” after “plume height”

Added.

Line 243: You are using plume heights that extend to 35 km asl, but NOAA NCEP Reanalysis 2 data extend only to a pressure level of 10 mbar, which corresponds to about 30+/-1 km in the atmosphere. Ash3d accommodates higher plumes by using wind vectors at the highest altitudes that are the same as the wind vectors in the highest meteorological pressure level. If you don’t want to add this caveat, it might be best to delete the plumes with heights of 31-35 km.

We analyzed the NWP data and found that the top-most pressure level (10 mb) was seldom lower than around 31 km in Northern Greenland and the three volcanoes of interest. We removed all the cases from the original 1,000 start-times for which the plume height was greater than 30.5 and ran additional simulations from the same uniform distribution to generate the full 1,000 cases. There were occasional cases in which the top-most pressure level was below 30.5, in which case we continued the data from the last pressure level. This treatment of the top-most layers is now noted in the text. Additionally, in reprocessing the data, we re-binned the plume heights so that they are symmetric about the reported value. Previously, plume height from 20–20.99 would be in the 20 km bin. In this correction, we assign plume heights from 19.5–20.49 to the 20 km bin. This change affected several of the minimum required plume heights reported.

Line 257: At the end of this paragraph, you should add a couple of sentences noting that the rate of fall and deposition of distal fine ash are controlled by many factors that are not well understood. Rates of tephra aggregation, particle interaction, and local fluid instabilities could all affect whether fine ash makes it this far; and if so, whether it is deposited or simply continues airborne to greater distance. These model results are crude attempts to see what is possible. Small differences in probability are unlikely to be meaningful.

We have added several sentences describing these more sophisticated mechanisms.

Line 267: what do you mean by the "validity" of small ash particles in polar ice cores? You are questioning whether fine particles seen in ice cores are actually volcanic ash?

In response to a comment by Reviewer 2, we have clarified the text in this section.

Line 279: add "the" before "potential"

Added.

Line 298: change "Antarctica" to "Antarctic"

Corrected.

Line 308: change "independently" to "independent"

Corrected.

Line 348: change "with the eruption" to "with the 88 CE eruption" (I assume that's the one you're referring to).

Added.

Line 355: change "implicate" to "imply"

Amended.

Line 948: I don't know what "field glass analyses" are. Is there a more widely understood term?

Text amended.

Line 975: change "for in the winter months" to "for the winter months"

Amended.

Figure 5: The description of the subplots doesn't correspond with their content. (b) is said to be Japanese volcanic zones but the plot legend gives data points for Turrialba and Tacana, for example.

The description of the panels has been re-ordered appropriately.