

Response to reviews/comments

We would like to thank our three reviewers for their thorough and constructive review of our manuscript. All comments (including additional suggestions made by the editor) have been addressed as follows:

RC1:

RC1 provided comments on a pdf file. All of the minor editorial comments have been addressed and changed as requested. The following comments have more detailed responses:

How certain are you that these are not from the same eruption but are heterogeneous - this seems important because of how common it is for our volcanoes to produce homogeneous melts - please address in some way so it is clear you addressed this possibility [A comment on this has been added to lines 434-440.](#)

This correlation seems stranger considering it has only been identified as cryptotephra with a very wide distribution and no known references to visible tephra and is young and therefore should have been identified elsewhere as a significant tephra....this just feels like a coincidence where geochem matches - do you have other supporting data - you mention shard shapes here but not for Aniakchak - was shard morphology used in correlation? [We agree that this is a strange situation and have highlighted this in the text and responded to this point with a new paragraph \(lines 372-380\).](#)

Line335: what is the meaning of this? why would that be the case - seems like a non-match if the SiO₂ values are dissimilar. [We have added an extra paragraph \(lines 400-409\) to our point here and clarify that we are still confident in this correlation.](#)

Line 504: Also by "reference" material - these are for some eruptions of those volcanoes but not the full record (which none of us have) so there will always be uncertainty in these records until more works are published and the db grows to give us more candidates to compare with...I think it is important to lay out what reference data you are using for comparisons. [We agree with this point and have added extra discussion in a new paragraph \(lines 602-609\) to show this.](#)

RC2

Comments

Line 90 - A reference to Figure 1 would be useful here to add context to the location of these volcanoes mentioned in this section Alaska and the lake. [Added.](#)

Figure 1 - This figure is very clear. Just a short addition of naming the Kamchatka- Kurile volcanic and Alaskan-Aleutian arc regions would help the reader with location of the lake in relation to these volcanic areas mentioned in the text. [Added.](#)

Section 2.3

Line 170 - Additional information on the sieve mesh size used would be useful here. Especially as many researchers change between 10 and 25micron yet still follow Blockley et al., 2005. [Added.](#)

Line 175 - How were the glass shards mounted for geochemistry? Were they hand- picked using a micromanipulator (using Lane et al., 2014 reference) or another technique due to how little number of shards there were? This information would be useful here for potential replication. [Details have been added on lines 164-165 and 179-184.](#)

Line 178 - I would suggest saying 'single glass shards were analysed on a JEOL...' to further emphasis single analysis. [Changed](#).

Line 181 - What were the peak count times for the individual elements? Did you use a PAP absorption correction method to quantify the geochemical results? A quick sentence and reference to these would be adequate. [Added details on lines 182-183](#).

Line 191 - Data filtering - what was the limit % of analytical total used to remove non- glass analyses? 93%? 95%? Please state here. What was the justification for this %? [Added](#).

Section 2.4

Line 202 – 'This is more effective than using statistical techniques...' - What statistical techniques are these? Is there a reference for this statistical bias? I think a bit more detail and evidence here would help the reader understand why you decide to use the visual technique to detect offsets. [This section has been restructured to reflect the larger changes in the paper, but has also been reworded to clarify the point raised here \(lines 218-225\)](#).

Line 206 - What calibration curve was used? Please state here. This is especially needed as Steen et al. reference IntCal13 in their radiocarbon calibration from the paper I could find on GChron. [Added](#).

Line 210 - Adapt reference 'Bronk Ramsey, 2013' to 'Bronk Ramsey and Lee, 2013' and correct in the reference section. [Changed](#).

Line 211 - Additional reference to Bronk Ramsey (2009) needs to be made here in reference to the Outlier model - Bronk Ramsey, 2009. Dealing with outliers and offsets in radiocarbon dating, Radiocarbon 51(3) 1023-1045 [Added](#).

Line 213 – After reading the whole paper, I believe additional information is needed on the use of 'combine' the different PSV models in the OxCal model. This is particularly important as reference is made to 'combine' in section 4 and in File S1. [With the larger changes – namely removing the PSV ages – the Combine function is now removed for all but one use \(for the 14C and 210Pb ages from the same cm sample near the top of the record\)](#).

Section 3

Line 216 - What was the total number of tephra samples examined? A quick reference to the number in the brackets. [Added](#).

Figure 2 – The figures throughout this paper are very clear, useful and appropriate for showing the necessary data. The figure captions explain the data presented in the figure very nicely. Just a quick change of 'filled grey triangles' to 'filled red triangles'. [Changed](#).

Section 3.1

Line 237 - I see 36 in the table not 38. Please adjust if necessary. [Changed](#).

Line 238 - Yes, homogenous geochemical results can indicate a primary deposition. However, I would like to see additional evidence provided about the glass shards sizes and morphologies in each tephra layer. I believe that this type of information would have been recorded at the same time as the shard count. Were the shards of similar sizes and morphologies within each sample? A quick sentence saying this here would further justify your comment on primary deposition. [Discussion of](#)

morphologies has been added to each tephra section and size data is summarised at the start of section 3.1 (274-276).

Table 3 - caption I don't understand this (#.##) and it is not in the table. Possibly a formatting problem? I believe just refer to 'S.D.' instead in the table and in the caption. Also say if it is 1S.D. or 2S.D? Please make this clear in the caption too. [Addressed.](#)

I believe 'Analytical total' should be displayed here rather than 'normalised total' as current. Using normalised total makes this column obsolete as 100% is repeated, though you can work out the analytical total from the remainder of the information. Many other tephra papers present analytical totals in tables in the main paper. [Normalised total has been removed as we agree it is redundant. However, we believe that analytical total would be misleading here as the reader may not realise the data is normalised – so instead, the H2O_d values are kept \(which as the reviewer notes, provides the same information\).](#)

Table caption should mention 'normalised major element...' to re-emphasise that the data displayed in the table is normalised. [Added.](#)

Table 4 caption – 'Age estimates... from Steen et al...' Steen et al. used IntCal13 in their paper to calibrate the radiocarbon dates. This is from the paper I could have access through on Geochronology using your reference (though Steen et al.'s paper title was different to the reference list). Are the ages for these tephra's present here also using IntCal13? If so, they should be updated to IntCal20 to make a similar comparison to the other ages presented here. However, I may be wrong in the paper I could have accessed. Either way, it should be made clear what calibration curve was used for the radiocarbon dates of Cascade Lake in the table caption. [This should no longer be an issue, but we have clarified throughout the paper that IntCal20 is used here.](#)

For each cryptotephra layer discussed in the remainder of this section - It would be advantageous to include additional information on glass shard morphology and size for every tephra cryptotephra layer discussed in this section. This has been done for CL-74. This would further emphasise the point that these peaks are homogenous and are primary (if being similar in size and morphology) as well as help with the correlation to which eruption. This might provide further evidence for why CL-105 has two populations due to Na loss but are possibly from the same eruption. [This has been added for morphology descriptions, and grain size data is summarised for the five samples in section 3.1.](#)

Generally, I would suggest following a similar style to what has been written for CL-74 for each of the cryptotephra layers. Information presented on the most distinguishable geochemistry that makes it clear which eruption it is correlated to, along with the glass shard morphology and size.

Line 271 – 'Chronologically, Aniakchak CFE II has been dated...' Sentence needs re- wording as there are too many and's. [Changed.](#)

Line 274 – 'The latter is supported using geochemically...' This sentence is confusing. How it is worded currently gives the impression that the ice cores also have tree rings. Please re-word, e.g. "... sulphate peaks. Additional evidence is also provided by tree rings..." [Reworded.](#)

Line 280 - highlight if error \pm 1sd or 2sd for NGRIP age [Addressed.](#)

Line 282 - Reference needed here for the Tau_Boundary, Bronk Ramsey (2009) [Added](#)

Line 385 - I also think that how the shard concentration graph presented indicates a possibility of problems with taphonomy such as bioturbation or secondary inwash. This is not mentioned in this paragraph when it is an important point to make. This side of the argument I believe should be

mentioned in a sentence at the end of this section as further discussion is made in section 5.1. [This is now mentioned in lines 263-268](#)) and addressed in detail in section 5.1.1. We do not believe this information needs to be repeated a third time.

Section 4

Line 403 - Please refer to why the potential reason for Steen et al. concluding that these ages were too old. Problems with hard-water effects? Not enough carbon to provide an accurate date? A little more detail will be provided further insight for the reader reading this paper. [This point is addressed in detail in the discussion \(lines 683-691\) and we do not believe it needs to be repeated here as well.](#)

Line 410 – ‘...therefore also removed as outliers.’ Again, what is the possible reason that these are outliers? Just a sentence as Steen et al. does explain it in detail. This will help emphasises why this paper only focused on the Holocene tephra record rather than the whole core. [As above.](#)

Figure 5 - This is a very clear figure. Very nicely presented and the caption is very detailed [Thank you.](#)

Section 5.1

Line 450 – ‘... ultra-distal...’ Can you provide a distance in km to further emphasis this point? [Added.](#)

Line 471 – ‘... deposition of shard before the reach the northern slopes.’ Additional references need to be made here to provide evidence for rain of shards due to a topographic barrier. E.g. Watt et al. 2015 or Stevenson et al., 2013. [Added.](#)

Line 471 – ‘Other factors may include...’ Example references need to be made here. E.g. Pyne-O'Donnell, 2011. [Added.](#)

Line 475 - This paragraph needs a concluding remark, e.g. that topography primarily influenced shard concentration at Cascade Lake and not lake characteristics. [Added.](#)

Line 476 - Closely space eruptions are not the only reason for this type of shard concentration profile. The possibility of taphonomic processes e.g. reworking, bioturbation are also a possibility. This needs to be emphasised here in a short sentence before the detailed discussion in the next session. [This is now mentioned in lines 263-268](#)) and addressed in detail in section 5.1.1. We do not believe this information needs to be repeated here as well.

Is there any additional proxy information that could help with providing evidence (proxies?) against or for this taphonomic argument from the core? If so, please use this here. [Sadly not, but we have added extra details about the visual stratigraphy \(as the sediments are laminated\) and other supporting evidence against this argument \(lines 623-638\).](#)

Line 501 – ‘... fallout on snow, sediment accumulation, hydrology...’ References to appropriate papers are needed here. [Changed.](#)

The remainder of the discussion is very good with arguments for each section presented clearly and supported by further evidence, maintaining a balanced view whilst strengthening this research's conclusions. Shortcomings of the research are also mentioned, providing a basis to advance further research in the areas of tephrochronology and using multiple chronometers in future investigations.

Supplementary information

- File S1 – there is an addition ‘.’ in “Curve("Bomb13NH1","Bomb13NH1.14c"); C_Date("2.25-3",1901,4.)” that causes the OxCal model not to run. It needs to be simply removed. [This file has been recreated and should no longer have this issue.](#)
- Please create a figure caption for each supplementary figure. Though the figures referred to are in the main paper, further detail could be provided with a simple caption describing what each figure is and any other information you believed to be important to understand the results. This is particularly for Figure S3. [Addressed.](#)
- Table S1 and S2 would be better as an excel file to allow other researchers to quickly compare their own data to yours. It is also typical within tephra papers that geochemical data is presented as normalised within the main paper and non-normalised within supplementary material. I would recommend non-normalised data to be presented here. A table caption should also make it clear it is non-normalised. [We agree – this has now been provided as a zipped folder with an excel file for the SI tables.](#)

RC3 & editor comments

In response to the discussion that focused on links with our companion paper (Steen et al., gchron-2021-19) we have removed references to that manuscript. Our manuscript has been cut back to 15ka (from 21ka) and references to Steen (2016) – the thesis this data was originally produced for – are made where appropriate. Several redundant sections have now been removed, and parts of the results and discussion have been heavily restructured to reflect this change. This addresses the previously stated concerns about the reliability of the PSV data and any potentially circular arguments or redundancies in the manuscript.

Specific comments:

L104: re-word “Bayesian statistics” as “Bayesian statistical methods”. (“Statistical” could then be removed from the start of the following sentence to avoid repetition.) [Addressed](#)

L134-137: I understand why you have chosen not to further discuss the 210Pb data... But it might be worth mentioning it briefly in the context of the confidence (or lack of confidence) that you have in the uppermost 14C date. N.B. you DO provide a 210Pb data point on Fig 6. [This is a good point! This sentence has been reworded, and an extra comment has been added \(lines 495-499\) to demonstrate this.](#)

L193: “as well as” [Changed](#)

L278: you mention the “correction” applied to GICC05 following Adolphi and Muscheler (2016) but, for readers who have not read that paper, could you summarise (in a sentence) the (10Be-14C) approach that Adolphi and Muscheler performed? (Nothing more detailed than that.) [Added \(lines 322-325\)](#)

L281-282: similarly, could you briefly expand on the “...updated modelled eruption ages produced using the Tau_Boundary function in OxCal v.4.4 with IntCal20”. You obviously cite the Supplementary Material and Davies et al. (2016), but again could you briefly (i.e., just extend the current sentence, or add another sentence) to explain the over-arching purpose of implementing the Tau_Boundary? More specifically, I presume that this is used as the upper Boundary on a (Single Phase?) model, resulting in an “exponential rise prior”? [Added \(lines 330-334\)](#)

L353: as above, could you just mention that the Tau_Boundary is utilised as the upper bound within a single Phase model (if that is indeed the case).

L584-585: “Only four of the eleven analysed samples were included in the final age-depth model and

the identified outliers were variably 500-5000 years too old compared to median modelled ages". Given that 7 of the 11 dates were significantly old, it does make one suspect that the remaining 4 dates were also old, albeit not demonstrably old (based upon the other age constraints input into the model) and/or "less old" (<500 years). Perhaps some very brief comment could be made to that effect? [Comment added \(lines 508-512\), and extra comments have been added to emphasise that the lower section of the model is provisional and would benefit from additional independent verification.](#)