Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-104-AC2, 2020

Interactive comment on "Cryptotephra from the Icelandic Veiðivötn 1477 CE eruption in a Greenland ice core: confirming the dating of 1450s CE volcanic events and assessing the eruption's climatic impact" *by* Peter M. Abbott et al.

Peter M. Abbott et al.

peter.abbott@climate.unibe.ch

Received and published: 23 November 2020

Response to Reviewer 2 Comments

Reviewer 2: The paper is well written and finally shines some light on the complicated 1400's volcanic record in Greenland. The figures are very helpful and are well done.

Response: We thank the reviewer for these positive comments and the constructive review.

Reviewer 2: Only a few very small things missing. The geochemistry needs some more explanation. Mainly rationale for the analysis type and why the disparity in MgO. Maybe find geochemical data from proximal sources with more geochemical variability.

Response: We acknowledge that the geochemistry does need some further explanation and hope that our responses to more specific comments from the reviewer address their concerns. The disparity in the MgO was an issue with exploring the correlation between the TUNU2013 tephra and proximal deposits that we fully acknowledged and tried to address. As such, we explored and proposed many possible explanations for the offset and outlined those in the manuscript, but at the present time cannot identify a definitive explanation. As we outline later in the paper this was supported by compiling a comprehensive dataset of V1477 analyses. Further work on characterising proximal deposits may show this geochemical variation. We have now also included analyses of a Veidivötn tephra made at the same time as the TUNU2013 analyses as a QUB internal standard. At the time of the analysis the specific eruption was uncertain, but during revision of the manuscript it has been confirmed as a sample of V1477. These analyses do not display the offset in MgO, providing further evidence that the MgO offset for the TUNU2013 analyses was not due to analytical uncertainty.

Reviewer 2: I am not an expert in dendrochronology and supplied general comments but cannot speak to the modeling. With some minor changes, this paper would be a great addition to the Northern Hemisphere volcanic and climate records.

Response: We thank the reviewer for this positive comment.

Reviewer 2: The abstract is long and covers 3 different thoughts that are not tied together well. Overview, characterization of tephra, and then further implications. Could be shorter. IDK why you chose 2500 yrs in the abstract when you only go back to 939 C.E. in your figures. I would remove the text about the coldest summers. The 1477 eruptions did not greatly affect summer temperatures and the text spends too much on things that were found in other studies.

Response: We have tried to improve the abstract to tie the different aspects of this study together. 2,500 years is not the specific time interval for this study, but a general comment regarding the time period over which studies of this nature – annual comparisons between tree-ring and ice-core records – can be conducted. This is due to the availability of well-dated records from both archives, sufficient replication and spatial coverage of tree-ring records and historical evidence for volcanic eruptions during this period. As suggested, we have removed the text regarding the summer temperatures from the abstract.

Specific Comments:

Reviewer 2: Line 66- The start of this paragraph does not flow well with the previous paragraph. Could this paragraph be more incorporated into the above paragraph? This paper deals with 2 unknown sulfate spikes (1453 and 1458) and the tephra/sulfate pair you are analyzing. . It would be nice if you introduced them here, individually, rather than lump them as the 1450s. 1453 has the same magnitude but shorter duration than 1458 in TUNU2013 but seems to be working together to cool the 1450's.

Response: We have tried to improve the clarity of this section. However, as we are trying to describe the evolution of the debate regarding this eruption, we don't introduce the idea of two eruptions until later. Initially low-resolution ice-core glaciochemical data pointed towards a single event that was attributed to the climatic cooling and the formation of the Kuwae caldera. Subsequently, high-resolution ice-core measurements could resolve annual variations in sulphate, providing evidence for and dating the two eruptions, hence why this issue is introduced later.

Reviewer 2: Line 76- You provide specific locations for climate reconstruction using dendrochronology but then say "northern boreal forest" for Briffa et al., 1998. This could use a little more context.

Response: We have made it clearer that Briffa et al. (1998) identified the climatic cooling at various sites in their network and that this was "circum-northern hemispheric"

in nature.

Reviewer 2: Line 105- I would try to keep things in chronologic order. Talk about 1452 first and then talk about 1458. There is a switch halfway through the sentence that makes it difficult to read.

Response: Done.

Reviewer 2: Line 119- add "glass" in front of shards. Want to be clean we are dealing with glass compositions and not mineral compositions.

Response: Done.

Reviewer 2: Line 154- Does "historical period" have a specific time frame or is it just the last 1000 years?

Response: We have added more context to highlight that the historical period here refers to the time since the settlement of Iceland.

Reviewer 2: Line 191- I would define what monthly resolution means here. It shows up later and implies that you know which month the eruption occurred. Monthly at this depth means âĹij1 cm resolution which you define later in results.

Response: We have added a statement to the text to define how we regard the data as monthly, or quasi-monthly, resolution and how it could be best described as sub-annual (see later clarification). Over our period of interest, the measurement resolution is about 1 cm, which for a 12 cm weq/y ice core relates to a quasi-monthly resolution, assuming that snowfall is constant throughout the year. The annual layer boundaries are defined using the nssS/Na ratio (see manuscript Figure 5) based on their opposing seasonality with a winter Na peak and S minimum (Maselli et al., 2017). Estimating the true month of deposition is difficult as errors arise from assigning the annual layer boundary and the seasonal distribution of snowfall. We estimate these to be $\pm$ 1 month and $\pm$ 2 months respectively and thus estimate the total error on absolute month assignment during this period to be $\pm$ 2.2 months.

As such we don't try to define the exact months of deposition for the sulphates and particles, but feel that based on the data we can fairly confidently state that they and thus the eruption occurred early in the year, i.e. winter, and based on the chronology and tephra identification can say it was 1477 CE. We have clarified our statement that the delay between the particle peak deposition and the sulphates was 2 months to "approximately 2 months" due to the uncertainties described above.

Reviewer 2: Line 205- microscope slide instead of microprobe. A microprobe was not used. Why did you decide to use EDS and WDS instead of a microprobe? You should put analytical conditions in the footnotes of S1a. Beam current, Acc. Voltage, counting times, beam size, etc. It is hard to tell what was analyzed with each detector or the rationale. Could you elaborate? Outside of secondary standards how could someone reproduce this data?

Response: Analyses were conducted in the instrument used at Queen's University Belfast for tephra glass analysis, which has been specifically enabled for precision analysis of fine ash particles, as demonstrated by Coulter et al. (2010). This has been clarified in the manuscript and analytical conditions have been included in the SI. We have added the dimensions of the glass slides used for mounting for clarification, but retain "microprobe slides" as this is a common way to refer to this size of glass size to differentiate them from other sized slides more commonly used for microscope work in tephra studies and shard quantification.

Reviewer 2: Line 272- I would cite Koffman et al., 2013 and Koffman et al., 2017 as they both deal with particle peak to sulfate peak differences in ice.

Response: Done.

Reviewer 2: Line 310- I would re-order this paragraph. The most important metric in this paper is the geochemical correlation of the glass shards. The sulfate offset is empirical and there is no known measurement for calculating that difference into a distance.

Response: Done.

Reviewer 2: Line 315- It would be nice to have the geochemistry of these other eruptions made available or discussed more.

Response: The geochemistry of these other eruptions and their similarities to V1477 are discussed later in the manuscript in Section 5.3. We do not feel it is necessary to present the previously published data further in this manuscript as the high chronological precision of the ice-core chronology implies that the TUNU 78.655 m tephra layer does not have chronological similarities to any of the other Veidivötn eruptions.

Reviewer 2: Line 330- I would move this up to Line 225 and get it out early. I was excited to see geochemistry for these shards. How come they were not analyzed? Could be an interesting story to have MSH in the core.

Response: We prefer not to move the text. The shards identified in these samples have not yet been analysed because of their small size, low concentration and low probability of yielding useful data due to the presence of microlites and flat morphology. As outlined in the manuscript the physical characteristics of the 1479 CE shards and their similarities to the 1477 CE shards strongly indicate they are of a mafic composition and thus highly unlikely to correlate to the rhyolitic material previously identified by Fiacco et al. (1993) or typically reported from Mount St Helens. Further work is needed for this event, but this is beyond the scope and focus of this manuscript.

Reviewer 2: Line 352- What happened to the second coldest summer? Was it not volcanically forced?

Response: The second coldest summer in 1699 CE (-1.49°C) is not listed as it was not volcanically forced and has been attributed to natural climate variability during a known cold period, the Maunder Minimum. In addition, there is no sulphate signal in the Greenland records consistent with a large volcanic eruption prior to this cold year.

Reviewer 2: Line 466- Did you look at more proximal records that show the volcanic succession? It would be nice to see compositions closer to the source. Maybe the more primitive compositions would be there. Maybe comparing the two-lobe directories would be good instead of plotting all the data in Figure 4. The big difference in MgO needs some more explanation.

Response: We conducted a thorough search of all V1477 deposit characterisations reported in the literature and did not identify more primitive compositions comparable to the TUNU2013 78.655 m shards. As we could not identify chemical differences between the lobes and strong similarities between all V1477 deposits (see similarity coefficient comparisons in Table S5) we don't think it is necessary to split the data in that way. Within Section 5.4 we have explored many potential factors for the difference in MgO to try to find an explanation and feel we cannot provide a definitive explanation at this time.

Reviewer 2: Fig. 1. – I know Sigl et al., 2013 says monthly resolution but it may be easier to use sub-annual as it is hard to see the small variations anyway when looking at 50yrs of data.

Response: Done.

Reviewer 2: Fig. 3- Secondary Y-axis says the data is the same but the lines look different. 1477 C.E. particle peak in a) âĹij0.10 and in b) âĹij0.38. Is the top x-axis correct? Seems like a big just in accumulation change from 78.7-78.6 (âĹij0.5 yrs) to 78.6-78.5 (âĹij1 yr.). I only notice the black shard in c). Is that you are referring too or is it all of the smaller clear shards. The dark shard really draws the focus. Might want to add more to this caption.

Response: The difference in the peak heights is due to the resolution of the data, with panel (a) showing annual resolution data and panel (b) showing sub-annual data, which mutes the peak heights in the annual data. The same is true for the sulphate data on these plots.

The top axis is correct. The change in accumulation rate at this point is due to the 1477 CE sulphate peak being used as an age marker and fix point in the chronology.

Further description of the glass shard (only one shown) has been included in the figure caption for clarification. Smaller clear shards are not present; the reviewer is perhaps referring to the texture created when the glass slide is frosted to allow the resin to bond to the surface.

Reviewer 2: Fig. 5- What is with the sulfate peak at 1469? It has a similar magnitude as 1459 and 1453.

Response: The 1469 CE sulphate peak in TUNU2013 is a volcanic sulphate deposition signal (with co-registered increases in acidity and liquid conductivity, not shown) with high concentrations but very short time duration of deposition (<4 nominal months) typical for tropospheric emissions close or upwind of Greenland and with a short atmospheric aerosol lifetime. It is not regarded as a significant event as it is not detected in other Greenland ice cores NEEM-2011-S1 or NGRIP (see manuscript Figure 1) unlike the signals in 1453, 1458/59 and 1477 CE and was not investigated here as there is not an associated microparticle peak. A consistent signal is observed around 1470 CE in several Greenland ice cores (manuscript Figure 1); however, it is not investigated in this study as it was not accompanied by a co-registered microparticle peak (see manuscript Figure 3b).

Reviewer 2: Fig. 6- Laki in panel b) is missing the x line denoting where 0 on the temperature anomaly is located. Not all of the blue dots are labeled in panel a). Also not referenced in the caption

Response: Done.

References

Briffa, K.R., Jones, P.D., Schweingruber, F.H. and Osborn, T.J.: Influence of volcanic eruptions on Northern Hemisphere summer temperature over the past 600 years. Nature, 393, 450-455, doi: 10.1038/30943, 1998.

Coulter, S.E., Pilcher, J.R., Hall, V.A., Plunkett, G. and Davies, S.M.: Testing the reliability of the JEOL FEGSEM 6500F electron microprobe for quantitative major element analysis of glass shards from rhyolitic tephra. Boreas, 39, 163-169, doi: 10.1111/j.1502-3885.2009.00113.x, 2010.

Fiacco, R.J., Palais, J.M., Germani, M.S., Zielinski, G.A. and Mayewski, P.A.: Characteristics and Possible Source of a 1479 A.D. Volcanic Ash Layer in a Greenland Ice Core. Quaternary Res., 39(3), 267-273, doi: 10.1006/qres.1993.1033, 1993.

Koffman, B.G., Dowd, E.G., Osterberg, E.C., Ferris, D.G., Hartman, L.H., Wheatley, S.D., Kurbatov, A.V., Wong, G.J., Markle, B.R., Dunbar, N.W., Kreutz, K.J. and Yates, M.: Rapid transport of ash and sulfate from the 2011 Puyehue-Cordón Caulle (Chile) eruption to West Antarctica. J. Geophys. Res. Atmos., 122, 8908-8920, doi: 10.1002/2017JD026893, 2017.

Koffman, B.G., Kreutz, K.J., Kurbatov, A.V. and Dunbar, N.W.: Impact of known local and tropical volcanic eruptions of the past millennium on the WAIS Divide microparticle record. Geophys. Res. Lett., 40, 4712-4716, doi: 10.1002/grl.50822, 2013.

Maselli, O. J., Chellman, N. J., Grieman, M., Layman, L., McConnell, J. R., Pasteris, D., Rhodes, R. H., Saltzman, E. S. and Sigl, M.: Sea ice and pollution-modulated changes in Greenland ice core methanesulfonate and bromine. Clim. Past, 13, 39-59, doi: 10.5194/cp-13-39-2017, 2017.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-104, 2020.