

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.

Anonymous Referee #2

Received and published: 20 October 2020

General: 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. 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. 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.

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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.

Specific Comments: 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. 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. 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. Line 119- add “glass” in front of shards. Want to be clean we are dealing with glass compositions and not mineral compositions. Line 154- Does “historical period” have a specific time frame or is it just the last 1000 years? 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 ~1 cm resolution which you define later in results. 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? 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. Line 310- I would re-order this paragraph. The most

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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. Line 315- It would be nice to have the geochemistry of these other eruptions made available or discussed more. 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. Line 352- What happened to the second coldest summer? Was it not volcanically forced? 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. 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. Fig. 3- Secondary Y-axis says the data is the same but the lines look different. 1477 C.E. particle peak in a) ~ 0.10 and in b) ~ 0.38 . Is the top x-axis correct? Seems like a big just in accumulation change from 78.7-78.6 (~ 0.5 yrs) to 78.6-78.5 (~ 1 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. Fig. 5- What is with the sulfate peak at 1469? It has a similar magnitude as 1459 and 1453. 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.

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