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Climate of the Past Discussions
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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.

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General Comments: Very well written manuscript, with high-quality figures and data presentation. The amount of new data provided is in some ways rather light to deserve a standalone publication (essentially only one tephra layer in one ice core with new data), but perhaps this is common in this field.

Please not that I do not have any expertise in dendrochronology to comment on the methods of Section 3.3 and results of Section 4.3, so hopefully other reviewers will.

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Specific Comments (mostly grouped per section):

Abstract: a bit confusing for the novice reader not familiar with the cores in the region. The geographic setting probably needs to be introduced a bit more clearly early in the abstract.

L59-60: with respect to the three papers cited, but “while most of the above-mentioned mismatches have been resolved” seems to be a very bold statement? Please comment some more

Section 2:

- I’m not an expert on Icelandic volcanism as such, but somehow, I have the impression there are a lot of “biggest historical eruptions” and also “one of the most active volcanic systems” in Iceland. It won’t really change anything, but some more context could be useful to back up these kinds of statements.

- Can you please elaborate some more on the geochemical fingerprinting of this tephra – if it is basaltic, is it then really that easy to distinguish from other basaltic tephra? What kind of data have the previous chemistry-based correlations mainly been based on (also only major elements on glass, or also other things like trace elements, analyses on crystals)?

- In first approximation tephra will be deposited sooner than the sulphate aerosols (L273), but of course this also depends on the longevity of the eruption. You only comment on this at the very end, in Section 5.5. How detailed are the historical archives? Typically, a fissure eruption may be a long-lived event (except perhaps apart from an intense opening phase that would send particles into the stratosphere). Can you comment on that some more, already in Section 2?

Chemical analysis (Section 3.2):

I am a bit surprised the analyses were performed on an SEM rather than EMP instrument? For major elements modern EDS detectors can indeed be tuned to provide

quantitative data of sufficient quality, but surely not for all elements analysed? You refer to another paper for the more detailed methods, but please elaborate at least a little bit on which elements were analysed using the EDS and which ones using the WDS detector(s), and also on the analytical conditions used (beam size, current, voltage), and how they were adjusted to deal with such fine-grained particles.

Please comment on the appearance of the glass shards, other than their size and brown colour, especially considering the slight mismatch for some elements compared to the previously known 1477CE tephra. Do they contain any microlites, or are they entirely glassy? Any signs of post-depositional alteration?

Samples: Fig 3c suggests the other intervals ii-iii-iv were also sampled for tephra; and these are indeed commented on in Section 4.1.2. It is a bit of a missed opportunity that these were not analysed. Without analysis, it is a bit speculative to discard their correlation to a different event, and simply say they may be remobilised. As commented on earlier: if this were a fissure eruption, could it not be the case that the event was long-lived, and had multiple highly explosive phases? Or is the historical evidence really conclusive that it was not? What other evidence would there be for reworking?

Chemical variability:

L465: if they did indicate a more primitive composition, other elements should also systematically vary consistently with general fractionation trends. Is that really the case?

The two possible explanations given to explain possible chemical variations (Section 5.4), do not seem entirely solid to me (or would need to be justified better): 1. If the two lobes have been identified, how does the chemical composition of the proximal deposits vary, if at all? In other words, do the lobes show any variability? If not, why would the distal deposits in Greenland? 2. If the existing plume of an ongoing eruption were to bifurcate, why/how would it experience chemical fractionation? 3. Alternatively: What is the grain size of the other distal deposits that have previously been analysed?

Is it comparable to the particle size in the studied core, or coarser? In principle one would not really tend to expect variations in glass composition with grain size to be induced by magma fragmentation, but given that there may be at least some local heterogeneity in the melt: is there a possibility that a very fine fraction of particles that happens to be more Mg-rich was preferentially distributed towards Greenland, e.g. due to variability in density?

Technical Comments L136: specify Changbaishan (N Korea) or rephrase – now it reads as if it is in Greenland.

L146: “Volcanic Explosivity Index” – cite Newhall & Self 1982

L210: technically it is not the oxides being analyses, but the elemental concentrations, which are then converted to oxide concentrations.

L307: something wrong in this sentence - remove “were analysed”

L435: for individual shards (bubble walls), vesicularity should not really play a role anymore, so I would suggest removing that.

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