

# ***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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Received and published: 23 November 2020

Response to Reviewer 1 Comments

General Comments:

Reviewer 1: Very well written manuscript, with high-quality figures and data presentation.

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

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review.

Reviewer 1: 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.

Response: We acknowledge that in comparison to other tephrochronological studies, certainly those aiming to build a framework for a set of archives or region, the reporting of a single new occurrence of a tephra horizon could be viewed as light. However, the study and reporting of a single tephra layer (often in only one ice-core) is not uncommon in this field especially when the implications go beyond simply reporting the tephra. For example, when the single tephra layer can aid the dating eruptions or provide insights into past tephra dispersal, or when the discovery is combined with other data such as new geochemical analyses of other occurrences to aid proximal-distal correlations, ice-core glaciochemical signatures of volcanism and reconstructions of the climatic impact of the eruptions. Recent examples of studies of this nature include Jensen et al. (2014), Sun et al. (2014), Dunbar et al. (2017), Cook et al. (2018), Hartman et al., (2019), Narcisi et al. (2019) and McConnell et al. (2020). As our publication goes beyond just reporting this single discovery and utilises the tephra layer to contribute significantly to settling a notable debate regarding the timing and climatic impact of the 1450s CE volcanic eruptions and we also explore the climatic impact of the V1477 eruption we feel it warrants a standalone publication.

Reviewer 1: Please note 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.

Specific Comments (mostly grouped per section):

Reviewer 1: 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.

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Response: We thank the reviewer for drawing our attention to confusion in the abstract; we have rectified our oversight not to highlight that TUNU2013 is an ice core from Greenland. In addition, reference to the NS1-2011 chronology has been removed from the abstract as readers might not be familiar with this notation.

Reviewer 1: 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

Response: To clarify this statement we have expanded the comment to “While most of the above-mentioned mismatches have been resolved using new ice-core chronologies and hemispheric-wide tree-ring based climate reconstructions”. We hope this clarifies the statement.

## Section 2:

Reviewer 1: - 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.

Response: We have added context that the statement regarding Veidivötn being “one of the most active volcanic systems” is based on the number of eruptions during this period. We don't feel further context is required as this is commonly accepted, and we just wish to provide some context for the V1477 eruption. If further context for this assertion is required, I would direct the reader towards the literature regarding Icelandic volcanism referred to in the manuscript. The same is also true regarding the description of V1477 as one of the most explosive historical eruptions. Where necessary we have added the caveat that it is “one of the biggest/most explosive” not the most explosive as there are many metrics than can be used to judge the size of eruptions, but feel that is sufficient context for the eruption in this manuscript.

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Reviewer 1: - 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)?

Response: We have added a comment to Section 2 to clarify that the correlation of V1477 tephra between proximal, medial and distal sites is underpinned by major element analysis of glass shards, but also stratigraphic and geochronological evidence. We also direct the reader to a later section (Section 5.3) where the issue of geochemical similarities between the basaltic products of the Veidivötn system is already highlighted alongside the importance of having chronological evidence to back up any correlations between Veidivötn deposits. We have also highlighted that the correlation between the Veidivötn proximal deposits and tephra layer “a” is based on geochemical evidence and field mapping.

Reviewer 1: - 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?

Response: The record of sulphates and tephra within the ice cores can relate to the longevity of eruptions, but as the reviewer also points out, the explosivity of an eruption is key for tephra particles to be injected high enough into the atmosphere to be transported to Greenland. Based on our review of the literature we have not found any evidence, geological or historical, that the V1477 event was long lasting or had multiple explosive phases. The explosive nature of the event is often highlighted and Larsen et al. (2014) highlight that the tephra fall is correlated to the early stages of the eruption, a point we have now added to Section 2, so we think it is fair to assume the tephra

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deposition also relates to that phase.

Reviewer 1: 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.

Response: The JEOL FEG-SEM 6500F used for the analysis is a system that combines electron microprobe (field emission gun) and scanning electron microscope capabilities through the addition of a Wavelength Dispersive Spectrometer (WDS) to the SEM. The system allows major element data to be generated using both WDS and EDS. As shown in the INTAV inter-lab comparison (Kuehn et al. 2011), several labs use integrated WD and ED analysis performed on an SEM for glass geochemical analysis, and Coulter et al. (2010) demonstrated the precision and reliability of the analyses produced by the FEGSEM 6500F and three electron microprobes. Furthermore, our secondary and internal standard glass data illustrate the precision of our analysis. For clarity, we have added details on the operational parameters (including which elements were analysed by WDS or EDS) to the supplementary information and we argue that the secondary glass standard speak to the precision of our results, irrespective of the instrument used for analysis.

Reviewer 1: 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?

Response: Most shards are plate-like or occasionally cuspatate, and shards containing

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microlites were present but rare. All shards were sharp edged, none showing any indication of physical alteration. These details have been added to the revised manuscript.

Reviewer 1: 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?

Response: The shards from these additional 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. We feel that it is appropriate to consider their correlation to the specific event of the 1479 CE tephra of Fiacco et al. (1993) unlikely based on the shards not having physical characteristics (i.e. colourless glass) indicative of the rhyolitic composition characterising that deposit. However, we acknowledge the reviewer's concern that we have limited evidence to attribute it to remobilisation so have adapted our assessment of that potential from "most likely" to "possible" and in the manuscript highlight that further work may help solve this unanswered question. We feel that such work is beyond the scope and focus of this manuscript and would not have an impact on the major conclusions of this work. Based on our review of the literature we have not found any evidence, geological or historical, that the V1477 event was long lasting or had multiple eruptive phases over a period of 2 years. Therefore, we do not feel that the shards identified in the 1479 CE annual layer derived from a later eruptive phase related to V1477.

Reviewer 1: Chemical variability:

L465: if they did indicate a more primitive composition, other elements should also

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systematically vary consistently with general fractionation trends. Is that really the case?

Response: We did not clearly observe fractional trends for other element pairs that would support this proposition and have highlighted this in the manuscript. Nevertheless, we feel it is worth highlighting the potential to fully explore all possible explanations for the MgO difference.

Reviewer 1: 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?

Response: As we outline in the manuscript geochemical differences have not been identified thus far along different dispersal axes; however, this is primarily based on proximal-medial deposits and there are insufficient data from far distal sites to assess if there is geochemical variation at those sites. In Section 5.4 we aimed to highlight that we cannot provide a definitive conclusion for the geochemical differences and to discuss issues that could be considered in future studies. This was important as the secondary standards did not suggest there was analytical uncertainty. 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 1: 2. If the existing plume of an ongoing eruption were to bifurcate, why/how would it experience chemical fractionation?

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Response: We have amended the text to refer specifically to changes in wind direction during the eruption.

Reviewer 1: 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?

Response: As grain-size data and more specifically the grain-size of the individual shards analysed for their major element composition has not been reported in the other studies it is not possible to explore this issue, but, we have added a statement to acknowledge that should be considered in the future.

### Technical Comments

Reviewer 1: L136: specify Changbaishan (N Korea) or rephrase – now it reads as if it is in Greenland.

Response: Done.

Reviewer 1: L146: “Volcanic Explosivity Index” – cite Newhall & Self 1982

Response: Done.

Reviewer 1: L210: technically it is not the oxides being analyses, but the elemental concentrations, which are then converted to oxide concentrations.

Response: Text changed to clarify this point.

Reviewer 1: L307: something wrong in this sentence - remove “were analysed”

Response: Done.

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Reviewer 1: L435: for individual shards (bubble walls), vesicularity should not really play a role anymore, so I would suggest removing that.

Response: Done.

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