

We would like to thank the reviewer for the positive assessment of our manuscript. Our responses to the points raised are described below.

“Page 17212 There is a description of what I believe is the use of water precipitation technique to size fractionate. These paragraphs could use a rewrite to enhance clarity. Also, it is fairly well known that aerosolization from water can cause a repartitioning of any soluble species present, leading to differences in particles from before and after this step. Put another way, a small fraction of the particles might have contained a high fraction of the total soluble material which, after this step, is repartitioned equally (per surface area) to all thereby affecting ice nucleation efficiency. I think this needs to be acknowledged here, especially given the ‘aging’ discussion at the end of the paper that acts the same way. In truth I doubt there is much soluble material on this fresh volcanic ash but without presentation of composition it can not be eliminated as a possibility.”

Reply: The reviewer is correct; the particles are allowed to settle in water with the aim of removing those larger than approximately 20 μ m. We have attempted to make these paragraphs clearer.

In this case, we do not need to worry about the repartitioning mentioned, as the samples described in these paragraphs are only used in the DSC, not in the ZINC, i.e. they are bulk samples, and no aerosolization is carried out. Furthermore, measurements of the melting point for the ash suspensions do not differ from that of pure water, indicating that no dissolved species are affecting the freezing/melting temperatures of the bulk samples measurably. We now mention this in the manuscript.

“Page 17225: This is an odd paragraph although I sympathize with the authors in that they are trying to make agreement of these two data sets. The truth is these results ARE clearly different from what has been presented by Bingermer et al. 2011. This paragraph would be much better with the elimination of everything starting with ‘However,: : :’ My assessment is that Bingermer is over-counting IN by a factor of roughly 10. There is almost certainly no need to invoke the strange conditions done in the last 3 sentences of this paragraph. Indeed, the ACPD discussion of Bingermer seems to have indicated this is likely and that even their simple budget of the aerosol acting as IN is inconsistent. My recommendation is to note that these studies are different and leave it at that (i.e., retain the first sentence, eliminate the next three). The onus is on Bingermer to explain such a strange dataset, not the other way around.”

Reply: We agree with the reviewer here, the conditions we describe under which our results could coincide with the measurements of Bingermer et al. are simply speculation. We have removed the last three sentences of this paragraph from the manuscript, as suggested.

“3. I was somewhat surprised to see that the lead authors of the previous ZINC papers, O. Stetzer and F. Luond, garner only an acknowledgement. I am left to assume that there is a student working on the instrument that serves as a co-author. None the less I note that it seems odd that those most expert on one of the central instruments used in this paper isn’t even on the author list.”

Reply: Olaf and Felix were offered co-authorship; however, at that stage they both indicated they would be happier being mentioned in the acknowledgements. We have approached them both again, and Olaf has now been included on the author list. Andre Welti, one of the co-authors, and Olaf’s student, is currently working with the ZINC.