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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice nucleation properties of volcanic ash from Eyjafjallajökull" *by* C. R. Hoyle et al.

Anonymous Referee #2

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Hoyle et al. consider the ice nucleation efficiency of volcanic ash using two methods that have been extensively used and described in the literature. The results are important and especially appropriate given the existence of the special issue on this volcano to which this manuscript is submitted. I have only very minor concerns outlined below.

1. 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.

Page 17225: "There is therefore a clear difference between the IN concentrations reported by Bingemer et al. (2011) and those presented here. However, the IN number densities measured by Bingemer et al. (2011) were not consistently enhanced during all of the ash episodes, suggesting the ash did not always contain large concentrations of IN. This may be related to a strong variation in the quality of IN emitted at different times during the eruption, or it may suggest an unknown atmospheric process which can lead to an increase in the IN activity of the ash during transport. Furthermore, the concentration of IN measured by Bingemer et al. (2011) was higher than usual during the second half of April, also in the absence of volcanic ash, hinting at the contribution of other aerosol sources to some fraction of the measured IN concentration."

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.

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.

In conclusion this is a timely topic and being submitted to a special issue on exactly this topic I recommend publication in ACP after these minor concerns are discussed.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 17201, 2011.