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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric ice nuclei in the Eyjafjallajökull volcanic ash plume" *by* H. Bingemer et al.

Anonymous Referee #1

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This is a timely topic, given the eruption in 2010 and the current interest in the formation of ice-containing clouds. The paper is well written, the figures are clear, and it is appropriate for publication in ACP. There are, however, some significant gaps and important omissions that need to be corrected before final publication. I suspect given the length of this paper that it was originally written for a letters type journal such as Nature or Science. If this is the case, given that it now appears in ACPD, some work needs to be done to expand the length and provide more description.

1. The methods section is exceptionally short. There are two major topics that need to be expanded: (1) The FRIDGE system is not adequately explained. I see there is a previous set of references but there needs to be a plot showing how this system compares to other previous instruments. Specifically there should be something like a

plot of ice nucleation conditions in FRIDGE vs. some previous standard (i.e. a mineral dust or perhaps AgI aerosol) as measured in e.g. the CFDC or AIDA. Second, a major point of the paper is post-FRIDGE analysis of ice IN. How were these separated from unactivated aerosol and collected? I understand this is a non-trivial step and yet this information is almost completely lacking and it is therefore impossible to judge the validity of these data.

2. The section on particle composition (page 2738, first paragraph) needs to be expanded. This is perhaps the central topic of the paper but contains little information beyond reference to other papers. IN are broken into four groups: volcanic particles, soot, sea-salt, and biological particles. Mineral dust is incorporated into the volcanic particles, but how are they differentiated? I think the paragraph says they are not and instead a non-volcanic average of mineral dust is subtracted. This 'wedge' should appear on the figure. Second, how are biological particles defined? I thought these were difficult to differentiate from other materials (humics, organics) using EM? Are there no other categories or 'undefined' particles? The pie graph seems very simplistic and clearly delineated and it is hard to believe the data so clearly break down into 4 groups and nothing more.

3. Expanding on this topic : I can not reconcile Figures 1 and 3. Looking at Figure 1 it appears that there are $\sim$700 IN / liter present during the first 'plume'. The high point of the non-volcanic periods is $\sim$100. In this case >600 would need to be of volcanic origin, meaning no more than 14% of IN would be non-volcanic. I note this is in the most extreme case, too, where one considers the HIGHEST non-volcanic abundance as opposed to the average which is an order lower. Yet in Figure 3 fully 35% and 25% of IN are in two cases in the 3 non-volcanic categories. How can this be reconciled? Are volcanoes producing sea salt and biological particles? Should not these categories be well less than 1/7th the total IN? And we haven't even considered the background mineral dust which is normally the most abundant non-volcanic IN type. More than anything this point needs to be explained before publication.

4. As the salient reference on ice nucleation by volcanic materials the work of Durant et al. 2008, which is later referenced, correctly deserves a place in the introduction. Furthermore, how do the activation points of Durant et al. compare to the activation points in this paper? This needs to be clearly explained since it is the previous data that most closely compare to what is presented in this work.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 2733, 2011.