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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice nucleation by combustion ash particles at conditions relevant to mixed-phase clouds" *by* N. S. Umo et al.

Anonymous Referee #4

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General Comment

This manuscript makes a fairly concise and straightforward assessment of the ice nucleating potential of various ash particles from combustion. The assessment includes a variety of ash types, and characterizes the particles in detail, so that comparison to mineral dusts can be made on an equal basis (surface areal site density). The point that these types of ice nucleating particles have not been often or readily distinguished from dust particles in the atmosphere, and that they require such assessment as potentially important atmospheric contributors is well taken. The statement that these particles could play an important role in primary ice formation in mixed phase clouds is not supported as yet on the basis of actual atmospheric measurements. It is enough to say that this deserves investigation. The basic findings can stand alone as an excellent

paper that will stimulate further research. All specific comments are rather minor.

Specific Comments

Page 28847, lines 6-7: The definition of deposition nucleation includes a statement that it occurs in a regime where bulk water cannot exist. I am not certain that the term bulk is appropriate in this case. Certainly aerosol water can exist and it can even potentially create an encapsulated particle in the regime below water saturation. I suggest to be clearer.

Page 28850, lines 8-10: One could infer from the statements here that 300C is a representative temperature for a wildfire. While that may be the flash point of wood, I believe that literature supports that this is far less than the high temperatures encountered in the flames of a wildfire.

Page 28859, discussion of Figure 7: I am not sure exactly where the discrepancy between some of the experiments that amounts to 1.5 to 2 orders of magnitude in the temperature regime from -20 to -25C is discussed. Is this what the discussion of larger droplets is about here? I do not consider these to be slight deviations. It would help if Figure 7 were more easily readable. Although there are a lot of experiments shown, the labels are just far too small.

Page 28861, lines 4 to 6: I found this to be a strange statement, suggesting that the lower activity of the ashes compared to mineral dusts is due to the absence of feldspar. It seems to be made as some kind of indirect support that feldspars are vitally important to ice nucleation by mineral dusts, but this paper is really about ash particles, which of course are not desert dusts.

Page 28861, line 12-14: The Wilson reference seems missing. Also, Archuleta et al. (Atmos. Chem. Phys., 5, 2617–2634, 2005) may be relevant for mention here due to inclusion of study of amorphous silicate particles. Studies were at cirrus temperatures, as were the others listed here.

Page 28864, line 2: Back trajectory correction is an awkward term since back trajectories have large uncertainties associated with them. Perhaps say back trajectory attribution? That at least does not suggest anything about the correct nature of the assigned trajectory.

Page 28864, last sentence: If persisting in making this statement, which is not needed, perhaps be more explicit about what work is needed, such as defining atmospheric concentrations in likely situations such as biomass burning.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 28845, 2014.