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Interactive Comment

Interactive comment on "Ice nucleation by soil dusts: relative importance of mineral dust and biogenic components" *by* D. O'Sullivan et al.

Anonymous Referee #2

Received and published: 14 October 2013

General remarks:

The paper deals with the investigation of the ice nucleation behaviour of soils dusts and here especially the relative importance of mineral dust and biogenic components. The manuscript presents very interesting results, the interpretation of which is sometimes somewhat speculative. Specifically the authors should be a little more careful in extrapolating their certainly valuable laboratory results to the atmosphere. The paper deals with a topic highly relevant to the field of atmospheric research and should be published after mayor revisions (see below).

My two major concerns are related to a) the transformations (converting active site mass to surface density) and scalings (scaling surface site density by Feldspar mass)

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Interactive Discussion

Discussion Paper

not being discussed thoroughly enough, and b) the “discussion” and “conclusions” sections being somewhat too speculative.

Specific comments:

Page 20277, line 7: The statement “These processes tend to shift the size distribution of hydrometeors in a cloud to larger sizes at lower concentrations.” should either be explained in a little more detail or be removed.

Page 20278, line 2 ff: The statement “. . ., but it has been suggested that due to thermophoretic effects, contact nucleation is favourable only in water subsaturated regimes, where cloud droplets disappear rapidly prior to freezing (Philips et al., 2007).“ should either be explained in a little more detail or be removed.

Page 20278, line 6: The term "relative efficiency“ needs to be defined.

Page 20280, line 26: The consequences and possible artefacts due to the wet sieving process should be discussed in detail.

Page 20282, line 3ff: The uncertainties due to particle shape effects should be addressed and estimated.

Page 20283, line 19: Assuming that a variation in volume by six orders of magnitude corresponds to change in surface area by around six orders of magnitude is only correct if the change in volume is achieved by varying the number concentration, and assuming the shape of the size distribution to be the same. If I understood it correctly, this is not true for the picolitre droplet freezing experiments. This issue needs to be discussed in more detail and resulting uncertainties need to be addressed.

Page 20286, line 18ff: The statement “. . . is expected to scale directly with surface area . . .” is not necessarily correct for biological IN as outlined in Hartmann et al., 2013 and Augustin et al., 2012. They suggest that the number of ice nucleating entities per droplet is the controlling parameter which may not be related to particles surface area. I suggest to cite these publications and weaken the statement.

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Page 20286, line 26ff: This description seems somewhat incomplete to me. Why are three bins used ? Why needs the bin centre to be estimated from the median of the droplet sizes enclosed within a bin ? Is the droplet population as polydisperse as suggested by the median been volumes? I think a little more explanation and discussion could be useful here.

Page 20287, line 15: Figure 4 could be larger.

Page 20287, line 23: Scaling ns by mass is an easy but maybe incorrect approach. To my understanding it at least implies that the particles are internally mixed and that the size distributions are similar. These assumptions should be discussed and proven if possible.

Page 20288, line 8: The authors state that "In this regime we estimate that most droplets do not contain particles with diameters above 0.4 μm despite these particles making up a significant part of the distribution. Hence, we were unable to determine nm or ns from these freezing data." This is correct, however in my opinion, the whole issue needs to be discussed earlier and in more detail (see above).

Page 20289, line 14: At this point immediately the question arises, why the influences of the high temperature IN is not visible at lower temperatures, i.e., in the nano and picolitre experiments. A reference to this question being answered further down in the manuscript could be useful, here.

Page 20291, line 18: Recent investigations by Hartmann et al. (2013) and Augustin et al. (2012) (please follow status of this paper) are newer publications which confirm these statements and were able to observe immersion freezing of droplets due to single macromolecules. It might be useful to cite these publications here as well.

Page 20294, line 19ff: I personally don't consider this paragraph important and suggest to remove it from the manuscript.

References: Hartmann, S., Augustin, S., Clauss, T., Wex, H., Santl-Temkiv, T., Voigt-

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laender, J., Niedermeier, D., and Stratmann, F.: Immersion freezing of ice nucleation active protein complexes, Atmos. Chem. Phys., 13, 5751–5766, doi:10.5194/acp-13-5751-2013

Augustin, S., Hartmann, S., Pummer, B., Grothe, H., Niedermeier, D., Clauss, T., Voigtlaender, J., Tomsche, L, Wex, H. and Stratmann, F.: Immersion freezing of birch pollen washing water, Atmos. Chem. Phys. Discuss., 12, 32911–32943, doi:10.5194/acpd-12-32911-2012, 2012.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 20275, 2013.

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