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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Biological residues define the ice nucleation properties of soil dust" *by* F. Conen et al.

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Conen et al. report an experimental study of the ice nucleating ability of soil samples from various regions around the globe. They present experimental evidence to support the hypothesis that the most efficient ice nuclei in soil are of biological origin and suggest that this may be an important source of ice nuclei in the atmosphere. This is an interesting study which is within the scope of ACP and I support its full publication once the following comments have been addressed.

1) There is an issue with the use of the term 'ice nuclei'. An ice nucleus is a discrete particle which can catalyse ice formation. Whether this particle contains a minor component which nucleates ice, active sites or its ice nucleating ability is just inherent to that particle the overall particle is the 'ice nucleus'. I think that this doesn't necessarily

come across in this way. Conen et al. report ice nuclei per mass of material. Is this not more correctly nucleation sites per mass of material? By reporting ice nuclei per mass the impression is given that there are x number of discrete particles per mass, but I think that it is not possible to state this. This is important because it affects how we should use the data and compare it to ice nucleation by other materials. The definitions need to be made clear and also the paper adjusted accordingly.

2) On a related issue, the authors cite Vali (1971) for the methodology of how the experimental data is used to derive the number of ice nuclei per mass of soil. Inspection of this paper does not clearly reveal how Conen et al. derived their results. I recommend that the authors include a brief description of their data analysis in the paper, including the equations they use. Also, Vali (1971) derives expressions for the 'Cumulative nucleus spectra' and also the 'Differential nucleus spectra'. It is not clear what Conen et al. are referring to with their 'number of ice nuclei'. In addition, Conent et al. report values per unit mass, whereas Vali reports values per unit volume.

3) In this article the authors have stated that they have used the singular model described by Vali which is a time independent model. It is well known that nucleation is time dependent , but it is often assumed that time is secondary relative to particle to particle variability and can therefore be neglected. This is a valid assumption in some situations, but not all (e.g. see Murray et al (Atmos. Chem. Phys., 11, 4191–4207, 2011) and Vali (J. Atmos. Sci., 51, 1843–1856, 1994). The authors need to justify their assumption that time is not important.

4) P16586, ln 22. The homogeneous freezing temperature of -38 C is a bit low. See Murray et al.(PCCP, 2010, 12, 10380–10387) for a compilation of experimental data. A number of -36 to -37 is closer to the mark.

5) P16588, ln 6. 'immersion...majority of nucleation....' This is stated far too strongly. I do not think this very general statement can be made on the basis of any work so far. It is certainly an important mode, but its importance cannot be stated so strongly.

6) P16589. ln 1-4. Does the H_2O_2 and heat treatment affect the mineral components in soil? The control experiment has been done with montmorillonite, but this is just one mineral found in soil. Could the others behave in a different way? Is it possible that a mineral component in soil causes ice nucleation and that this deactivates with heat and H_2O_2?

7) P16589, ln 11. Who states that montmorillonite is the most active mineral? This is a striking claim that I think cannot be justified.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 16585, 2011.