

Interactive comment on “Feldspar minerals as efficient deposition ice nuclei” by J. D. Yakobi-Hancock et al.

Anonymous Referee #2

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Review of “Feldspar minerals as efficient deposition ice nuclei” by Yakobi-Hancock et al.

In this paper, the authors report a nice systematic study of ice nucleation on a range of minerals in the deposition mode. One of the main findings is that feldspar minerals are the best ice nuclei, a finding that is complementary to the recent paper by Atkinson et al., Nature, 2013. The topic and results are important and well suited for ACP.

My major comments on this paper concern the method of producing particles (wet nebulization) and the washing procedure used by the authors. The authors need to justify better the methods for nebulizing the particles and the washing procedure and also discuss fully the possible artifacts/modifications the nebulization and washing proce-

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dures may introduce. In addition, the authors should consider carry out experiments with a dry nebulization technique to support their findings. Listed below are major and minor comments. Once these comments are adequately addressed the paper should be published in ACP.

Major comments:

1. I don't see the justification for doing washed samples. The authors indicate that this is done to remove soluble material. However, if the particles contain soluble material, experiments that include the soluble material seem most relevant for the atmosphere. Additional discussion on the atmospheric relevance of the washed samples should be given.
2. Page 17308, line 12-13. IF the outermost K⁺ ions of orthoclase are removed upon contact with deionized water, it is not clear how the washing experiments are applicable to deposition freezing in the atmosphere. Mineral dust will certainly be exposed to water during cloud activation, but after evaporation the water will be removed and K⁺ ions returned to the mineral, I assume? Again, please discuss the atmospheric significance of the washed experiments and conditions where mineral dust will be washed with water followed by deposition freezing.
3. Page 17306, line 24-26. The authors suggest the difference between Welti et al., 2009 and the current studies may be because Welti et al didn't wash their samples and non-washed samples may have IN-facilitating soluble material. First, some laboratory experiments suggest that soluble material will inhibit ice nucleation, not facilitate it (see (Cziczo et al., 2009) and references therein). Second, can the method of producing particles in the current study (generating particles by atomizing water suspensions) introduce insoluble material even if the water is clean? These points should be addressed in the manuscript.
4. Abstract and Page 17311, paragraph 1-2. The authors are discussing the results for Mojave Desert Dust (MDD) and Arizona Test Dust (ATD). For this discussion they only

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refer to the washed results. Please make it clear though out the document which results are being discussed (i.e. washed vs. unwashed). Also, why only discuss the washed results at these locations? Are the washed results more atmospherically relevant?

5. Page 17314, line 1-4. The authors argue that wet atomized particles is atmospherically relevant for deposition IN studies since particles in the atmosphere pass through regions of water saturation and cloud activation. I agree with this for single component minerals, but I am not convinced that wet atomized particles is atmospherically relevant for multicomponent mixtures such as MDD and ATD, since previous studies have shown that when multicomponent mixtures are wet atomized, soluble material can be redistributed on the particles in an unrepresentative way (Koehler et al., 2007). The authors should justify why the wet atomized experiments are relevant in light of the experiments by Koehler et al., 2007.

Minor comments:

1. The authors measure the electrical conductivity of the suspensions prior to nebulization. It would be beneficial to include this information in Table 1 or a supplemental table.

2. Page 17303, line 19-21. "although particle size selection was accomplished with the DMA at 200 nm, roughly 25% of the total number of particles exiting the DMA were multiply charged particles larger than 200 nm." What fraction of the total surface area in the experiments can be attributed to 200 nm particles? If most of the surface area is from particles larger than 200 nm, then is it accurate to indicate that the experiments in the current manuscript focus on particles with diameters of 200 nm? Please discuss in the manuscript.

3. Page 17302, 13-22. The point of this section is not clear. Are the authors making the point that only Zimmermann et al. and Atkinson et al. have studied extensively the minor components of minerals in a controlled manner? Please rewrite for clarity.

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4. Experimental procedure. The authors start by stating that each compound was washed twice prior to the freezing experiments. It would be more clear to state that for most experiments each compound was washed twice.

5. Page 17307, line 23-24. My understanding from the initial discussion is that kaolinite does not contain counterions. If this is the case, line 23-24 needs to be modified.

6. Page 17311, line 20-23. Here the authors indicate that the compounds were washed twice, implying all particles were washed twice. This statement leads to confusion since some of the particles were not washed. Also, my understanding is that soluble material inhibits ice nucleation, contrary to what the authors state here (see (Cziczo et al., 2009) and references therein).

7. Page 17313 line 7-24. The authors are making conclusions on the amount of soluble material based on the electrical conductivity measurements. To be exact, I think the authors should only make conclusions on the amount of soluble ions, not soluble material. Also on page 17304, line 10-12, the authors state that the amount of soluble material in each solution, was determined using a conductivity meter. The "amount of soluble material" should be changed to "amount of soluble ions".

8. Page 17314, lines 21-24. Can quartz also contain surface hydroxyl groups that can strongly hydrogen bond?

9. Table 1. The uncertainties are high for a few compounds (e.g. k-feldspar and ATD). Is this due to an uncertainty in the measurements of RH_i and temperature or a variability in the IN properties from day-to-day or sample-to-sample.

10. Table 1. I assume the authors want to organize the data by increasing critical RH_i. However, some of the rows are out of order. For example MDD unwashed should be the second entry in the table.

11. I don't think Figure 1 is referred to in the main text.

12. Since feldspar is one of the focuses of this paper, I would like to see an activated

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fraction curve for feldspar included in Figure 2.

References:

Cziczo, D. J., Froyd, K. D., Gallavardin, S. J., Moehler, O., Benz, S., Saathoff, H., and Murphy, D. M., Deactivation of ice nuclei due to atmospherically relevant surface coatings, *Environmental Research Letters*, 4(4), 2009.

Koehler, K. A., Kreidenweis, S. M., DeMott, P. J., Prenni, A. J., and Petters, M. D., Potential impact of Owens (dry) Lake dust on warm and cold cloud formation, *J. Geophys. Res.*, 112(D12), 2007.

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