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Interactive comment on “Laboratory studies of immersion and deposition mode ice nucleation of ozone aged mineral dust particles” by Z. A. Kanji et al.

Anonymous Referee #2

Received and published: 7 June 2013

Kanji et al. present new laboratory studies of immersion and deposition ice nucleation of aged mineral dust particles. Chosen proxies for mineral dust are Arizona Test Dust and Kaolinite. The novelty of this work is that particles were aged using various levels of O₃ exposure. Relatively minor changes in freezing temperatures were observed.

This is a timely study that is of interest to the readers of Atmospheric Chemistry and Physics. The methods are sound and the data are analyzed appropriately in terms of measurement uncertainty. The modeling and parameterization is commensurate with our current understanding of ice nucleation and the results are placed into the context of the currently ongoing debate in the IN community. My main concern is the

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writing style. In this respect, the manuscript needs to be substantially improved prior to publication. The structure convolutes results and discussion, the writing is often repetitive and explores tangential points that do contribute relatively little value to the main theme of the work. I therefore recommend that the revised manuscript should be significantly shortened. Furthermore, while the results are interesting and important and certainly worthy to be published, my personal interpretation of the raw data is that the shifts are insignificant relative to current measurement uncertainties, and probably also relative to its impacts on the atmospheric aging of dust.

Comments:

I don't understand why so much space is devoted to the O₃ uptake results. It is sufficient to demonstrate briefly that O₃ is taken up and that gamma values are approximately consistent with previous studies. A couple of paragraphs would suffice.

There is significant overlap between the introduction and discussion the material should be consolidated and presented only once.

I question the comparison with field experiments. The closure calculations performed for CRYSTAL-FACE and PACDEX are too poorly constrained to warrant inclusion here. It is encouraging to see that the that the results broadly make sense in the context of ambient measurements. Nevertheless, closure attempts that use properties from this aging study that are combined with poorly constrained aerosol composition and non-MD contributions to IN are premature. If the authors feel that such closure studies can be attempted they should devote a separate paper to it. Certainly even the most optimistic interpretation of a 3K shift attributed to O₃ ageing is too small to result in meaningful changes in predicted-vs-observed ambient IN comparisons. The results presented here stand well on its own and should focus simply on the observed results rather than making a giant leap to past field campaigns. The resulting statements regarding the parameterization conclusion stated in the abstract needs to be removed.

Comparisons to previous work: this is attempted in two places (Much of Section 3 and

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Section 4.5). The text isn't very clear how well the quantitative agreement is. Generally, comparing the active fraction as a function of freezing temperature gives (relatively) poor agreement when comparing across different studies, CFDCs, and other IN techniques. A specific example are comparisons between the ATD results from Welti et al., Sullivan et al. and Niedermeier et al. It is something we need to accept to be the case and quantify better. Personally, I don't think it is a big problem for this study as the changes are relative to the same instrument/technique. It would be helpful though if the comparison to previous results centered around a summary graph. Also the comparisons in the different sections should be consolidated and can be presented more concisely. Since the authors derive active site densities, they can account for size effects (or polydispersion), assuming that density only depends on surface area and that these properties do not systematically change with particle size.

I also concur with the excellent points made by the first referee.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 8701, 2013.

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