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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Towards parametrising atmospheric concentrations of ice nucleating particles active at moderate supercooling" *by* Claudia Mignani et al.

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General Comments

This is a welcome and concise contribution to the literature regarding parameterization of ice nucleating particle measurements. It suggests that the size required for best relating INP concentrations to aerosols deserves revisiting in general, depending on the conditions for INP processing, and it suggests that a larger size is required than used in past parameterizations using aerosol concentration-size relations as a parameterization basis (at least for the mineral dust related parameterization they investigate). This is not an entirely new recommendation, but is clearly presented here. I do not subscribe

to $-15°$C as the critical or only temperature needed for constraining parameterizations as relevant to wintertime precipitation. In general, the non-comprehensiveness of using only $-15°$C data is a limitation for any widespread application. I have some comments as well on clarifying the specification of size as being aerodynamic versus physical. This matters for the comparison to literature shown. In that regard, it should be made clear that the D15 parameterization is specifically for mineral dusts. The findings regarding the relation or not of apparent biological INPs to aerosol sizes is a new one to my knowledge, and is very interesting in its implications. This paper clearly motivates future studies to improve parameterizations overall through use of field data.

Specific Comments

1) Abstract: Lines 5-6: A key question for this might also be how divorced a surface site is from the free troposphere where clouds form? Or are the clouds always tied to the boundary layer and have cloud tops relevant to the measurements.

2) Introduction: Line 13: To be clear, this parameterization is for mineral dust as a single category only. It should not be expected to represent a multivariate INP population.

Lines 17-19: While this dive into the meteorology/climatology of winter precipitation is much appreciated, I want to note a caveat with regard to these studies. First, they were dominated by observations from over continental regions that were largely non-mountainous, unlike the site in the present study. Secondly, colder cloud top modes for precipitation are also noted in the referenced studies. This is consistent with earlier work by Rauber et al. (1987) from over mountainous regions in the United States, where both the $-15°$C maximum associated with the dendritic growth regime and colder cloud top precipitation events are noted. There is no way to know when colder-topped clouds may be impacting the persistence of liquid water in the $-15°$C regime, and thereby altering the microphysical scenario. Schultz et al. (2001) critiqued the inadequacy of this isotherm alone (in the Wetzel et al. scheme) for predicting heavy precipitation that can occur over a broad range of cloud top temperatures. So perhaps note this as "one of the modes" for winter precipitation. Hence, while it is reasonable to select this temperature for the present study, a caveat is needed because it is not possible to sit in any one location to make measurements and assume that the only relevant temperature for clouds passing over is a single value.

Line 20: What does the statement "$-15°$C or warmer" mean exactly? It is repeated in the Fig. 1 caption, but the "or warmer" part is not explained or justified. Also, with regard to the statement on the sizes of INPs relevant at this temperature, I would note that widespread measurements over continents in winter are, however, limited. Measurements of size-resolved INPs are even rarer in winter, at least to my knowledge of locations and the number of measurements that have been made of INP sizes. I understand that larger sizes indicate a better correlation in this paper, but I suggest care in assuming that the role of only larger INPs in this temperature regime is confirmed as relevant to winter clouds.

3) Material and Method

Line 33: Has the Coriolis collection method been compared to any other standard method, such as filters? This would be good to know.

Line 36: Can you clarify why there is an upper bound to measured concentrations?

Line 37: Does the detection limit mean the stated lower limit of quantification or what does it mean? I assume the background comparison would use particles per ml, rather than per L of air. Can you state values in that manner? Does it literally mean that no wells froze when you removed ultrapure water from the sampler and ran the same cooling rates?

Line 39: I am curious about the use of specific APS channel bin limits to define [n0.5] and [n2.0]. Aerodynamic size is different from physical size, and if using APS data to constrain values from a parameterization, especially D15, it requires conversion first to equivalent spherical physical diameter (using effective density or density and an

assumed shape factor), and then recalculation of the concentrations above 500 nm diameter. Otherwise the concentrations could be biased high for use in D15.

Line 46: Can you clarify what is meant by "amongst others" used here?

4) Results and Discussion

Line 59: As mentioned already, please clarify how [n0.5] as defined by the APS is used in D15. Also, I infer from Fig. 1 that you have not used the scaling factor derived in D15 for application in modeling total INP concentrations, but you have not stated that. It means that you do not assume the correction of continuous flow diffusion chamber data that was demonstrated valid for laboratory and transported ambient Saharan dust in D15. Please say so and perhaps justify.

Note on Figure 1: The listing of correlation coefficients for the fits shown, and perhaps also for the 500 nm concentrations (could list all of these in a table, for example), would be useful. This would put some quantification into the statement about "clarity of separation" of data when 2 microns is used as the reference. Also, the diamonds in the figure are extremely hard to resolve, but perhaps at a full page size the figure will be easier to read. Finally, please clarify again in the caption if the x-axis concentration values are for aerodynamic or physical size.

Line 68-69: Although I infer that you are getting at the enrichment of INPs that are inferred to be of biological origin following rain events, the statement "enrichment of the aerosol population with highly efficient INPs during precipitation" was a little confusing, because your data show that aerosol concentrations under rain events are typically lower on average.

Line 70: suggest "masses" for "mass"

Lines 73-75: While you have used a constant here to describe an assumed physical process whereby the production is independent of aerosols already in the air, I am curious if there is any dependence of this process on precipitation rate? Or in a complex way on that and existing surface wetness? Does it always produce large INPs?

Line 90: As stated earlier, I do not feel that such a strong statement can be made regarding the importance primarily of the $-15°$C INPs. Not without specific inspection for many sites around the world. And I believe that literature supports that this single temperature is not exclusive for predicting wintertime precipitation.

References

Rauber, R. M., 1987: Characteristics of cloud ice and precipitation during winter storms of the mountains of Northern Colorado, J. Clim. Appl. Meteor., 26, 488-524.

Schultz, D. M., Cortinas, J. V. Jr., and Doswell C.A. III, 2001, Comments on “An operational ingredients-based methodology for forecasting midlatitude winter season precipitation”, Wea. and Forecasting, 17, 160-167.

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