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Biogeosciences Discussions

Interactive comment on "Heterogeneous ice nucleation activity of bacteria: new laboratory experiments at simulated cloud conditions" *by* O. Möhler et al.

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General comments

This paper discusses physical simulations of how bacterial ice nuclei activate ice formation while freely suspended in an air parcel entering cloud. While the paper leaves room for improving methods and insights, I found it to be a reasonably good contribution to present knowledge. The paper would benefit from some additional detailed attention to organization so that superfluous details and repetition are removed and flow of reading is improved. I offer some specific suggestions and other critical com-

ments below.

Specific comments

Abstract:

1) It is not clear in the abstract or in the paper if the direct spray experiments had an explicit purpose or were just to take advantage of the fact that the cells had to be generated into the cold chamber volume and dispersed without a great amount of time passing. I was curious why the cells were not dried first prior to injection so that nucleation was not spontaneous. It is not clear what impact this injection process might have on ice formation. The thermodynamics do not reproduce any process expected in the atmosphere.

2) What is meant by the bacteria acting first as condensation nuclei and then "eventually acted as ice nuclei to freeze the drops." Can you really distinguish if the condensation preceded droplet formation versus happening simultaneously with it? This statement could be interpreted to suggest that freezing required time to occur. Although the paper appears to support that idea, it is not clear to me how time-dependent ice formation can be separated from that due to continued cooling and the effects of ice crystal sedimentation.

3) The abstract should probably indicate if the results confirm or support those from previous studies of Snomax.

Introduction:

1) Page 1447, lines 3-4: Have "several groups" validated the presence of P. syringae in clouds or at the level of clouds or a few groups? Two references are given. One is

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cloud data, but from a ground site. So, I feel that stating your point so strongly fails to highlight that there is only modest evidence at present. A study missing in this list is Jayaweera and Flanagan (1982; Geophys. Res. Lett., 9, 94-97). These authors found some cells to the higher altitudes typical of long range transport.

Preparation of bacterial cells:

1) Page 1448, lines 11-12: Please be clear on how the cell concentration was determined in the chamber. Was it simply calculated from volume dilution of amount sprayed? Or did you somehow attempt to integrate numbers from the aerosol size distribution, and how? This whole paragraph could be reduced in size through use of more concise statements.

Droplet freezing studies:

1) Page 1449, Section 3: Why were the droplet freezing studies done first? The point of this exercise should be stated. If to compare a standard method for assessing ice nucleation activity to a more realistic simulation, the results appear to show failure although the comparison is not comprehensive enough.

Cloud simulation experiments:

1) Page 1451, line 13: Could you say a little bit more about how the suspensions were sprayed (by what device) into the chamber and dispersed? Given that the particles were freezing upon adjusting to the chamber temperature, what is the likelihood that they were uniformly dispersed prior to detection as ice? Furthermore, repeating my earlier comment, why were these experiments done? That is, what do they tell us, and how do you know the time history of the activation behavior?

2) Page 1451, lines 24-25: A minor point here would be to clarify that the size range of

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interest that is not subject to sampling tube losses is that of the bacterial cells, not the total particle concentration.

3) Page 1452, lines 6-8: The non-spherical nature of the bacteria must surely complicate detection of first ice with the SIMONE instrument (Fig. 4 for example).

4) Page 1452, line 16-17: Do you have a reference for the expected size of bacterial cells. These sizes seem small. Is it because the cells have dried out?

5) Page 1452, line 26-27: Is fitting the distributions and integrating the larger mode the method used to obtain the number of bacterial cells mentioned earlier in the paper? If so, this clearly points to a need for better organization of information to separate procedures from results.

6) Page 1453, line 17: Is cooling in the chamber ever really *adiabatic*? It seems that it is always counteracted by diabatic effects.

Results and Discussion

1) Page 1455, line 16: Since a measurement was apparently not obtained, is it correct to term the lower detection limit for ice formation an "upper limit" for bacterial ice formation?

2) Page 1455, lines 19-20: It is possible to estimate an active fraction of SM cells at -6 degrees C based on the temperature activation spectrum measured by Ward and DeMott (1989) in their cloud chamber experiments. It is roughly 15

3) Page 1456, line 15: A question of clarification 8211; is the "deposition" nucleation mode defined as the relevant mode in this case simply because the relative humidity is below 100

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4) Page 1457, general comments: I find the single test of the validity of the spray method versus expansion cooling to be unconvincing. Extrapolation to validity at warmer temperature is not assured. Regarding removal efficiency of ice crystals, could some estimates be made of the expected removal times due to ice crystal growth and sedimentation based on the observed size of ice crystals? The chamber is quite large in volume and vertical extent, so it seems possible for sedimentation to impact both the apparent ice formation signal and the loss of particles. Without resolving this issue, inferences made to deactivation of cells must be considered very speculative.

Conclusions:

1) Page 1458, line 19-20: Was no significant activity obtained at warmer temperatures, or could it simply not be detected within the limits of the measurements?

Technical corrections

1) Page 1447, lines 12-15 need language correction: Replace "growth" with "grows" in the first sentence. Replace next sentence with "The warmer the freezing temperature the more time the ice particles have to take part in this sequence and the more likely they are to grow to precipitation size."

2) Page 1447, line 19: Suggest adding "number" before "concentrations".

3) Page 1448, line 7: Suggest replacing "industrial secret" with "proprietary information".

4) Page 1448, line 22: "The samples have again be tested for their INA...". Do you mean they were tested?

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5) Page 1449, line 25: Word "from" not needed.

6) Page 1452, line 6: "Also" is spelled incorrectly.

7) Page 1454, line 27-29: This is possibly the third mention of the Welas instruments, again suggesting more careful attention needed toward organization and details.

8) Page 1456, line 12: I suggest replacing "approved" with "confirmed".

9) Page 1456, line 16: Did ice number concentration increase or decrease with time due to settling?

10) Page 1456, line 26: Omit "at least".

11) Page 1457, line 6: bacterial cells have "been".

12) Figures 4 and 5: I would find it very helpful to increase the size of the labels in these plots and to repeat in the first figure caption the descriptions of the different parameters plotted.

Interactive comment on Biogeosciences Discuss., 5, 1445, 2008.