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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The ice nucleation ability of one of the most abundant types of fungal spores found in the atmosphere" *by* R. Iannone et al.

P. DeMott (Referee)

pdemott@lamar.colostate.edu

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General Comment

This manuscript provides an insightful, well written, and very well documented investigation of the ice nucleating properties of an atmospherically relevant biological particle type. Investigations of biological ice nuclei are of great current interest, and there is a great need to identify the most important particle types. In this case, it is very useful to have these findings available for one of the more common types of fungal spores. I have only an assortment of minor clarifications and concerns regarding statements and calculations presented.

Specific Comments

Abstract

The first sentence struck me as a little too strong for the current status of understanding of biological ice nuclei as a whole. I believe that it is safe to say, at best, that biological particles are a potentially important class of ice nuclei. I think this deserves qualification because the level of quantification of their importance at present, except by inference, is quite poor. For the same reason, I consider the statement that the results "do not appear to explain recent atmospheric measurements showing that biological particles are important ice nuclei" to similarly overstate present evidence. I suggest "participate as atmospheric ice nuclei" to replace "are important. . ."

1. Introduction

Page 24624, lines 18-19: You might add that while number concentrations per volume of air are of first order importance, the activation spectrum (proportion active versus relevant thermodynamic conditions) is also important for establishing atmospheric relevance.

2. Experimental

Page 24626, lines 15-16: Can you comment on whether any experiments were performed at different cooling rates and whether or not this mattered?

3. Results and discussion

Page 24630, line 16: The authors may or may not wish to comment here on the fact that these active fractions found for fungal spores do not seem all that different than found for pure kaolinite particles of similar spherical equivalent sizes by Murray et al. (2010).

Page 24630, lines 17 paragraph: Please clarify in words here that the data shown in Fig. 7 are either mean or median freezing temperature, whichever the case may be

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(terms seem to be used interchangeably in the following discussion).

Page 24630, lines 24-25: To be clear, are inclusion sizes determined by tagging each drop freezing event by location and then sizing these particles, or is the inclusion size inferred from the average of all spores used in each series of freezing tests? Part of my own confusion may stem from the fact that these "heterogeneous data" data must also be mean of median freezing temperature data. Please clarify and qualify each time the data are discussed after this point. For example, the last sentence on this page discusses an "increase in the freezing temperature," which should say "median freezing temperature."

Page 24632, lines 19-20: I may be cutting hairs here perhaps, but it was suggested by DeMott and Prenni that warmer temperature IN "could be" biological particles, a somewhat different assertion than proposing that biological particles do explain these IN.

Page 24632, lines 22-23: How was the proportion of spores active warmer than -15C estimated ("we can expect. . .")? The fraction seems clearly less than observable directly with the experimental setup.

Page 24632, lines 25-28: You might clarify that the estimate of number concentrations is an estimate for the boundary layer, and you might state exactly how much lower these concentrations are compared to IN numbers typically present at -15C (e.g., 100 to 1000 times lower).

Page 24633, lines 14-15: Should this say 0.001 percent or 0.001 as a fraction? The lowest measurement value shown in Fig. 6 is a fraction of about 0.004, so again I wonder about the 0.001 percent value and how it was estimated. Also, be careful about attributing any results to temperatures for which no data have been collected.

Page 24633, lines 22: Should this read greater than or equal to 700 nm for the residual aerosol sizes not assessed by the MS? Also, by the end of this section I began

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to wonder a little bit about the point of comparison to two studies that had limited detection capabilities for the primary mode-size of single spores. Is the point that the atmospheric studies might reflect the action of some other type of biological particles, just not spores? The point should be made clearer in any case.

Editorial comments

Introduction, page 24622, line 25: I suggest stating that freezing occurs “initially” by heterogeneous nucleation, to distinguish the fact that freezing in clouds includes other secondary processes not involving nucleation.

Introduction, page 24624, line 27: Please explain the meaning of the terminology "passively launched."

Results and discussion, page 24629, lines 19-20: “The freezing results for pure water droplets are consistent with results expected for homogeneous freezing.” The sentence is repetitive with statements made earlier in this paragraph and is probably not necessary. In the prior sentence I suggest clarifying as “classical homogeneous nucleation theory.”

Experimental, Page 24631, last sentence: Phillips

References (additional)

Murray, B. J., T. W. Wilson, S. L. Broadley, and R. H. Wills: Heterogeneous freezing of water droplets containing kaolinite and montmorillonite particles, Atmos. Chem. Phys. Discuss., 10, 9695–9729, 2010.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 24621, 2010.