

Interactive comment on “Ice nucleation and its effect on the atmospheric transport of fungal spores from the classes *Agaricomycetes*, *Ustilaginomycetes*, and *Eurotiomycetes*” by D. I. Haga et al.

Anonymous Referee #3

Received and published: 19 April 2014

This study combined laboratory measurements and modeling effort to investigate: 1) immersion freezing on 12 types of fungal spores and 2) possible effects of ice nucleation on the atmospheric transport and distributions of these species. The results show that ice nucleation ability of *Ustilaginomycetes* is higher than the other two classes. These fungal spores are less efficient than Asian mineral dust when comparing on surface area basis. Using a global chemistry-climate transport model, the authors conclude that the ice nucleation of these spores can influence their distribution in the atmosphere. Because of the limit ice nucleation data in the literature, this study pro-

C1556

vided substantial new data for fungal spores and should be archived. The paper is well written and contains interesting and original work. Therefore the manuscript is suitable for publication in ACP once the following comments/issues are addressed.

Major comments:

1), the manuscript gave a detail summary on the identification and abundant of fungal spores in the atmosphere and showed possible importance of ice nucleation on these spores. As shown in the Introduction section, the reported concentrations of these spores are often less than 100 L⁻¹ in the boundary layers. These are relatively low concentrations compared to other types of atmospheric particles. In addition, due to the large sizes of the spores and long term transport, what could be the possible fractions of these spores that would be transported to remote regions and higher altitude? The manuscript provided insufficient discussion on the concentration and distribution of investigated species in the model which may affect the modeling results. What emission rate was used in the model (page 5025, line 13)? It could be very informative to show the global and vertical distribution of the investigated spores, although using a single emission rate for all the land surface is oversimplified. It is not clear that whether model simulation included all the spores at once or separately. In Table 2, the simulation use $R_{nuc,ice} = 1$ in IN-Active case for T between -25 and -35C, this assumption clearly will result in overestimation of percentage change between IN-Active and IN-Inactive in mixing ratios due to ice nucleation (the results shown in Figures 5 and 6). $R_{nuc,ice}$ is temperature and spore type dependent which can be seen from the results shown in Figures 2 and 3.

2), could you comment on the possible effects of the ice nucleation competition between fungal spores and other atmospheric particles (for example, dust and marine diatoms for open ocean) on the redistribution of fungal spores? When co-existing with more efficient ice nucleating atmospheric particles and when ice crystals form more efficient IN, the fraction of fungal spores forming ice will be significantly lower than the current IN-Active calculations or assumptions. Thus the results shown in this study

C1557

could be overestimated if consider the realistic atmospheric implications.

In short, caution is needed to interpret the results from these model simulations with current assumptions. The conclusions from the modeling section need to be reworded or additional discussion is needed to clarify the potential bias.

Specific comments:

1, Since the modeling results (section 3.5) don't directly related to the discussion in section 3.6, it is suggested to move section 3.6 right after section 3.4. The paper may flow better. If the authors choose to do so, the order of corresponding figures and last two paragraphs in page 5019 also need to be changed.

2, Page 5024, Line 19-26, please provide a brief discussion or justification for using 0.05 for ice cloud in the simulation (Table 2). Due to the lack of data in lower temperatures (<-35C), ice cloud is not necessary needed in the simulations and the modeling investigation should only focus on mixed phase clouds.

3, Page 5026, Line9, since surface area data are critical, please provide the range of Spores/Drop in Table 1 or other percentiles. Also, it could be useful to include the surface area data in figure 4 as a second panel. Line 13-15, did the authors consider plotting the surface area vs. freezing temperature including each nucleation event? The freezing temperature vs. surface area plot could provide additional information on surface dependence.

4, Page 5026, Line 21, It is not very clear what is cumulative number of ice nuclei "per spore", why not using surface area for each nucleation event? The definition or the formula used here and in the paper by Haga et al, 2013 are slightly different compared to the formula used by Vali (1971). Please provide a brief description on the determination of cumulative number of ice nuclei per spore and give the formula. This could be placed in supporting document. This is easier for readers to follow the paper.

5, Page 5030, Line 25-28, as described in the Section 2.4 (page 5023,line 9), the dry

C1558

deposition is considered, so if consider the size of 3 and 8 micrometer of spores and dry deposition, what are the concentrations/mixing ratios at about, for example, 300 hPa and 900 hPa? Please see the comments above regarding the vertical distribution of fungal spores before any ice nucleation simulation. The impression is that the mixing ratios at 300 hPa level could be significant lower than those at 900 hPa, especially for 8 micrometer case. Even if all spores nucleated ice and are transported to lower altitudes, they won't account for all the percentage increases below 5 km (as for now, from Figs. 5 and 6, the absolute percent changes for high (negative) and low (positive) altitudes are similar). A back-of-the-envelope calculation may help to understand the contribution of downward transport (due to ice nucleation) to the changes in low altitudes. I could be completely wrong on this speculation, but current manuscript doesn't provide sufficient data or evidences to support the explanations and conclusions.

6, Table 2, missing degree signs in the table

7, Figure 1, the scale bars are not very clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 5013, 2014.

C1559