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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The contribution of fungal spores and bacteria to regional and global aerosol number and ice nucleation immersion freezing rates" *by* D. V. Spracklen and C. L. Heald

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General comments

This manuscript by D. Spracklen and C. Heald picks up a study by myself and colleagues which was published in 2010 (Hoose et al., 2010), using a different model and adding analyses about supermicron particles, CCN and the regional distribution of immersion freezing rates. The main results of Hoose et al. (2010) are essentially reproduced. This is certainly reassuring, as it means that they are robust with respect to different treatments of the aerosol dynamics, the cloud microphysics, the dust and soot

fields and the fungal spore emission function and size. The more subtle differences in the partitioning of the total freezing rate to the different species are appropriately discussed. The discussion of the regional contributions of PBAP to ice nucleation is essentially limited to one figure and one paragraph of the text. I would have liked to read more about this, especially as it is also emphasized in the title. The additional analyses of supermicron aerosol and CCN are also kept short but clear, maybe because the results were not thought to be very interesting, but I think this paper will be an important reference for these values which have never been calculated before.

The paper is well-written, very clear and a pleasure to read, and in my opinion it can be accepted for ACP once a couple of minor comments are adressed. Nevertheless, I think the article would be greatly enhanced if two points could be adressed which require some more work:

- quantification of the PBAP contribution to giant CCN
- an analysis of the contribution to simulated freezing rates at the sites of the field campaigns which have identified significant numbers of bio-IN (e.g. Prenni et al., 2009; Pratt et al., 2009; Prenni et al., 2013). This could be done e.g. as vertical profiles, seasonal cycles, ...

I leave it to the authors to decide whether or not these additional analyses are included.

Detailed comments

- page 32460, line 23-25: I would like to point out that in Hoose et al. (2010), we stated: "However, these results do not rule out the local, regional and seasonal importance of biological ice nuclei." Thus, I don't think that there is any contrast between the findings presented here and our earlier results.

- page 32461, line 22 and elsewhere: When citing papers about fluorescent biological particles (FBAP), it should be carefully distinguished between FBAP and PBAP.
- page 32462, line 15: Please add that these results refer to one flight.
- page 32463, line 22: I strongly encourage the authors to publish the fit coefficients of their fungal spore emission function (Heald and Spracklen, 2009), such that they are available to the community.
- page 32463, line 27: Can you explain why you are going for the upper estimate?
- The immersion freezing parameterization could also be mentioned in the "Methods" section instead of in the "Results" part.
- page 32466, line 23: I find the agreement and in particular the underprediction surprising as Burrows et al. (2009a)'s upper estimates for the emissions are used. Any comments?
- page 32477: I assume that the observations in panel a) are from (Sesartic and Dallafior, 2011), please add.
- page 32467, line 8: How well are other supermicron particles simulated? Is there any reference in which they are evaluated for GLOMAP?
- page 32468, line 9: I would be curious which fraction of the bacteria and the small/large fungal spores are activated at 0.2% supersaturation.
- Could you also give numbers for the contribution to CCN at a lower supersaturation (i.e. giant CCN)?
- The simulated global mass burdens of bacteria and fungal spores would also be of interest for comparison to published values.

- Are the immersion freezing rates shown here in-cloud values (if so, how is cloud presence diagnosed? RH? presence of liquid water?), all-sky values or are they calculated as a function of temperature only, irrespective of RH?
- If the immersion freezing rates as shown here depend on the presence of clouds, liquid water or RH, then the cloud scheme is also relevant. I assume that the cloud are not influenced by the calculated immersion freezing rates. This could lead to some inconsistencies (e.g. high freezing rates in the lower parts of a cloud but plenty of water above that). This should be mentioned.
- page 32469, line 25: This is interesting, but the question is how relevant these regions with very small immersion freezing rates are. The freezing rate of 10^{-14}cm^{-3}s^{-1} converts to less than 10^{-6}L^{-1}day^{-1}, which is extremely small and probably irrelevant for cloud glaciation and precipitation formation. I understand that the tropics are not shown in Fig. 6 for exactly this reason, but how high are the total freezing rates in the regions which are plotted? I recommend to show results only for regions with a total freezing rate above a physically motivated lower limit. This also depends on the answer to how the averages are calculated.
- I would add "regions where biological particles contribute substantially to **small** ice nucleation rates".

Technical corrections

- page 32461, line 25 and page 32467, line 12: Matthais-Maser → Matthias-Maser
- page 32464, line 25: Dallifor → Dallafior

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 32459, 2013.