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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Drivers of the fungal spore bioaerosol budget: observational analysis and global modelling" *by* Ruud H. H. Janssen et al.

Anonymous Referee #1

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Summary

This study presents two new schemes of fungal spore emissions and compares model results with the well-established parameterization of Heald and Spracklen (2009), as well as, with available observations. This work concludes that the new and more sophisticated emission schemes produce about one order of magnitude lower emissions than previously estimated. I find this paper well written and the conclusions very useful in exploring the uncertainties of different fungal spore emission schemes, along with the calculated atmospheric burden by global models. Some minor issues, however, can be addressed by the authors before the final publication in ACP, to help the reader to better understand the proposed parameterizations.

General comments

The authors state that the new parameterizations result in emission strengths of about an order of magnitude lower than the HS09 model and thus, fungal spores contribute less to the total organic aerosol burden in the atmosphere. However, the HS09 model presents results for both "PM2.5" (diameter < 2.5 μm) and "PM10" (2.5 μm < diameter < 10 μm) fungal spores, in contrast to the emission schemes of this work that are based only on spores with a diameter of 2.5 μm (σ = 1.5). Considering that the emission schemes are highly sensitive to the assumed size of the spores, I wonder whether such a comparison is fair by only referring to the total emitted masses and not also to the respective particle sizes. Further discussion is needed to support this conclusion since the size distribution(s) of the compared emission schemes (i.e., new vs. old) significantly differ.

Specific comments

1. The authors present the new fungal spores' emission schemes in Sect. 2. Although the HS09 parameterization is well established, a somewhat more extended discussion of that parameterization would be useful for the reader (a short discussion is, nevertheless, presented in Sects. 2.5 and 4.). For example, the authors could discuss more on the main differences between the old and the new schemes, i.e.: What is the main driver for the resulted overestimation of the previous scheme compared to the new schemes? Is it only the observations used (i.e., spores counts vs. mannitol concentrations) or/and the sizes of the observed fungal spores? Do the current parameterizations use more advanced statistical tools than previously? And possibly, what global emissions would have been derived if the authors had used mannitol concentrations, as in Heald and Spracklen (2009), on the spores considered in this study? Some of these issues were touched in the discussion section, but a more detailed analysis would be helpful.

2. Page 11, lines 7-12: The dry deposition and the sedimentation budget terms of the model should be more explicitly presented and discussed. Heald and Spracklen (2009) used two modes to parameterize the fungal spore emissions, i.e., a fine mode,

with a diameter < 2.5 μm, and a coarse mode, with a diameter < 10 μm. Although here the authors assume a fungal spore diameter of 2.5μm (σ=1.5), they also assume that the spores are present only in the coarse mode. Do the authors use a different size distribution scheme for this work compared to HS09? How different is this assumption with the one used by Heald and Spracklen (2009)? How do they compare? A more detailed discussion of the aerosol size distribution scheme(s) of the model is necessary to understand these differences.

3. Page 12, line 8: What molecular weight is used in the model for the fungal spores?

4. Page 12, lines 14-16: Considering that for the HS09 model, the fungal spores are present mostly in the coarse mode, sedimentation should be a significant process for their atmospheric lifetime calculation. For this, the respective annual budgets (ideally for all model simulations of this paper) should be presented in Table 3 and discussed in more detail in the manuscript. Besides, an additional Table with all simulations performed for this study would be also very useful.

5. Page 13, lines 25-26: Do the authors refer here to a new simulation (i.e., with fully emitted land-cover)? If yes, what is the impact on the calculated fungal spores' emissions and burdens? How much would that differ compared to the standard simulation? Is this correction applied only to specific boxes (i.e., those include the coordinates of the observation sites used in this work for model evaluation)?

6. Page 17, lines 4-5 & Page 18, lines 1-3: The authors discuss the impact of fungal spores' solubility assumptions (i.e., insoluble vs. fully soluble) on the long-range transport and global burden. Considering, however, a mean lifetime for all simulation of up to $\sim$2 days in the model, the conversion from insoluble to soluble via atmospheric processes (e.g., assuming 1.2-day e-folding conversion from hydrophobic to hydrophilic) may be potentially significant. A short discussion on fungal spores' aging would be here useful.

7. Page 17, line 19: "This suggests that fungal spores contribute less to the organic

aerosol budget. . ." Is this statement valid only for PM2.5 spores, or also for spores up to PM10, when the respective emission schemes are applied?

Technical corrections

i. Page 3, lines 23-28: A more detailed outlook paragraph at the end of Sect. 1 would be useful for the reader.

ii. Page 33: Figure 7 fits better in the supplement.

iii. Page 34: Please explain better in the caption the "simulated" vs. "calculated" emission fluxes.

iv. Page 35: Please explain better how the "normalized" vertical profiles are calculated.

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