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Biogeosciences Discussions

Interactive comment on "Global fungal spore emissions, review and synthesis of literature data" *by* T. N. Dallafior and A. Sesartic

Anonymous Referee #1

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General comments

In this manuscript, Dallafior and Sesartic have taken an interesting new approach to quantifying global fungal spore emissions. The manuscript summarizes published results on fungal spore concentration measurements in natural areas. These studies are aggregated by biomes and used to derive emissions estimates appropriate for use in global atmospheric models, using a promising approach that can offer an viable alternative to the parameterization by Heald and Spracklen (2009). Such an emissions parameterization is a useful new contribution, since global atmospheric models presently largely neglect the biological background component of the atmospheric aerosol, which is known to contribute an important fraction of the aerosol mass and number at some locations.

However, some major issues should be addressed before the manuscript can be recommended for publication. First, the authors should more fully address the data from tropical rainforests showing much higher fungal spore concentrations there, which indicate a larger contribution to global fungal spore emissions than is accounted for in the present results. In addition, the method by which the flux estimates are calculated should be explained in more detail. Finally, the manuscript would be improved by quantitative estimates, or further discussion, of the uncertainty due to key factors such as the culturability, particle size, and seasonal cycles in concentrations.

Specific comments:

Data from tropical forests: The authors correctly note that the assumption that tropical forests emit fungal spores at the same rate as higher-latitude forests is not accurate and probably explains a significant part of the difference with previous work Elbert et al. (2007) and Heald and Spracklen (2009). The authors point out that their results are consistent with those of Winiwarter et al. (2009), but it should be noted that that study focused only on European emissions of biological particles, and extrapolation of those results to a global scale similarly neglects the probably much greater emissions in the tropics. Data reviewed by Elbert et al. (2007) and the recent study of Zhang et al. (2010) show that concentrations of arabitol and mannitol in tropical forests are significantly higher than in extratropical forests, implying much higher fungal spore concentrations. The manuscript would be significantly strengthened if the tropical forests could be considered separately using these data. If the authors choose not to use these data, they should explain this choice to the reader. Perhaps they could also offer a quantitative estimate of the magnitude error introduced by not treating rainforests separately.

Grasslands emissions: Do the authors think it is realistic that the "best" emissions estimate for grasslands is an order of magnitude lower than for shrubs and crops – and indeed the "high" estimate for grasslands is still lower than the "best" estimates for shrubs and crops? Looking at Table A2, the measurements by Griffin et al. (2007) in

shrubs and grasslands found significantly higher average concentrations in grasslands. These results should be more or less directly comparable (same methodology).

Calculation of flux estimates: In Equation 1, please define the variable delta p and the value used for the air density. Please also explain the conceptual justification for this equation. On p. 8448, lines 25-26, the authors discuss the "escape fraction". Does the authors' estimate represent the escape fraction or the total emissions, including the 90% that do not "escape" beyond 100 m?

Treatment of data: While the approach of taking an average of all reported values has the advantage of being an objective criterion, some measurements are clearly more representative than others. For example, it seems inappropriate to give the same weight in the calculation of a global average to the measurement of Amato et al. (2005), with only two samplings, as to the measurements of Rodriguez-Rajo et al. (2005), which involved continuous sampling over an entire year. This probably leads to biases, for instance in the case of shrubs, the highest average concentration was observed by Burch and Levetin (2002) during four days of sampling, and this was at least an order of magnitude higher than almost all other observations in the "shrubs" ecosystem. One possible way to deal with this would be to exclude data points that are based on only a small number of samples.

Particle size: The assumed particle size affects both the estimation of their residence time in the atmosphere and the conversion between mass and number fluxes. Bauer et al. (2002) showed that both the volume and the carbon:volume ratio of fungal spores are highly variable. In light of the large variability in size among and between spores of various species, it is perhaps not surprising that Elbert et al. (2007) assumed a different average fungal spore mass for Ascomycota and Basidiomycota than did Winiwarter et al. for all European fungal spores. How much do the authors estimate that the uncertainty in the assumed particle size and mass will affect their results? Would the estimated global emissions still be inconsistent with Heald and Spracklen (2009) and Elbert et al. (2007) if they used particle mass assumptions consistent with those

studies?

Culturability: The authors treat data from culture-based studies equivalently to culture-independent methods. However, the culturability (or viability) of airborne fungal spores is perhaps between 20% and 40% on average (Burge, 1977; Lee et al., 2006, Reponen et al., 1998). How much uncertainty do the authors estimate this adds to their results?

Seasonal or daily cycles: Since it is known that fungal spore concentrations show significant seasonal and daily cycles, please comment further on the assumption of constant emissions (p. 8450, line 22).

Abstract and p. 8450, lines 12-13: It is mentioned that "More than 150 studies have been reviewed", but this seems misleading, since only 35 studies are used in the final analysis. Please mention which criteria were used to choose these 35 studies. It is mentioned later (p. 8453) that data from measurements with petri dishes (i.e. without the use of an aerosol sampling device) have been excluded. Were other criteria used as well?

Technical corrections: p. 8447, line 12: Jones and Harrison paper is from 2004, not 2003.

p. 8450, lines 14-15: "average mass of wet spore discharging Ascomycota" → "average mass of wet spore discharged by Ascomycota"

p.8451, lines 17-20 and Fig. 2: it is unclear to me how the ecosystem-based fluxes were attributed to the plant functional types. Why not use the Olson (2001) ecosystems, which were used for the flux estimates, to make the map?

p. 8453, line 2: Elbert et al. used mannitol, not ergesterol.

p. 8454, line 6: "retract"–> "retrieve"

p. 8454, lines 26-27: it is mentioned that Griffin et al. (2007) cite two other studies, which are unnamed. Please cite those two studies instead.

p. 8455, lines 14-15: Please provide a reference for the statement that the viability does not affect CCN or IN activity of fungal spores.

p. 8457, lines 15-18: The mass flux given for Elbert et al. (2007) seems to be based on the 17 Tg per year emissions of ascomycota, however the total global emissions of fungal spores were estimated by Elbert et al. to be about 3 times higher, i.e. $\sim$50 Tg per year.

p. 8458, lines 25-26: Since at least two other studies (Elbert et al. 2007 and Heald and Spracklen 2009) already have addressed this topic, please change "first step" to "step" (or similar).

Appendix, Table A1: For Bauer et al. (2002a) the sampling device should be filter samples. Pady (1957) is missing from this table (and perhaps others, as well). There are blank fields in the table for the sampling device for the Cote et al. (2008) measurements and the sampling period for the Kellogg et al. (2004) measurements. Is this information missing from the original publications? If so, it might be helpful to note this in the table.

Finally, it is noted in the manuscript that in some of these articles, the total number of spores was not reported, but the sum of a set of species or groups. Please note in the table for which papers this applies, and which families or groups were included (perhaps in a table footnote). This information is important to readers to be able to evaluate the reported results.

Table A2: I suggest that this table might be easier to interpret if the entries were sorted (primarily) by ecosystem, since the reader will want to compare the various measurements from each ecosystem. Also, I recommend adding a column to this table (or maybe table A1) noting whether the analysis was done using culture or culture-independent techniques.

Interactive comment on Biogeosciences Discuss., 7, 8445, 2010.