

Interactive comment on “Global modeling of fungal spores with the EMAC chemistryclimate model: uncertainties in emission parametrizations and observations” by Meryem Tanarhte et al.

Anonymous Referee #1

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Summary

The authors use the global model EMAC to simulate fungal spore and bacteria emission, transport and removal. The focus of the manuscript lies on the comparison of 3(4) fungal spore emission parameterizations and the evaluation of simulated spore concentrations with an updated (?) dataset of spore counts and with relatively novel measurements of fluorescent biological aerosol particles. For this, bacteria are also added. One of the emission parameterization is identified as the “best”.

The paper touches an important topic and adds to the still scarce literature in this field. One of the strong points is that the uncertainties related to the measurement

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techniques are discussed in detail. Furthermore, the attempts to use the FBAP data for evaluation constitute a step forward. Otherwise, the results are rather thin, and the paper includes various technical weaknesses (both related to the methods and the form). I strongly suggest that these have to be addressed before the paper is acceptable.

Main comments:

- As already mentioned in the pre-review, it does not make any sense to apply the HU emission parameterization over ocean. Please change this. The authors argued that this wouldn't change a lot, but looking at Figs. 2 and 3, I have some doubts.
- By hiding the table with the spore count observations (SI2) in the supplement, the data providers do not receive proper credit. I strongly suggest to move this table into the main text. This is also done with Table 3, which has about the same length, so it is not clear why they are treated differently.
- I did not find a clear description of how the observations are compared with the model output. I assume that the model output is taken at the nearest gridpoint and at the surface (even if this is not identical to the altitude of the observations)? More importantly, for observations which cover timespans shorter than a year, is this compared to monthly means from the 5 year simulations? Or seasonal means? Or just the exact dates from one of the years?
- This question is particularly important for the correlations shown in Table 2. It is unclear what the data vectors include. Average values for the stations (i.e. only a handful of datapoints)? Or time series for each station and then an average for the correlation coefficients is shown? This should be done and documented much more carefully.
- Why are no plots like in Fig. 4 shown for the FBAP observations vs. model simulations? It could well be that this gives a different results with respect to the performance

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of the parameterizations.

- The authors claim that the Sesartic and Dallafior (2011) dataset has been updated, but it is hard to find out what has been added (or removed?). Please go into detail.
- I have trouble understanding what size distribution is assumed for the spores. The text (page 3, line 34) says “lognormal distribution with modal-scale parameter $\sigma=1$ ”. Please refer to an equation. If this is not a monodisperse distribution, then the fact that the emitted mass flux is higher than in Heald & Spracklen (2009) and Hoose et al. (2010) is not surprising, because the spread of the distribution introduces an additional scaling factor.

Detailed comments:

- page 2, line 30: “interactions with PBAPs” – which interactions? The previous sentence mentions “interactions between biological and environmental factors”, so this doesn’t fit together.
- page 4, line 11: “from the year 2000 to 2004”: if all boundary data are climatological averages, these years are meaningless.
- page 4, line 30: “mean spore diameter”: is this the average over the number size distribution or the mass size distribution?
- page 6, line 24: “. . . observations that met our criteria” – which are these criteria?
- page 9, line 10: “overestimated . . . least by HO5. HO3 agrees better . . .” – this seems to be a contradiction.
- page 10, beginning of section 3.3: the beginning of this paragraph would fit better into the introduction.
- Section 3.3 distinguishes urban and non-urban sites. It should be documented in Table SI2 which site is which.

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- Section 3.3: see above, details on the calculation of the correlations is needed.
- Table SI2 needs a thorough workover: The abbreviations in the first column are never used. The order of the entries is neither alphabetical nor chronological nor geographical, please re-order in a meaningful way. Use dots instead of commas as decimal delimiters. Give units for ALT (and spell out as altitude). If MIN and MAX is given, a temporal resolution should be given as well.
- Table 3 gives the concentrations in 10^6 m^{-3} . This results in numbers with many leading zeros and is hard to read. Please change to 10^3 m^{-3} .
- There is an imbalance between the long discussion of the comparison to FBAP in section 3.4 (more than 2 pages) and the quite short discussion of the comparison to direct spore counts in 3.2 (1 page).
- Table 1: specify “global mean/total” in the caption.
- Table 2 caption refers to “bold” font, but I don’t see any.

Technical corrections:

- page 3, line 15: “fluorophores commonly present biological materials” – missing word?
- page 3, line 27: closing bracket missing after “Jöckel et al., 2016”).
- There are still numerous occurrences where a reference should be formatted as “A et al. (2009)” and not as “(A et al., 2009)”. Here’s an incomplete list: p. 4, lines 1, 4, 5 (2x), 14 (here it should be “evaluated in Jöckel et al. (2005, 2016)”), p. 6, line 1, p. 10, line 4, . . . such formatting errors are very distracting when reading the text (at least for me). Please check carefully and don’t leave this to the reviewers or copy-editors.
- page 5, line 1: “to match the emission estimates” – specify “mass emission estimates”
- page 5, line 16: give units for b1 and b2.

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- on page 7 in lines 2, 16 and 19, “e.g.” should be moved into the brackets.

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