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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 #2

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Review of acp-2020-569

Drivers of the fungal spore bioaerosol budget: observational analysis and global modelling

The present study uses a multi-year spore count dataset in order to derive spore number emissions. Two different emission models (a statistical and a population model) are fitted to these derived emissions. Finally, the two emission models are included into the GEOS-Chem aerosol transport model. The resulting spore masses are compared to the spore emission scheme that was already implemented in GEOS-Chem. Large difference were found between the old and the two schemes developed in this study. The three spore emission schemes were evaluated against the dataset that was used to derive the emissions and against ground-level FBAP observations and vertical

FBAP profiles. In general, the method to derive spore emissions is scientifically sound and promising for to give a more complete picture if more datasets become available to constrain the emission schemes. Furthermore, I really appreciate the study design involving a variety of different observations for model evaluation. However, in particular the sections discussing the GEOS-Chem results and evaluation (3.3. and 3.4) have some considerable weaknesses when it comes to give reasons and explanations for the findings and differences between the schemes and to give necessary information. For example it is not clear if the time period of the different observations was actually simulated or if the observations were compared to simulations of the year 2016 (and perhaps 2015), which is indicated by section 3.1. At least, I would need more information in order to evaluate the findings of this study. My concerns and questions are addressed in the comments below.

Major comments p.4, l.8: How does the sampling rate of 3 days a week relate to the filtering by rain? How long is the collection time in these cases? From the description, I assume 2-3 days. Was the datapoint excluded if rain was found on any of the days of the collection?

p.5, l.6-7: Does this also involve rainfall in the surrounding grid cells?

p.6, l.13-16: I think, the method applied in this work is correct, however I don't understand the reasons given in these sentences. The description of the method is difficult to follow and could be extended by providing more information on certain assumptions. I can't follow the explanation here. Can the authors please explain why short atmospheric lifetimes necessarily lead to smaller concentration differences in the horizontal compared to the vertical? I would have argued that long lifetimes lead to small concentration differences due to longer time for mixing. Since horizontal wind speed is usually orders of magnitude stronger than vertical wind speed, already concentration differences smaller than the assumed here for the vertical (factor of 0.1) may result in the same order of magnitude horizontal advection. Having a read in Bakwin et al., 2004, one can find that they assume horizontal advection to be neglected due to the averaging period of 1month, which perhaps might be the case in regions with no prevailing winds and horizontal relatively homogeneously distributed sources. It's more the long averaging time that smoothes out horizontal differences. By neglecting horizontal advection, the authors implicitly assume long-term horizontal homogeneity, which is also the reason for why only subsidence velocity is applied. I think that this needs to be better described in the text.

p.12, l.32: "...are mainly caused by the occurrence of wet deposition". It sounds reasonable. However, can the authors please show the modelled wet deposition rates similar to Figure 7 or at least modelled rainfall in their answer to this comment? According to p.10, l.17-18, the population model has a "delayed response to temperature and LAI, due to the growth and mortality of the fungi". Can that also play a role here? How long is this delay?

p.13, l.5-7: I don't see the strong difference that is described here. Actually, it seems the July and August concentration of statistical and population model are quite similar (0.04-0.05 μg m-3).

p.13, l.14: Since wet and dry deposition are the only described loss processes (if I understand correctly), and both loss processes scale directly with the available concentration (i.e. the relative loss is independent of the actual concentration), this statement ("Due to the strong emissions of the HS09 model, emissions and concentrations have similar cycles.") seems wrong. Can the authors clarify what they meant to express?

p.13, l.16-17: Isn't that what the sentence before is saying for the HS09 model? So, I don't understand what different interplay between wet deposition and HS09 and wet deposition and the other two models the authors are targeting at here. For me, the different size assumptions, as explained in the sentence before, seem to be more the cause for different response to the same modelled rainfall rate. However, plotting modelled wet deposition rates would give proof to this speculation.

p.13, l.25-26: There are a number of stations close to the coast. For me, not nec-

essarily, filling no-emitting water surface with emissions leads to a better comparison between model and observations, since also in the observations in terms of spores presumably clean marine air masses might lead to low concentrations. Furthermore, also the observational sites experience spores from probably a variety of land covers in their surrounding. Can the spore counts really be estimated to be dominated by local sources without a strong contribution of long-range transport (at least from around 200km)? Based on the lifetime shown in Table 3, long-range transport over this distance is occurring frequently. Can the authors please show a comparison of their "fully emitting land cover" simulation and the simulation using the original land cover at least for the stations whose land cover has been changed by this assumption? If I understood correctly, the original land cover was used for the results shown in e.g., Fig. 6.

p.13, l.26: Does "fully emitting land cover" also involve grid cells containing nothing but ocean? From the text, this is not clear.

p.13., l.27-29: I would have guessed that in particular the variability of meteorology in different years is one of the main causes of differences. Since different years were observed and simulated, how is the correlation coefficient calculated, i.e. which timeseries is/are or population mean are compared against each other? On p. 14, l.5, low "skill in reproducing seasonal variations at the AAAAI stations" is determined by small correlation coefficients. Even for long-term averages such as seasons, the meteorological driving variables might be different in different years. Can the authors please show a comparison of the 2016 meteorological conditions and the observational years?

p.13, l.34: The 25% / 75% share between fine and coarse mode is reported for the mass in the abstract of Heald and Spracklen (2009). So, for spore mass, this statement absolutely makes sense. However, in Fig. 8, I think the number emission flux is shown, right? Does the HS09 scheme really show a larger number of coarse mode spores than number of fine mode spores? Heald and Spracklen (2009) actually state "...we obtain number concentrations of 10ˆ5 m-3 (fine) and 8 x 10ˆ3 m-3 (coarse)."

(p. 4, paragraph [9]). This does not seem to be inconsistent with the observed size distribution, which the authors refer to in the next sentence. Furthermore, it suggests that the overestimation is either general (i.e., both in fine and coarse mode) due to the applied method by Heald and Spracklen (2009) or at least in the fine mode. Can the authors please clarify what they meant to express here?

Section 3.4: I assume that the observed FBAP concentrations are from other years than 2016. Are the same years simulated or is the 2016 simulation used instead? This is not stated in the text and should be mentioned. In section 3.1 it is written that only the years 2015 and 2016 were simulated. If other simulated time periods than the observed ones were used, the knowledge gain from the results presented in this section is limited. I would not necessarily expect good comparison for single peaks in the seasonal cycle and timeseries statistics as different meteorological conditions and timing can vary greatly between different years. The section should include clear statements on these limitations if other years were simulated than observed. In particular it should then be checked how the meteorological variables that drive the emissions compare at or close to the sites between the different years.

p.15, l.15-16: Can the authors clarify what is meant here? For me, it looks like that at Karlsuhe (which I believe the statement is referring to) HS09 and observations peak right at the same time in June, however HS09 is just overestimating. In case you mean that the maximum concentration in HS09 is in June whereas it is Aug-Oct in the observation, I recommend to revise this sentence.

p.16, Section on the comparison to vertical profiles of FBAP: Were the same time periods simulated in which the obersvations took place?

p.16, l.15-16: Which modelled time period was compared to these observations?

p.17, l.2-3: How does this compare for the other years with observations (2015 and 2017)? At least, 2015 was simulated.

p.17, l.17-18: “These differences are largely the result of different assumptions about size...”. If I understood correctly, the HS09 is emitting the mass. So size does not play a dominant role for the mass concentration. Is the HS09 emission in GEOS-Chem implemented as mass- or number-based emission scheme?

Other comments p.4, l.10: What does “no local spore or pollen sources” mean? How close is local in this context?

p.5, l.17: Do you mean subsidence of cold air? If not, then please further explain what is meant here.

p.6, l.4: The abbreviations BL and FT have not been introduced, yet.

p.6, l.7: I’m not sure if the abbreviation for local time is well known.

p.6, l.9-10: I assume that the boundary layer height is also taken from NARR data? Further, the authors write "mean height of the afternoon boundary layer", however, in the Figure caption of Fig. 2 they write "c) maximum daily boundary layer height". Can the authors please clarify, which was used?

p.6, l.10: Double “daytime” in “the daytime mixed-layer during daytime”.

p.6, l.33-34: How was this calculated?

p.8, l.23-25: This sentence is rather complicated. It could perhaps be split into two sentences for easier reading?

p.8, l.31: I think, referring to section 2.5 where the fitted parameters are presented or even to Table 2 already in the beginning of section 2.4 is beneficial for the reader here.

p.9, l.22: Perhaps nothing to bother before type setting, but the font seems to have changed.

p.10., l.17-18: Double "in the population model".

p.10., l.25: Perhaps name it "derived spore emissions" to better make clear that the observed emissions are meant.

p.11, l.5: Can the authors please show the comparison of LAI for 2008 and 2016 in an answer to this comment?

p.12, l.10: For me, burden is usually a column quantity or mass / number in the whole atmosphere. However, shown in Figure 7 is a concentration [μg m-3] according to the axis label. I therefore recommend to use "concentrations" instead of "burden", as it is done later in this section.

p.13, l.18-19: This sentence reads a bit difficult. First, at this point, it is not clear (since it is the beginning of a new paragraph) that with "both schemes" the authors mean statistical and population model. Second, the fragment "as well as the HS09 scheme" is badly placed / written for easy understanding.

p.13, l.19-20: I get your point, but written this way, it seems like a rather strong statement. I would have not assumed it, especially since a different time period is simulated (2016) than observed (2003-2008).

p.13, l.30: The reference to Figure 8 should already be given somewhere in the beginning of this paragraph.

p.14, l.6: Just for my understanding. The comparison and correlation coefficients in Fig. 4 are for the timeseries of the emission mean of all stations? If so, I suggest to write this clearer in the respective section.

p.15, l.24: Actually, the population model stays rather low. I would not call the increase "sharp" for this particular model.

p.15, l.29: Why was the temperature threshold not introduced for the HS09 model?

p.18, l.8: Please consider revising this statement ("continental outflow of bioaerosols"). Only spores were simulated.

Table 1: Assuming [q] = g/g (or similar) and [LAI] = m2m-2 the unit of b1 and b2 should

be m2s-1?

Table 3: Typo: "Table. 3" -> "Table 3".

Figure and Table captions: Probably not something to bother at this stage: Sometimes dots are missing at the end and non-capital letters in the beginning of Figure and Table captions, in both the manuscript and the supplement.

Figure 2: a-e missing in the Figure itself, but it is referred to in the text and the figure caption.

Figure 3: In the Figure caption, "2" is not in superscript in rˆ2.

Figure 4: Perhaps typo in the figure caption (I'm not a native speaker)? "20-day running derived-emission flux" -> "20-day running mean derived emission flux".

Figure 4: "fit parameters shown inset". This is not the case. However, it is not needed since fit parameters are presented in Table 1 and 2 very nicely.

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