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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Regional-scale simulations of fungal spore aerosols using an emission parameterization adapted to local measurements of fluorescent biological aerosol particles" *by* M. Hummel et al.

Anonymous Referee #2

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This study compares a series of model simulation of fungal spores based on previous parameterizations in the literature with field observations (made using UV-APS and the WIBS instruments) in four locations. The authors then suggest that model underestimates at these sites motivate the development of an additional emissions scheme (a modification of one of the schemes) and explore the model performance using this adjusted parameterization.

There are a two major concerns with this study:

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Interactive Discussion

Discussion Paper

1. There is insufficient statistical support for the arguments put forth in this study. The authors need to fully quantify model performance for each scheme (R=correlation coefficient and normalized mean bias statistics for Figure 3-7). In doing so, it is critical that the authors distinguish between bias and skill. Does the new scheme actually add to the model skill (i.e. improve R2) or simply eliminate bias? Are there other potential causes for the bias (i.e. LAI, qv, constants used in the model)?

2. The modeling in general is not very compelling and it seems like a missed opportunity to investigate/comment on the factors controlling the variability of PBAP. In particular, given the general skill of the model (e.g. Figure 3), it would be useful to try to separate in the model how meteorology and emissions contribute to variability (i.e. perform a simulation with constant emissions). This may be the first time that a model has been compared with high resolution PBAP observations. This is the primary unique direction of this work, but it is insufficiently explored in the current manuscript.

Minor Comments

1. Page 9907, lines 27-30: Regarding the role of PBAP as IN, the authors may also want to comment on the work of Hoose et al, 2010 and perhaps Creamean et al., 2013 which seem quite relevant here.

2. Page 9909, lines 1 and 10 seem to contradict each other, FBAP cannot be a lower limit for PBAP if it may be contaminated by other fine particles.

3. Section 2.1: much of this is section (page 9909, line 25 through all of page 9910) is basic model treatment of aerosols that does not need to be included in a scientific manuscript. I suggest the authors trim this (equations are not necessary).

4. Page 9911, line 14: typo “gases at particulate” should be “gases and particulate”

5. Page 9911, line 23: what are “Anthropogenic primary aerosols”?

6. Section 2.2: line 1 and line 27 incorrectly suggest that the H&S parameterization is a constant emission rate. The authors have described how it is a dynamic function of

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q and LAI, therefore is is by definition, not constant in time or location.

7. Page 9913, line 3: Please quantitatively compare the impact of your different size assumption (3 um) here with Hoose et al., 2010a (5um) and Heald and Spracklen, 2009 (PM2.5). Are your totals for mass, number emissions comparable? How did you scale your constant (c) term to account for the smaller size range from Hoose et al.?

8. Page 9913, equation 6: This equation does not explicitly match the parameterization of H&S (unless one assumes that their constant=c/(LAI_max qv_max). Why is the parameterization given this way here? Is there a physical justification?

9. Page 9913, line 11: what is the time resolution for qv (i.e. the meteorological time step)?

10. Page 9913, lines 13-18: This section is confusing. There are 3 sentences that mention the IC/BC for aerosols, and it's not clear what the authors mean by "No initial and boundary concentrations are predefined for aerosols or gases." Please clarify or simplify this text.

11. Figures 3-6: missing statistical quantification of model performance

12. Figures 3-6: all 3 schemes co-vary. To what degree does variability reflect meteorology (PBL height, mixing, and precipitation) rather than variability in emissions driver (q, LAI)? This can be diagnosed in the simulation (statement on page 9919 lines 15-16 isn't quite true. Some of these effects can be deconvolved in the model). What is the correlation between FBAP and PBL height?

13. Page 9919, line 1: Can you show the observed precipitation from the sites in the Figures?

14. Page 9920, lines 7-9: Is there any evidence of this phenomenon in the observations you are exploring here?

15. Page 9921, line 9-10: Figure 7a does not support the statement that the simulated

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concentrations are “systematically underestimated”. Nor does Figure 7b demonstrate the improvement suggested on page 9924, line 8. Please show statistics to support these claims.

16. Page 9922, line 3-4: what is the lifetime in the model simulations?

17. Page 9922, line 7: This lifetime is very short. What physical mechanism could justify such a rapid removal rate?

18. Page 9923, equation 8: Are the variables used here independent? Have you verified that you are not over-fitting?

19. Figure 9: Can you overplot the observations using the same assumptions?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9903, 2014.