Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Global modeling of primary biological particle concentrations with the EMAC chemistry-climate model" *by* Meryem Tanarhte et al.

Anonymous Referee #1

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This study explores the skill of three parameterizations for fungal spores, implemented in the EMAC model, to reproduce observed fungal spore counts, as well as (in combination with parameterized bacteria and pollen) fluorescence observations. Given the challenges in interpreting the observations (undercounting of spore counts, varying sensitivities of fluorescence), the study struggles to conclude as to the skill of these schemes as comparisons with the two datasets lead to opposite conclusions. Given this ambiguous result, I was disappointed that the authors did not pursue more exploration on the modeling side. I provide some specific suggestions below which would expand the utility of this study and ensure that it meets the standard for publication.

MAJOR

1. Expand modeling

- The results from none of the 3 fungal spore simulations is very satisfactory. Can the authors suggest (and possibly test?) improvements?

- Page 8,10: the authors claim that differences in bacteria from Burrows et al. may be the result of using the MODIS ecosystem distribution rather than the Olson distribution. This seems like something that could be easily confirmed with the model.

- Page 13: the authors highlight the deficiency of not including seasonal or diurnal variability for bacteria. Could they perform a simple sensitivity test to explore how imposing seasonal variation might impact their results?

2. Aerosol size assumptions

- Page 3, line 28: Why are fungal spores and bacteria treated as monodisperse? This seems an unrealistic assumption.

- Page 3, lines 32033: comment on the size dependence (if any) of these processes

- Page 5, line 17: 5 um seems large for fungal spores. Heald and Spracklen include fine and coarse mode particles, so this assumption does not seem consistent. Please comment.

- Page 8: While the authors claim that their results are not sensitive to the assumed size, it would certainly impact the conversion from number to mass. Might this help explain differences among previous fungal spore estimates discussed on page 9? If the authors feel these differences are not the result of assumed size, can they offer some explanation for these substantial differences? And why do the distribution and magnitude of bacteria agree better with previous studies than for fungal spores?

- Figures 3a, 4a, 5a seem to suggest more export of pollen and bacteria than spores (though this may be a false impression due to the color bar). Can the authors confirm

this by including global mean lifetime numbers for the 3 classes of PBAP? If lifetimes do differ, why is this the case when the authors indicated that removal processes are not dependent on size?

MINOR

1. Page 1, Line 19: measurements are spores, not all PBAP

2. Page 1, Line 23: meaning of "reflects a greater difference" is unclear. Compared to what? Observations? Or do the authors mean the ratio of bacteria to fungal spores varies more widely? Please modify text.

3. Page 1, lines 27-28: "of fungal spores and pollen", why not include bacteria in this sentence?

4. Page 6, line 9: what is the time span of the averaging? Is it possible to separate seasonal averages?

5. page 6, line 27: what is the upper size limit of these instruments?

6. Page 10, lines 24-26: why are the Borneo and Nanjing data exceptional? The authors need to justify why they would remove these from the comparisons

7. Section 3.5: the presentation of these results is a bit confusing. It would be helpful if the authors first discussed how many datasets are available for each season and commented on the observed seasonality before discussed the model performance.

8. Figure 6: It is very hard to see the data on this figure, suggest better use of scale (max value set to 0.1) to see data more clearly and using a color other than yellow/green which is hard to distinguish from white on the panels. The season labelling should be explained in the captions.

9. Page 11, line 1: unclear what "discrepancy" the authors are referring to. The measurements don't distinguish these two classes of PBAP.

10. Page 11, lines 25-28: clarify if these measurements are all for the same size ranges

11. Page 12, line 19: reference of justification for assumed mass per particle needed

12. Page 12, line 20: specify that these means are for the surface

13. Page 12, line 28: The Poschl et al. numbers are averages over the deployment and only for supermicron particles and so cannot be directly compared to annual means of all aerosols from the model. Suggest that you compare to relevant month, coarse fraction only. If the large difference holds when the correct time of year is compared can the authors speculate as to why there would be such a substantial difference?