**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Bacteria in the global atmosphere – Part 2: Modelling of emissions and transport between different ecosystems" *by* S. M. Burrows et al.

Anonymous Referee #1

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This paper accompanies Burrows et al., (2009) which synthesizes measured bacteria concentrations in the atmosphere. In this manuscript (Part II), the authors use these observations with a 3D model simulation of generic 1 um particles from ecosystems to try to optimize a first set of regional emission fluxes. This is an important first attempt to estimate global emissions of bacteria (here at 1.4 Tg/yr). This study highlights where additional observations may be particularly beneficial for constraining this component of global PBAP emissions. Overall, the paper is thorough and interesting. I offer below a few suggestions, primarily to shorten the text for clarity and brevity.

MAJOR COMMENTS

1. Page 10834, lines 1-2 and throughout: given that the lifetime in the NO-ICE-SCAV simulation are so unrealistically long, I recommend stating this as here and then removing this simulation from all further discussion (including Figure 2c and 3c).

2. Overall: Could you comment on what the effect would be of including the seasonality of observed bacteria concentrations in your optimization? In your Part I manuscript, Table C1 shows that many sites have considerable summer vs. winter seasonality. Given that simulated lifetimes are generally shortest in the summer (Fig. 2) and concentrations are highest here, the bulk of emission may occur in this season.

3. Section 4.4 and throughout: I would recommend that you remove further discussion of the exact solution in your text. Considering that you are optimizing 10 elements in a state vector with exactly 10 observations, it is perhaps not surprising that the optimization results in some negative elements. As you point out, this is unphysical, and I think you can confidently apply a non-negative a priori constraint to your optimization.

4. Page 10843, line 9-11: Related to the point above, I would recommend using the Method 2 solution range as your final estimate than the exact solution (which includes negative coastal emissions). This is consistent with your use of the Method 2 best fit values in Section 5.

5. Table 5: It appears to me that there is an error on the 'Mean global load (cells)' row where the homogeneous emissions are 4 orders of magnitude higher than the adjusted emissions (whereas the row below when converted to Gg is only different by a factor of two).

MINOR COMMENTS

1. Page 10832, line 15: Could the authors justify their particle size choice here in a sentence? – perhaps with some appropriate evidence from the literature that this 1 um assumption is likely representative for bacteria.

2. Page 10832, line 17: Please give emission rate here, not just in caption of Figure 3.

3. Page 10832, line 25: It's unclear why a 3 year spin-up was required for the bacteria tracer to reach quasi-equilibrium when these tracers are so short lived (2-5 days for the CCN simulations). Is this spin-up rather required for climate stability? Please explain this.

4. Page 10833, lines 3-13: missing a reference to Figure 1

5. Page 10833, line 23: the "global loads" are not included in Table 2

6. Page 10835, line 1: perhaps clarify that this is not the column density of realistic "total bacteria" but rather the column density of the homogeneously emitted bacteria tracer.

7. Are both Figures 5 and 6 necessary? They are not discussed in the text and the information is somewhat redundant to Table C1 and Figure 4. You may consider cutting for brevity.

8. Page 10842, line 17: typographical error "eensemble"

9. Page 10842, line 18: typographical error "fluxa"

10. Figure 8: in line with major comment #3 above, you might consider removing the exact solution here and expanding the scale.