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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.

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We would like to thank Ana Sesartic and the two anonymous referees for their careful reading and thoughtful comments. Because several issues were raised by more than one referee, we will address the comments cumulatively.

1 Major comments

Three major issues were addressed in all three referee reports. These can be summarized under the headings 1) particle size, 2) seasonality of emissions, and 3) presentation of numerical methods.

1.1 Particle size (e.g. page 10832, line 15)

The referees requested that the choice of 1 μm particles should be better justified, or further discussion should be included of how a different choice of particle size would affect the results, perhaps with sensitivity runs.

We recognize that the size of bacteria and of any particles to which bacteria may be attached is an important parameter in determining their transport lifetime in the atmosphere. Field studies (e.g. Shaffer and Lighthart 1997) suggest that the median size of particles containing bacteria is larger than 1 μm and probably closer to 3 μm. This finding is discussed in more detail in Section 6 of the companion paper (Burrows, 2009, Part 1).

In the revised manuscript, we will include additional data on the sensitivity of model output to the assumed size. We believe that this approach will represent a significant improvement to the paper. We will also discuss the choice of size in more detail in the revised manuscript.

1.2 Seasonality of emissions (e.g. page 10838, line 25)

The referees asked for further discussion of how the seasonality of emissions might affect the results, possibly with sensitivity runs.

We recognize that the emission rate of bacteria to the atmosphere is unlikely to be con-

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stant throughout the year. Anonymous Referee #1 correctly points out that the literature data companion paper shows significant seasonal differences in measured concentrations in some studies (Burrows, 2009, Part 1, Table C1). However, we conclude that the published field studies do not provide enough information about the seasonality of emissions to make a well-founded assumption about seasonality for use in the model, for the following reasons:

1. Very few field studies have measured surface fluxes of bacteria, and none that we are aware of have measured the annual cycle of the surface-to-atmosphere flux.

2. The few field studies that have measured concentrations during different seasons at the same location do not suffice to draw general conclusions about the seasonal cycle, especially of the emissions. While the observed concentration in many regions is highest in summer, at some locations it is highest in spring or autumn. Similarly, the lowest concentrations are often observed in winter, but sometimes observed during autumn (Harrison et al., 2005). These data suggest that the seasonal cycle is likely to vary across ecosystems (as also pointed out by Referee #2), depending perhaps on local climate, vegetation type and other factors.

3. Finally, it should be noted that the variability of observed concentrations within a season is at least as large as the differences in concentration between seasons (Part I, Figure 2).

4. Extrapolating from concentrations at a particular time of year to the seasonality of the sources is tricky, since the lifetime also varies seasonally.

Thus, we believe that based on the currently available information, a more complex assumption about the seasonality of emissions would not necessarily be a better rep-

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resentation of reality, and would make interpretation of the model output more complex. We will add a brief discussion of this issue to the revised manuscript.

To gain more insight into the possible effects of seasonality, we can estimate the atmospheric residence time of particles emitted in each hemisphere separately to focus on summer/winter differences, as (hemispheric load) / (hemispheric emissions). Because the residence time of the particles (up to ten days) is much shorter than inter-hemispheric transport times (> 1 year), this should be a good approximation. The revised manuscript will include these plots and discussion of their implications for the conclusions of the study.

1.3 Presentation of numerical methods (e.g. section 4.4, pages 10838-10840)

The referees asked for clarification of why two different numerical fitting methods, as well as an exact solution, are used to estimate the sources, as well as a clearer discussion of how the results of these methods should be interpreted. Anonymous Referee #2 recommends omitting the constrained weighted fitting procedure "Method 1", since it is not used in the results. Anonymous Referee #1 recommends using the Method 2 solution range as the final estimate rather than the exact solution, an issue also mentioned by A. Sesartic. Finally, Anonymous Referee #2 recommends omitting detailed discussion of the exact solution.

We think it is important to include results from both the exact solution and the "maximum likelihood" fitting, but agree that "Method 1" can be omitted, since it doesn't play a role in the results. This will help make the presentation clearer and more concise. Also, in section 4.4 we will try to explain better why we include both the exact results and the fitted results. We will also adopt the recommendation of consistently using the "Method 2" solution range as the final estimate.

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2 Additional major comments

- Following the recommendation of Referee #1, we will remove most discussion of the NO-ICE-SCAV simulation (including Figures 2c and 3c), in light of the fact that the lifetimes are unrealistically long.
- A. Sesartic and Referee #1 noticed an error in Table 5, where the homogeneous emission are 4 orders of magnitude higher than the adjusted emissions (while in the row below, when converted to Gg, the difference is only a factor of two). This will be corrected in the revised manuscript.
- Referees #1 and #2 ask for clarification of the bacteria emission rate in the text (page 10832, line 17). In the revised manuscript, we will state at this point the emission rate that was used in creating Figure 3, although this differs from the emission rate used in the original simulations. However, note that the emission rate used in Figure 3 (1 m-2 s-1) was chosen only for mathematical convenience, and is far lower both than observed rates of emission and the rate of emissions estimated in this study (for clarity, the revised text also includes a reference to the estimated emissions in Table 5). Because the concentrations are linearly dependent on emissions, the simulated concentrations can be scaled to be consistent with any desired emission rate – this is the premise behind the optimization procedure.
- Referee #2 objects to our statement that we investigated the "transport" between ecosystems because we do not present a source-receptor matrix to describe transport. Our "weighting" matrix describes transport, but in a slightly different way than a source-receptor matrix (see Table C1 and Section 4.2 of the manuscript). Our weighting matrix relates the emissions in one region to *concentrations* in the near-surface air of another region, while the source-receptor matrix relates emissions in one region to *deposition* in another region. We chose

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to use concentrations to examine transport because these can be directly related to observations. In the revised manuscript, we will explicitly discuss the relationship between our weighting matrix and the source-receptor matrix in Section 4.2. Also, where transport between ecosystems is mentioned in the introduction, we will include a reference to the weighting matrix.

- Referee #2 comments that the land use categories (Table B1) lump together some areas that are very different from one another. We recognize this, and believe it is already adequately discussed in Section 5.3 (Limitations and sources of uncertainty).

- A. Sesartic made several comments about the discussion of the column density data presented in Figure 3. This figure shows the column density of the homogeneously emitted bacteria tracer (Figure 3), not the "realistically emitted" bacteria (this is now more clearly stated in the text). The comparatively high column densities in the polar regions are due to the lack of removal processes there, but they are reduced when more realistic values are used for emissions (Figure 9). Similarly, the high column densities in the tropics as seen in Figure 3 are purely the result of transport and removal processes (not of greater biological activity and thus greater emissions of bioaerosols), since the tracer was emitted at the same rate everywhere.

- Page 10838, lines 25 and 26 : A. Sesartic and Referee 2 ask whether the negative fluxes could indicate a net particle sink. Clearly, this is possible, which is why we include the results of the exact solution in the article. In the revised manuscript, we will discuss in more detail in Section 4.4 how these results can be interpreted. In short, we believe that the deposition simulated by the model has a higher degree of certainty than the data used for optimization. A negative flux resulting from the optimization would imply that the net deposition is greater than the simulated deposition.

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- Page 10834, lines 9 and 10, 20 and 21: A. Sesartic comments regarding the atmospheric residence times of particles that because the mean residence time is short for particles emitted from seas and oceans, she would also expect the mean residence time to be short for particles emitted from coastal regions. In our model simulation, the residence time for particles of coastal origin is similar to that for many other land ecosystems, and much longer than for particles emitted from oceans. We assume that particles emitted from the open ocean experience very different are affected by very different patterns of transport and removal than those emitted from land or near the coast, and the open ocean dominates the behavior of the ocean tracer.

3 Minor comments and typographical errors

- A. Sesartic requests a source for the claim that bacteria "remain in the atmosphere for an average period of a few days." (page 10831, line 3) This claim is based mainly on the typical lifetimes for particles of the size range to which bacteria belong. Similarly, Referee #2 objects to the citation of a textbook to support the claim that "The lifetime of a few days for CCN-ACTIVE and CCN-INACTIVE bacteria is consistent with theoretical expectations for particles of 1 μm diameter (Roedel, 1992)." (Page 10833, line 25). Certainly, a textbook cannot be expected to provide insight into recent research, but it can be expected to contain the canon of "common knowledge" of the relevant field, which we think applies to the claim that particles in this size range have lifetimes on the order of a few days.

- Referees #1 and #2 comment that much of the information presented in the tables and figures is redundant, and that this is confusing to the reader. Also, Referee #2 comments that placing the tables in a table appendix is unhelpful because it disconnects the text from the supporting material. The point is well taken. The

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redundancy will be reduced in the revised version by eliminating some figures, and some tables will be moved from the table appendix to appropriate locations in the text, while others will be moved to an online supplement. In addition, we simplified several of the figures and tables by removing the results from the NO-ICE-SCAV simulation and from the "Method 1" fitting, as suggested by Referee #1.

- A. Sesartic suggests explaining more clearly whether we consider the total bacteria in the atmosphere, or only the species which exhibit ice-nucleating capabilities. The bacteria tracers simulate the total concentration of bacterial aerosol found in the air, including both ice-nucleating and non-ice-nucleating species. For the purposes of this study, we assume that bacteria are scavenged at least as efficiently by mixed-phase and ice clouds as are aerosol particles in general. The small fraction of bacteria that are IN-active may be scavenged at a higher rate, but this should not affect the overall conclusions of this study, which deal with the total concentration of bacteria, rather than the IN-active fraction. The revised manuscript will mention this in the model description (Section 2).

- A. Sesartic requests that we explain why we chose to investigate the CCN activity and not the IN activity. This will be discussed in the model description section in the revised manuscript, (Section 3).

- A. Sesartic suggests citing Caristi et al (1991) with respect to the concept "bio-precipitation" (Page 10848, line 5). We will include this citation in the revised manuscript. (Sands et al. (1992), which she also mentions, is already cited here.)

- Page 10844, lineS 16-17: Referee #2 commented that Jaenicke's estimate of global PBAP emissions is presumably not meant to be very robust. When referring to this estimate in the revised manuscript, we will qualify it as a "rough" estimate to indicate this.

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- Referee #2 also remarks that it seems odd to mention the author's affiliation in the acknowledgements. We have revised the acknowledgements, removing the Max Planck Institute for Chemistry (MPIC), but still mentioning the International Max Planck Research School, which is a graduate school associated with the MPIC.
- We thank the referees for pointing out several typographical and other additional small errors, which will be corrected in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 10829, 2009.