

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 #2

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S.M. Burrows and co-authors.

The discussion paper presented by Burrows et al. provides an interesting approach of an inversion technique. The method is presented convincingly and provides interesting results. But a large number of open questions remain, which may affect the overall conclusions. The main problem is that the way of presentation chosen by the authors may affect a positive reception of their study, and may also hamper the interpretation of the meaning of the results obtained.

Before final publication in ACP, the authors should at least cover the main topics pre-C1468

sented below, but probably they will also wish to address the other issues raised. This will require major revisions.

Main topics:

1) There is a general inconsistency in the approach, when a “constraint fitting procedure” (2 methods) is being developed but in the end is disregarded for the result, and instead the simpler “exact solution” is used to arrive for the global totals I see two ways to resolve this: either delete the “constraint fitting procedure” and all results connected to it completely (there is no need to repeat a failed methodology even if it is mentioned in the research report) OR explain in detail why the procedure as been selected and also why it has not been found useful

2) Parametrization of bacteria by aerosol tracers not only is poorly described (full description should be page 10832 line 17, not just in the header of Fig. 3), but moreover the choice of parameters seems totally arbitrary. The justification (on p. 10845) that *E. coli* is larger than this size and huge differences in bacteria size exist does not say anything about airborne bacteria. I believe that Bauer et al. (2002) provide a figure of 17 fg C for bacteria, which also may be questionable – but using it would yield (with some extrapolation on C content in total mass) a diameter of airborne bacteria in the range of 0.45 μm .

In an ideal world the authors would have started with a diameter that is based on well-founded arguments. Here what one could expect is a careful analysis of the impacts smaller bacteria sizes (like half of the value selected) would have on the results.

3) Huge differences exist for the land use categories “land-ice” and “wetlands”, pointing to the shortcomings of the model and the caution to be taken with respect to interpreting results. But as concentrations (and related emission fluxes) for “land-ice” have been taken from Bauer et al. (see also: Companion paper), this is not surprising: Even if Bauer et al. possibly provide information on bacteria concentrations over snow covered surfaces, the situation hardly resembles Antarctica or Greenland. This needs to be

made clear in the discussion, possibly supported with a sensitivity analysis in emission fluxes.

4) I can not see where “transport between different ecosystems” is covered – yet the paper title pretends just this has been done. Change the title!

Results presented in Table C1 (and repeated in Figs. 5 and 6) are source oriented, not receptor oriented (at this stage, emissions are normalized to 1 cell/m²/s for all ecosystems). A source/receptor matrix to describe “transport” can be calculated in this modelling system, but I can not find where this would have been done.

Further questions and comments, in the order of appearance:

Figures/Tables: the paper is full of quite complex tables and graphs which partly repeat the same material. What are Figs 5 and 6 good for when just repeating Table C1? Likewise, information presented in Table C4 may fully be covered in Fig. 4. Note it is quite difficult for a reader to understand each graph/table, which therefore should only be used when visualizing and explaining the message of the paper.

Abstract, line 14: the “upper boundary” of 4600 contradicts the value presented in section 6.2 (4000) even if everything can be explained by a close look into Table C6. Please harmonize the methods instead of misleading the reader. The concept to separate tables into a table appendix is anyway not helpful as it disconnects text from supporting material (see also note above).

P. 10831 lines 16ff: see also (4) above – I cannot see where the receptor part of bacteria transport between ecosystems is being shown.

General to method: Note (and discuss) that also the temporal profile of emissions will differ between ecosystems – how far can this affect the results?

Section 3.1: It is already an interesting result that NO-ICE-SCAV is unrealistic, indicating that ice nucleation always occurs. But it is not clear which theoretical expectations a textbook (Roedel) could provide.

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Section 4.3: Could the negative surface fluxes possibly indicate a higher deposition rate than emission rate? Negative net fluxes definitely can be real.

Section 5.2: When referring to Jaenicke’s estimate on global emissions, one may consider that the figure is presented just at the end of a footnote in the original paper, and possibly is not meant to be very robust.

One final remark: it seems odd to me that the authors’ institution needs also to be mentioned in the acknowledgements. It is mentioned in the affiliation anyway.

Due to all the points mentioned above, it is quite difficult to grasp the very valuable ideas and concepts presented. This is a pity, as a lot of valuable material thus remains hidden to the public. I would strongly recommend the authors to seriously consider the suggestions presented here. Then the paper will be able to provide an important contribution to better understand the global role of bacteria in the atmosphere.

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

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