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Atmospheric Chemistry and Physics Discussions

Interactive comment on "

Bacteria in the ECHAM5-HAM global climate model" *by* A. Sesartic et al.

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We would like to thank anonymous referee 3 for the helpful comments and careful evaluation of the manuscript. The referee's comments and our responses follow.

1 Major Comments

I strongly recommend to extend the present paper, for example by addressing the second major point raised in the referee comment by Dr. Hoose in some detail.: As already

mentioned in our reply to Dr. Hoose, we limited ourselves to adding an explanation on the detailed bacterial coagulation mechanism, the freezing parametrization and the contribution of bacteria to contact and immersion freezing compared to other aerosols. However, we will consider the valuable suggestion of including bacteria as CCN for a future study. This exceed the scope of this study, which focuses on bacteria as IN.

2 Specific comments/questions/suggestions

1. Several assumptions regarding scavenging, coating with H2SO4, coagulation with dust, etc. of bacteria are rather uncertain. This is true, however, there are unfortunately no better data available. We have added a more detailed description of the treatment of bacterial coagulation in the model. The assumptions were based on plausibility arguments. We explained them better in the revised manuscript.

2. The presence of ice-nucleating bacteria can act either to increase or to decrease ICNC. Increases in ICNC due to ice-nucleating bacteria can occur where they outnumber other types of IN. Decreases in ICNC could be due to the fact that some bacteria nucleate at higher temperatures than other (possibly more numerous) IN. Do both of these effects actually occur in your simulations and, if yes, could you try to better sort out their respective roles? Our simulations seem to confirm both effects taking place. We now additionally diagnosed the contribution of the different aerosol species to the contact and immersion freezing and discuss them in the manuscript.

3. In the second case, i.e. when ice nucleating bacteria lead to a decrease in ICNC, even a relatively small number of bacteria IN could potentially play a fairly large role. The importance of this effect is, however, limited by (a) the spatially inhomogeneous distribution of bacteria IN in the atmosphere and (b) the condition that ice crystal growth needs to be fast enough in order for super saturation to be sufficiently depleted before other (possibly more numerous) IN start playing a role. Could you provide some esti-

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mate regarding the relative roles of (a) and (b)? This is only relevant for cirrus clouds on which bacteria have no influence in our model. In our study we look only at mixed-phase clouds. However, this is a valuable comment which we will consider in future studies when bacteria are also allowed to influence cirrus clouds.

4. Would a change in the color scale of Figs. 7 and 8 help? We changed this. You might also want to mention the Arctic here. We already do by writing "boreal regions", which cover the area around the Arctic circle. However, we now also mention the Arctic for clarification.

5. p. 1460, l. 25: you could also cite Grützun et al. (2008). Done.

6. Sect 2: Please make clear that the ECHAM5 version you are describing here differs from the one described in Roeckner et al., 2003. Done.

7. p. 1462, line 9: which variables are nudged? Is water vapor among them? Only temperature, surface pressure, vorticity and divergence are nudged. We added this information.

8. p. 1465, line 8: You make it sound as if the observations do not reproduce the observed variability. Please re-formulate. We assume that the comment should mean "the simulations [. . .] do not reproduce the observed variability". As we write in the manuscript, they do, but not in a sufficient manner. Another problem is the fact that the amount of existing observations is insufficient.

9. p. 1565, line 15: on the danger of outing myself as a nitpicker: but this sounds as if standardized long-term observations with world wide coverage could help the fact that the model underestimates the variability in the bacteria concentrations? And: would setting up such a worldwide network of measurements be something that you would recommend based on your findings (other aspects aside)? No and yes. The model is insufficient. Part of this is related to not being able to distinguish the emissions for different forest types and that information for some biomes are missing. As for the

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second point, we added those recommendations to our conclusions.

10. p. 1466, lines 1-7: Good point. Think about mentioning it in the abstract/conclusions. Done.

11. p. 1466, l. 26-27: Could it be that the increased OLR is due to the reduction in Ni in this run (Table 4)? How do you know that it is due to the decrease in LWP? Yes, that is indeed the case and it is corrected in the manuscript.

12. Table 3, 3rd column: why do dry and wet deposition not add up to the total deposition for for 100BT-100? This was an error and is now corrected in the manuscript.

13. Table 3, 4th column: what does 6.9o E×10−12 mean? This is an observational value at that location.

14. Table 4: is it straight forward to find the Nl number directly from Han et al. (1998)? I must have overlooked this. The Nl was calculated by Ulrike Lohmann from raw data obtained directly from Han et al.

15. Fig. 5a: Is there any physical meaning in the patchiness of this figure or could it be that an averaging period of one year is too short even for a nudged run? During testing we conducted a five-year free run and observed a similar patchiness. The patchiness is due to differences in meteorology, even in the nudged run.

16. Please cite Hoose et al. 2010a.: Done.

17. Please address the issues raised in the referee comments by Dr Hoose thoroughly.: Done.

3 Technical comments

1. Table 4, caption: corrected

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2. p. 1463, l. 24: corrected

3. Table 4, Fig 5: ICNC is used consistently throughout

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 1457, 2011.