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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.

P. Amato (Referee)

pierre.amato@univ-bpclermont.fr

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Comments on "Bacteria in the ECHAM5-HAM global climate model" by Sesartic et al, submitted to Atmos. Chem. Phys.

This paper reports the parameterization of airborne bacteria as ice nuclei in the model of climate at global scale ECHAM5. Data have been selected in the literature for IN activity of bacteria and for estimations of the emission fluxes of bacteria by surfaces. Then, simulations considering that 1% to 100% of the bacteria were IN were ran, along with a control simulation in which no IN bacteria was present, and a simulation in which emission fluxes of bacteria were increased by a factor 100. Results show that the pres-

ence of bacteria does make only minor changes in cloud formation and precipitation, but that this likely plays a role on the amount of ice, respect to liquid water, in clouds, especially at high latitude in the North hemisphere. Aerobiology and its implications in atmospheric processes are of increasing interest. Many more or less recent papers reported the presence of IN bacteria in the atmosphere and in hydrometeors, raising the question of their participation to precipitations and to the formation clouds. Within the last few years, a couple of papers have been published in which it was attempted to numerically simulate the presence of such bacteria on the behavior of clouds using atmospheric models. They had to deal with an obvious lack of observational data and with a certain heterogeneity of these data; Sesartic et al. also has to face this problem. Based on the same dataset and using a very similar parameterization as in Hoose et al (2010) for emission fluxes, the conclusions here are also rather similar: there would be no significant impact of the presence of bacteria on precipitations and cloud cover in average at a global scale, but regional effects are likely. To my mind, this work is pioneer in the modeling of atmospheric processes and aerobiology and deserves to be published in ACP. There are some issues dealing with the description of the parameterization of the emissions of bacteria and of their IN activity, both linked to the fact the authors are modelers and physicists before being microbiologists. In compensation, please note that this review is the point of view of a microbiologist, and that I have certainly missed obvious details and constraints linked to modeling and physics parameterization.

Concerning the parameterization of the emission of bacteria, a similar scheme as defined by in Hoose et al. (2010) has been used. Since there is a real lack of data about this, extrapolations have been made to cover all surface types. I am mainly concerned here by the fact that, if I understand well, emission fluxes of bacteria are considered similar for all forest types, from boreal to equatorial. Also tundra appears as a quite strong source of bacteria for the atmosphere (fig 2), explaining the difference in IWC in the Arctic (Fig 5). It sounds rather weird to me that this is not latitude dependent and could explain why adding IN bacteria to the model make a big difference especially at

low altitudes North to 60°N (fig 5), where cold temperature facilitate freezing. Authors have discussed this point briefly, but no real criticism about the parameterization of the emission of bacteria has been made while this is apparently one of the key parameters in this work. Furthermore, more details should be given concerning the experimental data used for parameterization of bacteria as ice nucleators because it is still rather hard to figure this out in the manuscript. I understand that overestimating their activity could be seen as a sensitivity test, but it seems to me too much overestimated (and not really discussed) for several reasons: First, what does exactly mean 1%, 10% and 100% of the bacteria active as IN? This depends on the bacterial species and on the temperature, so this needs to be specified. I believe that these correspond to the relative abundance of cells belonging to species potentially ice nucleators (meaning for example that there was 1%, 10% or 100% of Pseudomonas syringae in the bacterial community). I guess that in a further step, this proportion should be made variable, if this is computationally possible, as a function of the temperature. Second, the parameterization was based on that used in Diehl and Wutzer (2004) and Diehl et al. (2006) for modeling clouds, itself based on experimental data from Levin and Yankofsky (1983). In the last reference, the proportion of frozen droplets containing "about 5×106 bacteria/drop" was investigated and expressed as a function of temperature. So, because of the very high concentration of bacteria, this did not take into account the heterogeneity of the population of cells, especially at the warmest temperatures (which I guess are the most important in this kind of study because this is where bacteria could make a difference). However, this is of primary importance at low concentration of bacteria, as it is in natural clouds. Indeed not all the cells of a population are IN at the same temperature, even in a pure culture. Because of this, we cannot consider that a droplet containing 1 cell of an IN bacterial species will have the same probability to freeze as a droplet containing a million more cells. So I wonder how the authors converted a fraction of frozen drops into INA/cell at a given temperature (as in e.g. Wolber et al., PNAS 83, 7256-7260, 1986), i.e. a parameter that can be used for bacterial IN parameterization. Such issues and the parameterization of ice nucleation by bacteria have to be

explained more precisely in the manuscript. Authors affirm that 10% of bacteria being IN is a good estimate (line 198), but if we consider the data in the literature, even 1% appears overestimated (but again, this depends mainly on the temperature). A paper by Santl Temkiv is referenced but this corresponds to an abstract of a conference that is not accessible: they reported 9% of the bacteria in rain samples to be INA+ (at what temperature?). I would like to have more information about this if they exist: fraction of IN/cell and dependence to temperature? Is the value of 9% related to the total number of bacterial cells in the sample, or to the relative number of colonies isolated by culture, or again to the number of isolated species, as in Constandinidou et al (Phytopathology 80, 1990)? In the latter case, this would make a huge difference if we consider that less than 1% of the bacteria in the atmosphere can generally be cultivated (Amato et al., Atmos. Environ. 41, 2007) and that among them, Pseudomonas are generally rather easily cultivated. This would lead to an important overestimation of the proportion of IN bacteria. The paper by Christner et al (Science, 2008) is mentioned (p 1460, line 10) but apparently not reported correctly: I do not see where the concentration of 500 cells/L in fresh snow comes from. In the same paper, I found that Christner et al. reported 1.5×104 to 5.4×106 cells/L, of which less than 102 were identified as bacterial IN at -7°C. In this paper, Christner et al. estimated that "0.4% of the cells in mid-latitude snowfalls were ice-nucleating active at temperatures between –7°C and –4°C".

I have also some minor comments that are listed below (including errors in the references; it would be a good point to be more careful when citing published work):

- Overall the manuscript (and in the first sentence of the abstract): not ALL bacteria are IN at warm temperature, but only a limited number of species. So replace "bacteria" by "some bacteria" or equivalent.

- Page 1459, line 12: the reference Matthias-Maser and Jaenicke (1995) is not appropriate here (for 74% (by volume, according to Morris et al., 2008 (BGD)) of bioaerosols over the Amazon forest, cite rather Graham et al (J. Geophys. Res., 108(D24), 4765, doi:10.1029/2003JD004049, 2003).

- Page 1459, line 17: the reference Amato et al., 2006 does not exist. You may want to use the following reference, instead: Amato P., Parazols M., Sancelme M., Mailhot G., Laj P. and Delort A-M. (2007). An important oceanic source of micro-organisms for cloud water at the puy de Dôme (France). Atmospheric Environment 41, 8253-8263.

- Page 1459, line 17: it is not question of bacteria in the ref Burch and Levetin (2002).

- Page 1459, line 18: To my knowledge, all, and not most, IN bacteria known so far are gram-negative.

- Page 1459, line 19: Why would the surface area of gram-negative bacteria be larger than that of gram-positive bacteria? cell size does not depend on the composition of the membrane.

- Page 1459, line 25: a reference should be provided for the CCN activity of Pseudomonas syringae.

- Page 1460, line 4: I guess that the last sentence of the paragraph is not on the right place.

- Page 1460, line 9: where does this value of 500 particles (that are actually cells?) per litre of snow come from?

- Page 1460, line 25: Möhler et al. (2008) was an experimental model in cloud chamber, while other refs were numeric models. Please distinguish the two approaches.

- Page 1463, line 10: why does the number of aerosol modes was increased from 7 to 9? Are there 9 size modes of distribution for airborne bacteria? If so, please give a reference. And what sizes do they correspond to?

- Page 1463, line 24: the mass used for bacteria was set to 10-15 kg, which seems to be a little bit high. Indeed, considering the given density of 1.2, this corresponds to a cell diameter of 2.5 μm if I did the calculation right. Eventhough this could be represent an aggregate of several cells, as they commonly are in the air, bacteria are generally

considered to be around 1 μm in diameter. I do not know what the impacts of cell size in the model are, but maybe this would deserve to be specified and discussed quickly.

- Page 1465, lines 6-7: Clarify this sentence (the use of “observed” in confusing).

- Page 1465, line 29: why does the presence of bacteria impact homogeneous freezing (fig 5)?

- Page 1466, line 22: the difference in LWP between the CTL and 100BT-100 reported in Table 4 is not 7% as mentioned but 4%.

- Figure 4: The concentrations of bacteria in the literature were obtained using various methods (i.e. culture or microscopy). Plotting them on the same graphs does not make sense since this actually shows different parameters (i.e. total cells, viable cells). This is mentioned in the text briefly, but I would recommend removing this figure.

- Figure 6: It seems here that the total amount of condensed water (LWP + IWP) is different from a simulation to another. Why is that? And if I can easily conceive that the presence of IN bacteria modifies IWC and LWC, I have more difficulties to see how a difference in the number of IN bacteria affects the total amount of condensed water. Could you please explain this?

Pierre Amato

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