

## ***Interactive comment on “Impacts from ice-nucleating bacteria on deepconvection: implications for the biosphere-atmosphere interaction in climatechange” by V. T. J. Phillips et al.***

**G. Vali (Referee)**

vali@uwyo.edu

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General comment:

This paper explores the linkage between biogenic ice nuclei and precipitation. With authors both from the fields of atmospheric science and microbiology, the paper can confidently deal with the gamut of considerations that enter into this complex situation. Yet, I am afraid that I find this paper disappointing, perhaps because it is not rigorous enough on either side of the link. The core of this study is the use of a cloud model to

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explore changes in cloud characteristics as a result of assumed variations in the input of bacterial ice nuclei. Thus, the focus is on a storm, yet one learns very little about it, at least I can't find it in the cited references. As it is, the storm details are eluded to but not manifested, the biological details are summarized from previous publications with no new material. This is what leads to the paper being unconvincing. More of the reasons for this impression are detailed in the following.

Specific comments:

The title directs the reader's attention to 'impacts .. on deep convection' and to 'climate change'. These strike me as too ambitious. The impacts examined in the paper are not on convection per se, but on the precipitation and radiation characteristics of a single simulated cloud. The title should indicate that a numerical model is at the core of the paper. Climate impacts need more than a single case to assess in more than a speculative way.

The abstract starts with the caveat that this paper is a "framework" and that it is a "preliminary simulation". This does excuse the authors from describing the simulation in detail, but isn't quite consistent with what really is contained in the paper. The framework, the model itself, was the subject of other papers. This paper is a utilization of that framework (model structure) for a given set of assumptions.

The last paragraph of the Abstract appears to me as a good motivation for this and similar studies, but is not elaborated in the paper in any substantive way. The same point is made as (3) in the preceding paragraph, and that is probably all that is needed.

The Introduction is too much of a recital of basic cloud processes and impacts to be useful, as detailed below. The paper would benefit more from a well-directed lead-in to the main thrust of this paper. Why start with broad questions when the paper is focused on a specific issue in a rather narrow context? A few sentences and references regarding tropospheric ice nuclei and the uncertainties caused by the paucity of solid knowledge about their origins, followed by the material in Section 2 would make a more

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effective, and shorter, introduction.

Section 2 rounds out material given in section 2 of Morris et al. 2008b and in section 7 of Moehler et al. 2007. While this is useful to some extent, repetition could be avoided. While it is understandable that the authors press their points, speculative statements appropriate for the overview papers are a distraction here. Here the focus should be on likely abundances in air that could influence storms, and not on the processes that determine what those concentrations are on leaves.

Section 3.1 sets out a crucial part of the model structure, but is given in qualitative form only. The model results can hardly be evaluated by the reader without knowing what these input parameters were.

Section 4 details the model runs. As already mentioned, it is difficult to learn much from this paper about the storm being simulated. Also, little is given in the paper that would convince the reader of the reliability of the model, beyond the reference to another case of considerably different character. Since the process investigated here is ice initiation, the model predictions on that score need to be assessed. How good is a 2D simulation of such a long-lived situation? How important is the motion of the storm over the period of time?

The presentation is uneven. In section 5, the tone shift to one that would be appropriate in the Journal of Atmospheric Science but will be hard to decipher for the biologists.

Technical comments:

pg 1036, ln 20-26: One wonders whether another re-statement of these fundamentals is really needed.

pg 1037, ln 2-8: Again, these general statements are fairly superfluous. Citing his own work in support of these statements is an assignment of originality that is hardly warranted.

pg 1037, ln 10-13: The parenthetical and quotation-marked phrases, and the subse-

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quent sentence, are misleading in my opinion. Yes, either or both phases may be present at those temperatures, there are important differences between all-liquid, all-ice or mixed clouds. The implication here seems to be that they are always mixed.

pg 1037, ln 13-15: Mixed-phase clouds have no unique role with respect to the radiation budget. There is a continuum of clouds of different compositions with their radiation impact depending on much more than just the ice-water mix. In total, justifying the importance of cloud phase on the basis of radiation impact alone is misleading, specially for deep clouds which have major water-cycle impacts.

pg 1038, ln 5: What the authors mean by 'always' here? Does this sentence really contribute to the paper?

pg 1038, ln 5-15: The statement that PBA are water-insoluble, or have been before getting coated, is too much of a simplification according to many investigators. There is ample literature on this. Since this paper deals only with bacterial sources, a fuller discussion of this topic is not warranted, so the authors should not deal with the issue here.

pg 1040, ln3: Is the Phillips et al 2008 reference the most appropriate source about the concentrations of airborne microorganisms? How about other contributions to the Special Issue?

pg 1040, ln14: What is meant by "... it was rife ..."? Has there been a change?

pg 1040, ln 20: "the same clone .." is that the right characterization?

pg 1042, ln 23: It'd be simpler to write : " .. to include a prognostic aerosol component." Similarly, a few lines down, the double definition via a parenthetical quote is redundant.

pg 1043, ln 13-18: The same thing is being said in the active role (nucleation) and then in the passive (nucleation scavenging). Why? If precipitation (line 17) scavenging by washout is included that is different, but it is not clear whether this was in fact done.

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pg 1043, ln 10->: Numerical definitions are absent.

pg 1044, ln3-4: This sentence makes no sense to me, mostly because of '... without freezing ...'

pg 1044, ln 6: What is meant by "... aerosol freezing ..."?

pg 1044, ln 13: The sentence starting with "Accretion ..." should be reworded; it is now garbled.

pg 1044, ln 27: In light of earlier discussions in the paper, why is the possibility excluded that the bacteria have soluble coatings or components, so that they can play a CCN role?

pg 1044, ln 27: What do "crystals" refer to here?

pg 1045, ln 7: If there is a fixed proportion of 6% of ice that originates from bacteria, than the relative effectiveness of bacteria versus other ice nucleating substances is a priori eliminated from the model. That seems to be an unrealistic approach and probably undermines the significance of the whole study, so the authors should justify why they have chosen to use this assumption.

pg 1045, ln 14: "... agrees..." is given a rather liberal interpretation here. In fact, one wonders, whether the curve shown in Fig 1. is significantly different, or not, from the assumption of fraction =0 for  $T > 3^{\circ}\text{C}$  and fraction =  $10^{-5}$  for  $T < 3^{\circ}\text{C}$  ?

pg 1048, ln 1: What is given in this figure - an average over the lifetime of the storm or some specific time? Same question for the other figures.

Fig. 3. What are represented here? Averages over the whole domain? Updraft? ?????? Same question for all figures.

Because I am uncertain about what has been demonstrated in section 5, I cannot comment on the conclusions.

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