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Discussions

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.

V. T. J. Phillips et al.

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Overview

We thank the reviewer, Gabor Vali, for the comments and many valuable suggestions made. In the light of the reviewer's comments, a revised and improved version of the manuscript will be prepared. The reviewer's shrewd observation that a large sample of clouds is needed to assess impacts from aerosols was a motivation for using a cloud-system resolving model (CSRM) in our study. CSRMs simulate many clouds of many types over a wide area.

A key criticism by the reviewer is that there is insufficient detail about the simulated case and the cloud model. In the revised version of the manuscript, Sections 1 and 2 will be shortened and more detail provided about the case and model.

Serious misunderstandings about our paper are apparent in the review. For instance, the reviewer interprets the paper as assuming that 6% of all heterogeneous crystals are from bacteria in all our cloud simulations for Oklahoma. His interpretation is not correct (see point (m) below). It follows that his claim about the paper's work being undermined is also not correct (see point (n)). That fraction refers only to a particular aerosol scenario used by Phillips et al. (2008, JAS) when constructing the ice scheme. Also, somehow, the reviewer assumes that the model does not represent CCN activation of bacteria (point (k)) when it actually does. This point will be further stated in the revised paper. Finally, the reviewer suggests that what our CSRM has simulated is a "*single simulated cloud*". But his interpretation is not correct (see point (a) below). Instead, the CSRM simulates a 4-day episode of convection, including many clouds of different types, over an area of 170 km in width. This is about half of the width of the state of Oklahoma. The sample of clouds is large.

Changes planned for the revised paper include:- (1) new simulations of the other two ARM sub-cases so as to enlarge the simulated period to almost two weeks; (2) analysis of the cloud dynamics (e.g. with vertical velocity histograms) of simulations; (3) a revised title (see point (b)); (4) clarification as detailed here, so as to avoid misunderstandings, and an augmented model description; and (5) more validation of the control simulation.

The present paper depends critically on the empirical parameterization for a representation of biological ice nucleation, described in detail in a longer article in press at Journal of Atmospheric Sciences (Phillips et al. 2008, JAS). The paper will appear soon, and a copy may be sent to reviewers here at BGD on request.

In summary, several misunderstandings about our paper are evident in the review. In response, we offer clarification and will provide extra explanations in the revised paper.

Detailed Responses

(a) The reviewer's statement that *"The impacts examined in the paper are not on convection per se, but on the precipitation and radiation characteristics of a single simulated cloud"* is false. In fact, the plotted profiles of cloud statistics provided in the paper are from an ensemble of many clouds of many types (stratiform and convective) and are certainly not from a single simulated cloud. A large sample of deep convection over 4 days has been simulated. The CSRM domain is very wide (170 km), spanning about half of the average width (from west to east) of the state of Oklahoma. Many clouds are resolved over this area. Sensitivities of radiative and cloud-microphysical statistics are thoroughly examined in the paper. Nevertheless, we will provide further explanation in the revised paper.

All plotted vertical profiles are conditional averages over visible cloudy regions, averaged throughout the entire duration of simulation (excluding regions when and where upper-level cirrus is present, for some plots). In the revised version of the paper, the averaging procedure will be explained in more detail.

(b) The reviewer's statement that *"Climate impacts need more than a single case to assess in more than a speculative way"* requires clarification. From the context to this comment, it is clear that "*single case*" refers to the reviewer's assumption that only *"a single simulated cloud"* is studied in the paper. As stated above (point (a)), that assumption is not correct. There is no suggestion anywhere in the paper that the impact from biogenic aerosol on the climate system is being evaluated. The aims of the paper are to assess impacts on cloud ensembles, and to discuss general implications for the atmosphere-biosphere interaction. The title may be altered slightly (for example, perhaps to "Numerical modeling of impacts from ice-nucleating bacteria on clouds:

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implications for the biosphere-atmosphere interaction") to clarify the purpose of the paper and make it more accessible.

(c) The reviewer's comments that *"The framework, the model itself, was the subject of other papers"* and that "*This paper is a utilization of that framework (model structure) for a given set of assumptions*" require clarification. Actually, there have been important modifications to the model that are described for the first time in the present BGD paper. There is now a treatment of how aerosols are depleted by nucleation and replenished by evaporation, with their amounts predicted in air, cloud and precipitation for each chemical species. There is now wash-out of activated aerosol by accretion of its cloud-particles onto falling precipitation (what we have termed "in-cloud nucleation scavenging"). Inclusion of the prognostic aerosol component is the crucial change described for the first time here. The recent changes will be organised more clearly in the revised version of the paper.

(d) The reviewer's statement that *"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*" requires clarification. The point will be elaborated in more detail in the revised paper, as suggested. The *"open question"* mentioned in that paragraph, about the possibility of a feedback between the local climate and bacterial growth/emissions, follows logically from model results documented in the paper. Impacts are predicted from INA bacteria on cloud properties and on the precise meteorological conditions known to control bacterial growth on leaves.

(e) The reviewer states that the *"introduction is too much of a recital of basic cloud processes and impacts to be useful"*. Yet what is obvious to an atmospheric specialist such as the reviewer is not necessarily clear to a reader of "Biogeosciences" from another disciplinary background. It is of paramount importance here to define the basic concepts and terminology used later to explain model results. Such essential definitions may be shortened, however.

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(f) The reviewer states that *"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"*. Concerning the case of deep convection over Oklahoma simulated here (ARM(C)), simulations of it with an earlier version of our CSRM were validated by comparison of predicted radiative fluxes with ARM observations by Phillips and Donner (2007, QJRMS). Fluxes were averaged over the entire domain and duration of each simulation and over an ensemble of 84 different simulations (perturbed randomly at initialisation). The 2D domain and resolution were the same as applied in the present BGD paper. The fractional errors of predicted average fluxes were lower than about 10% and 5% in the short- and long-wave respectively, both at the ground and at the top of the atmosphere. Such fluxes are sensitive to the extent and properties of clouds.

Yes, it will be possible to provide extra validation of our simulation of this case for the present paper when we revise it. Yet no aircraft observations of crystal number concentration are available, as far as we are aware, for ARM(C). Consequently, it does not seem possible to validate the simulated ice initiation for this case of deep convection over Oklahoma.

The general accuracy of previous versions of our CSRM has been demonstrated in comparison with satellite, surface-based and aircraft observations for a total of 6 cases of deep convection by Phillips and Donner (2007, e.g. Table 2 therein) and Phillips et al. (2007, JAS). These include 3 land cases (ARM (A-C)), one of which is simulated here, and 3 cases from the tropical Pacific (TOGA-COARE, KWAJEX) and Atlantic (GATE) oceans. Validated quantities include radiative fluxes for all 6 cases in the above manner, and cloud (microphysical, dynamical) statistics for the oceanic cases. In particular, predicted number concentrations of crystals and droplets have been validated against aircraft observations by Phillips et al. (2007, JAS) for cases from the tropical Pacific ocean simulated by our advanced double-moment CSRM. Such extensive validation

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of the representation of ice initiation is unprecedented for CSRMs with double-moment bulk microphysics schemes. Our advanced double-moment CSRM has predicted radiative fluxes, in the shortwave and longwave at the surface and top of the atmosphere, with average errors of less than about 1-2% over the tropical Pacific ocean (Phillips et al. 2007, JAS).

(g) The reviewer's statement that *"citing his own work in support of these statements is an assignment of originality that is hardly warranted"* requires clarification. No assignment of originality is actually made to that cited paper, and the citation is done in this style: "e.g. .... et al". Nonetheless, in the revised version, extra citations of other earlier papers will be provided in addition.

(h) The reviewer states that *"Mixed-phase clouds have no unique role with respect to the radiation budget"*. From global simulations by Senior and Mitchell (1993), clouds just above the freezing level appear to be unique insofar as they have the potential to change phase, and hence, their radiative properties, during sufficient tropospheric warming. Senior and Mitchell find that this provides an important cloud-radiation feedback in climate change. Perhaps what ought to have been written is "clouds in the mixed-phase region" instead of "mixed-phase clouds", and this will be corrected in the revised version.

(i) In response to the reviewer's comment that *"justifying the importance of cloud phase on the basis of radiation impact alone is misleading"*, we will mention other impacts in addition. However, there is no suggestion anywhere in the paper that the radiation impact is the only one. We will provide further explanation in the revised paper.

(j) The reviewer asks why the *"same thing is being said in the active role (nucleation) and then in the passive (nucleation scavenging)"* (pg 1043, ln 13-18). By "in-cloud nucleation scavenging", we meant the process by which aerosols are depleted by their activation of cloud-particles that are then removed from the atmosphere by accretion onto precipitation. This removes aerosol from the atmosphere. Yes, what the reviewer describes as *"precipitation scavenging by washout"* is indeed included. It is a necessary process for components of each aerosol species to be explicitly predicted in air, cloud and precipitation. The text will be clarified in the revised version and our terminology standardised.

(k) The reviewer's statement is false, about the paper excluding the possibility *"that the bacteria have soluble coatings or components, so that they can play a CCN role"*. The model actually does represent both the IN activity by the insoluble component of INA bacteria and the CCN activity by their soluble coatings. That is stated in the model description section of the paper (pg 1043, ln 22-24) and will be highlighted further in the revised version. It is also why the basic concept, of PBAs' composition including both water-soluble and water-insoluble material, is introduced early in the paper.

(l) The reviewer questions whether Phillips et al. (2008, JAS) is the most appropriate source in the literature about concentrations of airborne microorganisms. Prior observational papers cited by Phillips et al. (2008, JAS) will also be cited in the revised version of the present paper, as suggested. The above reference is relevant as it provides a broad summary of the literature on measurements and assessments of biogenic aerosols.

(m) The reviewer's statements are false, about the cloud model having *"a fixed proportion of 6% of ice that originates from bacteria"* so that *"the relative effectiveness of bacteria versus other ice nucleating substances is a priori eliminated from the model"*. In fact, that proportion (6%) applies only to one particular aerosol scenario of air sampled in Colorado (Phillips et al. 2008, JAS) when constructing the empirical parameterization of heterogeneous ice nucleation. This fraction applies to that background-troposphere scenario only. It does not apply to all aerosol scenarios in general, such as those simulated over Oklahoma for the present BGD paper. The empirical parameterization takes account of the aerosol concentration and chemistry for a wide range of aerosol scenarios in general when predicting crystal numbers, and requires no such fixed proportion to be assumed for all scenarios. Clearly, the way the text was writ-

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ten allowed such a misunderstanding to arise. The text will be clarified in the revised version.

(n) It follows that the reviewer's very next statement that this seems *"an unrealistic approach and probably undermines the significance of the whole study"* is also not correct. Nothing about the paper is undermined. We plan to add sufficient details describing our numerical modeling approach. The text will be made clearer in the revised version of the paper, to avoid such misunderstandings.

(p) The reviewer criticises our claim that there is agreement between the laboratory observations and our heterogeneous ice nucleation scheme in Figure 1 by asserting that we give a *"liberal interpretation"* to the phrase, "agrees with". This requires clarification. Over most of the experimental temperature range (about -1 to -15 degC), our empirical parameterization (red line) agrees with the independent experimental data (blue points only) because it lies within the range of their values. Over the entire experimental temperature range, the prediction is within the spread of all the experimental data (red and blue points), being not very far from their geometric mean. No problem with the ice scheme can be seen. This will all be clarified in the revised paper.

At temperatures between -10 and -15 degC, independent data (blue points) are scarce and display higher freezing fractions than predicted by the scheme. That is to be expected because single bacterial strains that were highly effective at nucleating ice were preferentially selected for those laboratory studies.

(q) Other comments by the reviewer will be clarified in the revised version of the paper.

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