

***Interactive comment on “Effect of bacterial ice nuclei on the frequency and intensity of lightning activity inferred by the BRAMS model” by F. L. T. Gonçalves et al.***

**Anonymous Referee #2**

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**General comments**

This manuscript deals with the impact of bacterial ice nuclei (IN) concentration on liquid and ice content, and precipitation. Through numerical simulations with varying IN concentrations, it is shown that including ice nucleation activity from *P. syringae* reduces slightly precipitation at the ground, and increases the cloud ice content. The authors also investigate the potential impact of these IN on lightning activity with a flash rate parameterization based on microphysical and dynamical parameters. They show that taking into account bacterial IN in ice nucleation induces an increase in the estimated flash rate.

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The paper deserves to be published but after consideration by the authors of the following substantive issues.

**Specific comments**

This manuscript should be carefully read before next submission.

Title

My first point of concern is about the title. In my sense, it does not reflect the article content. This paper is first focused on the impact of bacterial IN on liquid and ice water content. Then, the flash rate is parameterized to get an idea of the impact of these IN on the total flash rate. When you first look at the title, you get the feeling that an explicit cloud electrification and lightning flash scheme (Helsdon et al., 1992; Mansell et al., 2002; Barthe and Pinty, 2007) is used, and that lightning is the major focus of this paper. The authors should make clear in the title that they also investigate the effect of bacterial ice nuclei on cloud properties.

Introduction

More attention should be paid to the introduction.

- p 26147, l 13: from the beginning of the introduction, the role of microorganisms as ice nuclei is discussed. Thus, it is difficult to understand why there is a reference to a paper dealing with mineral dust (Levin et al., 2005). The transition should be made clear.

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- p 26147, l 25: more references should be added concerning the non-inductive mechanism as the main process responsible for cloud electrification. Numerous observational or modelling study have addressed the importance of this mechanism: Reynolds et al. (1957); Williams and Lhermitte (1983); Dye et al. (1989); Rutledge et al. (1992); Carey and Rutledge (1996); Petersen et al. (1999); Latham et al. (2007), among others.
- p 26148, l 1: "Due to gravitational force...". This sentence is not clear.
- p 26148, l 5-8: Stating that an increase number of ice crystals will increase the number of rebounding collisions, and thus the lightning activity is, in my sense, oversimplified. Indeed, you can also consider that the parameterizations of this process (Saunders et al., 1991; Takahashi, 1978) predict higher separation rates per collision for larger crystals (see Mansell et al. (2005)).
- p 26148, l 9-13: this paragraph should not be there, between two paragraphs dealing with cloud electricity.
- p 26148, l 16: add references. See for exemple Schumann and Huntrieser (2007) for a review of lightning-produced NO<sub>x</sub>. See also references for ozone production (Section 2.6 Importance of LNO<sub>x</sub> for atmospheric chemistry) in this review paper.
- In the last paragraph of the introduction, I would have expected a short description of the objective(s) and the outline of the paper.

## Methodology

This study entirely deals with numerical modelling, therefore the simulation set-up deserves more discussion. Section 2.1 should be divided into 2 distinct parts.

In 2.1 Numerical set-up, the authors would describe the model they use, the domain and resolution, the way the simulation is initialized, the physics (do you use a

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turbulence scheme?). In the current manuscript, some details of the simulation are missing: the time step, the duration of the simulation, the vertical grid (how many levels? resolution at the bottom, at the top? altitude of the higher level?)

Section 2.2 should be devoted to Ice nucleation parameterization. Before describing the different scenarii, you should describe how ice nucleation is parameterized in BRAMS. In your simulation, do you consider only bacteria as ice nuclei?

Is it realistic to consider IN concentrations homogeneous over the whole domain? Do you mean horizontally and vertically homogeneous? Please argue why you hypothesize there is no IN depletion.

In Section 2.3 (Cloud electrification), several points need to be clarified.

I do not understand why you use the horizontal divergence instead of the vertical velocity. Deierling et al. (2008) used the horizontal divergence at the top of their domain to get an estimate of the vertical velocity, but their study is based on radar data. With your model, you have a direct access to the vertical velocity at each grid point and each time step.

Why do you consider snow and aggregates are non-precipitable species?

Please check the unit of constant  $C$  ( $\text{fl min}^{-1} \text{kg}^{-2} \text{m}^{-1} \text{s}^{-2}$  instead of  $\text{s fl}^{-1} \text{kg}^{-2} \text{m}^{-1}$ ?). If you use  $\text{fl s kg}^{-2} \text{m}^{-1}$  instead of  $\text{fl min}^{-1} \text{kg}^{-2} \text{m}^{-1} \text{s}^{-2}$ , the value of  $C$  should be modified.

You should refer to Barthe et al. (2010) (published paper) instead of Barthe et al. (2007) which is an abstract and does not contain any formula.

## Results

A set of figures showing how the cloud develops would be appreciated since the reader even does not know what this convective cloud looks like. What is the altitude (temperature) of the cloud top height? What is the maximum vertical velocity? Is it a realistic convective cloud for the Sao Paulo region? Are these conditions favorable for lightning production? Do you know if some lightning activity was reported for the 3 March 2003 storm?

Why do you use the total mixing ratio rather than the total mass of each species?

In Section 3.2, you should conclude about the potential influence of bacterial ice nuclei on the estimated total flash rate. In the conclusion (p 26157, l 7), you state that "the bacterium *P. syringae* (...) induces an increase in cloud electricity". The term "cloud electricity" is too general. Since you investigated the impact of bacterial ice nuclei on the parameterized total flash rate, you should rather use the term "estimated total flash rate".

## Technical corrections

p 26145, l 21: (Pouler et al., 1992))

p 26146, l 19: *P. syringae*

p 26147, l 16: enhance

p 26148, l 3: electric potential

p 26148, l 7: electric charges

p 26150, l 20: define INA

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p 26153, l 6: in Table 2, I do not see the total precipitation at ground level.

p 26153, l 7: Table 2

p 26153, l 14: this sentence is incomplete. You should precise that "The last result agrees with Levin et al. (2005) who observed rain reduction at the ground when they allowed mineral dust particules to act as efficient ice nuclei".

p 26154, l 3, and Figure 3: how can you get the temporal evolution of vertical profiles if you integrate "over the horizontal and vertical extents of the cloud"?

p 26154, l 6, l 7, p 26155, l 2: maxima

p 26154, l 13: "several minutes later": give a value

p 26154, l 25 and 28 ; p 26155, l 3 and 24 ; p 26156, l 24: riming

p 26154, l 29, p 26155, l 23: accretion

p 26155, l 18: similar scenario to S4

p 26155, l 21: "a smaller concentration" or "smaller concentrations"

p 26156, l 13: Fig. 3 → Fig. 4

p 26156, l 20: lighting → lightning

p 26156, l 22: "that produced non-precipitable ice mass flux 3 orders of magnitude greater than S1".

p 26156, l 23-25: "The low production ... in Sect. 3.1": this sentence is not clear.

p 26157, l 14: "These authors ..." → "Phillips et al. ..."

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 11, 26143, 2011.

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