3rd REFEREE

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

Received and published: 16 December 2011

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.

C13266

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

A**: We separated it in order to be clear.**

• 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); Petersen et al. (1999); Latham et al. (2007), among others.
A: We added them.

• p 26148, l 1: ”Due to gravitational force...”. This sentence is not clear.

A:We modified it.

• 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)).

A: We added them.

• p 26148, l 9-13: this paragraph should not be there, between two paragraphs dealing with cloud electricity.

A:Ok. We modified its position to the next.

• p 26148, l 16: add references. See for exemple Schumann and Huntrieser (2007) for a review of lightning-produced NOx. See also references for ozone production (Section 2.6 Importance of LNOx 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.

A: We added it.

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 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.

A: We added one section more, dealing separately with RAMS set-up and ice nucleation modeling.
We added as well more details about RAMS schemes as suggested by the other referees as well Additionally most of the answers are in our article Gonçalves et al. (2008). There is no turbulence scheme.
The IN concentrations used is based exactly on RAMS default. We only changed concentrations in order to evaluate IN. Therefore, it is homogenous vertically and horizontally, as all other IN concentrations. We also answered the other referees concerning depletion, where there is no depletion due to the cloud dynamics.

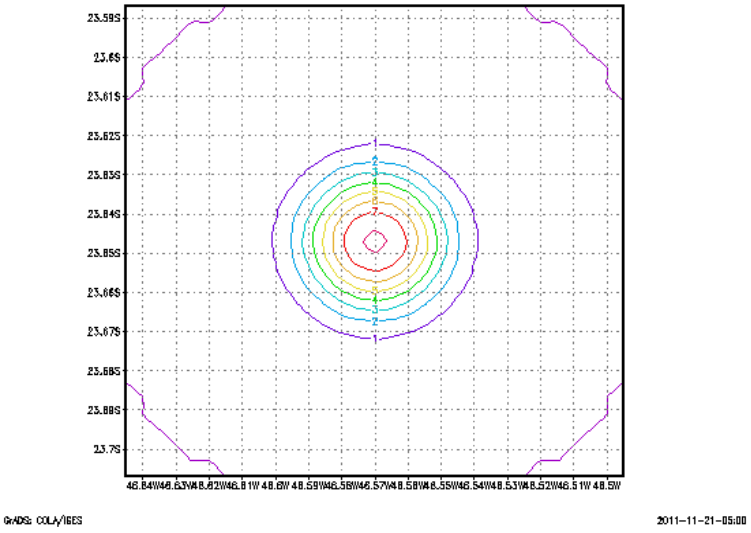
In Section 2.3 (Cloud electrification), several points need to be clarified.
I do not understand why you use the horizontal divergence instead of the vertical velocity. Deierling et al. (2008) used the horizontal divergence at the top of their domain to get an estimate of the vertical velocity, but their 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 (fl min−1 kg−2 m−1 s−2 instead of s fl−1 kg−2 m−1?). If you use fl s kg−2 m−1 instead of fl min−1 kg−2 m−1 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.

A**: The several points were also focused by the other referees and therefore directly answered in the text. The answer to them is:**: **The hypothesis of equation 1 says that $f_{NP} = m_{NP}$ w (Deierling et al (2008); Blyth et al. (2001); Latham et al. (2004)). However, w cannot be directly inferred from Doppler radar data, so Dierling et al. used the horizontal divergence wind velocity to calculate f_{NP} as $f_{NP} = m_{NP}\partial w / \partial z = m_{NP}\left(\nabla_H \rho_0 \vec{V}\right)$. This is fully explained in section 3.2 from Deierling et al. (2008) paper. Although we do have w at the model simulations, we had to infer w from horizontal wind divergence as in Deierling et al. to be consistence with their relationship ($F = 9.0\text{x}10^{-15} \cdot f_P \cdot f_{NP} + 13.4$). We have included a few lines about this in the text to better explain the methodology used to calculate *F*.**

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.

A: the cloud figures can be seen through hydrometeor figures which shows the shape of the cloud as well as cloud top (highest level with ice in Figure 3). The vertical view

is very inconvenient due to the fact that it is very difficult to see differences among the scenarios. See answer to the 2nd referee and the following figure



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?

A: The maximum vertical velocity is shown in Table 4 (maximum *w*). The top height is shown in figure 3 as explained above. Yes, it is realistic as we can see the added Radar Figure 1c and it shows favorable condition with hail. Unfortunately, we do not have lightning reports at this time.

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

A: Table 1 shows the mixing rate for each species separately, with total mass.

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”.

A: We changed “cloud electricity” for “flash rate”

<u>Technical corrections</u>

Most of the technical corrections are modified in the text. Further considerations are shown as it follows specifically.

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

A: It is rain. All hydrometeors are shown as a sum in the entire domain.

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 particles 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”?

A: We integrated spatially along 3 hours which is shown Table 2,. On the other hand, Figures 3 show 2 minutes integration. The analysis result is similar even shown different situations.

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.

A: The low production of ice hydrometeor (pristine, etc) implies in a low amount of hail and graupel, because the last ones are formed by the first ones. We think the sentence is clear enough n the text, therefore we kept it.

p 26157, l 14: ”These authors ...” –> ”Phillips et al. ...”

References

Barthe, C. and Pinty, J.-P.: Simulation of a supercellular storm using a three-dimensional

mesoscale model with an explicit lightning flash scheme, J. Geophys. Res., 112, doi: 10.1029/2006JD007484, 2007.
Barthe, C., Deierling, W., and Barth, M. C.: Estimation of total lightning from various storm parameters:
A cloud-resolving model study, J. Geophys. Res., 115, doi:10.1029/2010JD014405, 2010.
Carey, L. D. and Rutledge, S. A.: A multiparameter radar case study of the microphysical and
kinematic evolution of a lightning producing storm, J. Meteor. Atmos. Phys., 59, 33–64, 1996.
Deierling, W., Petersen, W. A., Latham, J., Ellis, S., and Christian, H. J.: The relationship between lightning activity and ice fluxes in thunderstorms, J. Geophys. Res., 113, doi:10.1029/2007JD009 700, 2008.
Dye, J. E., Jones, J. J., and Winn, W. P.: Observations within two regions of charge during initial thunderstorm electrification, Quart. J. Roy. Meteor. Soc., 114, 1271–1290, 1989.

Helsdon, J. H., Wu, G., and Farley, R. D.: An intracloud lightning parameterization scheme for a storm electrification model, J. Geophys. Res., 97, 5865–5884, 1992.
Latham, J., Petersen, W. A., Deierling, W., and Christian, H. J.: Field identification of a unique lobally dominant mechanism of thunderstorm electrification, Quart. J. Roy. Meteor. Soc.,133, 1453–1457, 2007.
Mansell, E., MacGorman, D. R., Ziegler, C. L., and Straka, J. M.: Charge structure and lightning sensitivity in a simulated multicell thunderstorm, J. Geophys. Res., 110, doi:10.1029/2004JD005 287, 2005.
Mansell, E. R., MacGorman, D., Ziegler, C. L., and Straka, J. M.: Simulated threedimensional
branched lightning in a numerical thunderstorm model, J. Geophys. Res., 107, doi:10.1029/2000JD000 244, 2002.
Petersen, W. A., Rutledge, S. A., Cifelli, R. C., Ferrier, B. S., and Smull, B. F.: Shipborne Dual-
Doppler operations during TOGA-COARE: Integrated observations of storm kinematics and
electrification, Bull. Am. Meteorol. Soc., 80, 81–96, 1999.
Reynolds, S. E., Brooks, M., and Gourley, M. F.: Thunderstorm charge separation, J. Meteor., 14, 163–178, 1957.
Rutledge, S. A., Williams, E. R., and Keenan, T. D.: The down under Doppler and electricity experiment (DUNDEE): Overview and preliminary results, Bull. Am. Meteorol. Soc., 73, 3–16, 1992.
Saunders, C. P. R., Keith, W. D., and Mitzeva, R. P.: The effect of liquid water on thunderstorm
charging, J. Geophys. Res., 96, 11 007–11 017, 1991.
Schumann, U. and Huntrieser, H.: The global lightning-induced nitrogen oxides source, Atmos.
Chem. Phys., 7, 3823–3907, 2007.
Takahashi, T.: Riming electrification as a charge generation mechanism in thunderstorms, J.
Atmos. Sci., 35, 1536–1548, 1978.

Williams, E. R. and Lhermitte, R. M.: Radar tests of the precipitation hypothesis for thunderstorm electrification, J. Geophys. Res., 88, 10,984–10,992, 1983. Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26143, 2011.
C13273