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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Evaluation of a new lightning-produced NO_x parameterization for cloud resolving models and its associated uncertainties" *by* C. Barthe and M. C. Barth

C. Barthe and M. C. Barth

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We would like to thank the referees for their interest in our work and for the helpful comments. The response to the specific comments are below and our manuscript has been revised based on these comments. The referee's comment is italicized and our response is in normal font.

Response to Anonymous Referee 1

General comments

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The manuscript presents results from the inclusion of a complex lightning NOx parameterization in a cloud-resolving model. The parameterization estimates both flash rate and the amount of NO produced. Observed flash rates are reproduced remarkably well, and the temporal evolution of flash rate is better reproduced by the new parameterization than by Price and Rind (1992) who estimated flash rate based on peak updraft velocity. Simulated NO mixing ratios also compare favorably with aircraft observations. This new approach represents a valuable contribution to studies of atmospheric chemistry, both in cloud-scale and larger-scale models. In addition to the results of the reference simulation, results from a number of sensitivity studies are presented. The impact of flash rate, flash length, the number of CG flashes, NO production by short-duration flashes, vertical and horizontal distribution of LNOx, and NO production per flash are considered. While many studies have considered one or two of these quantities, there has been no similar study which tests the sensitivity to so many parameters within a single framework. These results will be a useful reference for future studies of LNOx production and are suitable for publication for ACP. My main concern is that the sensitivity to flash length is not adequately addressed because the two simulations testing this have the same mean length (21 km). Because great uncertainty surrounds flash length estimates and they are the basis for NO production in this study, I think another simulation assuming a significantly different flash length would be very useful. I also think the discussions of CG ratio and CG production might be better addressed by combining section 5.2 with the second half of section 5.7 (CG NO production).

Specific Comments

p. 6609, line 6 - I am curious to know if any sensitivity tests were done to evaluate the impact of the choice of the 15 m/s threshold for lightning activity in a convective cell. If such tests were done in the development of the parameterization, it would be nice to

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include some discussion of them.

We did some sensitivity tests about the maximum vertical velocity for lightning activity in a convective cell. However, for this storm, the 15 m s^{-1} threshold does not have a significant influence on the LNOx production. For midlatitude, continental storms, the identification of convective cells will not be sensitive to the 15 m s^{-1} threshold because these severe storms have updrafts easily exceeding this value. The most sensitive storms would be tropical convection over land (Zipser and Lutz, 1994) which have peak updraft velocities straddling the threshold updraft. Note, that Zipser and Lutz (1994) compared midlatitude, continental convection, tropical oceanic convection, and tropical convection over land.

It is worth reminding that this threshold is used only to find the number of cells that could produce lightning flashes. Using the precipitation and non-precipitation ice mass flux product as a proxy for the total flash rate in each "detected cell", the lightning activity can still be null even if the maximum vertical velocity threshold is reached.

p. 6610, paragraph 1 - Were any sensitivity tests of the size of the lightning triggering region and the flash propagation area done? This may also be useful.

For this part of the parameterization, we relied on the LNOx parameterization developed by Ott et al. (2007). Since the average flash length they used (21.5, 27.9 and 31.4 km) is higher than in our study, it was decided to use a cylinder with a 4 km radius instead of 5 km radius as done by Ott et al. (2007). The 5 km radius used by Ott et al. (2007) is based on the horizontal extent derived from interferometer data. We didn't do any sensitivity test on this, but in a future work, it could be interesting to do it and to vary the size of the cylinder with the length of a flash.

p. 6618, line 1 - Is this decrease in NOx in the boundary layer due only to transport? It is very large. I wonder if chemical conversion may also be playing a role here?

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Yes, chemical conversion plays a role in the NO_x depletion in the boundary layer. It is the NO2 + OH –> HNO3 reaction. The sentence has been modified in the revised manuscript.

p. 6621, lines 17-18 - The authors state that the simulations producing more NOx in the convective core region (transect 1) have more NOx transported into the anvil (transect 2). It looks to me from Figure 6 that the FR_PR92_CELL simulation produces much higher spikes of NO in transect 1, but in transect 2, the peak values of FR_PR92_CELL are slightly less than FR_OBS. I assume this is due to the timing differences between the observed and PR92 flash rates, but I think the sentence may need to be modified to discuss this.

The reviewer is right. This sentence has been modified.

p. 6622, Section 5.2 - Is NO production per flash estimated in the same way for CG and IC flashes? Are flash lengths calculated using the same distribution for both IC and CG flashes? This needs to be clarified. If NO production from CG flashes is treated the same as IC flashes except for the vertical distribution, then it is not surprising that the factor of 10 larger CG ratio predicted by PR93 has little impact on NOx mixing ratios.

Yes, the only difference in the treatment of IC and CG flashes is their vertical distribution. In the case of IC flashes, a bimodal distribution is used, while for CG flashes, a Gaussian distribution is applied (see Figure 1 in DeCaria et al. (2000)). This is now detailed in the revised manuscript.

p. 6625-6626 - I don't think the impact of flash length on LNOx production is adequately addressed. The simulations presented are the reference simulation which assumes a lognormal distribution of flash length with a mean of 21 km, and a simulation which assumes this mean of 21 km for all flashes. Essentially this is testing the impact of

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flash length variations, but not not flash length itself since the means are the same (21 km). I think a simulation assuming the Defer et al. (2001) value of 34 km (mean with no short duration flashes) would be interesting here, especially because flash length is such an uncertain quantity but is the basis of NO production in this approach.

The objective of this sensitivity test was to address the impact of the flash length distribution on the LNOx production and distribution. In other words, do we need to have a detailed description of the flash length distribution (like the lognormal distribution) to correctly represent the LNOx production? Or a representative constant value is sufficient? Then, we can conclude that in the framework of this storm and of this parameterization, the flash length distribution does not impact significantly the LNOx source. In flash length section, the objectives are now explained better.

We tested the impact of increasing the constant flash length to 34 km. We found by increasing the flash length value by 62% that the NO increased by 63%. Thus, it is a linear increase and is similar to changing the number of NO molecules produced per meter of flash. A paragraph has been added reporting these results in addition to the contribution to NO production from long and short flash lengths.

p. 6630, paragraph 1 - The sensitivity tests for the CG NOx production were done using the NLDN observations which showed a very small number of CG flashes (~ 2%). I think this test would be more useful if combined with the CG ratio test presented earlier, specifically the ~ 20% CG ratio derived from the Price and Rind (1993) relationship. While this ratio is not realistic for this storm, it is closer to the climatological CG ratio derived from satellite and ground-based observations (e.g. Boccippio et al., 2001) and I think would give a sensitivity to the CG NOx production that is more broadly applicable.

The objective of this paper is to study the impact of each single parameter on the NO distribution and to compare to observations. Doing the sensitivity test that is

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suggested would make the comparison to observations and thus the evaluation of the realism of this test nearly impossible. This test should be done in a future study on a more appropriate storm with a high CG rate.

Technical Comments

Abstract - I think the authors should be a bit more careful about specifying either grid cells or convective cells. For example, "First, the convective cells that can produce lightning..."

We now specify if a cell is a grid cell or a convective cell.

p. 6605, line 17 - Change "For each step..." to "For each component..."

"For each step" has been changed to "For each component".

p. 6605, line 29 - The authors may want to add that observations of total lightning for a particular storm are difficult to obtain because ground -based networks (like the NLDN in the U.S. and BLIDS in Europe) typically record CG flashes primarily.

This sentence has been added.

p. 6606, lines 27-29 - It is mentioned that Wang and Prinn (2000) tested two estimates of NOx production per flash but the authors do not include whether this study concluded that either estimate was realistic.

Wang and Prinn (2000) did not have observations to evaluate their method. Thus, they did not conclude about the realism of the two estimates of NO_x production per flash. However, they stated that the run using the Franzblau and Popp (1989) values gives

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very high modeled NO_x mole fraction that is suspect. Further, Salzmann et al. (2008) pointed out that Franzblau and Popp (1989) is unrealistically high compared to other values reported in the literature (note that in the late 1990s there was not much more information available).

This information has been added in the revised manuscript.

p. 6607, line 29 - The authors may want to add the range of production values for IC flashes as well since that is mainly what is examined in this paper.

The sentence has been modified to include the range of production values for both IC and CG flashes.

Fig. 3 caption - I think the caption for (b) should be "... ice mass flux product (green curve)" and for (c) should be "... in each individual cell (red curve)."

The caption has been modified.

p. 6616, line 2 - Add more specifics on how much earlier supercell stage in simulation begins as well as how much stronger simulated mass flux was.

The text has been modified.

Fig. 4 - Need to add (a)-(f) labels since these are referenced in text.

The (a)-(f) labels are already included in Fig. 4 (upper left corner).

p. 6625, lines 2-3 - Change to, "...is more readily transported to higher altitudes..."

"available" has been changed to "transported".

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