

Interactive comment on “The impact of lightning on tropospheric ozone chemistry using a new global parametrisation” by D. L. Finney et al.

Anonymous Referee #2

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Summary

This article presents results from a global chemistry-climate model to show how ozone distributions and production rates change when the authors' new lightning parameterization (which is based on upward cloud ice flux) is used compared to the previous lightning parameterization that is based on cloud top height and implemented in several chemistry-climate and chemistry-weather models. Lightning produces nitrogen oxides (NO_x) in the middle and upper troposphere and therefore impacts troposphere ozone (O₃) especially in the upper troposphere. One weakness in our ability to predict the production of NO_x from lightning is to be able to predict the lightning flash rate. Finney et al. developed a new lightning flash rate parameterization for global models, and showed that it compares well with observations (in both a previous paper using a model driven by reanalysis data and in this paper using a climate model). In this paper,

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the authors describe how this new flash rate parameterization, which tends to predict fewer flashes in the tropics and more flashes in the continental extratropics, affects tropospheric ozone distributions and production rates. Because many models use the previous parameterization, it is important to publish this evaluation of the new lightning parameterization on tropospheric ozone chemistry.

The paper does a nice job of evaluating the model results to observations, comparing results between different sensitivity simulations, and showing that the ozone production efficiency is less for regions with high flash rates relative to regions with low flash rates. Although they make good recommendations, e.g. focusing field campaigns during particular months, I think there could be some additional analysis work on specific regions that can also guide future field campaigns and regional-scale model simulations. There are parts of the paper that need to be clarified. I would recommend that the authors pay attention to paragraph construction, being sure the paragraph begins with a topic sentence and is followed by supporting information linking sentences from what the reader knows to the new information (e.g. Schultz, Eloquent Science, 2009). I recommend minor revisions before publication.

Specific Comments

Title. To convey what kind of parameterization, I would suggest modifying the title to say, “using a new lightning parameterization”.

L. 126. How does the lightning-NO_x scheme differentiate between cloud-to-ground (CG) and intracloud (IC) lightning? Does it need to make this difference if the production of NO per flash is the same for CG and IC flashes and the vertical distribution of NO sources is the same for CG and IC flashes?

Section 3. Both lightning flash rate schemes depend on how well the model predicts cloud top height or ice mass flux. Has there been an attempt to evaluate these parameters (or a proxy) from the chemistry-climate model to a climatology of reanalysis data?

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Section 3.1 has a nice analysis of lightning flash rates in different latitude bands, and remarks upon differences in continental regions. I wonder if an additional figure showing how the model performs for different continents could be included and discussed. For example, showing the annual cycle of North American, South American, African, India and East Asia, and Australia (and maybe tropical oceanic region) lightning should give peaks at different times of the year. This type of figure would be a natural follow on to Figure 1 because the eye is drawn to each of these regions when viewing Figure 1.

L. 252. While both lightning-NO_x schemes show a general underestimate of ozone in the middle and upper troposphere of the tropics, they are both within the variability of the observations (while no lightning-NO_x is outside that variability). In fact, the northern tropics appears to have quite good agreement. If you want to point out the underestimation, restrict the comment to the southern tropics. Second, what is the variability in the model results?

L. 284. I like the conclusion from the analysis that point to April and October as specific months to focus field campaigns. However, aircraft field campaigns can only cover a region (and not a latitude band). Can you recommend where field campaigns should focus? A similar analysis of continental regions would be helpful.

L. 297. I assume that the major Ox production is through oxidation of NO by peroxy radicals. This should be clarified to avoid confusion with NO + O₃ producing NO₂. It is curious that Table 2 discusses production and loss rates of Ox, but burdens of O₃. I assume that is because O₃ is the dominant Ox species (although it is of equal size to NO₂ and O(1D) in Figure 5). It would be good to clarify in this paragraph why you discuss O₃ burdens juxtaposed with the Ox production and loss rates discussion.

L. 303-304. Perhaps the characterization of the ZERO case could be revised. I think it should be described as the following. There is less production of Ox (or O₃) without lightning-NO_x emissions, resulting in a smaller O₃ burden and therefore reduced

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Ox losses and shorter O3 lifetime. Can anything be said about linear or non-linear responses? For example, it seems that the lifetime decrease is less than the Ox loss rate decrease, and both are less than the decrease in Ox production.

Section 4. In addition to the comparison of the ZERO case with the two other cases, there should be a statement pointing out similarities between ICEFLUX and CTH, including the point at the beginning of Section 5.

L. 341-344. I think it would be helpful to the reader to repeat how the NO lightning emissions are placed vertically for each scheme. It is also not clear to me how the horizontal distribution affects the vertical distribution. My interpretation is that ICEFLUX predicts lower lightning-NOx emissions in the tropics based on the storm parameters and more in the extratropics. While the magnitude of NO emissions is less in the ICEFLUX scheme for the tropics, those emissions are still distributed according to the Ott et al. (2010) curves to cloud top height (lines 126-128; and cloud top height should be the same in the two simulations). However, I think the authors are trying to say that the ICEFLUX scheme produces a lot of lightning-NO emissions in storms with lower cloud tops. There is also the point that because the CTH scheme has greater NO emissions in taller clouds, there is a substantial difference in where the NO emissions are found vertically. I think this could easily be supported by a plot of lightning-NO emissions versus cloud top height for different latitude bands.

L. 369-375. Could this be clarified? It was already established that a reduction in Ox production decreased O3 mixing ratios and therefore Ox loss rates (Section 4). However, in these lines it says there is an increase in Ox production in the middle and lower troposphere but a reduction of O3 concentrations, when comparing ICEFLUX and CTH results. Is Ox partitioned differently, meaning there is more HNO3 that can be removed? What loss process dominates (O3 chemical loss or Ox wet deposition)?

L. 405-410. I was surprised that the ICEFLUX lightning flash rate frequency distribution was not discussed. Also, although it is not the point of section 5, I wonder if it would be

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useful to include LIS/OTD frequency distribution in Figure 9.

L. 460. It is an interesting finding that Ox production efficiency is less for higher flash rates (at least initial Ox production). Could the authors speculate why this would happen? Or suggest analysis that could be done in order to explain why. I would imagine the HO₂ and RO₂ abundance might play a role. Are there connections between flash rate and location to VOC sources? For example, Barth et al. (2012) showed more O₃ produced from storms occurring over VOC-rich regions (e.g. southeast U.S.).

L. 465. How did the authors translate the Ox production efficiencies to Ox produced per mole of NO?

L. 477. Here, the authors argue that more Ox is produced by the CTH scheme because NO_x has a longer lifetime at higher altitudes. However, the analysis is for the initial Ox production (“at the time of emission”)? How does the NO_x lifetime affect the Ox production shown in Figure 10, which is “at the time of emission”?

Technical Comments

L. 9 Insert “NO” before emission.

L. 17 Replace “-“ with “;”

L. 16-18 I suggest adding a caveat that more ozone production can subsequently occur from the high flash rate regions.

L. 21-22 Change to “for comparison between models and observations . . .”.

L. 27 NO₂ lifetime may be shorter in the upper troposphere because its photolysis rate is greater. I think it would be better to rewrite the sentence to say NO_x lifetime is longer in the upper troposphere (rather than the individual species).

L. 51 Could a reference be cited supporting that the upper troposphere is the region with most efficient ozone production?

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L. 53 Please delete “simplified”. I find cloud chemistry models to be rather complex.

L. 63-64 It would be better written as, “. . . of low flash rates, which are unrealistic compared to observed flash rates. This results in low NO_x concentrations and greater ozone production efficiency . . .”

L. 86 Please add more information about the chemistry represented in the model. Is it the “standard troposphere” chemistry or does it have the added isoprene chemistry, both described in O’Conner et al. (2014)? I suggest including number of species, stating it describes methane, ethane, and propane (and maybe isoprene) hydrocarbon chemistry.

L. 147-151 Could this be rewritten? It appears that only lightning flash rates are scaled to obtain a global values of 46 fl/s, because the NO production per energy is the same for both cases. Is the energy per flash changed? I suggest rewriting to first address the scaling for the flash rates, including the comment that the scaling factor is very similar to Finney et al (2014). Then discuss the scaling applied to get 5 Tg N per year globally.

L. 164 I think it would be good to include in the text what is said in the caption of Figure 1 regarding the satellite data are regridded to the model grid.

L. 174 The model ozone column is regridded. I assume that it is placed on the same grid as the satellite climatology (which is what in degrees latitude and longitude?). Could the sentence be clarified? “. . . is regridded to the satellite grid of x by y degrees and then compared on this grid. The model ozone column was not sampled the satellite track. (perhaps this last sentence is placed before the previous sentence).

L. 178 Hard to believe Thompson (2003) included data until 2011! It looks like 2011 should be 2000.

L. 179 Perhaps add values of latitudes for the 4 regions.

L. 187 What does “. . . extension of the evaluation over a smaller region . . .” mean? I assume that this paper evaluates lightning over a larger region than what was used by

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Finney et al. (2014).

L. 275 Insert “NO_x” before emissions.

L. 303 Add “in the ZERO simulation” in stating which case has reduced deposition.

L. 305 is not clear. Is not the ZERO simulation corresponding to a reduction of N emissions by definition? That is, it is how the simulation is configured. What is the point of “less than the range of estimates for lightning emissions”?

L. 315 Use “whole” instead of “total” to be consistent with table.

L. 315-319 Why not just say “less than by 13 Tg” instead of “difference of -13 Tg”? I think your meaning may become clearer. Likewise, for the other differences stated in this paragraph.

L. 309-324. Consider revising the construction of this paragraph, which is making the point that location of the emissions (tropics versus extratropics) matters because production of O₃ in the tropical upper troposphere will result in more O₃ transported into the stratosphere. Previous studies found this result, and your results do as well. Implement basic paragraph construction: Topic of paragraph (or point being made), support of this topic, concluding sentence.

L. 326-333 Remind the reader that although the ICEFLUX and CTH simulations were designed to have the same magnitude of lightning flashes and lightning-NO_x production, the location of the lightning and lightning-NO_x differs between simulations, citing Figure 1 or other supporting information.

L. 355 add “by peroxy radicals”.

L. 358-359. Change to “O_x precursors are transported downwind of convection before they form ozone”.

L. 361-363. The last sentence of the paragraph should be the first sentence of the next paragraph.

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L. 473. When the authors say, “at the time of emission”, do they mean within the model time step? In other words, 15% of the Ox production associated with lightning occurs within 20 minutes of the lightning flash (or NO emission)?

Table 1. Add units for RMSE and mean bias.

Table 2. Add information about values in parentheses.

[Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-59, 2016.](#)

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