

## Comments to the Authors:

Review of MS with titled "*Assesing and improving cloud-height based parameterizations of global lightning flash rate, and their impact on lightning-produced NO<sub>x</sub> and tropospheric composition*" by A. Luhar *et al.*, submitted for publication in Atmospheric Chemistry and Physics.

### General remarks:

The paper by Luhar et al proposes a new CTH-based lightning scheme that considerably improves the maritime behaviour of the original CTH parameterization proposed by Price and Rind in 1992 (PR 1992).

There is a relatively clear introduction (**section 1**). Then in **section 2** the authors first describe the chemistry-climate model used followed by a description on how the lightning scheme and the NO per flash were implemented in the model. In **subsections 3.1/3.2/3.3** they describe three previous CTH-based schemes (including the one proposed by Boccippio in 2002 that inspired the authors' new lightning scheme) and the new lightning parameterization proposed (**subsection 3.4**). In **subsection 3.5** the different model runs to be compared in the paper are commented. **Subsection 3.6** is devoted to compare the lightning flash rates derived from four model runs with those from satellite observation. The modelled LNOx, its vertical distribution and verification are described in **section 3.7.1** (global LNOx), **3.7.2** (adopted vertical distribution of LNOx) and **3.7.3** (tropospheric NO<sub>2</sub> verification), respectively. Finally, **section 4** of the article is devoted to comment the impact of the new lightning scheme on some key chemical components of the troposphere with specific subsections for NO<sub>x</sub> (subsection 4.1), O<sub>3</sub> (subsection 4.2), OH (subsection 4.3) and CO (subsection 4.4).

The paper is overall well written but requites some clarifications. The figures need some improvement. In particular, the numbers inside Figures 10, 13, 14 and 17 are not readable and should be larger. Also the number in the vertical and horizontal axes of Figures 5, 6, 10, 11, 13, 14, 15 and 16 are small and not very visible. The numbers in the color bars should also be bigger.

### Some more detailed comments:

#### Section 1

*What do the authors mean in line 5 of page 4 with "... The performace of the PR92 flash-rate parameterizations has not yet been tested properly for their land and ocean components separately" ?.*

There are previous works indicating that the PR92 scheme exhibit large land-ocean biases. This has been already been pointed out by Finney et al 2014, 2016 and others as the author themselves state in the lines 3-5 of page 4. *Please rephrase this sentence or make it clearer.*

#### Section 2

What is the time step of the ACCESS-UKCA model used?.

The authors state that their ACCESS-UKCA setup includes some additional modifications compared to the base UM-UKCA v8.4 model. These changes seem to produce an increase (see line 20 of page 5) in the tropospheric O<sub>3</sub> burden of about 12 %. *Have the authors compared this increased O<sub>3</sub> burden with observations?.*

## Section 2.1

What is the convection scheme used in the ACCESS-UKCA model?. This is important and should be clearly stated since any lightning scheme will be sensitive to the chosen convection parameterization. Please write it in the manuscript for the sake of clarity.

Did you use / implement the spatial calibration factor (c) introduced in PR92 and shown in equation (3)? This is not clearly stated.

The authors should advise readers that the use of the method suggested by Price and Rind GRL 1993 to distinguish between CGs and ICs was only derived considering a number of thunderstorms in the US. However here the authors assume worldwide applicability. The authors should mention the restrictions and assumptions underlying such method. Also, it would good if authors could say something about how the assumptions of the PR93 method could affect the results of the paper.

## Section 2.2

The authors seem to assume that the energy of CG and IC flashes is the same. Is it so?. If yes, please state it clearly and add appropriate citations supporting this assumption (for instance Ridley et al 2005, Ott et al 2010 and / or others. It is also assumed that 330 moles NO / flash is produced independently of whether the flash is CG or IC. Why 330 ?.

In this paper the amount of NO per flash is prescribed to 330 moles NO / flash. What is the underlying reason for choosing 330 moles NO / flash instead of the 310 moles NO / flash concluded by Miyazaki et al 2014?.

By using equation (15) in Price et al JGR 1997 the authors could estimate (assuming the energy per flash) the number of NO molecules produced per joule. This an interesting magnitude to show and it is possible since they have computed the amount of global LNOx (Table 4, equation (21)) and are assuming that the energies of CG and IC flashes are the same, and that approximately 75 % of predicted total flashes per second (Table 1) are CGs while 25 % are ICs.

In line 16 of page 7, the authors comment a little bit how the produced amount of LNOx is vertically distributed. It is mentioned that it is distributed evenly vertically from 500 hPa (approx 6 km) to the cloud top for IC flashes, and from surface to 500 hPa for CG flashes. What is the rationale and / or the physical, chemical and transport reasons (and / or possible observations) for choosing / supporting such vertical distribution ?. This is a bit obscure to me.

## Section 3.1

In line 15 of page 9, it is said "... by substituting Eq. (8) into Eq(13) ...", wouldn't it be the opposite?.

## Section 3.5

According to the authors ACCESS-UKCA was setup as a free running simulation for 2 years (2005 and 2006), and the simulation was started using the model initial conditions taken from a nudged model run (see line 10 in page 13). I think the authors should be a bit more specific on this technical matter. I understand that the nudging somehow guarantees / ensures that the basic dynamics in the lower-middle atmosphere is identical in simulations in which other changes (implementation of a lightning scheme for instance) are made. Is the nudging applied to all altitudes (pressure levels of the model)? Also, it is not clearly indicated whether lightning was included (or

not) in the first free running simulation. Was it?.

I consider that to "see" the influence of **lightning only** in a CTM one should proceed as the following: **First** run your code in a free-running dynamic mode **without** considering lightning. Then run a **second** model simulation **also without lightning** but now using the nudge, that is, the horizontal wind and temperature fields in the tropo-stratosphere are nudged at each model time step of the first free-running dynamic ACCESS-UKCA run. **Then**, what I would do, is to run a third nudged ACCESS-UKCA simulation (**with the lightning scheme on**) that is nudged to the first free-running dynamics ACCESS-UKCA run. **Finally**, I would **repeat the third** simulation for each of your lightning schemes (PR92 and your new one) and **will always compare** their output with the results of the second nudged ACCESS-UKCA runs. In this way you will ensure that you are really carrying out comparisons between simulations of the atmosphere with and without lightning that are not biased by dynamical effects.

It is not completely clear to me if your "nudged model run" is really free of dynamical effects. Please comment on this and try to be more specific.

### Section 3.6

As a remark, by looking at Table 1 I see that the output of RUN 1 (PR92) gives quite low global lightning flash frequency (32.92 flashes / s). Note that the UKCA-UM model was already used by Finney et al 2016 and applied a scaling factor of 1.44 to match PR92 global flash frequency to LIS/OTD observations. In your case the scaling factor would be 1.40.

Could you please explain a bit the underlying reasons for your model runs (including RUN 2) to underpredict in spring in the SH and NH and overpredict in autumn in the SH (see Figures 3 a/b) ?.

Both PR92 and the new lightning scheme proposed fail in accurately describing the tropical oceanic flash rate (see Fig. 4c). There is a considerable overestimation of RUN 1 (PR92) and RUN 2 (new lightning scheme). **What is the reason for this?** This has consequences on the simulation results shown in Fig. 5a (observations) and Fig. 5d (new scheme) where the tropical oceanic overestimated flash rate is apparent. Please comment a bit on this behaviour.

Regarding land, note that North America, the Indian and Australian continents are not very much well described either in RUN 1 and RUN 2 (new scheme). Please give reasons for this.

In commenting the use of scaling factor for flash frequency (line 1 and 2 of page 20) **you should also cite** the works by Tost et al 2007, Finney et al 2016 and Clark et al 2017 (among others) that applied such scaling factors in different models.

Regarding scaling for NO produced per flash, **authors have prescribed** an amount of 330 moles NO/flash which immediately conditions the desired lightning generated NOx (LNOx) as can be clearly seen from equation (21). Any comment on this ?.

The authors are assuming that all lightning flashes produce 330 moles NO / flash (no matter if CG or IC and independently of occurring in land or ocean). **However**, it is known that CG strokes over water usually carry more charge into them which leads to a higher transported current. This is an indication that, on average, CGs over water are more energetic than CGs over land and, consequently, CGs over ocean would produce a larger LNOx (see the paper by Nag and Cummings in GRL 2017). The latter is an indication of different land / ocean convection regimes. This is not considered by any lightning scheme (quantifying the occurrence rate, not the energy). Authors should add comments on these deficiencies so that readers can have a fair perspective of the many

limitations of lightning schemes (any).

## Section 3.7

### Subsection 3.7.1

In my view the lack of scaling flash frequencies (and the fact of using a prescribed  $P_{NO} = 330$  moles NO per flash) artificially magnifies the difference between the PR92 LNOx (4.8 Tg N / yr) and the one resulting from the new lightning scheme (RUN 2) leading to 6.6 Tg N / yr. If authors would have scaled (to match observations) the flash frequencies of each tested lightning scheme (especially the one of PR92), the resulting LNOx of PR92 and TS1 would have been much closer.

In connection with this, I miss a deep discussion on the reasons for selecting 330 moles NO / flash. For example, there are recent papers (not cited by the authors) by Bucsela et al JGR-Atm 2019 and Allen et al JGR-Atm 2019 where, based on OMI + WWLLN observations, find that LNOx can be 180 moles NO / flash +- 100 in midlatitudes summertime NH. Complementarily, the paper by Allen et al 2019 finds that LNOx can range between 70 and 270 moles NO / flash in the tropics.

**I disagree** with the sentence in lines 11-12 of page 22 that the new flash-rate parameterization (Fig. 6b) agrees better with annual LNOx distribution obtained by Miyazaki et al 2014 (Fig. 6c). There are large land geographical regions (North America, Australia, India, EuroAsia) where the predicted LNOx by PR92 and the new scheme are pretty similar and very different with respect the LNOx distribution derived by Miyazaki et al 2014 (Fig. 6c). This is mainly due to the very different flash densities of both PR92 and RUN 2 (Fig. 5c and 5d) compared to observations (Fig. 5a). So, the global flash frequency (and LNOx) could be similar to considered observations (LIS / OTD and Miyazaki's 2014) but, to me, a more demanding comparison would require detailed comparison of flash frequencies (and LNOx) per continental region (North America, South America, Africa, EuroAsia, ...). **Could the authors provide a Table showing such comparison?.**

### Subsection 3.7.2

The vertical distribution of LNOx is crucial. The authors compare their chosen vertical distribution with those of Pickering et al 1998 and Ott et al 2010. The paper adopts an alternative vertical distribution closer to Ott's. However I miss a full discussion explaining / supporting the reasons that moved the authors to use the vertical LNOx distribution (blue dots) shown in Fig. 7.

Please comment and justify your election of vertical distribution. **Do you have supportive observations?. Why do you use these profiles?.**

The relative energy of global ICs with respect to global CGs has consequences and / or conditions the LNOx vertical distributions. For instance, the vertical LNOx introduced by Pickering et al 1998 is consistent with their election of IC flashes being 10 % as energetic as CG flashes. In fact, according to Pickering et al 1998, if global IC flashes contained less than 10 % energy as CG flashes, the upper troposphere (UT) peak (upper part of the "C-shaped" distributions) in the mass profiles might not be as pronounced. Consequently, if ICs are equally energetic as CGs (as authors have assumed) the UT peak would be even more significant and this is not consistent with the vertical distribution used by the authors that rather seems to be a kind of mean between Pickering's and Ott's distribution. **But, again, what are your physical / chemical / transport reasons supporting such profiles?.**

### **Subsection 3.7.3**

While I understand the authors' reasoning for tropospheric NO<sub>2</sub> verification, I do not fully agree with your conclusions of this section.

As I see it, the conflict in your procedure starts in line 6 of page 25. Here you indicate that since N\_v\_trop\_180 is not available from observations, you take the average of the curves (in Figure 8) showing the predicted N\_v\_trop\_180, that is, the mean tropospheric NO<sub>2</sub> column vs latitude resulting from RUN 1 (PR92) and RUN 2 (new scheme) over the reference longitude of 180 degrees in 2006. Doing this somehow "contaminates" the reference, that is, the CAMS data. This "contamination" leads to curves like the ones shown in Fig. 9 where, inevitably, RUN 1 and RUN 2 for global, land and oceanic scenarios are strangely close to the CAMS values (considered as reference).

Do Fig. 9 show total NO<sub>2</sub> columns **or only** the lightning contribution to the zonal annual-mean tropospheric NO<sub>2</sub> column ?. If total, please state it clearly.

I miss comparison of your NO<sub>2</sub> values (shown in Fig. 9) with NO<sub>2</sub> values reported in Bucsela et al 2019 (see Fig. 3(a) there) from OMI + WWLLN observations in northern midlatitude regions.

Please elaborate on this a bit.

## **Section 4 / Impact on chemical tropospheric composition**

Let me start by indicating that in this section I miss a more detailed discussion on explicit chemical reactions and species in the context of the production / loss of the lightning affected species (NO<sub>x</sub>, O<sub>3</sub>, OH and CO) in the different geographical regions. As mentioned in line 32 of section 2, the model includes 306 chemical reactions and 86 species. This chemical set (plus the aerosol chemistry) is quite rich so that key chemical processes could have been pointed out. This is not really done.

Please try to indicate the key processes that, according to the model's reaction set, play the most important role(s) for the formation / loss of each of the species investigated. This is very important and illuminating for the readers.

### **Section 4.1 (NO<sub>x</sub>)**

As said above I think that the comparison between modelled tropospheric NO<sub>2</sub> columns and observations shown in section 3.7.3 is not completely convincing.

Here you compare the total tropospheric NO<sub>2</sub> columns resulting from PR92 and the new lightning scheme and its difference. I think it would have been clearer for readers to show only the corresponding lightning contributions to the tropospheric NO<sub>2</sub> column.

### **Section 4.2 (O<sub>3</sub>)**

Could you please indicate the explicit chemical mechanisms that (according to the adopted chemical set) are controlling the balance of O<sub>3</sub> at 20 m and at 6400 m due to lightning activity ?. What are the key chemical processes controlling ozone population at the two considered reference altitudes?. Are they the same or different?. This is an interesting information not commented in the paper.

Why, according to the authors, the new lightning scheme is not really able to account for the O3 observations in Fig. 12(c) and Fig. 12 (e)?.

### **Section 4.3 (OH)**

Could you please show only the lightning contribution to the total OH tropospheric column ?. It is also important to show readers what are the crucial chemical reactions due to the increase of OH at 20 m and 6400 m.

The authors openly admit that the UKCA StratTop configuration produces an overestimation of OH. It would be interesting for readers if the authors could dig into their chemical scheme and indicate what chemical processes could be playing a role (or could somehow explain) the modelled overestimation.

Please comment.

### **Section 4.4 (CO)**

Are the authors showing in Fig. 17 **the total** annual-mean tropospheric CO or only the one due to lightning ?.

### **Recommendation:**

This paper reports on a improved CTH-based lightning scheme with the maritime lightning flash frequency being more realistic than the one of the PR92 lightning parameterization. The paper could be published in ACP but only after the authors have appropriately answered the questions and comments that I have addressed. There a number of points that need clarification and improvement before this manuscript can be accepted.