Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-885-RC4, 2020

Atmospheric Chemistry and Physics Discussions
Open Access

Interactive comment on "Assessing and improving cloud-height based parameterisations of global lightning flash rate, and their impact on lightning-produced NO_x and tropospheric composition" *by* Ashok K. Luhar et al.

Anonymous Referee #2

Received and published: 2 November 2020

Luhar et al. 2020 implement an alternative lightning flash rate parameterization following Boccippio 2002 in the ACCESS-UKCA global chemistry climate model. The new parameterization is evaluated by comparison to satellite observations of lightning and showed that it yields a better agreement than the default lightning parameterization (PR92). This study then assesses the impact of the new parameterization on the model simulation of NOx, O3, OH and CO. Not surprisingly, the results demonstrate that the relatively small amount of NOx emitted by lightning, leads to a disproportionately large impact on middle-to-upper tropospheric chemistry. Accurate representation of lightning and lighting NOx thus is essential to accurate chemistry and climate models.

I have two major comments. First, a model run utilizing the parameterization following Boccippio 2002 (referred BO02 hereafter) is missing. Table 1 lists out the four parameterizations discussed in this study. The new alternative parameterization proposed in this study is quite similar to BO02. As I read it, the only significant difference is changing the linear coefficient by a factor of 2 for lightning parameterization over ocean (Fo). Sec 3.6 shows that switching from PR 92 to this new parameterization improves model's performance on producing lightning flash rates. However, it's unclear that whether this improvement can be achieved by just switching to BO02 or not. This addition of a model run using BO02 is needed to demonstrate that the modification on BO02 suggested in this study is essential.

Second, in Sec 4.2 and 4.4, the authors incorporated ground-base in-situ observations of O3 and CO and compared the model's simulations against the observations. They concluded that the model using the new parameterization outperforms the one using PR92 and yields better agreements of O3 and CO with in-situ observations. However, the results shown from Figure 12 and Figure 18 are not convincing enough to support the conclusions. The lightning parameterizations only lead to marginal changes in monthly averaged O3 and CO, and these effects are not obviously responsible for reconciling the difference between model and observation.

Overall, this paper appears an incremental improvement but it does offer some new insights and should be published after attention to these comments and the ones below.

Other specific comments:

Page 4 Line 5-6: All cited papers listed above evaluate performances of PR92 over either land or ocean, or both. This statement doesn't hold with respect to all existing studies.

Page 5 Line 4: What's the chemical timestep? I understand the model timestep of 60

Printer-friendly version

Discussion paper

min as too large to solve chemistry properly. But if I am incorrect, some extra words to explain would be helpful.

Page 6 Line 10: The threshold of 5km for cloud thickness looks arbitrary, authors should discuss this in relation to the estimate of lightning flashes.

Page 8 Line 12: Based on the discussion of electrical dipole, cloud thickness seems to be a better parameter representing dipole separation and size of charge centers. It is not obvious to me that it links to cloud-top height.

Page 25 Line 11-13: The argument of better agreement over ocean using model run 2 is not convincing in terms of large uncertainty in the calculation. Nv,trop from CAMS is calculated using the average of the two curves as Nv,trop,180, which leads to overestimate compared to Nv,trop calculated using model run1 (PR92) and underestimate compared to Nv,trop calculated using model run 2 (TS1) over the tropical region. Note in Figure 9b, the column NO2 ($\sim$0.5X1015) within the latitudes $\pm$ 30° is comparable to column NO2 ($\sim$0.3X1015) over the reference longitude shown in Figure 8. The better agreement between model run2 and CAMS shown in Figure 9b may be predominantly attributed to the uncertainty introduced in Nv,trop,180. To make the result more convincing, two model runs should be compared to two CAMS column NO2 datasets calculated using Nv,trop,180 from model run1 and run2, respectively.

Figure 14: It's very hard to conclude that the new parameterizations lead to modelled ozone closer to the observations from this figure. A better visualization is suggested, for instance, set zero to white color, use relative difference plot etc.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-885, 2020.