Geoscientific Model Development Discussions

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Comment on gmd-2022-39

Anonymous Referee #2

Referee comment on "Introducing new lightning schemes into the CHASER (MIROC) chemistry–climate model" by Yanfeng He et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-39-RC2, 2022

Four lightning NO_x production schemes were investigated in the CHASER (MIROC) chemistry climate model: ICEFLUX scheme, the original and modified ECMWF schemes, and the CTH scheme (native to the CHASER model). The model performance was evaluated using OTD (lightning flashes), ATOM (air composition), TROPOMI (NO2 and O3 columns). As the most abundant NOx sources in the mid to upper troposphere, it is important to include LNO_x in global models by implementing and testing LNO_x production schemes. The manuscript is generally well written and clearly presented, but some important pieces were missing or not properly addressed. A major revision is needed before it can be published in GMD for the following reasons:

- In half of the simulations, the global LNO_x emissions were adjusted to 5.0 TgN/yr using different adjustment factors, but no descriptions were given regarding what are the adjustment factors and how was it done? A detailed description is necessary to understand the process.
- The NO emissions per flash were set to 111 moles NO per IC (intro-cloud lightning flash) and 1113 moles NO per CG (cloud-to-ground lightning flash) as parameters drawn from work reported by Price et al. (1997). These values were quite outdated, and there are many publications to update the lightning production efficiency (PE) in recent years. And the number 1113 is considered the upper limit of the reported values and is not realistic. In recent years, the commonly recommended values for CG flashes are in the range of 150-350. Please justify your use of these numbers. At the very least, please discuss the uncertainties that may be caused by using different PE values.
- In the validation of the lightning schemes, the 1996-2000 OTD climatological data was used to compare with the simulations during 2007-2011. As a qualitative measure, it is probably okay to use the climatological data for a broad range comparison even though the time periods were completely separated. However, since the LIS data was available for the simulation years, albeit it was in a narrower latitude range, it should be used to evaluate the model's performance.
- In recent versions of the TROPOMI NO2 column data (http://www.tropomi.eu/data-products/nitrogen-dioxide), there is increase of the tropospheric NO2 columns under some weather conditions. Please provide the detailed version (and level) information of TROPOMI data used in this research and discuss potential uncertainties associated with it.

- The Taylor diagram showing the prediction accuracy of the various lightning schemes doesn't seem to present any significant differences among the schemes. With mean values across the globe and spanning so many years, correlation coefficients of 0.80 and 0.79 probably don't mean much difference. Please provide the significant test and, if possible, a confidential interval would help.
- I would suggest replacing Figures 4 and 5 using LIS data with the same period as your simulation years. The peaks (most lightning occurrences) appeared between +38° and -38° latitude that the LIS had coverage anyway.
- The trend analyses for lightning density (Section 3.4) and NO_x and O_3 columns (Section 3.5) were only based on simulation results and compared to the CTH lightning scheme. Because no observational basis, all the analysis is hypothetical and there is no way to know which scheme or schemes could reproduce (or even close to) the truth. Understandably limitation exists in time and space to employ observation-based analysis, but it is still helpful if limited analysis based on observations could be done. For example, for the lightning trend, the LIS/OTD satellite data could be used from the years when the data was available to derive the climatological trend coupled with the trend of temperature during the period. While for NO_2 and O_3 columns, the OMI/TROPOMI data could be employed to examine the trend over the years.

Specific Comments:

- Page 1 Line 8 and other places, "the most dominant NOx source in the upper troposphere", I would say mid to upper troposphere.
- Page 2, Lines 40-47, not only in global models, the studies of LNO_x in regional scale models have also made significant progress in recent years (e.g. Kang et al. 2019: https://doi.org/10.5194/gmd-12-3071-2019 and https://doi.org/10.5194/gmd-12-4409-2019 and 2020: https://www.nature.com/articles/s41612-020-0108-2), please update the knowledge with references in the discussion.
- Page 2, Lines 51-55, "However, some new … against the LIS/OTD lightning observations". These two sentences are basically stating the same thing, please combine and simplify these sentences.
- Page 2, Lines 57-58, "The two new lightning schemes …", please provide references.
- Page 2, Line 58, "The new lightning schemes must be validated …", the expression "must be" is too strong, consider rewording.
- Page 19, end of Figure 14 caption, the "without nudging" used twice. One of them should be "with nudging".
- Page 21, Line 543 and other locations, "Two new lightning schemes". I would say Three new lightning schemes: the ICEFLUX, two ECMWF schemes (original and modified). It would be more straightforward to me to just say 3 new schemes.
- One comment on the ICEFLUX as it displayed the downward trend of lightning flashes with rising temperature. Since the rising temperature would mean less ICE and less ICE FLUX, that would result in fewer lightning flashes. This probably suggests the intrinsic shortcomings for this scheme to simulate climate lightning trends.