Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2022-39

Anonymous Referee #1

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-RC1, 2022

General comments

This study uses the CHASER chemistry model, along with a number of observational products of lightning and atmospheric chemicals from aircraft and satellites. A number of lightning parametrisations were implemented and compared. The study concludes that several lightning schemes reduced certain biases when compared to the often-used, cloud-top height lightning scheme. 20-year trends were also compared, both with and without nudging to reanalysis meteorology. The show a wide range of trends from negative trends to some that are much higher than found with the cloud-top height lightning scheme.

Overall the paper is well written. It provides a useful demonstration of these different lightning schemes, and explores to a high level of detail how they interact with atmospheric chemistry. Notably, considering how trends in lightning NOx emissions impact trends in column NOx and ozone. There are a few general points that I think need addressed before it is suitable for publication:

- The lightning observations compared to are not for the same time period. I appreciate this is to try and look at near-global observations, but I propose below suitable sensitivity analysis to assess the uncertainty introduced by the inconsistency in time period.
- Whilst differences in correlations/stats can be, and are, described, you should acknowledge where these differences are sufficiently small that they may not be real (e.g. correlations within a few 0.01s of each other). Assess whether it is appropriate to draw conclusions from differences of this magnitude, especially given uncertainties in observations (even if you were using the same time period).
- The modelled results are actually fairly similar in many cases, as such it should be acknowledged that the input variables for some (e.g. cloud-top height) might be better observed and with a better understood response to climate change, than others (e.g. cloud ice). As this may be a more important factor for some modellers, than the small

differences in bias found here.

Specific major comments

L23/section3.4. I don't agree that you can call this "long-term" trend analysis. 20 years is considered to be on the scale of internal variability of climate, and not even as long as the normal averaging period for looking at trends, let alone looking at the trend within that time period. I think it is helpful to show the trends but I would be more refrained in how you describe them. they may indicate the response to warming but cannot be assumed to be the same as the climate response. I would avoid saying "under global warming" and instead say it could be a "short-term response to surface warming" since if there is anything other than decadal variability in the signal, then it is the transient climate response not the equilibrium response. You should note this if you are comparing to other estimates, because they may be looking at the equilibrium response.

L128. I'm not sure where is best to acknowledge it, but I think you need to highlight the issues around using a constant isobar for climate change simulations. As much as it is not an issue for your simulation necessarily, you are trying to relate your results to climate change and if there is significant climate change the same isobar may not be appropriate. In Finney (2018) a different isobar was used for 2100 RCP8.5. A potentially neater solution is to use an isotherm as proposed by Romps (2019). If you have tried this it would be good to include what you found, but if not I think it would be helpful to at least acknowledge the isotherm-alternative as a more flexible approach to applying the ICEFLUX parametrisation.

L245. Whilst I think it is of interest to have compared to OTD, it is a completely different 5 year period. It is quite possible that interdecadal/annual variability would influence results. I strongly recommend checking this by reproducing results within +-45degrees. For this domain you can compare against LIS for the same years, and then against OTD just in that domain. The difference in the statistics compared against OTD, opposed to LIS, will show you how much error may be in your +-75 degree statistics. Alternatively, is there not a full LIS/OTD climatology product, which would mean at least you were using a good number of years (>10years) for the tropics climatology.

Fig14. If both green and red lines use the same LNOx emissions in 2001, how can they have different column NOx in 2001? What is the differences between the models? Spinup? This is particularly concerning in panel h. You need explain these lines begin, and even end up offset, whilst the other panels don't.

North Pacific analysis. I can't see that this is particularly interesting. I would remove. Whilst there are some notable trends in the north pacific, the absolute amount of LNOx emission is small so it's unlikely to have a major effect on column NOx and O3 in this region. As indeed you have found. Your own fig7 shows that none of the schemes result in a big effect from lightning on column NO2 in the north pacific.

L597. I don’t think you can use a review from 2008 (or 2009 in the bibliography!) to say what recent estimates say are expected. Recent estimates (within the last decade of studies) are highly conflicting, and it’s hard to say that any particular trend can be expected. You could say the majority of estimates show a positive trend, and tend average close to 10%/degree, but I would struggle to see a justification for anything stronger.

Specific minor comments

L11. “, also” to “is based on X, and has been…” At the moment it sounds like ICEFLUX is also in ECMWF model but I don’t believe that’s what you mean. And you need to say what the underlying input variables of the ECMWF parametrisation are

L19. “observations” of what?

L30. “the reproductivity of long-term trends of lightning” to “trends of lightning over longer time periods” or something similar

Introduction. I think somewhere (intro or discussion section maybe) it needs to be acknowledged that whilst ice-based parametrisations are appealing they have greater uncertainty associated with inputs, especially with regard to the microsphyics scheme used. This paper can provide a good basis for that https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021JD035461?casa_token=XPjZ7-BBFZEAAAAA%3AfbdQYuwknvDDXqNO5TJGvAEiUNFpzLMnbt2VnnKcHMC4QXtaq1hUzOs3-PYNAOZvSowW3J3vLuvB

L50. I think it would be worth referencing this paper, regarding what chemistry models are currently doing. It is much more up to date (though no updates to lightning schemes) and it includes some nice looks at the chemistry/radiative impacts of lightning in models. https://acp.copernicus.org/articles/21/1105/2021/

L57. As in abstract you don’t say what the input variables are for the lopez scheme.

L76. A decrease for convective mass flux scheme was also found for CMAM model in Finney (2016a). Not necessary to include but would demonstrate it wasn’t only one model in which this has been seen.

L72. You refer to a USA specific result but actually the tropics is where the dominant LNOx production occurs. The following paper considers a cloud ice based parametrisation for lightning of Africa and finds a relatively small response of lightning to climate change over the continent https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020GL088163

L139. I don't see that Q_R is a "flux" as there's not per second in there. Please include the units. I think its kg^2/kgm^2? A bit unusual so not sure how to describe it. You might almost have a flux with Q_R*sqrt(CAPE) as the sqrt of CAPE has units of m/s. But still not sure it works out so I suggest coming up with more precise terminology for what Q_R is.

L140. Can you give a reason for the "1.8" in the min function? What are the units of that? Km?

L170. Eq13 is quite different to Eq8, and changes the meaning of this variable quite a bit. For the better potentially – I can at least see the logic behind it more clearly. I'm not sure I'd call this a modification of the ECMWF param, I'd say it's probably different enough to have its own identity.

Table 1. Please review your use of "frozen precipitation convective flux". I do not think it makes sense. Stick to the variables present in the main equation for the parametrisation. ECMWF-mod (Eq 11/12) is based on column precipitating ice and CAPE so I think you should only have two bullet points. I also don't think it's a appropriate term for the ECMWF-original inputs but as per L139 comment I'm not really sure what you'd call that.

L186. What is the C-shaped scheme, Pickering et al.? reference?

Table 2. Are experiments 3-6 actually different to experiment 3 or have you just selected out the relevant years? If they are different you need to say how, if not then it seems overkill to call them separate experiements.

Section 2.3.2. I presume you have sampled the specific flight track and timings from the modelled data, for comparison to observations? Can you explicitly state that.

L238. "profile as per Ott(2010)". Add reference and suggest just get rid of L186-190 which is confusing as it sounded like you were suggesting you'd explored both profiles. If you have then it would be good to know what you found.

Fig3. Showing separate diagrams for full global, ocean-only and land-only could draw out your points about the differences. All schemes, currently lie quite close to each other.

L365. Is this model bias the total global model bias? Or just over the regions in fig9? Is it on an annual or monthly basis?

L370-379 and L548. The differences between these correlation coefficients are generally quite small, and the differences probably not significant. I would go easy on firm conclusions here and at least acknowledge that the correlations are actually very similar (this is something you should consider throughout when referring to differences in correlations).

L437. The majority of these references are looking at the equilibrium response to warming. Clark had a transient simulation but I suspect averaged over a number of decades to show the trend. You are looking at variations over a much smaller timescale. It is interesting to see, but it is comparing apples with oranges to directly compare these. You should acknowledge that they are measuring different kinds of response, over different timescales. You would be better going back to the Williams paper that looks at temperature response on shorter timescales, and relating your results to that… https://ui.adsabs.harvard.edu/abs/2005AtmRe..76..272W/abstract

Fig12. Worth noting in caption why ICEFLUX doesn't have shading towards the poles?

Technical corrections

L9. "improve" to "improving"

L65. "CPAE" to "CAPE"

Fig6, table3. Say clearly in the caption that these are looking at NO. Currently it doesn't state.

Fig 10. State in the caption that these are relative to the CTH simulation.

Fig11. Please say what the black blue and red lines are in the caption.

Fig14. Is this accessible to people with red green colour blindness?