Rebuttal to Rewiever 1

We thank the reviewer for the time spent in reviewing this article and for the provided comments. In this document, we respond to all the points in blue text.

This study runs a number of simulations with a chemistry-climate model (with several of those in a chemistry-transport like setup, where the meteorology is independent of the chemistry simulation). Three lightning parametrisations are used to explore the sensitivity of results to that choice. The primary focus of the study is on the implementation of a LNOx emission per flash that is dependent on the flash rate, normally it is constant. The relationship is based on a previous study using satellite data over the midlatitudes that found an inverse relationship between flash rate and LNOx emission per flash. The authors report that the NOx concentration reduces in some regions typically higher in LNOx emissions, such as upper troposphere, and increases in regions with typically lower LNOx emissions, such as the lower and mid troposphere. They also report a number of other effects on atmospheric composition.

The Bucsela et al. (2019) finding of an inverse relationship between flash rate and LNOx per flash is an interesting one, and warrants investigation of the effects within lightning chemistry parametrisations. It is good that the authors have investigated this, and the results can provide a useful reference for all atmospheric chemistry modellers.

We thank the reviewer for these encouraging comments.

I have a few issues with the work as it stands and the authors would need to make changes for me to feel like this was ready to publish. I particularly note that in some cases I wonder whether the small changes described are actually insignificant, and therefore null results. The authors are not clear on this point, but I would encourage them to be, and I would encourage the editor to publish (once other comments have been addressed) whether there are significant or insignificant results. For this study, null results are as useful as significant results.

We have included a discussion of the relevance of the reported results in the new version of the manuscript. In particular, we have added a comparison between the obtained variation in the tropospheric ozone and the obtained interannual variation of the Tier 4.1 product. In addition, we have added a discussion on the significance of the obtained variations in the methane lifetime.

Major comments

Use of Bucsela et al. (2019) - There is not sufficient acknowledgement and discussion of the focus of Bucsela study over mid-laittudes, whilst you are applying it over the whole globe. Please add more text discussing the potential issues with this.

Allen et al. (2019) also reported "evidence for a decrease in PE with increasing flash rate on a regional basis" within the tropics, including the oceans. Although this reference was already cited in the manuscript, we have highlighted its importance in the introduction and discussion sections.

Description of different parametrisations – you use 3 parametrisations. They are reasonable choices, but you are not describing them sufficiently. I know there are references, but at the very least say what the input variables are (e.g. updraught mass flux for the Grewe). Please include some text to elaborate on that.

We have added the input variables in Table 1 and the simulated annual flash density in Figure S1. The parameterizations were described, implemented into EMAC and compared with OTD/LIS observations by Pérez-Invernón et al. (2022). For more details, we refer to this publication in order to not lengthen this manuscript.

Description of NOx per flash parametrisation – Given its new and the focus of this study, I'm amazed you have not included at least the equation, if not a plot, of the LNOx per flash equation you have used. I appreciate the plot is in Bucsela, but you should at least include in the methods, your implemented equation. If other modellers want to implement this, they should quickly and easily be able to apply the same parameters and form that you have used.

We have included in the revised manuscript how we derive this relationship.

L72 – How do you do this on a 1x1 deg grid when the model is simulated at a coarser resolution? Also, what allowances do you make for grid cell area varying with latitude as this would vary the flash rate purely because of an area change (do you actually use flash rate density in some way?)

In the model, we calculate both flash rate (flashes per second) and flash rate density in every cell. We have included in the revised manuscript how we use the relationship derived for 1x1 degree resolution into our model.

L130-132 – It is not obvious to me which spatial map is best. I suggest you refer to particular features that have made you reach this conclusion. It is awkward that I had to look at another paper to corroborate your conclusion, given that it seems a pretty key bit of evaluation – is it not possible to have a figure in the introduction that reproduces relevant panels from Bucsela2019? Then you would be able to refer the reader to it throughout your paper, instead of drawing key conclusions based on material not in your manuscript.

We refer to Bucsela et al., (2019) for a map with the observations. We consider this manuscript is already too long and we prefer not adding more figures if not necessary. However, we have extended the discussion on the comparison between simulations and observations. In particular, we have highlighted that the agreement is better over ocean.

Fig1 – Although I'm loathed to say someone should use a rainbow-based scale (as Bucsela has), in this case, it would help the reader compare your results to their figure if you used the same colourmap.

We have used the rainbow-based scale in the revised manuscript.

L236 – It's not obvious to me if any of the methane lifetime changes are significant. This is a general issue throughout the paper that the authors quote small changes without testing significance. I suggest this should be done for results the authors consider most key (I would say methane lifetime is one of those). Null results are fine and useful so please just be clear on that.

We have included a discussion about the significance of the differences in the methane lifetime: "When using the mean and standard deviation as metrics to evaluate the significance of differences in methane lifetime, the results indicate that the differences between CTR and LNOfs are significant in the P and G simulations. In contrast, no significant differences are observed in the L simulations."

In addition, please note that the numbers of Table 2 have slightly changed in the revised version of the manuscript. During the revision, we identified a minor error in the calculation of the means that excluded the months of December from the calculation.

Fig14 – include a plot of the observations in the figure so the necessary material for your conclusion is here.

We refer to Jöckel et al., (2016, Fig. 29) for a map with the observations. We consider this manuscript is already too long and we prefer not adding more figures if not necessary.

Figs10-13 – Broadly the biases are not affected by the new scheme. Have you checked if the temporal correlation is? It is not easy to tell by looking at different plots of each season. You could make an equivalent zonal plot of temporal correlation between the model and obs, and then panels with differences in correlation for your different schemes. It would be interesting to know if there was any significant improvements.

We have added a comparison between the obtained variation in the tropospheric ozone and the obtained interannual variation of the Tier 4.1 product in Section 3.3. This comparison shows that the obtained variations are spatially different than the interannual variations of the tropospheric ozone.

Minor comments

Title – I find the title is not precise enough for the novelty of this work to be clear. I would say that all lightning NOx parametrisations are based on lightning frequency in that the more lightning there is the more NOx. It is specifically the per flash parameter that you are varying and which is novel. It's hard to think how to frame this in such a way as to be precise but also meaningful without detailed explanation. Maybe something like "...composition to applying an inverse relationship of NOx emission per lightning flash"?

We changed the title accordingly.

L39-49 – There is a lot of text on Lightning and ozone here that is not obviously useful. It mainly seems to be saying lightning affects ozone but different schemes introduce different biases when simulating it. I think that can be said in a couple of sentences. If there's something in here relevant to your results then I think it would make more sense for the reader for it to come in a discussion section.

We consider this information useful. In particular, we have found that the new parameterization of LNO_x based on flash frequency affects the L parameterizations less than others because the L parameterization already includes a modification over the oceans.

L82 – Are there any scaling factors applied to the different paramtrisations (as discussed extensively in Tost et al (2007)? If so list them here.

The scaling factors have been included in Table 1.

Table2 – It would be quicker for the reader to take this in if it were a figure with three line plots.

Done. See new Figure 1.

L103 – why is only the Luhar percentile result mentioned?

We have mentioned other percentiles.

Throughout - “Injection of LNOx” terminology is not something I’ve seen much. It seems strange because it is not coming from outside the atmosphere, and therefore is not injected. It is a result of reactions within the atmopshere. Most commonly, I see it referred to as LNOx “emissions”. Or the term “production” seems most precise to me.

We agree with the reviewer. We have changed “injection” by “emissions” and “production” accordingly.

Sec3.3 - Why are the Luhar results are not shown, or at least discussed, along with the other parametrisation results? Up to this point, I thought there was a sense that it was the better parametrisation, though I’m not sure. At least explain to the reader in the text, if and why you are deciding to focus on certain results.

We considered that including zonal and seasonal analysis of the differences between the simulations CTR_L and LNOfs_L would significantly lengthen the manuscript without providing extra information. The reason is that the differences are qualitatively similar to the differences between the LNOfs_P and CTR_P simulations, but smaller in absolute numbers. We have explained this in the revised version of the manuscript.

L125 - “active” to “intense”? (normally I’d think of active as related to frequency of events, but I don’t think that’s what you mean. You mean few events that are more intense, I think).

Changed.

L129 - “the largest amount” I don’t think you mean. You mean “relatively more”.

Changed.

Fig10 – It is not a white “line” but a white “region”, that shows the straosphere.

Changed.

Technical comments

L33 - “rate” to “rates”

Corrected.

L49 – I suggest yo umight want a new paragraph at “Previous studies...”

Done.

L71 and throughout - “bucsela2019midlatitude” citation typo

Corrected.

L125 - “sparsed” to “sparse”

Corrected.

L155 - “lead” to “leads”

Corrected.