REPLY TO REVIEWERS

Dear editor and reviewers:

Thanks for your time and comments for our manuscript entitled "**Contributions of lightning to long-term trends and inter-annual variability in global atmospheric chemistry constrained by Schumann Resonance observations**" (Manuscript Number: EGUSPHERE-2025-370). The manuscript has certainly benefited from these insightful revision suggestions. Below we provide point-wise response to reviewers′ comments. In the manuscript, revised or newly added sentences are highlighted in yellow color which relates to the reviewers′ comments.

[General comment]

In this manuscript, Schumann resonance observations are used to create a scaling factor that further constrains the typical GEOS-Chem lightning parameterization based on cloud-top heights and the climatology of satellite observations of lightning. The trends and variability in global lightning activity, LNO_x emissions, O_3, and OH are then compared between GEOS-chem model runs with the Schumann resonance constraint, GEOS-chem model runs without the Schumann resonance constraint, satellite lightning observations, and ground-based lightning observations. Overall, the paper is well organized, and the incorporation of Schumann resonance data is a novel approach to modeling lightning and lightning chemistry. However, there are a few questions I would like to see addressed before I recommend publication.

Reply:

We thank the reviewer for constructive suggestions!

[Main comment 1]

Line 91 states: "…and manually exclude the disturbed days (usually manmade activity) from the dataset." Could you provide more information about the disturbed days, e.g. How do you know when a day is disturbed? What are the

causes of disturbed days (i.e., what specific manmade activities lead to disturbances)?

Reply:

Thanks for your question. We have modified the sentence as follows:

Following Bozóki et al. (2021), we apply the weighted average method to obtain the intensities of the first three SR modes, and manually exclude the disturbed days from the dataset. Such disturbed days are either of natural (e.g., local lightning activity, see Tatsis et al., 2021) or manmade (e.g., nearby human activity, see Tritakis et al., 2021) origin. The removal of these data is based on the evidence that the diurnal variation of SR intensities is usually very similar within a given month (Bozóki et al., 2021; Sátori 1996). Therefore, days with unusual diurnal variation are considered to be disturbed and are removed from the dataset during the manual sanitation process.

New references:

Sátori, G. (1996). Monitoring schumann resonances-11. Daily and seasonal frequency variations, *J Atmos Terr Phys*, *58*(13), 1483-1488, doi:10.1016/0021-9169(95)00146-8.

Tatsis, G., et al. (2021). Correlation of local lightning activity with extra low frequency detector for Schumann Resonance measurements, *Science of The Total Environment*, 787, doi:10.1016/j.scitotenv.2021.147671.

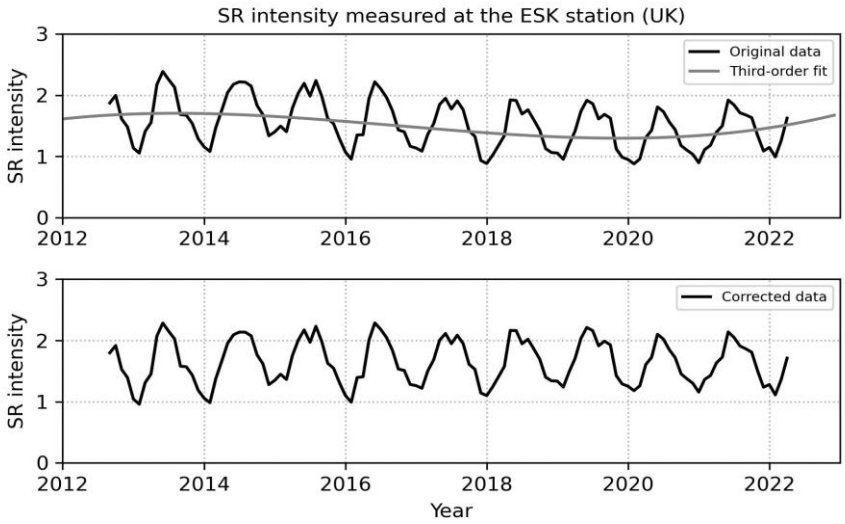
Tritakis, V., I. Contopoulos, C. Florios, G. Tatsis, V. Christofilakis, G. Baldoumas, and C. Repapis (2021). Anthropogenic Noise and Its Footprint on ELF Schumann Resonance Recordings, *Frontiers in Earth Science*, 9, doi:10.3389/feart.2021.646277.

[Main comment 2]

Line 94: How does applying a third-order polynomial fit on the data remove the influence of solar cycle variations?

Reply:

Thanks for your question. The following figure demonstrates how this process works:



The solar cycle appears as a slowly varying trend in the measured SR intensities, which is not related to lightning but to the changing propagation conditions of ELF waves (see Bozóki et al., 2021). This trend is successfully removed by the applied third-order polynomial fit.

[Main comment 3]

Figures 4, S1, S2: Could you explain a little more how the correlation coefficients in these figures are determined? For example, in Fig. 4a, how is the OP lightning contribution positively correlated with the non-lightning contribution if the OP lightning LTT is positive and the non-lightning LTT is negative?

Reply:

Thanks for your question. We now clarify in the captions that the correlation coefficients are computed between annual anomalies of lightning and non-lightning contributions, after their respective LTTs are removed.

[Main comment 4]

Line 217: "The sharper decrease predicted by the SR simulation is in better agreement with the trends of tropospheric NO_2 columns from satellite observations during the period (Fig. 5a-b)." This interpretation does not seem consistent with the Figure 5 for the following reasons:

- **Looking at Figure 5, the NO_2 anomalies determined from the SR method**

appears much more similar to those from the OP method than to the OMI observations. For example, both the OP and SR results underestimate the 2015-2016 NO_2 anomalies and overestimate the anomalies in 2018-2019 by about the same amounts relative to the OMI data. Thus the SR simulation does not substantially improved the agreement with the OMI data compared to the OP simulation. Are the differences between the OP and SR trends (-5.4% [SR] vs. -3.8% [OP] for global and -10% [SR] vs. -8.3% [OP] for the Northern Hemisphere) significant?

- **It is true the magnitude of the SR trend (-7.0) is closer to the OMI trend (-7.1) than to the OP trend (-5.4) in Figure 5a, but the OMI trend is not significant, while the OP and SR trends are both significant. Again, this result suggests that the SR results are not really capturing the OMI trend any better than the OP results.**

Perhaps the conclusion is that some other factor besides lightning is the reason for the differences between the model results and satellite observations for the NO_2 anomalies?

Reply:

Thanks for your valuable comment. We agree with the reviewer that the annual anomalies of NO_2 are heavily affected by non-lightning factors. The mismatch between simulated and observed annual anomalies are not corrected by our update of lightning parameterization. We now clarify in the text "The sharper decrease predicted by the SR simulation is in better agreement with the trends of tropospheric NO_2 columns from satellite observations during the period (Fig. 5a-b), though the simulation still does not fully capture the inter-annual anomalies possibly due to errors in non-lightning factors."

In this figure, however, we intend to focus on the comparison of long-term trends, which are improved by the SR simulation. To guide the readers, we now add trend lines of the annual data in Fig. 5. We also denote significant ($P < 0.05$) and moderately significant ($0.05 \leq P < 0.1$). In the case of global OMI NO_2 trend, it falls into the category of "moderately significant". The observed trend is less significant compared to

simulations because of its larger inter-annual variability, which, as discussed above, is likely due to non-lightning factors that are not captured by the model simulation.

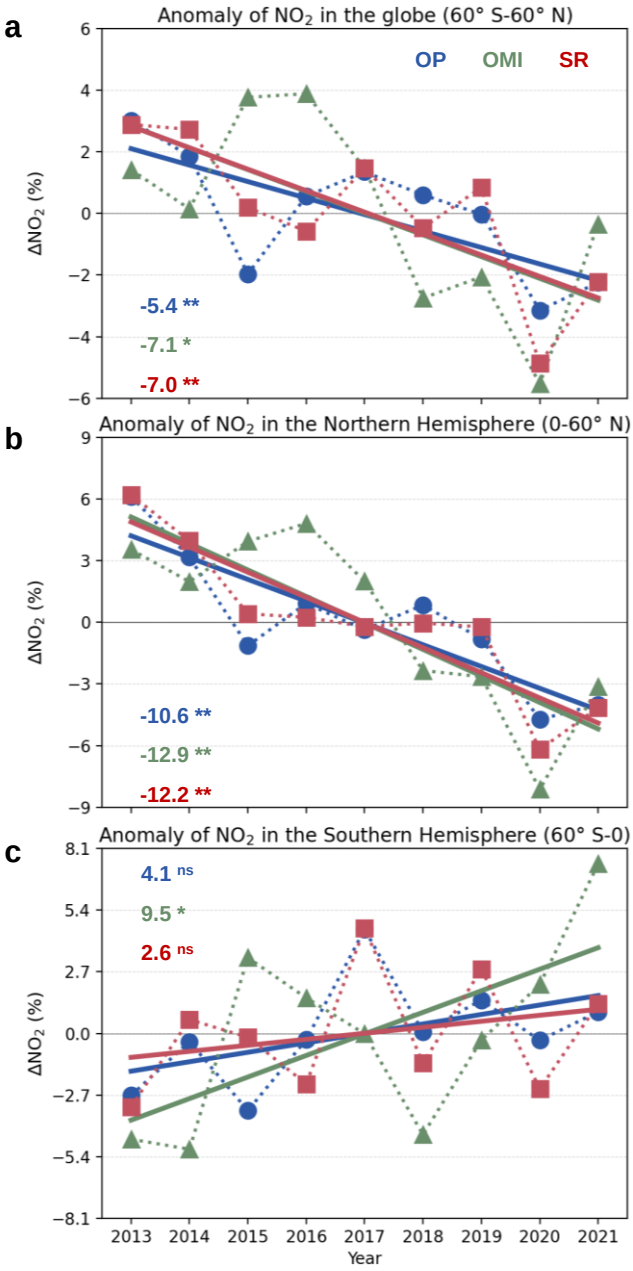


Figure 5: The anomaly and linear trend of tropospheric NO_2 vertical column densities averaged over the globe (a), Northern Hemisphere (b) and Southern Hemisphere (c) during 2013-2021 for OP and SR simulations and OMI satellite observations. The trends are shown in the unit of % dec^{-1}. ** denotes significant trends ($P < 0.05$), * moderately significant trends ($0.05 \leq P < 0.1$) and ^{ns} insignificant trends. Following Qu et al. (2021), we filter the data using quality flags reported in OMI data product. We exclude data affected by the row anomalies and retain those with cloud fraction < 0.2, surface albedo < 0.3, solar zenith angle < 75°, and view zenith angle < 65°.

[Main comment 5]

Fig. 7a: What is causing the positive change in NO_2 anomaly for the OP method? According to Fig. S3a, LNO_x determined from the OP method also decreases from 2019 to 2020, just not as much as the SR method LNO_x. So it is a little surprising to see a positive NO_2 anomaly from the OP method in Figure 7a.

Reply:

Thanks for your question. Wildfire emissions contributed ~50% of the positive change in NO_2 anomaly for the OP method, with meteorological factors explaining an additional ~30% of the variation.

[Main comment 6]

Throughout the paper the trends are often described as decadal or the changes put in terms of % per decade, but the study range only covers 8 years of data, so describing the trends as decadal (occurring over 10 years) seems not quite right. Perhaps "overall" trend or similar wording would be more appropriate?

Reply:

We now use LTT or trends throughout the text. We clarify that we report the trends in % dec^{-1}.

[Technical Comment 1]

Line 54: "…lightning observations from satellite-based the Lightning Imaging Sensors (LIS) and Optical Transient Detector (OTD)…". The words "the" and "satellite-based" should be switched: "…lightning observations from the satellite-based Lightning Imaging Sensors (LIS) and Optical Transient Detector (OTD)…".

Reply:

Thanks for your kind suggestion. The corresponding revision has been provided as follows:

This limitation is improved by incorporating lightning observations from the satellite-based Lightning Imaging Sensors (LIS) and Optical Transient Detector (OTD) to constrain the distribution of LNO_x emission provided by the parameterization (Murray et al., 2012).

[Technical Comment 2]

Line 87: "Geoagnetic" should be "Geomagnetic" ("m" is missing).

Reply:

Thanks for your valuable suggestion. This mistake has been corrected as follows:

We use SR measurements at the Eskdalemuir Geomagnetic Observatory (ESK) in the UK operated by the British Geological Survey (Beggan and Musur, 2018; Musur and Beggan, 2019) between 2013 and 2021.

[Technical Comment 3]

Both Line 192 and Line 257 use similar phrasing that is difficult to follow: "occurs in a decrease" and "occurs in a huge decline". Simplifying the wording here would make these sentences flow better, e.g. "…global lightning activity decreased by ~10% from 2019 to 2020 (Fig. 2a)." and "…global lightning declined by ~10% from 2019 to 2020."

Reply:

Thank you for pointing this out. Following the suggestion, we have revised the corresponding expressions:

Lines 197–198: Noteworthily, SR and ISS LIS observations conformably show that global lightning activity decreased by ~10% from 2019 to 2020 (Fig. 2a).

Line 266: As mentioned in Section 3.1, global lightning declined by ~10% from 2019 to 2020.

[Technical Comment 4]

Supplement: There is a typo in first sentence of Text S1: "mothed"- maybe should be method?

Reply:

Thanks for your kind comment. It has been corrected as follows:

We evaluate the performance of SR observation method with the space-based tropospheric monitoring instrument (TROPOMI) NO_2 observations by cloud-slicing

technique (Figures S3a-d and S4a-b).