We appreciate the reviewer for the time spent revising this manuscript and for providing valuable comments. We have addressed the reviewer's comments below (in blue font).

In addition, we want to point out that we have re-run the chemical simulations because we detected a small error in the code implementation related to the vertical profile of NO_2 and N_2O injection. Despite this change, the results obtained are very similar.

Reviewer 2

The paper presents new and interesting results on the global chemical effects of sprites. It is well written and focused. It addresses relevant scientific questions within the scope of ACP, and deserves to be published. For the final revision, I'd like to ask the authors to take into consideration these points:

We thank the reviewer for his/her encouraging comments.

Line 132: Please comment on the ratio of LCC-flashes to all flashes. How does your 20% relate to the 1/1000 sprite-to-flash estimate by Arnone et al. (2014)?

We (Pérez-Invernón et al., 2022) reported that among $3.5x10^6$ flashes recorded by ISS-LIS over one year, only $2.6x10^4$ flashes have a continuing current lasting more than 18 ms (about 7.4/1000 flashes). We set that only 20% can produce sprites (1.5/1000). Finally, we set that only flashes taking place during nighttime can produce sprites. Therefore, our 20% relates quite well with the 1/1000 sprite-to-flash estimate by Arnone et al. (2014).

Added:

"Arnone et al. (2014) estimated that about 1/1,000 flashes could produce a sprite, while Pérez-Invernón et al. (2021) found that 7.4/1,000 flashes reported by LIS during one year have a continuing current lasting more than 18 ms. Therefore, the approximation of 1 sprites per 20% nighttime LCC(>18 ms) lightning flash is of the same order as the 1/1,000 sprite-to-flash estimate by Arnone et al. (2014)."

Table 1 states 'SPRI-M … HOx by Malagón-Romero et al (2023)' Are there HOx production estimates in that paper? If so, please give numbers like for the cases Yamada et al. / Winkler et al.. If not, please correct the reference.

No, there were not HO_x production estimates in that paper. We have changed the reference to Winkler et al. (2021).

Line 144: As far as I see, that wasn't an 'electrodynamic' model?

We agree, because this model is 0D. We have removed "electrodynamic".

Line 154: After 'single sprite' add 'streamer'.

Done.

Section 3.2: Please compare your results to the model results of Arnone et al. (2014).

Added:

“We obtained a marginal increase of approximately 0.007% in the background concentration of NO_x at an altitude of 70 km. This increment is notably lower than the perturbation estimated by Arnone et al. (2014) due to sprites, which falls within the range of 2% to 20%. The variance in results can be attributed to the disparity in assumptions made by Arnone et al. (2014), who considered an injection of NO_x molecules ranging from 1.5×10^{23} to 1.5×10^{24}. In contrast, our study assumes a more conservative injection of 6.46×10^{22} NO_x molecules. In addition, the sprite-NO_x perturbation profile in this study is linear between the altitudes 45 km and 80 km, while the profile adopted by Arnone et al. (2014) peaks at about 65 km altitude.”

Figure 6 caption: ‘globally averaged’ might be wrong.

Done.