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Comment on acp-2020-1228

Anonymous Referee #2

Referee comment on "Model simulations of chemical effects of sprites in relation with observed HO_2 enhancements over sprite-producing thunderstorms" by Holger Winkler et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1228-RC2, 2021

Referee report for manuscript "Model simulations of chemical effects of sprites in relation with satellite observations" by Holger Winkler et al.

Very recently, Yamada et al. (2020) reported first-time observations of mesospheric HO2 enhancements in regions of proven sprite activity. The manuscript by Holger Winkler and colleagues is a timely contribution to give a model interpretation of these observations. It is a very detailed model study of HO2 changes related to sprites and includes a much-needed modeling of the dispersion of the air masses affected by the perturbing events, therefore bridging between sprite-streamer chemistry predictions and air masses actually sounded by the satellite. The observations with Winkler et al.'s interpretation could in principle give a constraint to the several models developed over the past 2 decades on sprite chemistry, a source that is as yet poorly constrained and of interest to the broader atmospheric community. I think the study is well developed and discussed, mostly well written and with high quality figures. There are some improvements that could be applied and I invite the authors to consider the following comments before acceptance for publication in ACP.

GENERAL COMMENTS

The main finding of the study is that modelled sprite HO2 cannot explain what sounded by the SMILES instrument, unless an unrealistic number of sprites were contributing. The difference between model and observations is of 3 to 4 orders of magnitudes. Given that typically one expects a few tens of sprites over a thunderstorm (in a relatively compact volume since it is sounded by one SMILES measurement), a few orders of magnitude difference persists. I miss a thorough discussion of what factors are at play in the model that limit the HO2 production. Several factors are then cited as possible shortages, although there was no quantitative analysis of what parts of the study could lead to order of magnitude increases. I think such a detailed study could really give guidelines on where the discrepancies are to be found.

In Yamada et al., Fig 2, there seems to be a tiny decrease in ozone consistent among the three cases. Even though very limited, would a decrease be consistent with model predictions? Is this the only further species detected by SMILES? It would be of great help to look also at other species, which may help to better relate observations and model predictions.

The observational uncertainties are only shortly introduced in the table. I think there is a need to further explore these uncertainties to help reconciling observations and model predictions. How are the observing geometries affecting Yamada et al. estimates? Could there be a contamination of the HO2 spectral features? How is the sprite HO2 production further diluted in the large volumes sounded by the instrument along its lines of sight? Furthermore, the transport study shows that only fractions of the airmasses affected by the sprites are sounded, but no quantitative consideration is made of its further dilution effect. Are these expanded/transported airmasses consistent also with a multiple-sprite scenario? The apparent dilution along the line of sight should be considered also in this case.

DETAILS

Title: I would find the title more attractive if it represented better the focus on HO2

L41: " These are the first direct observations of chemical sprite effects”. I would be more careful with such statements. Yamada et al. were the first observations of HO2 enhancements in regions of proven sprite activity, not direct measurements of chemical changes through a sprite. The lack of consistency between model and observations seem to further require this caution.

L44: A few words of comments would be helpful on the decrease predicted for HO2 by Hiraki et al. 2008. Isn’t this relevant to Yamada et al. observations? Yamada et al. reported observations up to 80 km altitude so some cases would see a reduction of HO2 whereas the other cases an increase?

L45: Yamada et al. 2020 already presented model predictions but these are not mentioned here in the introduction. Why? It should be clarified whether the model and simulations presented in this manuscript are different (and how) from those presented in Yamada et al.

L60 and around. The observational results are affected by uncertainties, which are only reported in table 1 and not presented in the paragraph. Because of the discrepancies found between model and observations, I would find it useful to anticipate here a detailed description of all possible sources of these discrepancies. For example, limb sounding measurement is affected by spread of information along the line of sight. How large is this spread? How are the averaging kernels? 3-400 km as for other instruments? What is the pointing error? How accurate is the geolocation? It is mentioned that Yamada et al. estimated advection of a few 100 km. In what direction?

L69 it's - -> its

L94 data from SABER are used as climatological background. Are there no other measurements directly from SMILES? Please add a comment.

L120 the impact of changing vertical transport speed in the model_JPL estimates is very large. H2O at 80 km altitude (i.e. one of the case studies) changes from 1.5 to 4 ppmv. Large differences are found as stated/shown also in ozone and atomic hydrogen. It may be difficult for the reader to understand here and in the following whether these large discrepancies have an impact or not. I assume that water abundance is so large that these starting differences have no impact, so I would anticipate it here.

L140 MLS data points were averaged over a very large region. It seems therefore appropriate to give an estimate of the variability of these measurements. Since this works attempts to describe conditions found in the three case studies, it is essential to understand the range of background conditions that could be reasonably found and how these impact on the results: therefore, the scatter should be considered, both due to measurement errors and actual natural variability.

L141 Section 5. This section is very rich and the full description with no breaks become very difficult to follow. I recommend introducing subsections or an alternative approach to split the flow into a few blocks to help the reader to quickly understand the main points.

L220 and following. How this compares to the findings by Hiraki 2008? Were there similar mechanisms linked to the changes at 80 and 70 km altitude?

L240 Since there is such a stringent constrain on the timing of the SMILES measurement and previous sprite activity, an analysis of lightning activity of the three thunderstorms would be very helpful. Can we reasonably expect sprites in the few hours prior to the SMILES passage? This is mentioned in L274-281 but only qualitatively. Given the relevance of this point a quantitative estimate should be considered.

L241-245 SMILES cases A and C had tangent points at 75 and 80 km altitude. Why mentioning only the 77 km one? The discussion continues focusing on case B. It would be useful to clarify this and add a comment on the other cases studies.

L245 I would split section in subsections for example here.

L248 Is the 850 m diameter consisting of a volume completely filled by an individual streamer channel or simply be a volume with a variety of branches of different scales? Would this change the estimates that follow? I would specify this is the text.

L252 This is a clever approach. How robust and variable are these estimates? Are photons from the internal parts of the sprite expected to escape undisturbed or should one expect an onion-like shielding effect? Can this increase the amount of excess HO2 molecules? Is this approach better than that used by Arnone et al. 2014 that was cited? They used the current moment, shouldn't the two approaches lead to consistent estimates? This is a key point in this study and I feel it should be better explored in its limitations and giving a possible range of the adopted estimates.

L263 There is no mention of the direction along the line of sight. The signal is being integrating over likely a few hundred km (please give robust estimates for this), so that a further important dilution of the predicted sprite HO2 enhancement occurs (likely of the order of 30 km / 300 km, which is a factor 1/10). This decreases the amount of enhancement that SMILES would have seen due to a single sprite. I think this is an important point that was missed and should be quantified.

L266-273 there is little effort in estimating how and by how much a larger HO2 enhancement could be obtained. I think the three points that were identified "missing chemical processes considered by the streamer model, inaccurate electric field parameters or reaction rate coefficients" should be further investigated giving quantitative estimates. For example, the very interesting approach of multiple models shows that the different rate coefficients considered have no significant impact (only a change in the first couple of hours).

L277-280 Here the 3 cases are recalled, although no mention is made of case C at 80 km tangent altitude. In Yamada et al. 2020, also case C shows clear enhancements of HO2, how would this be possible given the negligible predicted HO2 production?

L282 The authors discuss the possibility that a large number of sprites contributed to the observed HO2 enhancement. This point certainly deserves a discussion but given the 3 or 4 orders magnitude difference between the modelled sprite HO2 production for 1 sprite and that observed by SMILES, "large" is rather unrealistically large. I suggest reviewing the text to make clear since the beginning that one could expect a few tens of sprites per thunderstorms (up to a few hundred in extraordinary cases) and so 3 or 4 orders of magnitude differences cannot be reconciled.

L291 I would discuss this part in terms of the airmasses interested by the sprite event rather than introducing the expansion of the sprite body since the sprite lasts a few milliseconds.

Fig 7 and 8. Could you add a thin line at zero?

Fig 11. Could you please add a thin vertical zero line? Why is only the SMILES 77 km altitude tangent point plotted in the graph? The three case studies are at 75, 77 and 80 (cases A, B and C respectively). I think having all the three lines would be more appropriate.

Fig 12: It would be helpful to report the time difference between the sprite event and SMILES measurement directly in the figure. Also, the figure could be completed adding a contour map of a snapshot of horizontal winds.