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Comment on acp-2022-274

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Referee comment on "Enhanced sulfur in the upper troposphere and lower stratosphere in spring 2020" by Laura Tomsche et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-274-RC2, 2022

This manuscript is overall an excellent overview of the BLUESKY mission, and presents some rather interesting measurements of the UTLS in a peculiar case (the beginning of the COVID-driven decrease in emission). It has the potential to be a rather important reference going forward, but clearly the manuscript needs more work, and to be cleaned up a bit.

I generally agree with all the comments already laid out by Reviewer 1, so I find it pointless to repeat them one by one: but in particular, I found the possible causes of enhanced SOâ□□ to be a bit too much handwaved in section 4.

For instance, at L. 283, the paragraph starts with "Beside the conversion to H2SO4..." but then continues by mentioning that most of the SOâ□□ would be reduced by its conversion to sulfates through OH oxidation. Something you clearly go back to at the end of the paragraph, by simply repeating the same thing. But no convincing proofs are given. Even just the addition of a vertical profile of humidity from ERA5 reanalysis (which is what van Heerwaarden et al. uses) would go a long way into proving that, indeed, SOâ□□ lifetime has been increased by lower OH concentrations.

Something similar can be said for Section 5.

A rather long list is given, but I don't find a compelling critical assessment of possible causes. Starting from the bottom, sure, we know COS contributes to the stratospheric aerosol layer, but what causes would have produced an increase significant enough to be observed in this campaign (noting that photolysis of OCS happens much higher up, so its eventual conversion to aerosols and signal at the lower altitudes would not be as instantaneous as for SOâ□□). So in all of this, Figure 6 is really not that helpful (also, the legend in panel b) seems to be hiding the 2017 data...). Are we sure those different measurements are performed over similar regions? The Andrés Hernández (2022) reference seems to be targeted at lower altitudes, and I can't find SO4 measurements there, so it's hard to judge if the comparison is correct or not - and it's the only point offered for comparison! This hand-waviness is reproduced as is in the Conclusions, where the phrase " The enhanced stratospheric sulfate aerosol, which was observed, was likely impacted by the volcano Raikoke, and smaller sources" doesn't really say anything at all.

In a future revision, both Section 4 and 5 need to be strengthened and mroe analyses need to be performed to make sure this really reads like a Research Article. Otherwise, the editor and the authors could consider shortening those parts and limiting the scope of the work to a "Measurement report" type of manuscript for ACP.

One more typo: "One major source of SO4 2- in the stratosphere are volcanic eruptions" should be "is" and not "are"