We thank the reviewer for their valuable feedback on our paper. We respond here to some of the larger comments the reviewer has detailed in their review.

The reviewer makes some valid and valuable points in reference to our article. However, we feel that he misjudges the value of the paper. While we agree that some of the results of the paper are not ground-breaking and new, they do confirm the existing literature with a complete sulfur chemistry scheme, which we then extend to isotope modelling. It is a first attempt at fully incorporating the isotope scheme in a 1D model, with all the S-molecules included. This an important step before incorporating the isotope scheme in a 3D model, in which the spatial and regional effects can be better scrutinized. We would like to point out that modelling of S-isotopologues in atmospheric transport models is not a trivial task, and we think that this 1D modelling study is a valid first step.

Regarding the COS contribution to SSA; we are limited in the case of SSA with a lack of isotopic measurements, however this issue would also be a problem in a 3D model. We do agree that for SO_2 and COS there are enough measurements available (e.g. satellite data) that we can add in our paper to better validate it. We would like to add some Michelson Interferometer for Passive Atmospheric Sounding (MIPAS) satellite observations in the case of COS (Glatthor et al., 2017, Ma et al., 2021).

Furthermore, relating to the point of reviewer 1 on the budget of sulfur isotopes in the stratosphere, we would like to point that the highlight of our paper comes from the isotopic aspect of it, where we are able to show the vertical profiles of the sulfur gases in the stratosphere and troposphere and calculate the contribution to the isotopic budget. For instance, what we notice in the sulfate isotopic budget is that sedimentation makes the pool of stratospheric S enriched by removing the lighter S. Therefore, it is definitely an interesting finding to see that all the S compounds in the stratosphere are enriched in this model. The isotopic budget calculations are also not trivial, as they describe the contribution of each process to the isotopic signature of each molecule in the atmosphere, something that is worthwhile to implement in a 1D model first before moving to a full 3D implementation. We feel that these novel aspects of our paper are not valued enough. We are of course willing to restructure the paper so as to better highlight the isotopic aspect of the paper.

To conclude, we agree with some of the more substantive remarks of this reviewer. However, the reviewer clearly undervalues some of the novelty of the paper. We think that – with the restructuring we propose above – the paper provides enough interesting material for the scientific community.

References

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