

Firstly, thank you to the anonymous referees for taking the time to evaluate this work.

Here we address all major or general comments made by the two referees.

Please find all responses to minor comments specific to the text in the supplementary document attached.

RC1

1. The main concern from RC1 here is the insufficient use of recent references in the introductory material, as well as using primarily out-of-date fluxes and data in the model.

"References to recent studies are missing in the introduction." - We address these concerns by firstly updating the introduction to include a broader review of available literature to better represent recent scientific findings for OCS.

"Using the inventory of Kettle et al. (2002) is also a major weakness as many studies have provided new OCS flux estimates since. Therefore, the conclusion that TOMCAT_{OCS} gives better results than TOMCAT_{con} does not seem relevant when TOMCAT_{con} is based on out-of-date estimates (except for the anthropogenic emissions from Zumkehr et al., 2018). " - To address this concern (and others detailed in 1.) we include an additional model run for comparison with TOMCAT_{OCS}. This new model is called TOMCAT_{SOTA} and makes use of flux inventories developed between 2017 - 2022, including work by the following: Stinecipher et al. (2019), Zumkehr et al. (2018), Lennartz et al. (2017, 2020) and Maignan et al. (2021). See the text for more specific detail. This new model run provides an additional comparison for TOMCAT_{OCS}, one that is more relevant to recent findings and strengthens the conclusions made about the quality of TOMCAT_{OCS}.

"In Section 3.3, why choosing to use a single constant LRU value while several studies provide PFT-dependent LRU values?" - On a global scale it seems unlikely that a varying LRU would make a large difference, which is highlighted by Hilton et al. (2017): "the methodological simplification of treating LRU as a constant introduces considerably less error than GPP uncertainty (as estimated by inter-model GPP differences)". While this will be included in the scope of future work, the primary goal was to present a suitable model that compares well with measurements.

" In Section 3.4, the OCS fluxes that were adjusted to obtain a balanced budget should be contrasted with more recent estimates. For example, choosing to scale CS₂ oceanic emissions to 439 GgS/y is not supported by Lennartz et al. (2020) who estimated a total source of 70 GgS/y from CS₂. An oxic soil budget of 322 GgS/y is also not in line with the recent estimates of Kooijmans et al. (2021) and Abadie et al. (2022) based on the mechanistic soil model of Ogée et al. (2016). " – Firstly, thank you for bringing these works to my attention, the method and discussion sections feature more references to recent literature in line with this request. However, from an actionable perspective this comment poses several challenges. Firstly, the use of CS₂ is utilised purely due to its spatial distribution over the tropics from the Kettle et al. (2002) inventory – this is mentioned several times in the text. Secondly, while Kooijmans et al. (2021) does suggest soil uptake of 89 – 146 Gg S/y using SIB4, this paper was published in December 2021 during the final stages of preparing this manuscript. Furthermore, the discussion for Abadie et al. (2022) started in November 2021, again during final preparations for this work and was actually published nearly 2 months following (11 May 2022) the initial

submission of this work (18 March 2022). Finally, it is worth highlighting that the combined biosphere flux presented in this work, used in TOMCAT_{OCS}, is inline with inversion studies by Ma et al. (2021) and Remaud et al. (2022).

“More recent studies should be added in Table 2 to compare to the OCS budget from this work, such as Maignan et al. (2021), Kooijmans et al. (2021), Remaud et al. (2022).” – Remaud et al. (2022) is now also included.

2. The main concern from RC1 here is that optimisation of the fluxes would be better achieved using an inversion framework and that balancing the budget is unnecessary.

“The scaling of OCS fluxes to better match estimates made after Kettle et al. (2002) and to obtain a balanced OCS budget seems quite arbitrary. Such adjustments should be made using an inversion framework as done in Ma et al. (2021) or in Remaud et al. (2022). Without an analytical inverse system that optimizes the fluxes, why aiming at a balanced COS budget? A balanced OCS budget is also not required if analyzing the detrended OCS concentrations.” – This is a fair statement but does not factor in the accessibility to an inverse framework at the time of undertaking the research. Also, while a balanced budget is indeed under debate (less so in the past 5 or so years), it has been shown for the majority of the period between 2004 and 2018, that the OCS budget is indeed in balance or with a weak trend, depending on latitude.

“Moreover, this scaling assumes that the OCS flux spatial distribution of each component is not modified compared to the control inventory, which might not agree with flux distribution obtained in more recent studies.” – This is indeed a limitation of the method of scaling in this work and is acknowledged by the author(s). The compromise proposed here is to have included the third model run, as discussed in 1. This provides an additional comparison with up-to-date spatial distribution of fluxes, including maps presented in the supplementary material. The quality of the vertical comparison with ACE-FTS suggests the troposphere, surface and atmospheric fluxes are modelled well.

3. The main concerns here were regarding the calculation of vegetative uptake of OCS, particularly CO₂ and OCS concentration.

“Not considering interannual variations is a strong assumption that should at least be better justified. The study from Chen et al. (2017) does not conclude that interannual variability in GPP amplitude can be neglected. GPP interannual variability could easily be included in this work as GPP is modelled by JULES. Considering only the year 2010 does not reflect the yearly increase in atmospheric CO₂ concentration and the fertilization effect.” It was assumed that including variability in CO₂ concentration is likely to be a smaller source of error or variation in F_{ocs} than other factors, such as GPP product and OCS concentration. Like LRU this would be included in the future scope of this work.

“Otherwise, could the impact of not considering OCS flux interannual variability be quantified? For example, OCS vegetation uptake could be defined as a first order relationship with OCS mixing ratio. Therefore, inter-annual variations in OCS vegetation flux might have a strong impact on the simulated atmospheric OCS concentrations.” – The calculation of vegetative uptake is actually calculated at each time-step in the model (every 6 hours). The text has been updated to make this clearer. But we do establish a first order relationship between vegetative uptake and OCS concentration.

4. RC1 main points here are that the paper lacked direction and conclusion, and that some of the comparing TOMCAT_{OCS} to TOMCAT_{CON} was a challenge and a bit arbitrary.

“It is not clear what the goal of this study is, and the title is confusing.” – The goal of the study was to build a model that simulates OCS well compared to novel satellite observations and surface measurements. In turn to extract information about the implications of the OCS budget estimated here and how this compares with recent literature. TOMCAT_{SOTA} helps provide a benchmark for recent work and how a collection of bottom-up estimates does not necessarily yield sensible seasonality at the surface. Hence, highlighting there is still substantial uncertainties in the overall OCS budget. This work agrees with the missing source originating from the tropics but falls short of concluding its exact origin. As mentioned in the text, future work will aim to use an inverse system in combination with data that offers vertical information, such as ACE-FTS, but preferably also in the lower troposphere, like NDACC. Connecting the troposphere to the surface could be the key to improving surface flux estimates.

We have recommended a change in title from:

Modelling atmospheric carbonyl sulfide using gross primary productivity to constrain vegetative uptake

to

Constraining the budget of atmospheric carbonyl sulfide using a 3-D chemical transport model

“Should this work focus more on the advantage of using ACE-FTS compared to other available OCS concentration observations? Or on the information that can be retrieved from ACE-FTS about the modelling of OCS atmospheric sinks?” – The quality of TOMCAT_{OCS} comparison with ACE-FTS is highlighted more so in the revised manuscript, with an underlying message that a good comparison throughout most of the atmosphere suggests that the gradients of OCS are well represented by the model. Therefore, so are the surface and atmospheric fluxes driving it.

RC2

“First, when the results are described in the paper, often hand-waving argumentation is used to explain the deviations between model and observations. “Likely caused”, “could be attributed”. Here, we have to believe the judgement of the authors, since rarely additional arguments are presented.” – The author has improved on this by either removing speculation or better comparing and referencing conclusions, particularly in the results and discussion sections.

“Likewise, the underestimation of modelled OCS in the tropical stratosphere is explained by too fast removal. These observations call for additional simulations to verify whether speculations hold true. Two suggestions here: (1) a simulation with tagged tracers to be used in a more detailed analysis of e.g. seasonal cycles (2) a simulation with reduced photochemical removal in the tropical stratosphere. Presentation of the results would give the paper more body.” – We include an additional model simulation for 2010 only that reduces atmospheric photolysis. The intention of this simulation is to test if this does resolve the issues in the stratosphere. What we find is while it does reduce negative bias in the Southern Hemisphere, it introduces model bias elsewhere, i.e. positive in the Northern Hemisphere tropics. So we draw the conclusion that the convection scheme or transport is causing the issue. An experiment

with 0.75 photolysis rate is presented in the manuscript and an additional one with 0.5 photolysis is shown in the supplement.

“It remains unclear what is taken for the OCS mixing ratios here.” – As described in point 3 of RC1, we utilise the OCS concentration at each time-step in the model to calculate the vegetative flux. The model is initialised at the start of the spin-up period (1994) using 500 ppt however.

“The paper therefore misses quite some recent references that are relevant for the work. The authors should update the reference list (and discussions) with more recent papers.” – The introductory material has been updated to include more recent literature. More frequent use of up-to-date references are used throughout the text.

Minor Comments

RC1

Abstract:

L25: “At the surface, the model captures background concentrations at most of the surface sites to within the maximum and minimum of the seasonal measurements”. It does not seem to be a strong condition to satisfy. It might be better to highlight results on the seasonal cycle amplitude or the phase. – *The error bars represented in Figure 1 are now standard deviation, rather than maximum and minimum. Results of SCA are highlighted in the new abstract.*

1.Introduction:

L49: “The main source of atmospheric OCS is oceanic emission”. This could be replaced by "one of the main sources" as anthropogenic emissions are also a major source of OCS. For example, Zumkher et al. (2018) estimated an anthropogenic OCS source of about 400 GgS/y. Aydin et al. (2020) suggested that this estimate could be underestimated with an anthropogenic OCS source of about 600 GgS/y. – *Updated. Line 64.*

L57: “with estimates ranging from 210 to 2400 Gg S yr⁻¹ (Kettle et al., 2002; Sandoval-Soto and Stanimirov, 2005; Suntharalingam et al., 2008; Berry et al., 2013; Glatthor et al., 2015; Kuai et al., 2015; Launois et al., 2015b; Ma et al., 2021)”. Add references to more recent studies such as Maignan et al. (2021), Kooijmans et al. (2021), Remaud et al. (2022) for vegetation OCS uptake estimates. – *Updated. Line 75-77.*

L61: “OCS hydrolysis also occurs in soil, again catalysed by carbonic anhydrase”. Note that OCS can also be consumed by other enzymes in soils, such as nitrogenase, CO dehydrogenase, or CS₂ hydrolase (Smith and Ferry, 2000; Masaki et al., 2021). – *Updated. Line 81-82.*

L63: “with an estimated annual loss of 127-355 Gg S (Kettle et al., 2002; Montzka et al., 2007; Berry et al., 2013; Glatthor et al., 2015; Kuai et al., 2015)”. More recent studies should be mentioned, such as Kooijmans et al. (2021) and Abadie et al. (2022) that lead to smaller soil OCS budgets. – *Updated. Line 83.*

L65: “Soil has also been observed to act as an emitter of OCS in warm conditions (Maseyk et al., 2014)”. This was not observed only for warm conditions. OCS emissions have also been related to soil types (Whelan et al., 2013), nitrogen content, light radiations reaching the soil surface (Spielmann et al., 2019; Kitz et al., 2020). – *Updated. Line 85-88.*

L69: “the latter of which has been used as a benchmark for more recent studies”. Please add the references of the studies. – *Updated. Line 92-93.*

2. Observations:

Section 2.1: Please provide and detail the uncertainties associated with ACE-FTS retrievals. – *Updated. Line 145-147.*

3. Chemical transport modelling of OCS:

Section 3.1: What is the timestep used to run the TOMCAT model? – *6 hours. Updated. Line 172-173.*

L139 to L224: Please make it clearer which fluxes have been used for TOMCAT_{con} and which one have been used for TOMCAT_{ocs}. – *Updated. TOMCAT_{con} uses inventory in Section 3.2 and TOMCAT_{ocs} uses inventory in Section 3.3.*

L153: “The three sink terms are an oceanic sink, soil uptake and a vegetative sink”. OCS photolysis in the stratosphere and OCS oxidation by OH radical in the troposphere should also be included in OCS sinks, as atmospheric OCS reactions are not explained before in section 3.1. – *We include a summary of OH and Photolysis loss in Section 3.1, before describing all the fluxes. It is clear the same scheme is used for all model runs. Line 166-170.*

Equation 1: Precise the unit for each term of this equation. What is used for OCS background concentration? – *Updated. Line 222-226.*

L173: Please replace “LRU is the normalised ratio of OCS assimilation rates to CO₂ at the leaf-scale. This is then normalized by background concentrations of the two gases” by “LRU is the ratio of OCS assimilation rates to CO₂ at the leaf-scale, both normalized by their respective concentration”. – *Updated. Line 223-224.*

L180: “but is slightly under half that of the largest estimation of 1115 Gg S in Table 2 from Montzka et al. (2007)”. Launois et al. (2015) estimated a larger plant OCS uptake than Montzka et al. (2007) for the ORCHIDEE land surface model. – *Updated. Line 242.*

L204: “at Northern Hemisphere (NH) NOAA-ESRL sites”. Please precise which sites and whether they receive air masses mainly coming from the ocean. – *Updated. Line 277-278.*

Table 2: Why were more recent studies not included in this table for comparison? Such as Maignan et al. (2021), Kooijmans et al. (2021), Remaud et al. (2022). – *Updated to include Remaud et al. (2022).*

4. Results:

L243 to L245: “TOMCATCON was initialised using OCS values in each grid box from TOMCATOCS, after 10 years (1994 – 2003) spin-up. Only 2004 monthly mean mixing ratios from TOMCATCON have been included, as this flux inventory has a 245 net negative budget and therefore a negative trend over longer periods”. Should this be in Section 3 as it is related to the method? – *Moved to section 3. Line 187.*

L244: “Only 2004 monthly mean mixing ratios from TOMCATCON have been included, as this flux inventory has a net negative budget and therefore a negative trend over longer periods”. Please precise the net negative budget. The trend in atmospheric COS concentrations should not be an issue if you remove the trend and compare the detrended atmospheric concentrations. – *Updated. Line 188. Figure 2 now presents monthly anomalies.*

L249: “Error bars associated with the observations represent the maximum and minimum values for each month at every site”. Representing the standard deviation would be a better indication of the uncertainty of the mean value. – *Updated. Figure 1 error bars are now represented by standard deviation.*

L253: “Comparisons between TOMCATocs and TOMCATcon are shown here to emphasise the improvements made by the flux inventory developed in this study”. How could the improvements obtained with TOMCATocs on atmospheric OCS concentrations be compared to the improvements made when using inversion systems such as in Ma et al. (2021) and Remaud et al. (2022)? – *We compare TOMCATOCS to a new model run TOMCATSOTA, which utilises fluxes that are more up-to-date in the literature and similar or the same as those used in as prior fluxes by Ma et al. (2021) and Remaud et al. (2022). See discussion and supplement.*

L254: “The root mean square error (RMSE) for the entire period is shown for each site, alongside the seasonal cycle amplitude (SCA)”. Precise that in the following you also compare the phases of observed and simulated seasonal cycles. – *SCA is now displayed in Figure 2.*

L255: “Generally, there is an improvement in RMSE across all the sites, but in some cases, there is a degradation, which is mostly attributed to background concentration, rather than the model’s ability to capture a suitable seasonal cycle, hence both are shown.” By “background concentration”, do you mean the average concentration? If so, could you please show that the degradation in RMSE is due to the average concentration? – *I did mean average concentration. Figure 1 shows TOMCAT_{OCS} and TOMCAT_{CON} monthly mean model values, but Figure 2 shows monthly anomalies. So we are able to distinguish the impact from poor estimation of average concentration and of the seasonality.*

L264: “This seasonal cycle resembles that of CO₂, hence GPP is a suitable proxy for calculating OCS uptake”. Please rephrase as similar seasonal cycle is not reason enough to use GPP as a proxy of vegetation OCS uptake. – *I agree. This was rephrased. Line 377.*

L275: “Here we show realistic amplitudes in the seasonal cycle from TOMCATOCS, 76 ppt at LEF and 71 ppt at HFM, compared to observed values of 123 ppt and 128 ppt, respectively”. Please rephrase as the SCA at these two sites are still largely underestimated. – *Rephrased. Line 399-401.*

L279 to 282: The constant LRU value used in this study could be compared to other LRU estimates for the same vegetation types found at LEF and HFM to see if it could be underestimated. If a constant OCS mixing ratio was used to compute OCS vegetation uptake, this could also affect the SCA. – *I have updated the text in places to make it clear we are using OCS concentration from online within the model. A brief discussion of LRU in the literature is made in Section 5: Line 565-570.*

L329: “OCS values decline above and below the UTLS due to removal by photosynthesis at the surface”. Soils can also absorb OCS at the surface. – *Updated. Line 456-457.*

L345: “potentially attributed to slower surface OCS uptake”. It could also be due to an underestimated surface OCS uptake. – *Updated. Line 472-473.*

Figure 2: What could explain the net distinction between higher mixing ratios in NH compared to SH found in TOMCAT in JJA and SON? – *Weaker removal of OCS in the SH.*

L364: “this suggests the upper atmospheric sinks are modelled well by TOMCATocs”. Isn't it in contradiction with the steeper gradient of TOMCATocs in the stratosphere mentioned above? – *Slightly rephrased to play down the performance. Line 492-493.*

Section 4.2 attributes model-observation mismatches to OCS sources or sinks, what about the potential mismatches from TOMCAT transport? – *This is mentioned in Section 4.3. Line 516.*

5. Discussion:

L376: “the TOMCATocs simulations of atmospheric OCS concentrations and the vegetative flux, which are dependent on one another in the model”. The simulated OCS concentrations are dependent on vegetation OCS fluxes that were transported, but it is not said which OCS concentrations are used to compute the vegetation OCS fluxes. – *OCS concentration from the model is used in the calculation of vegetative uptake. See Line 228.*

L385: “inverse modelling of OCS fluxes shows that some combination of a larger tropical oceanic source and vegetative sink resolves the budget”. Kooijmans et al. (2021) also show that considering a variable atmospheric OCS concentration reduces the vegetation sink in the tropics, meaning that a smaller tropical OCS source would be needed to close the budget. – *See above.*

L392: “such as calculating OCS uptake using a constant LRU value of 1.6 is not representative of reality”. It could be interesting to give the range of values proposed for LRU in the literature, to illustrate how LRU values can vary. – *Updated. Line 565-575.*

L394: “Our estimation of vegetative uptake in this work does not replicate OCS uptake universally and it is unclear if this is due to localised differences in LRU or on the GPP fields themselves”. Could you provide a global map of vegetation OCS uptake obtained with your approach using a constant LRU value and compare it to similar maps found in the literature (Berry et al., 2013; Kooijmans et al., 2021; Maignan et al., 2021) to analyse the spatial distribution of the fluxes? Could you also provide a map of TOMCAT simulated atmospheric CO₂ concentrations used to compute vegetation OCS uptake? – *See the supplement for these additions.*

L396: “the distribution is based on work by Kettle et al. (2002) and has since been updated, for example by Ogée et al. (2016)”. It has also been updated by Sun et al. (2015). It could be interesting to compare the spatial distribution of your soil OCS fluxes to other maps based on the mechanistic approach from Ogée et al. (2016) (Kooijmans et al., 2021; Abadie et al., 2022). – *See the supplement for these additions.*

6. Conclusion:

L453: “Therefore, further study following on from this work will be to derive an a posteriori set of fluxes using an inversion scheme based on an up-to-date prior, and surface observations and a dataset containing vertical information near the surface”. Important drawbacks are acknowledged by the authors as this work does not rely on an optimization framework and uses out-of-date OCS fluxes. These drawbacks should explicitly appear in the abstract. – *Abstract has been mostly rewritten.*

Minor comments:

L19: “To compensate for this larger vegetative sink”. I would not use “larger” here as it has not been explained yet that it is larger than the vegetation OCS uptake from Kettle et al. (2022). – *Updated.*

L30: Is this really “Hawaiiin” and not “Hawaian” (replace everywhere if needed)? – *Updated to Hawaiian everywhere.*

L56: Please replace “Vegetative uptake is the most important atmospheric sink of OCS” by “Vegetative uptake is the most important sink of atmospheric OCS”. – *Updated. Line 74.*

L107: Please develop the abbreviation HITRAN. – *Updated. Line 144-145.*

Table 1: Please replace “Barrow” by “Utqiagvik (formerly Barrow)” and “Cape Grim” by “Kennaook / Cape Grim”. For PSA and SPO stations, please precise “Antarctica (United States)”. – *Updated.*

L135: Should “surface emission fields” be replaced by "surface flux fields" as surface OCS fluxes are not only sources? – *Updated. Line 175.*

L136: Please replace “six sources and three sinks” by "six net sources and three net sinks" as soils can be both a sink or a source of OCS for example. Please also name the sinks and sources here. – *Updated*

L147: Remove "an" in this sentence “Eleven anthropogenic sources of OCS were an quantified by Zumkehr et al. (2018)”. – *Updated*

L175: Please develop the abbreviation WATCH. – *Updated*

L235: “so we only compare the main simulations”. Please precise that it is TOMCATocs. – *Updated*

L301: Please remove “Gg” in the following “from 17.7 Gg ppt to 3.4 ppt”. – *Updated*

Figure1: Please improve the resolution of the figure to be able to read the RMSE scores. – *Updated*

RC2 (from attached pdf document)

Section 1

L19: confusing, since you say "towards the lower end", so expect "smaller". – *Abstract has been mostly rewritten.*

L39: define lifetime: global burden/stratospheric loss or stratospheric burden/loss? – *Updated. Line 48-50.*

L55: confusing. "DMS accounts for OCS oceanic emissions". DMS is emitted and an uncertain fraction is oxidized to OCS. refer recent findings: Jernigan, C. M., Fite, C. H., Vereecken, L., Berkelhammer, M. B., Rollins, A. W., Rickly, P. S., Novelli, A., Taraborrelli, D., Holmes, C. D., & Bertram, T. H. (2022). Efficient production of carbonyl sulfide in the low-NO_x oxidation of dimethyl sulfide. *Geophysical Research Letters*, x, 1–11. <https://doi.org/10.1029/2021gl096838> - *Updated. Line 71-73.*

L66: recent update: @article{abadie2022global, title={Global modelling of soil carbonyl sulfide exchanges}, Kooijmans, L. M. J., Cho, A., Ma, J., Kaushik, A., Haynes, K. D., Baker, I., Luijckx, I. T., Groenink, M., Peters, W., Miller, J. B., Berry, J. A., Ogée, J., Meredith, L. K., Sun, W., Kohonen, K. M., Vesala, T., Mammarella, I., Chen, H., Spielmann, F. M., ... Krol, M. (2021). Evaluation of carbonyl sulfide biosphere exchange in the Simple Biosphere Model (SiB4). *Biogeosciences*, 18(24), 6547–6565. <https://doi.org/10.5194/bg-18-6547-2021> - *Updated. Line 85-88.*

L84: I find this sentence strange. Normally one would use OCS vegetative uptake to estimate GPP.....so this becomes rather confusing. – *Removed this line. While we do clarify that the LRU approach does suitably estimate vegetative uptake, this had been shown previously.*

L92: are – *Updated.*

Section 2

Section 3

L153: do I miss the chemistry terms here (OH, and photolysis)? – *Line 166-170.*

L169: I think you have to be clear about units here. GPP and Fcos differ by orders of magnitude... – *Updated to be more clear on units. All appropriate scaling is performed in the calculation. Line 223-227.*

L178: I think Ma et al. showed that is even more important to account for OCS mixing ratios. Now it remains unclear how [OCS] is used to convert GPP to F_{OCS} – *We use OCS from the model at each timestep. The text has been updated to make this more clear. For example on Line 228.*

L194: fluxes?

L195: uptake?

L197: I understand, but should be something like: due to the resulting improvements

L206: ocean emissions

L219: I think OH loss is mostly tropospheric. – updated.

Table 2: I read the Ma et al. table well, this is the imbalance in their budget.... – *This has been updated in Table 2.*

L236: did not read that in section 2.1, nor in section 3.1 – updated.

Section 4

L237: This does not make sense. Why not compare both simulations? – As the budgets of the other model runs are negative, it would make correction throughout the entire atmosphere very challenging.

L243: mm, this was not stated in the method section. Actually, this belongs in the method section, and the 10 year initialization is mentioned in the TOMCAT_CON description. – Updated.

L248: please do not repeat the method section here. – Updated.

L250: What I do not understand why monthly means are compared, while ACE-FTS is co-sampled. I think co-sampling is important at the surface. Also, I find the metric max-min to estimate the monthly error in observations misleading. – Figure 1 updated to use standard deviation.

L272: This improvement is rather disappointing. This implies that budget terms and their seasonality are not OK yet? – *It is not an excellent improvement. However, when compared also to TOMCATSOTA, we see that it is a fairly good improvement. While TOMCAT_{SOTA} does well in the NH in terms of SCA, it performs poorly at LEF and HFM.*

L279: Here it is really vital to investigate the modeled diurnal cycle. And during strong uptake OCS goes down, and the LRU formulation might break down. This is very handwaving argumentation. – *This sentence was removed. But our work does indeed suggest the LRU approach still underestimates at heavily vegetated regions.*

L282: Well, also handwaving: was JULES NEE validated with flux measurements at this location (of at Harvard Forest)? – *JULES GPP was validated against FLUXNET. The text has been updated accordingly. Line 405-407.*

L287: did you check this? Or is this speculation. Could anthropogenic emissions influence MHD is some month?

L289: The big question hanging here is: is this due to model shortcomings, or due to the applied COS fluxes. Given the fact that inverse models capture the seasonal cycle at MHD, you would be tempted to say that fluxes are not yet optimal, e.g. too high ocean emissions over the Atlantic... – *This is likely the cause and text has been updated. Line 415-419.*

L298: did you check (e.g. with tagged tracer run), or can you provide a reference. – *Updated as above.*

L301: ??

Figure 1: increase font size... AND What are the Blue error bars? CAPTION: is. – Figure 1 has been updated and the blue bars now represent standard deviation.

L337: Again: here you speculate. It would be good to perform tagged tracer runs to backup these statements. – This was not considered in the scope of corrections.

L342: I assume you mean something like the maximum concentration in the vertical.

L343: ?? If ACE cannot measure, this does not make sense. – *Removed.*

L347: There is nothing between these two seasons? – *Updated Line 473.*

L359: There is abundant speculation in the paper. Some things really need to be checked betterOne thing is this "faster removal". What about reducing the photolysis rates to see whether the disagreement disappears? – *The author improves on argumentation and discussion. We include an additional model run to test the comments on photolysis loss in the stratosphere.*

L364: Not sure if this is the ACE uncertainty, since this is the variability in observations. Does ACE provide an observational error? – *Amended to be standard deviation.*

L366: see comment above

Section 5

L386: I think you overstate the quality of your simulations here. There are significant remaining deviations that apparently are resolved when emissions are optimized. Now the seasonal cycles are sifted and mostly underestimated (Figure 1). – *Amended the text to suggest that our flux also points towards a missing tropical source, like the inversion studies referenced. Less so that our model compares as well as theirs.*

L388: ??? – *Removed.*

L394: and sensitive to the unknown OCS mixing ratio at the uptake locations... – *Updated. Line 565-566.*

L397: see also new paper Abadie (2022) – *Updated. Line 570-571.*

L410: Again,, OH mostly acts in the troposphere. – *Updated this sentence. Line 539-541.*

L413: Again, a speculation that can be easily tested.. – *Removed this sentence.*

Section 6

L448: d – *Updated. Line 613.*