Reviewer comments in black
Author response in blue
Text in the revised manuscript in italic

Reviewer #1, Jürgen Kesselmeier

The paper was greatly improved. The reader can now reach an overview about environmental factors affecting physiological background for COS-uptake much easier. The content as removed from the supplement and added to the manuscript helps to follow the description and discussion. The additional figures are fine. I have only one minor remark, which may be regarded as a technical correction. The authors discuss the role of other sinks than trees within the boreal ecosystem. However, when regarding the potential role of soils and cryptograms they should not mention "mosses" only. The term "cryptograms" as mentioned in my former review comprises algae, lichens, mosses, and ferns. Furthermore, lichens can provide a large biomass in boreal regions and they can easily reach an ecosystem sink strength of soils (Kuhn et al. 1999, Atmospheric Environment 33, 995-1008). I propose to use the term cryptograms or to write "mosses and other cryptograms".

We thank the reviewer for pointing this out and apologise for the inaccuracy. We have now revised the text as follows: "*Mosses and other cryptogams can also significantly take up COS during the night when the soil is wet (Rastogi et al., 2018). Nighttime soil uptake in Hyytiälä was ca. -3 pmol $m^{-2}s^{-1}$ (Sun et al., 2018) while the ecosystem scale nighttime uptake in our study is ca. -10 pmol $m^{-2}s^{-1}$. However, we did not measure the contribution from cryptogams. The nighttime COS uptake in Hyytiälä is thus likely a combination of soil and cryptogam uptake, but also has a larger contribution from the canopy (Kooijmans et al., 2017).*"

Reviewer #2
I find that the manuscript has been significantly improved. The authors replied to my comments in an appropriate manner and followed my main suggestions. They added more explanations in the "materials and methods" section and performed additional evaluations of the COS fluxes simulated by the SIB4 Land Surface Model against measurements at Hyytiala. There are still some minor points that need to be clarified before publication.

Page 4, line 100: Add a reference as for the provenance of the equation 1.
Reference to Kohonen et al., (2020) added.

Page 5, equation 6: Replace T by T_a.
Corrected as suggested.

Page 6, section 2.6 : Add a sentence precising that the year 2014 was not used in determining fitting parameters.
Year 2014 was also used in determining the fitting parameters.

Page 7, line 197: Consider adding a sentence saying that *e* is believed not to be gas dependent.
Corrected as suggested: "*Parameter e = 0.18 was fixed before optimizing the other parameters according to a previous study by Peltoniemi et al., (2015), since parameter e is related to ecosystem phenology specific to the site, and is believed not to be gas dependent.*"

Page 7, section 2.7 : Although the authors added some clarifications in the review regarding the e parameter, they do not appear in the manuscript! Add a few sentences explaining the reasons why e

is equal to 2.1. "The in-situ LAI is the all-sided lead area index, while SIB4 LAI is projected leaf area index. For this reason, e in SIB4 is fixed to 2.1".
We apologize for missing to add this clarification also to the manuscript, now corrected.

Page 8, lines 220-224: The authors calibrate now the SIB4 meteodata based on in situ measurements but the calibration method is not mentioned clearly in the manuscript. Add a sentence explaining how the SIB4 meteodata are calibrated with a reference to Figure S10.
Corrected as suggested: "*To obtain COS biosphere fluxes for the whole boreal region based on the FCOS observations in Hyytiälä we calibrated the SiB4 PAR, LAI, VPD and T_a for the grid cell where Hyytiälä is located against observations. The obtained calibration is shown in Fig. S10. The in-situ LAI is the all-sided leaf area index, while SiB4 LAI is projected leaf area index, which explains the large difference between the two LAI data. We then applied the parameterization represented in Eqs. 7-11 to the whole boreal region (based on the ENF grid cell selection described in the previous paragraph) using the SiB4 meteorological and phenological data.*"

Page 10, legend of the Figure 2 : "show daily gap-filled averages (see Text S1)" Where is the Text S1?
Thank you for spotting this, it was an old reference. Corrected the reference now as "*(see Sect. 2.2)*".

Page 11, line 297: "avoid including radiation related-correlation". Explain why there is a radiation related correlation.
Added clarification "*since VPD and T_a are highly intercorrelated with PAR.*"

Page 14 ,Table 1: Given the high non-linearity underlying the equations 7-11, the statistical analysis would require a random forest approach as done in Maignan et al., 2021 to deal with the high non linear interactions between variables.
As explained in the previous author response, the linear regressions give some insight to which parameters are important despite non-linearity of some of the interactions. Often highly non-linear correlations also have higher linear correlation than when there is no correlation at all. Since the regression analysis is not the main focus of the study but only gives some background information to the COS flux variations, we have decided to leave the multivariate regression analysis as is. However, we have now added a sentence discussing this: "*While some of the interactions are non-linear, as seen from Fig. 3 and Eqs. (8-11), the linear regression analysis still provides information on the relative importance of the environmental variables, as non-linear correlations usually have a high linear correlation as well.* "

Reviewer #3, Mary Whelan

This manuscript presents the first very long term record of carbonyl sulfide (OCS) eddy flux covariance over any ecosystem. Additionally, measurements of OCS exchange in the boreal region are nearly as rare. Even without any analysis, the dataset is valuable to our scientific community. That said, the analysis performed here is the first step of many. A typical motivation for measuring OCS over ecosystems is to reveal new information about the carbon cycle which can be in turn used to constrain the representation of land carbon uptake in land surface models, mentioned in the introduction. The analysis here incorporates the role that stomatal conductance and leaf-affecting parameters play in OCS uptake by vegetation; however, the data is not brought back around to compare to CO2 fluxes. I hope to see this in a future effort!

In the response to my earlier review, you note that deriving stomatal conductance from OCS measurements is "a very difficult task to do from EC" and requires its own paper. Rick Wehr may have already written this paper in 2017. As far as I can tell, the most difficult part of applying Wehr et al., (2017) approach here is coming up with a reasonable estimate of mesophyll conductance for Scots Pine, which has experienced recent advances (see Stangl et al. 2021). Wehr and Saleska (2021) have also developed an improved method for estimating stomatal conductance from CO2 EC measurements that can be compared to OCS-based estimates.

Thank you for your continued effort in improving this manuscript.

Mary Whelan

References

Stangl, Z.R., Tarvainen, L., Wallin, G. and Marshall, J.D. (2022), Limits to photosynthesis: seasonal shifts in supply and demand for CO2 in Scots pine. New Phytol, 233: 1108-1120. https://doi.org/10.1111/nph.17856

Wehr, R., Saleska, S. (2021) Calculating canopy stomatal conductance from eddy covariance measurements, in light of th energy budget closure problem. Biogeosciences, 18: 13–24. https://doi.org/10.5194/bg-18-13-2021

We thank the reviewer for these comments. This is indeed a first step of many with this data set, that can be used in a multitude of different analyses in the future, not possible to fit in one paper.