

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2021-281

Anonymous Referee #1

Referee comment on "Global modelling of soil carbonyl sulfide exchanges" by Camille Abadie et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-281-RC1>, 2021

Abadie et al. implemented a mechanistic and empirical soil model of COS exchange into the ORCHIDEE land surface model and compare those with observations of soil COS fluxes at several sites, representing different soil types. Through a sensitivity study they find the most important parameters for the soil COS flux and optimize those parameters with observations at two sites to improve the COS soil flux simulations. Finally, the authors provide an updated global soil COS budget, including both oxic and anoxic (wetland) soils. This is a very complete and thorough study that I find very well readable. My comments are hence minor.

Abstract:

P1, L44: -576 Gg S yr⁻¹ for vegetation+soil, or only the vegetation?

Introduction:

P2, L62-63: This sentence reads weird.

P2, L63-64: The numbers 700-1100 GgS yr⁻¹ sound like a very large gap. If I'm correct, Berry et al. (2013) added an additional ocean flux of 600 to close the gap, and Kuai et al. (2015) added 559 Gg S yr⁻¹. Do the values 700-1100 GgS yr⁻¹ represent the total ocean emissions? So not only the emission gap? A reference to a more recent inversion could be added (Ma et al. (2021) with a total gap of 432Gg S yr⁻¹).

P2, L78: better say something like: "they have usually not been considered in atmospheric

COS budgets”.

P3, L87: form = from

P3, L113: For clarification, consider adding something like: “at the different sites **that will be used for evaluation in this study**”.

P3, L119: More important to the mechanistic model than to the empirical model?

P8, anoxic soil COS production: Did you consider to use the formulations of Meredith et al., 2018? These are similar to that of Whelan et al., 2016, but then with the α and β parameters specific for peatland/wetland soils. That is, $f_{ca} = 3700$ for boreal peatland (Meredith et al., 2019) and α and β for peatland from Meredith et al., (2018,). It would be interesting to compare those COS production estimates for wetland soils.

P10, L330: “...the same training method than the one used in Spielmann et al.” should be “...the same training method **as** the one used in Spielmann et al.”

P10, L333: If I understand the description in Wehr et al. (2017) right, the soil COS fluxes at US-HA are not based on eddy-covariance fluxes. It can be better described as flux-profile measurements, connected to CO₂ soil chamber measurements and profiles.

P10,L350-351:” The stations located in the northern Hemisphere sample air masses coming from the entire northern hemisphere domain above 30 degrees.” The stations cover mainly North-America and actually Eurasia is hardly covered, so I would not agree with this statement.

P11, L384: what does “d” stand for?

P12, L412: spell out “DA”.

Results:

P13, section 3.1.1. I think the authors could put more emphasis on the potential role of

nitrogen fertilization on soil fluxes. E.g. the results of IT-CRO, an agricultural site, could be emphasized in this context. Also the overestimated COS uptake at AT-NEU and ES-LMA could be discussed in light of nitrogen fertilization.

P13, L468 (Table 3): I would consider showing Table 3 as a figure. The same also for Table 4, which could even be combined with a Fig. from Table 3.

P14, L486-488: Or the division by PFT is not sufficient, and more specific information on e.g. nitrogen content is needed.

P14, L496: globally = generally?

P14, L503-505: This seems to be a repetition of line 500-501.

Fig 4: Can you show the same plots for observations?

Fig 5: Can the numbers at the end of the parameter names be replaced with an abbreviation? It is not entirely clear to me what the numbers represent, are they a PFT and soil texture number?

P16, L555: Can you remind the reader what PFT 15 is?

Figure E1: Can you explain the green and blue points, which are prior and which are posterior?

P17, L600-605: It is very interesting to read that the optimized parameters not only improve the simulated soil COS flux, but also the soil hydrology! Can you give some more details on the improvement of the soil moisture, e.g. with a figure or numbers?

P17, L612-613: It may be worth discussing the resemblance of the global distribution of COS soil fluxes of oxic soils with that presented by Kooijmans et al. (2021) (see their supplementary material). It is nice to see that the implementations of both the empirical and mechanical models show very similar global distributions in ORCHIDEE and SiB4.

P19, L683-684: So the soils do not seem to explain the biases at high latitudes, so can we conclude that the vegetation sink is underestimated at the higher latitudes?

P19, L699-702: But at the same time it is inconsistent with comparisons at AT-NEU, ES-LMA, IT-CRO and US-HA, and the marginal model-observation biases can not explain the too high atmospheric concentrations, so I find this sentence out of place and would remove it.

P19, L709-710: Instead of showing the Launois et al. results in Fig. 10 you could consider to include that of Maignan et al. (2021), which to me seems to be a more fair comparison.

Discussion:

P20, L717: It would be relevant to compare also with recent global soil COS sink estimates of Kooijmans et al. (2021) and to include those in Table 5.

P20, L738: Please, specify that this is about the lack of seasonality in the COS soil flux.

P21, L759-772: The authors here talk about under- or overestimations, but it does not read as if this is compared to actual observations. So I do not think under- or overestimations are the right term here, they are simply higher or lower than other estimates.

P22, The authors briefly touch upon the role of nitrogen fertilization in the discussion of section 4.3, but I think the authors could (and should) put more emphasis on the potential role of nitrogen fertilization in this manuscript.

P22, L788-789: More recent references such as Kaisermann et al. (2018) would be appropriate here.

Kaisermann, A., Jones, S. P., Wohl, S., Ogée, J. and Wingate, L.: Nitrogen Fertilization Reduces the Capacity of Soils to Take up Atmospheric Carbonyl Sulphide, *Soil Syst.*, 2(4), doi:10.3390/soilsystems2040062, 2018.

Kooijmans, L. M. J., Cho, A., Ma, J., Kaushik, A., Haynes, K. D., Baker, I., Lijkx, 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., Wohlfahrt, G., Berkelhammer, M., Whelan, M. E., Maseyk, K., Seibt, U., Commane, R., Wehr, R., and Krol, M.: Evaluation of carbonyl sulfide biosphere exchange in the Simple Biosphere Model (SiB4), *Biogeosciences Discuss.* [accepted], <https://doi.org/10.5194/bg-2021-192>, 2021.

Meredith, L. K., Boye, K., Youngerman, C., Whelan, M., Ogée, J., Sauze, J. and Wingate, L.: Coupled Biological and Abiotic Mechanisms Driving Carbonyl Sulfide Production in Soils, *Soil Syst.*, 2(3), doi:10.3390/soilsystems2030037, 2018.

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