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Comment on bg-2021-281

Anonymous Referee #2

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-RC2, 2022

Abadie et al. simulated soil COS fluxes on a global scale using a mechanistic soil model in a land surface model ORCHIDEE, and evaluated the simulation results, from both the mechanical model and an empirical model based on scaling soil respiration, against 7 sites in the Northern Hemisphere. Furthermore, an atmospheric transport model LMDZ was used to investigate the contribution of different soil flux products to the latitudinal gradient of atmospheric COS concentrations. Moreover, sensitivity analyses were performed to reveal the importance of various parameters, which is useful to understand the control mechanisms of the soil COS fluxes. Note that the mechanistic model has been previously developed and published. Nevertheless, implementing the existing model in ORCHIDEE to study the global soil COS fluxes is desired. This is a very nice model study. The paper is well structured and very well written and is certainly suitable for the journal of Biogeosciences.

As has been noticed by the authors, the available field observations of soil COS fluxes are very limited, which is especially true when global COS fluxes are the focus of the study. In fact, all 7 sites are located in a narrow latitude range of 42 – 62 °N, and do not cover a full seasonal cycle, which makes it difficult to evaluate the simulation results on a global scale, and raises many questions around whether the presented simulation results are justified, e.g., whether smaller global COS soil flux than previous estimates is trustworthy, whether very large emissions in part of the tropics exist, not to mention the validation of the seasonal cycle and the diel cycle of the simulated results. On the other hand, these are also nice topics to be followed on. For this manuscript, I strongly feel that these points need to be better clarified in the revised version before publication.

Regarding the selection of the field sites, why were the soil flux measurements in an agricultural field in the Southern Great Plains by Maseyk et al., 2014 not used?

L25: remove “budgets” after “atmospheric COS”

L37 specify the region of the tropics, otherwise, it sounds like high emissions in all tropical regions

L170: please briefly discuss why the steady-state condition is valid? what assumption has to be made to make the steady-state valid?

L457-458: Clearly, the mechanistic model predicts nearly no seasonal cycle except for large production signals in the summertime, as is shown in Figure 2. However, this implies that relatively large net soil uptake exists in northern high latitude in winter times, when the temperature can be rather low and the land is covered by snow. This makes me wonder what the applicable range for the parameters shown in the method section, e.g., the valid temperature range of f_{CA} in eq. 16, the valid temperature range for ð□□¼ and ð□□½ in eq. 17.

L471-473: Note that vegetation was also removed for the FI-HYY site, as is in Sun et al., 2018 "The moss layer or any other vegetation was removed to expose the humus layer inside the chambers." This contradicts the statement. If the assumption would be true, what would be the mechanism for artificially enhanced COS production?

L478: The diel cycles of simulated COS soil fluxes by the mechanistic model shown in Figure 3 are totally not supported by the observations. Note that when relatively large uncertainties in the observations are considered, a minimum net soil COS uptake in the observations is not significant at all. Actually, the large discrepancy calls for a better understanding of the mechanistic model: what causes the diel cycles in the model but not shown in the observations.

L568: Section 3.1.5, although it is nice that the authors have made an effort to optimize soil COS flux, it may be premature. As the results are not used in the following results, I suggest leaving this section out or putting it into the Appendix.

L645: Section 3.2.2 Temporal evolution of the soil COS budget. It is expected that oxic soil COS sinks would decrease when atmospheric COS concentration decreases, and one could even expect that the decrease is, to the first order, proportional to the decrease of COS concentrations. However, the sharp decrease from 2016 is far beyond this. What are then the main reasons that can explain the sharp decrease in the mechanistic model?