

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2021-192

Anonymous Referee #2

Referee comment on "Evaluation of carbonyl sulfide biosphere exchange in the Simple Biosphere Model (SiB4)" by Linda M. J. Kooijmans et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-192-RC2>, 2021

The paper presents an update of the modelling of the carbonyl sulfide biosphere fluxes within the SiB4 model. The soil model of Ogée et al. (2016) is implemented and a spatially and temporally varying COS atmospheric concentration is considered. This latter modification is shown to have a large impact. The new models are evaluated against field observations and a revised budget for COS is given at global scale, reducing the missing source. The authors make valuable recommendations, with a fine study on the alpha parameter of the COS vegetation uptake model.

The paper is well built and well written, with clear figures, and represents an important new contribution to this research field linking COS uptake and GPP.

I have only minor comments and some requests for clarification.

Page 2, line 65: "the Lund-Potsdam-Jena model (LPJ) and the Community Land Model (CLM4) (Launois et al. 2015a)" -> Launois et al. cannot be a reference for a possible implementation of biosphere COS exchange for LPJ and CLM as, as you further state in Table 1, they only scaled the GPP using a leaf relative uptake approach to estimate vegetation COS fluxes.

Page 3, line 95: Vesala et al., in prep -> Vesala et al., 2021. They mention that "e is the original e multiplied by 2.1, the average ratio of Hyytiälä and SiB4 LAI data". How do you reconcile having a factor 2 on LAI with however correct simulations of GPP (Figure S3a) and COS fluxes (Figure 2a)?

Page 7, lines 177-178: "These numbers were later updated to alpha = 1400 and 8862 for

C3 and C4 species, respectively, after updates were made to the SiB model." -> Could you explain a bit more what were these updates and whether these parameters were recalibrated against measurements?

Page 8, lines 198-199: "These effects of nutrient fertilization on soil COS exchange were initially not simulated in the SiB4 model." -> Do you mean they are simulated now?

Page 9, lines 226-228: "We chose the tortuosity functions of Deepagoda et al. (2011) for air and Millington and Quirk (1961) for water, as these functions do not require a pore-size distribution parameter, which facilitates its implementation in SiB4." -> Do you mean you have a constant porosity per soil type?

Page 9, line 233: "kuncat varies with soil pH" -> How is soil pH prescribed/computed in SiB4?

Page 9, equation (6): Is the production term valid for both oxic and anoxic soils?

Page 11, line 284: "CO₂ mole fractions were held constant at 370 μmol mol⁻¹ during spinup and simulations" -> Why don't you at least use varying annual means to get the CO₂ fertilization effect in the simulations?

Page 11, lines 289-290: "To compare SiB4 with site observations (listed in Table 3), we run the SiB4 model with 3-hourly output for only the grid cells (at 0.5° x 0.5° resolution) in which the sites are located" -> Why didn't you use the local meteorology available from the FLUXNET, ICOS or AmeriFlux sites? This comment is also valid for your remark Page 15, lines 384-388 that the model temperature (from MERRA?) in 2012 was higher than the observed one at the US-ARM site.

On the same order, this means you are using a soil type defined at a 0.5° spatial resolution, what if the local soil type is different?

Page 12, lines 316-317: "Ecosystem fluxes are corrected for storage of COS in the canopy airspace using collocated canopy COS profile measurements when available (FI-HYY and US-HA1)." -> Can you explain this a bit more?

Page 21, lines 544-545: "922 ± 11 Gg S yr⁻¹ over the years 2000-2020" -> How do you compute this uncertainty? Is that the interannual variability?

Figure S7: The diurnal cycles of COS soil fluxes seem to be often in opposite phase between observed and simulated (for ES-LM1, AT-NEU notably). Do you have an idea why?