Biogeosciences Discussions

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Comment on bg-2022-210

Anonymous Referee #1

Referee comment on "Technical note: Novel estimates of the leaf relative uptake rate of carbonyl sulfide from optimality theory" by Georg Wohlfahrt et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-210-RC1, 2022

This technical note proposes a new theoretical approach to provide estimates of the Leaf Relative Uptake (LRU) of carbonyl sulfide (COS) with respect to CO_2, along large-scale bioclimatic gradients. It is based on plant optimality and coordination hypotheses. The LRU is useful to estimate biosphere COS fluxes based on gross primary productivity (GPP) and is often used for atmospheric inversions against COS atmospheric concentrations. The paper is well built and well written, with a literature review quite up to date, and clearly defined objectives. Plus, the derived LRU maps, as well as the scripts, are made available on a repository, which is quite commendable.

This study will be of interest to the whole COS community. The new estimates are intriguingly quite low as compared to previous ones, this will most certainly fuel interesting discussions to understand why, and what the consequences are for the biosphere COS and GPP budgets, for the closure of the global COS budget and for atmospheric inversions. As often in the COS field, the authors advocate for more in situ observations in more biomes, needed to correctly evaluate the predictions of this new framework. I recommend the publication of this study and have only minor comments.

L63: The P-model is applicable only to C3 plant species. The authors should add something in the legend of Figure 2 or mask grid cells where C4 plants are predominant.

L68: The authors could add a short analysis to quantify the sensitivity of LRU to the beta parameter. Wang et al. (2017) indeed show that beta (with a former slightly different

formulation) varies when they account for the mesophyll conductance, and they also suggest that beta is assumed a constant but could be varying with plant functional traits.

L80-81: c*=0.41 seems to be based on two numbers (Jmax/Vcmax = 1.88 and chi = 0.8) following Stocker et al. (2020). Stocker et al. (2020) also mentions that Smith et al. (2019) use another Jmax modelling. Again, as stated by Wang et al. (2017), c* could vary with functional traits and the authors could add a sensitivity analysis of LRU to c*.

L92: "Kooijmans et al. (2019; only data from chamber #1 were used)": is there a specific reason why the data from chamber #2 were discarded from the validation?

L104: "data were filtered for PAR > 1000 µmol m-2 s-1". Could this (partly) explain why the authors find lower LRU values, as compared to estimates by land surface models that calculate mean LRU values over all PAR conditions? Could the authors quantify the effect of this filtering?

L110-112: This part is not crystal-clear, and neither is the corresponding argumentation in Stoker et al. (2020). Yet I believe it is fundamental to explain what is leaf-level and what is canopy-level, and what information is exactly put in the ð□□□$_0$ in this study (as opposed to the P-model version). Plus, later the authors indeed compare their results both with leaf-level observations and with canopy-level LRU estimates from land surface models. The authors should detail and clarify this section, and maybe say something on how a canopy-level LRU compares with a leaf-level LRU, is one systematically higher than the other?

L129-131: The authors mention a large correlation across plant functional types between

the LRU of this study and the ones derived from the ORCHIDEE land surface model. I guess this is expected as both approaches are using very similar models driven by meteorological fields. The figure seems to show that the difference is not constant but proportional to LRU. A scatter plot with a regression line could help in this analysis.

Typos

L63-64: hypotheses (plural, twice)

L83, L101: The letter chi is (erroneously?) used instead of c for the ratio of concentrations.