Biogeosciences Discussions

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Reply on RC1

Georg Wohlfahrt et al.

Author 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-AC1, 2022

We thank reviewer #1 for his/her comments - please find out reply to each comment below starting with "R: ...":

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.

R: Figure 2 will be modified to indicate the presence of C4 plant species.

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.

R: will include a sensitivity analysis of LRU with regard to the beta parameter

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*.

R: the c* parameter affects both Vcmax and Jmax (see Eqs. 6 and 7 in the manuscript); a reduction in c* increases Vcmax and thus (remember that the model assumes co-limitation by RUBISCO and electron transport) GPP and thus, because Ci is unaffected by c*, causes a proportional increase in gs; since gi is proportional to Vcmax via Eq. 8, the ratio of gs/gi in Eq. (3) remains unaffected by changes in c* - in other words: LRU is not sensitive to changes in 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?

R: no specific reason … Figure 1 and the corresponding text will be updated to include both chambers of the Kooijmans et al. (2019) study

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?

R: yes, the filtering for light-saturated conditions may explain the higher values of Seibt et al. (2010) and Whelan et al. (2018), as the data underlying these studies were not filtered for radiation; with regard to the comparison to the results by Maignan et al. (2021), the bigger issues is a difference in scale, as their values are integrated over the depth of the plant canopy and thus, because of the decrease in radiation and VPD, should be higher; the text will be updated to include a discussion of these issues

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?

R: the entire methods section will be re-organized and partially re-written in order to convey more clarity; it should then be clear that model simulations represent the leaf-scale only and in the one case when we compare against the canopy-integrated LRU simulations by Maignan et al. (2021) we will discuss the issue of leaf-to-canopy scaling and why canopy-scale LRUs must be expected to be higher than at leaf scale;

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.

R: as suggested, a scatter plot will be added to Figure 3, which shows a positive relationship between bias and LRU – this finding will be discussed in the updated text; while both models are driven by presumably similar meteorological fields, the stomatal conductance model used in ORCHIDEE, a variant of the Ball-Berry-Woodrow model, is quite different and we will modify the text in order to emphasize this point

Typos

L63-64: hypotheses (plural, twice)

R: will be corrected

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

R: will be corrected