Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-381-RC1, 2020

Interactive comment on "Carbonyl Sulfide: Comparing a Mechanistic Representation of the Vegetation Uptake in a Land Surface Model and the Leaf Relative Uptake Approach" *by* Fabienne Maignan et al.

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Received and published: 12 November 2020

Maignan and co-authors present a simulation study using the ORCHIDEE model comparing a mechanistic formulation of the leaf COS uptake with the established LRU approach. They do so by confronting site-scale simulations with eddy covariance flux measurements, but also simulations at global scale and then compare against measurements of COS concentrations at globally distributed atmospheric measurement sites. While they find that at global and monthly scale, the difference between the two approaches is a minor issue compared to other processes in the global COS cycle,

they provide interesting evidence into the spatio-temporal variability of LRU and the performance of the mechanistic model in reproducing observed physiological behavior at two field sites. The paper is mostly well written (where it is not this can be left to the final copy-editing) and certainly within the scope of BG and presents important progress beyond previous work using similar approaches. I am really intrigued by Fig. 8, which clearly demonstrates that there is substantial variability in LRU which we need to better understand and represent if we are to further develop COS into a defensible proxy for GPP. Most of my detailed comments are probably minor and should be straightforward to deal with.

The one comment that may require more thought and in fact some additional work by the authors relates to text around Fig. 2 and Table 4, where the authors attempt to disentangle the drivers of simulated changes in gs_cos and gi_cos. Given the high non-linearity of the underlying equations I find Fig. 2 to be highly uninformative and wonder how well the simple partial regression analysis presented in Table 4 is able to capture the underlying highly non-linear interactions. In my view this analysis, which is another of the highlights of this study, would require a different approach, something like a Global Sensitivity Analysis or similar (e.g. Pianosi et al. 2015), in order to properly account for the many highly non-linear interactions. Given that ORCHIDEE is quite a complex beast of a model, not sure how feasible such a GSA is given the need to execute a large number of model evaluations.

One more comment, or maybe rather a suggestion, regards the LRU concept, which has to be understood as the simplest possible, but still process-based, closure possibility in order to use measurements of the COS flux to estimate GPP. In choosing some LRU value, implicit assumptions are made about the ratio of conductances, which is dealt with in this paper, but also about the Ci/Ca ratio (Seibt et al. 2010, Wohlfahrt et al. 2012). This has always been a major criticism of the LRU concept, as it amounts to ignoring the demand side of the CO2 diffusion equation. I am thus wondering whether the authors could make a useful contribution here by expanding their analysis to look into the spatio-temporal Ci/Ca ratio, given that Ci must be calculated by their mechanistic model, and confront these with our understanding of how Ci/Ca varies across the globe and seasonally.

Detailed comments: l. 82-83: fluxes and mixing ratios in μmol/m2s and μmol/mol, respectively, result in tiny numbers – why not use units of pmol/m2s and pmol/mol which are typically used in practice? l. 85: . . . if Fcos, [CO2]a and [COS]a are available . . . l. 85-96: this is all well-known – the real issue in my view is that we are seeing a large spread in LRU values even after normalizing for PAR that we cannot explain; also note that Yang et al. (2018) clearly show that low-light periods, when LRU deviates most, typically contribute little to the daily and longer-term integrated GPP l. 95: and possibly other factors we do not yet understand l. 97-100: this is essentially a technical objective – can the authors come up with an objective that aims at providing an advancement in scientific understanding and/or even some hypotheses of what they believe will result from adopting the mechanistic as opposed to LRU approach? l. 139: ". . . is generally not produced by plants (but see Gimeno et al. 2017)." l. 140: suggest to remove "to heat and " as this sounds as if the boundary layer conductance to heat would affect the flux of COS l. 164: the seminal paper to nighttime transpiration and residual conductance in my view is Dawson et al. (2007) l. 173: does the model then also calculate H2O gas exchange at night or only COS? l. 195: What does "Vegetation COS flux direct or derived measurements . . ." mean? Suggest to reformulate Eq. (4) and (5): what about some statistics which allow evaluating systematic biases? l. 226: this is the first citation of a table and thus table number should be 1 l. 310: wouldn't it make sense to compare modelled and measured canopy-scale COS fluxes before embarking on a detailed analysis of the underlying processes, i.e. move sections 3.1.2.1 and 3.1.2.2 here? Fig. 1: what is the area reference for the units – m2 leaf area or m2 ground area? l. 336-340: alpha values depend on Vmax, which I suppose changes with phenology? If so, would it make sense to also investigate Vmax? l. 346-347: I would turn around the argumentation here, i.e. say that due to the way of how gs is simulated according to Yin and Struik (2009) there is a linear relationship with A ... l.

354-355: but isn't Vmax strongly driven by phenology as well and wouldn't this be the main seasonal driver? Fig. 4: it looks like there is a data gap in May 2013, but the dashed line is not broken and instead interpolates through the gap, compared to 2012 when the dashed line stops during all gaps l. 395: there also appear to be issues in terms of phenology, e.g. in April 2013 l. 396: can the authors estimate the magnitude of the noise (I presume they refer to the random variability of EC flux measurements) and whether this noise could be responsible for the observed variability? l. 403: I would replace "diurnal" with "daytime", which I believe better fits with what the authors mean Fig. 6: what do the model simulations of LRU refer to – the canopy scale? If so what do the PAR values refer to – the PAR incident at the top of the canopy? If so then I would say that there is a scale issue affecting the comparison to the branch-chamber measurements, as the canopy LRU is the integral over all leaves, both sunlit and shaded, which experience very different PAR values; having said this, I however would expect canopy simulated LRU values to lie above the measurements at low PAR, as when PAR incident on the top of the canopy is say 200 μmol/m2s, a large fraction of the leaves will be in the shade and experience much lower PAR values and should thus actually have a higher LRU; in any case the authors should critically reflect on whether any scale issues affect the interpretation of the comparison between measured branch-chamber and simulated, presumably canopy-scale, LRUs Fig. 7 and 9: appropriate x- and y-axis scale and text needed Table 5: can the authors add the time frame to the table to which the different studies refer to? l. 459: and you use the same parameterization of COS uptake?! Fig. 7: can the authors avoid the figure legend to overlap with the plotted lines? l. 546, 549: isn't Barrow in Alaska/USA? l. 561: to what does the citation Whelan and Seibt refer to? l. 598-599: agreed, but it should be acknowledged that at present the spatial coverage of biomes is very poor (some biomes not having been sampled at all, for others n=1) and also temporal coverage is poor, with the exception of Hyytiälä and Harvard forest most COS flux measurements being confined to less than a year, leaving a big question mark with regard to inter-annual variability l. 603-606: unclear what this sentence tries to say l. 633-634: can you elaborate on which data would be required to that end? l. 645: nighttime instead of nitghttime l. 666: here the authors might refer to some recently published experimental canopy-scale LRU estimates, such as Spielmann et al. (2019, 2020) who provide estimates for deciduous forest, savanna, C3 crop and C3 grassland l. 669: another issue, mentioned first in Wohlfahrt et al. (2012), is that leaf chambers are typically vigorously ventilated and LRU values inferred from these are thus implicit with the correspondingly large boundary layer conductance; under real-world conditions boundary layer conductances vary with wind speed (temporally but also with depth of the canopy) and transport across the boundary layer may not necessarily be dominated by forced convection as in leaf chambers; that is to say that for this reason leaf-chamber LRUs may not be easily transferable to real-world conditions l. 678: in this section you might cite Spielmann et al. (2020) who show, with a simple linear perturbation analysis, that variability in [COS]a does in fact affect LRU

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