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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.

Anonymous Referee #2

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Maignan et al., develop a parameterization for including OCS uptake within a well-known land surface model (ORCHIDEE). Since OCS has been proposed as a proxy for GPP (currently not observable at large scales), this work is an important step towards estimating global GPP. The authors do a very thorough job of combing the OCS literature for published values of leaf relative uptake (LRU) and validating modeled uptake at two temperate sites where OCS flux is measured. I think this work should eventually be published in Biogeosciences. However I am currently a little confused with regard to how the paper is framed. The authors pose their formulation of OCS flux

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using a conductance based approach against the well-known LRU based relationship between FOCS and GPP. The LRU approach is also based on conductance, with simple assumptions regarding the relative role of mesophyll and stomatal conductances (Seibt et al., 2010). Thus the differences in flux resulting between the two approaches are not surprising, and don't really manifest themselves once transported and compared against OCS flask measurements from NOAA. This is an interesting finding, but I think the authors must focus on a more scientific question.

The other main comment I have is about modeled gs,ocs and gi,ocs. I agree with the authors that the role of gi is important and often ignored, but I am somewhat skeptical of the large diurnal variation in modeled gi, which is related to temperature. As I understand, gi is estimated from Vmax of Rubisco, but Rubisco response to temperature is not thought to be that large, particularly at temperatures observed at the temperate NH sites (see Sage and Kubien 2007). Moreover, while estimates of diurnal variability of mesophyll conductance hasn't been reported much, a recent study showed that the diurnal variability in gm (which is similar to gi in this study) is much smaller than gs (Strangl et al., 2019; which is also a high latitude coniferous forest, so somewhat similar to Hyytiälä). This could serve as a mechanism for plants to modify (increase) water use efficiency and therefore continue assimilating carbon even as gs declines due to high VPD commonly observed after mid-day (see also Buckley and Warren, 2014). See also recent work by Gimeno et al., (2020). Thus, I am quite surprised that at Hyytiälä it seems like gas exchange is most often limited by gi and not gs. I believe some sensitivity analyses could be done with regard to the formulation of gi. The temperature dependance of gi is also not uniform across plant species (von Caemmerer and Evans, 2015), with obvious implications for the global formulations presented here. I think a more in-depth discussion of the implications of gas exchange most limited by gi should be added, replacing the current and mostly qualitative discussion currently.

One way to understand the relative roles of gs, gi, and the differences between the mechanistic model and the LRU approach is to look at values of intercellular and chloro-

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plast CO2 concentrations (ci and cc respectively). These should be standard outputs of the model. ORCHIDEE ci and cc could be compared with "inferred" ci and cc using modeled A (GPP) and gi,cos and gs,cos following Seibt et al., 2010. It would be interesting to see if those differences can explain the difference in FOCS flux obtained from those two approaches (for e.g., in Figure 9).

In general, I find the manuscript too long and perhaps some figures (e.g. Figure 2) can be removed. Similarly, perhaps figures 3 and 4 can be combined. I greatly appreciate all the work that has gone in to this, but perhaps the authors ought to split this in two papers. One that describes the modeling framework in ORCHIDEE and another that focuses on the transport modeling? This would allow the authors to delve more deeply in the important findings such as Fig 8. Similarly, I can imagine that Fig 11 discussion can be greatly expanded upon.

I find it a little odd that LMDZ doesn't match the seasonal cycle of CO2 at MLO. Is. this a known issue of the transport model? In summary, I am not entirely convinced of the transport analyses since inferences could be flawed due to erroneous transport. Some quantification of transport error/uncertainty should be presented (perhaps using withheld/independent observations?).

The writing in the results section is often very qualitative and not informative (e.g., line 329: "The conductances drop in the afternoon to reach minimum values at night"). I find most of the discussion unnecessary. It seems like a re-hash of the methodology and results section (e.g., Sec. 4.1.1. and 4.1.2), with inferences that I don't think are always supported by the results (for instance lines 653-656. This is an example where the writing could be improved to make a very compelling argument about the use of OCS flux). Some of the work suggested (e.g in Sec. 4.1.3.) is well within the purview of this study and can be performed (ie., some sensitivity analyses of min gs and gi, and impact on simulated flux.).

The writing overall really needs to be tightened and the conclusions seem a bit weak.

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This could be improved with better framing. Thus I believe that with a much clearer presentation of figures and text, this could be a much more compelling manuscript.

Comments about figures and tables: Figure1: Conductance seems to be highest at 12 am ie., midnight? Growing season midday at Harvard Forest is shown to be limited by gi but Wehr et al (2017) measurements show that gs is the limiting conductance to OCS transfer. How do these numbers compare with Wehr and Kooijmans measurements (of gi)? You could also add total conductance to OCS (the quantity multiplied to [OCS]a in eq 3). In general, since you don't seem to be explaining month by month variations, maybe compress these to show 3 month means (e.g. JJA, SON etc)? Currently, it is impossible to see in detail the diurnal variations, specially when one is trying to discern at what times of day gi > gs and vice versa.

Figure 2. I believe this figure should be replaced by scatterplots or a table listing correlations between gs, gi and aux variables (which already exists as Table 4). Why would gi be expected to scale with soil moisture? It is strange that PAR is highest in May at Hyytiälä.

Table 4: gs is more related to Tair than to VPD. Stomatal conductance has been shown to be related to VPD (See for e.g., Oren et al., 1999). This would be worth examining.

Figure 4. Doesn't seem that Harvard Forest 2013 observed fluxes match simulations all that well. For instance, fluxes seem to peak in May-June and Aug-Sep in 2013, but peak in July in the model. There is a mention of "noise" in EC based measurements in the text but these should be quantified or at least described.

Figure 5. It would be easier to view this figure as two separate panels instead of one plot with two y axes.

Minor Comments: Note, I stopped providing minor comments because of the length of the manuscript.

Line 70: remove "then" in COS is then hydrolyses

Line 77: mention that soils can be a sink or a source, and cite appropriate studies (cite appropriate studies e.g., Maseyk et al, 2014; Berkelahmmer et al, 2014; Kitz et al., 2020). Also mention the role of epiphytes in the OCS budget (and cite Kuhn et al., 2000; Gimeno et al., 2017 and Rastogi et al, 2018) .

Lines 78-85: Add some discussion about possible scaling issues for LRU from leaf to canopy and ecoregion scales. I think some of this framework can be found in Wohlfart et al., 2012

Line 90: summer is likely only valid for ecosystems that exhibit seasonality. Explain diurnal and seasonal variability in LRU.

Line 95: What do you mean by 'time'?

Line 112-13: remove these lines.

Line 121: Briefly describe improvements in the Farquhar model.

Lines 208-210: Provide scientific names for these species at Harvard Forest.

Lines 218-220: Awkward phrasing. Also, please elaborate what you mean by "when possible".

Line 339: change 'air surface temperature' to 'air temperature'.

Line 339: Very minor comment: 'modelling' and 'vapor'. The first is a "British" spelling and the second is "American". Please pick one and be consistent throughout.

Line 348: This isn't true based on Fig 2 as PAR peaks in May but gs in June at Hyytiälä.

Lines 356-358: Needs citations.

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