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Biogeosciences Discussions

Interactive comment on "Stomatal control of leaf fluxes of carbonyl sulfide and CO_2 in a *Typha* freshwater marsh" *by* Wu Sun et al.

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Sun et al. present here the first field dataset for COS and CO2 leaf relative uptake (LRU) collected in situ during continuous measurement over the peak of a growing season. The authors chose a typical wetland plant (Typha latifolia) and report continuous measurements of CO2 and COS uptake under varying environmental conditions, mainly light (photosynthetically active radiation) and vapour pressure deficit (VPD). They demonstrate that the strong dependency of LRU with PAR observed under laboratory conditions is also observed under natural conditions. The authors explain that strong stomatal control of both processes (COS and CO2 uptake) underlies the observed patterns. Interestingly, the authors report lower LRU values in natural conditions than those previously measured under laboratory conditions. This constitutes a

very valuable contribution as it is the first dataset of LRU collected at the leaf level, in situ, under natural conditions and for more than a month. The paper is very clearly written and the figures and results are nicely presented.

Yet, I do not believe that this paper deserves to be published as regular 'research paper', rather these results would be more appropriately presented as a technical note or rapid report. The reason is that the authors report data from one chamber that measured continuously one single set of six leaves. Their results and conclusions are relevant as they constitute a strong proof of concept, but continuous measurements over a period with limited climatic variability (one campaign with homogenous meteorological conditions, P6L11-15) on a single set of leaves from the same plant (presumably) are not sufficient to constitute a whole research paper. Even more so taking into account the environmental heterogeneity of the light environment (P6L19-21).

In addition I have some major technical concerns and another major concern related to the result interpretation and theoretical framing of the study.

In the methods, the authors claimed that they used a 'flow-through (dynamic) chamber' (P4L5). Yet, during the 5-minute measurement period the chamber acted as a static enclosure (P4L13) and the authors calculated COS and CO2 uptake and transpiration from the slope of the progressive drawdown (or accumulation) of the different species over time (P5L4-15). This approach should be valid provided good mixing, but that is hard to achieve for a >10 L chamber without a fan. The authors need to provide the flow rate entering the cuvette over the measurement period and discuss to what extent they can warranty thorough mixing inside their chamber. In addition, the authors report that they characterised the blank fluxes of (presumably) the same chamber and that these were negligible (methods P4L27). Still, they do not specify how often were these characterised and under what conditions. More important, although it is not stated specifically, it appears that they authors calculated transpiration rates from H2O vapour concentration measured with the QCLS. If that is the case, I assume the authors did not operate their QCLS coupled to a Nafion drier (or other type of water trap) and

thus they need to correct for the interference associated with the water absorption line (Kooijmans et al. 2016 Atmospheric Measurements Techniques 9:5293-5314).

The authors claim that simultaneous stomatal control of both CO2 and COS uptake underlies the coupling between these processes and the changes in LRU observed under low light and high VPD. Indeed, stomatal control lies at the heart of the discussion and the theoretical framing of the paper, but no data are shown. Also, the authors claimed that they monitored leaf temperature (P4L29-30) and they also had transpiration fluxes (Figure 3c). Still, no calculations of stomatal conductance have been performed. Later in the discussion, some calculations of stomatal conductance are mentioned (P8L20, P9L24), but the authors do not detail how these were obtained. Given that the authors have all the ingredients to calculate stomatal conductance, but yet these are missing, I wonder if this is due to poor mixing inside the chamber, which would have affected all other measurements. This needs to be clarified. In addition, comparing estimates of stomatal conductance derived from COS-uptake measurements with independent quantifications of stomatal conductance from transpiration and leaf temperature would allow to further demonstrate the tight stomatal control of COS uptake. These issues need to be clarified. In addition to these major issues, I have some additional concerns:

Title Remove the term 'Stomatal control' unless you decide to include stomatal conductance measurements, otherwise I suggest "Effects of light and vapour pressure deficit on the coupling of leaf fluxes of carbonyl sulphide and CO2 in a Typha freshwater marsh under natural conditions", or something similar.

Abstract P1L3-4 I think, here, you could be more specific with respect to what we have learned so far: 'LRU is known to increase under low light' P1L15-17 reduce the emphasis on the role of stomatal control.

Introduction This section is interesting and very clearly written. P2L2-10 maybe consider shortening this section, these concepts have already been amply discussed in the literature. P2L2 'COS has been shown to be a unique tracer' P2L8 'The approach

to estimate photosynthesis from COS fluxes' P2L12 'COS and CO2 follow the same diffusional pattern' P2L22 'environmental variables that regulate diffusional limitations, mainly stomatal conductance, including photosynthetically active radiation (PAR) (. . .) and vapour pressure deficit (VPD)'Also provide a citation here (e.g. Leuning 1995 that you already cite). P2L25 'In contrast to the CO2 flux, at night, COS uptake might continue. . .' P2L27-28 'Night time COS uptake has been observed..' L29-31 This is not entirely clear. I think here what you mean is that the cited studies inferred vegetation COS uptake from ecosystem-scale measurements instead of direct measurements. Also, please note that both Maseyk et al. (2014) and Commane et al. (2015) found not only evidence for COS uptake, but also emission, this should be briefly mentioned here. P3L16 'We need direct measurements of how LRU. . .' P3L19-23 Pleas try to specify the research objectives more clearly, or even better formulate two hypotheses (e.g. LRU will decrease under low light in natural conditions) instead of stating the questions that motivated the study.

Methods This section is also very clear and nicely written, but some key details are missing (see major concerns above). P4L5-14 Could you please provide a schematic drawing of the gas-exchange chamber? P4L29-30 Where are the data for leaf temperature? P5L17-18 'Conspicuously unrealistic data points in the meteorological data were removed.' P5L18 'independent criteria to filter measurements' P5L21 'were also discarded' P5L23 'these filtering criteria' P6L2 so if LRU was only calculated during the daytime, why do present the 24-h mean LRU in figure 6?

The results section is very clear and I only have one minor comment: no need to repeat the definition of LRU (P7L1).

Discussion and conclusions In my opinion, this section turned out to be the least interesting of the paper. It is nicely written, but it only consists on a mere repetition of the results and ideas previously presented in the introduction. It can be shortened significantly and I believe the results and discussion section should be merged into one, which would be a much more adequate format for a technical paper. I provide some

further specific details below. P7L19 Provide a citation to support light-independency of COS hydrolysis. P7L17-26 This paragraph is a long compilation of ideas presented already in the results and in the introduction. P7L27-P8L2 this paragraph belongs to the results section. P8L20 provide the details for these calculations in the methods. P9L6-10 Shorten this section, most of these ideas are repeated elsewhere in the paper. P9L16-22 Again repeated ideas, this belongs to the introduction. P9L23 The discussion is not a section appropriate for introducing new equations, move this to the methods. P9L24 detail how this was calculated in the methods. P10L8-10 this discussion on the variation of LRU among species is very relevant. Note that T. latifolia has a very particular physiological behaviour, often exhibiting very high rates of carbon uptake (e.g. Yavitt & Knapp 1998, 139:495-503 or Jespersen et al. 2017 Functional Plant Biology 44:774-784). Thus it is not surprising to find lower LRU value than those previously reported for other plants. Maybe also consider comparing your measurements of leaf CO2 uptake with previous measurements as you seem to have measured much lower values than those previously reported, although this might be simply due to the differences in environmental conditions among studies, most likely light environment. P10L11-24 I do not think it is relevant to discuss the differences between day-time LRU and 24-h averaged LRU. The parameter LRU is useful to estimate GPP from COS uptake, thus it is only relevant during day-time. Please remove this section and the corresponding values from figure 6. P11L2 remove 'that is only stomatal conductance limited'. P11L4-6 rephrase, are 'midday' and 'early afternoon' the same? Because you use them interchangeably here! P11L7-9 I am not quite sure I understand the logic behind this statement. In the afternoon, presumably, PAR does not limit stomatal opening, instead stomatal opening would be limited by high VPD and thus COS and CO2 would both be constraint and hence LRU would not respond to VPD. In fact, I cannot appreciate a change in LRU at midday in figure 3d. I think this conclusion might be a bit misguided by an earlier interpretation of the measurements. P11L10-15 this should be the opening, not the closing paragraph of the discussion.

Figure 4. The data points do not appear colored.

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