

Interactive comment on “Stomatal control of leaf fluxes of carbonyl sulfide and CO₂ in a *Typha* freshwater marsh” by Wu Sun et al.

M.E. Whelan (Referee)

mary.whelan@gmail.com

Received and published: 3 January 2018

Leaf relative uptake (LRU) of COS and CO₂ is a parameter that is often used to estimate plant CO₂ uptake from observed ecosystem fluxes of COS. There are other sources and sinks of COS in ecosystems, though they are typically small compared to uptake through plant stomata. One important exception is wetland soils, which tend to be a relatively large source of COS. In non-wetland or agricultural systems, measurements of net CO₂ and COS concentrations and fluxes are sufficient to make an estimate of GPP with an approximation of LRU.

Here, Sun et al. present a dataset of H₂O, COS, and CO₂ flux and concentration measurements from a single leaf chamber in a wetland over about 36 days. This type of

C1

data is an important contribution and will be undoubtedly useful for other studies. However, the interpretation would be aided by greater attention to stomatal conductance, as the title implies, rather than LRU.

The trouble with focusing on LRU is the matter of scale and applicability. Work by Hilton et al. (2015) demonstrated that, for regional GPP estimates, LRU is not the most important source of uncertainty. On the leaf scale, a direct measurement of CO₂ uptake can be made, though it includes photorespiration. At the tower-level scale (1 km²), I am not sure that COS-based GPP estimates are more accurate than recent approaches relying on CO₂ measurements alone, though the Wehr et al. (2017) study in a temperate forest demonstrated COS-based estimates of canopy stomatal conductance were consistent with other measurement approaches in that system. In short, LRU is not the most important question on large scales, not employed in and of itself on leaf scales, and has some applicability still under development at the site scale. While having a better description of LRU variation with PAR would be an improvement, it is not the urgent next step that the text here describes.

The second issue is applicability to other ecosystems. This dataset was collected from a chamber containing leaves of a plant typically found in wetlands. The COS-GPP tracer technique is not usually applied at the site level in wetlands because of often substantial COS production from wetland soils. Also, some wetland plants have interesting adaptations to tolerate suboxic soil environments. For example, *Typha* have well developed aerenchyma to allow oxygen to diffuse into the root zone. Aerenchyma can also transport reduced gas compounds to the surface, circumventing oxidation in the water column. This has been shown for methane and Whelan et al., (2013) suggested a similar route for carbonyl sulfide. The data do not necessarily show COS release from the parts of the leaves enclosed in the chamber, but teasing apart uptake from other sources of COS in the system would probably be a challenge. It is confusing to carry out an LRU study in one of the few ecosystems where applying LRU to back out GPP is an exception to the simplicity of the approach.

C2

While using LRU is probably the most popular method of calculating GPP from COS measurements, it is not the only method. The SiB model, for instance, has a “mechanistic” uptake representation that does not rely on an LRU number. The applicability of COS measurements to carbon cycle studies does not depend solely on LRU.

Motivating this study interpretation with the vagaries of leaf conductances would be of greater interest. Already, Sun et al. show that nighttime stomatal conductance is occurring and that daytime conductances change with evaporative demand. Sect. 4.2 should be expanded to include the broader literature on nocturnal stomatal conductance, rather than restricting the discussion to focus only on COS studies. Graphically comparing an established method to the COS-based method of estimating stomatal conductance could reveal possible mismatches and highlight the strengths of each approach, even if leaf temperature was not measured precisely. Re-working the figures to this effect would be beneficial.

Small technical concerns include publishing chamber blank results and also the exact equation that was used for the QCL water correction. There are a growing number of researchers using this make of QCL and water is a problem for the older models.

In short, this is a good dataset, but the interpretation could perhaps avoid the concept of LRU entirely.

Sincerely,

Mary Whelan

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-431>, 2017.