

Interactive comment on “Observations of the uptake of carbonyl sulfide (COS) by trees under elevated atmospheric carbon dioxide concentrations” by L. Sandoval-Soto et al.

Anonymous Referee #1

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This paper discusses results from a long-term study of trace gas exchange over two different tree species under ambient and elevated CO₂ concentrations. These unique results are potentially interesting for the scientific community to project the influence increasing ambient CO₂ concentrations might have on processes such as evapotranspiration, CO₂ exchange, and trace gas exchange of other gases such as COS. For trace gases predominantly removed from the atmosphere by uptake through plant stomata (particularly COS), changes in lifetimes and environmental impacts are potentially significant. Unfortunately, I have many concerns with the current manuscript that I believe need attention before this paper could be published.

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*The conclusions drawn by the authors don't appropriately represent the ambiguity in the results obtained. The authors approach this study with expectations on how the exchange between COS and plants should be affected by increased CO₂ concentrations. These expectations are reasonable given our understanding of this system, but the important question regarding this manuscript is whether the new data add to the evidence supporting those assertions and how those results are interpreted by the authors and conveyed to the reader. Unfortunately, the signals they measure in most respects are not robust so that the expected relationships do not appear to be generally confirmed by the new data. The authors' results do show that CO₂ depositional velocities increase by factors of 2-4 when the trees were grown for an extended period at 800 ppm vs 350 ppm CO₂ concentrations. The conductance data, however, only fit expectations half of the time (conductance reduced under elevated CO₂). Expectations suggest that enzyme activity (carbonic anhydrase only, as it is relevant for COS) should be important for interpreting trace gas results, but those measurements do not clarify the situation for CO₂ or COS (none of the measured activities were significantly different under the two CO₂ concentration environments). Finally, in the case of COS uptake, the difference signals are typically not significant ($p > 0.05$ in 4 of 6 measurement periods). Where the differences are significant it seems that they are on the order of a 15-20% increase in deposition velocity. As a result, the paper is full of discussions of differences that are explicitly stated as not being significant and the primary conclusions don't adequately reflect the ambiguity in the results. Comments regarding expectations and possibilities are included in the conclusions where the data don't robustly add support. I think an accurate conclusion section would convey the ambiguity in these results and their limitations in re-formulating our understanding of this plant-trace gas system. My take-away thoughts: expected signals are small compared to unexplained variability. Some speculations are provided to explain the anomalous observations, but these explanations seem arbitrary and, in one case, not supported by other information the authors present.

*The use of the compensation point concept seems misleading. A non-zero compen-

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sation point implies emissions of COS, yet the authors explicitly state that there was no indication of COS emissions from these trees. How should the reader reconcile these conflicting points? Whether or not there are COS emissions from vegetation is an important point for budget considerations for this gas. The use of a new term "virtual compensation point" doesn't add to this discussion, in particular because it isn't clear what it represents and it isn't used consistently throughout the paper. Does the flux go to zero at lower COS concentrations because the outlet COS concentration in the chamber is zero? If so, I think the model for using this concept becomes inappropriate.

*How is it that the fluxes are influenced by seasonality when the plants were kept at 25 C, consistent light intensity with 12 hours of daylight and 12 hours of sunlight (section 2.1)? The influence of seasonality is used in places as a speculative explanation for anomalous observations. This potential influence and the issue of non-concurrent measurements make me question the usefulness of this new data even in a revised manuscript.

*Indirect inferences made here regarding the potential for competitive uptake by vegetation between COS and CO₂ are in direct contrast to a study that explicitly measured this influence (Stimler et al). It is hard to determine if the data and analysis presented here add significantly to the discussion of this topic, though given the indirect nature of the approach taken here, my impression is that they do not.

Experimental details could be clearer. For example, how many individuals of each tree species were tested? How does seasonality influence the trees? What do the 2nd and 3rd columns represent in Table 8, coefficients or p values?

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