Biogeosciences Discussions, 2, S73–S76, 2005
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European Geosciences Union

Biogeosciences Discussions

Interactive comment on "Global uptake of carbonyl sulfide (COS) by terrestrial vegetation: Estimates corrected by deposition velocities normalized to the uptake of carbon dioxide (CO_2)" *by* L. Sandoval-Soto et al.

Anonymous Referee #1

Received and published: 11 March 2005

The budget of COS sources and sinks to the atmosphere is not completely understood, and the vegetative uptake of COS is one of the least understood elements of the atmospheric budget. The authors address this weakness by conducting laboratory experiments of COS uptake by vegetation and combine this with previously reported studies to produce a revised global estimate COS uptake by vegetation. Their laboratory experiments demonstrate that plants do not uptake COS and CO2 in a fixed ratio. Rather, when normalized by their respective atmospheric mixing ratios, there is a preferential uptake of COS compared CO2. This is an important finding because most previous estimates of global COS uptake by vegetation are based simply on the fixed ratio of the atmospheric concentrations. The paper is concise and clearly written, and this scientific result is interesting for atmospheric and Earth system modelers.

Because the paper is clearly written, it was easy to identify its strengths (given in the previous paragraph) and weaknesses. I present a list of the points to improve below. I believe that these should be addressed before the paper is accepted for publication.

1. I do not believe that the paper does an adequate survey of the existing literature. As I write this review, I have a second published paper in front of me: "Carbonyl sulfide and dimethyl sulfide exchange between lawn and atmosphere" by Geng and Mu. This second paper also deals with the uptake of COS by vegetation, but no mention is made in the introduction of the Sandoval-Soto manuscript. Likewise, the data from the earlier study does not appear in Table 2 (a literature survey) of the Sandoval-Soto manuscript. In Table 4, there is no mention of the modeling study of Kjellstrom (1998).

2. Geng and Mu identify and explain the concept of the COS "compensation point" by vegetation; i.e., the fact that there is a bidirectional exchange of production and destruction of COS by vegetation. Under ambient COS mixing ratios this concept is not important for quantifying COS uptake by vegetation. However, when ambient or atmospheric mixing ratios are very low, a situation arises where the plant actually emits COS to the ambient air. Geng and Mu clearly state that the compensation point concept for COS was previously expanded and developed within the Kesselmeier research group. However, there is no mention of the COS compensation point in the manuscript under review.

3. The authors make a very curious comment in their concluding paragraph about "one-dimensional COS uptake" as contrasting with "the bidirectional exchange of CO2". If I read the manuscript correctly, their measurement techniques could only detect net uptake of both COS and CO2, and that their experimental results do not preclude the bidirectional exchange of both COS and CO2 through the stomata. Because there is greater relative uptake of COS relative to CO2 in the stomata, this does not prove that there is no COS leaving the stomatal pore space as part of a bidirectional exchange.

BGD

2, S73–S76, 2005

Interactive Comment

It might be that there is a slightly higher chemical destruction of COS relative to CO_2 within the stomatal fluids. Possibly, most of the molecules entering the stomata are also leaving after spending a few microseconds in the surface microlayer of the stomatal fluid. This could be proven or disproven using principles of statistical mechanics.

4. As a related issue, it is difficult to understand why they belabor the point of "gross primary production". If their instruments generate net uptake data for COS and CO_2, and the book they use to make the global extrapolation also uses net primary production (for CO_2 uptake), then why confuse the uptake issue by introducing the gross primary production concept?

5. The authors do not comment on the importance of temperature in the uptake of COS by vegetation, and they do not mention it in their global extrapolation procedure. Geng and Mu mention that temperature was important in their experimental uptake studies.

6. The authors should explain briefly how they calculated their deposition velocities and uptake numbers. The reader can imagine the experimental setup where there is a mixing ratio difference of COS and CO_2 in the ingoing and outgoing air through the cuvette containing the plant. However, it is not explained how they get from these mixing ratios to the deposition velocity and uptake.

7. In Fig. 1, I was impressed by the way that the plant acts almost like a mechanical switch with respect to the assimilation and conductance parameters. There are two clear plant states corresponding to light and dark. By contrast, there is a lot of scatter in the COS uptake measurements. Also, when the light is switched on, there seems to slow steady increase of COS uptake up until when the light is switched off again, as if the plant has some memory of the previous dark period. More disturbingly, during the light periods when there is highest COS uptake, there almost seems to be a double line of COS uptake points, as if the plant has a memory for every second or third point. Possibly, this resulted from memory effects in the automatic sampling system where there is a small amount of sample carry-over from one cryogenic sampling sequence

to the next. If each sample is cuvette exit air, then this effect is probably not important. However, if the machine was programmed to alternately sample cuvette input and output air (perhaps with a calibration gas in the sequence), then there might be some real cause for concern. The authors might comment on this.

In addition, there were some minor points that the authors could improve:

1. In Fig. 1, the author use technical terms that are not defined, and this makes it difficult for the reader. For example, in Fig. 1 is "conductance" the same as "deposition velocity"? I might guess that it is after studying the units carefully, but "conductance" is used nowhere else in the paper. Is "assimilation" the same as "CO2 exchange"? Again, I could guess on the basis on the units, but "assimilation" is used nowhere else in the manuscript.

2. On p. 191, l. 9 the sentence fragment "...recalculated the COS sinks adequately" should be changed to "...recalculated the COS sinks (accordingly or appropriately)".

3. The uptake numbers are reported with four significant figures, indicating a level of precision that would make an analytical chemist envious. I would report the uptake numbers with two significant figures, or maybe just one.

4. The author list is interesting with people of very different research specialties: COS biogeochemistry, industrial processes, atmospheric oxidation of complicated organic molecules, stratospheric chemistry, and upper ocean chemistry. I point out to the editor that are other COS scientists in that research group.

Interactive comment on Biogeosciences Discussions, 2, 183, 2005.