Biogeosciences Discussions, 2, S70–S72, 2005
www.biogeosciences.net/bgd/2/S70/
European Geosciences Union

Biogeosciences Discussions

BGD

2, S70–S72, 2005

Interactive Comment

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 #2

Received and published: 9 March 2005

General comments: This manuscript represents an interesting and important addition to the literature on vegetative uptake of COS, and its relation to CO2. The authors have included new measurements and have reassessed older results to refine global loss magnitude of COS to vegetation. This refined vegetative loss estimate is quite larger than most previous compilations (Watts, 2000 and Kettle et al., 2002, for example) have suggested. The text is adequate, though there are many sections in which some improvements are warranted (see below). Without them, I'm afraid the main point, and ultimately the importance of these results, could be lost on many readers. Finally, I would hope that some additional experimental details could be included.

The authors might mention one important implication of their results: if losses are indeed as large as is suggested, source magnitudes must be substantially underestimated!

Specific comments: On the estimate of Vd for COS and CO2: Given the fast loss of COS to vegetation, the question can arise as to whether uptake rates were affected by the total loss in the cuvette. In other words, were the losses small so that most all leaves were exposed to a COS mixing ratio that was similar to the initial mixing ratio? It would seem that only if this were true would normalization to the initial COS mixing ratio be appropriate. It might be useful to indicate the reaction (uptake) extent somewhere in the text or tables. Other details that would be useful to add for the non-expert: How were leaf areas determined? How exactly were deposition velocities determined from the concentration differences? Were any corrections made to the CO2 exchange owing to respiration fluxes observed in the dark (clearly small for the oak, but perhaps not so for other vegetation types)?

I can think of a number of other possible reasons for inter-species variations in the relative deposition velocities of COS and CO2 that might be worth exploring and/or mentioning. Does there appear to be any correlation between the COS/CO2 Vd ratio and the photosynthetic scheme used by the plant (C3 or C4)?? Perhaps the COS/CO2 Vd ratios vary in part because the magnitude of respiration that occurs through leaves is not constant across all plants?

Technical comments and those related to clarifying text:

The second half of the abstract could be presented more clearly. An estimate of COS loss based upon NPP is given, then an estimate based upon GPP is presented, though no comment as to why the authors present two different estimates is given. Nor is there any mention of which estimate might be more accurate or reliable. Furthermore, in the final sentence, the authors give advice on how accurate and reliable estimates can be derived with NPP, though I believe the authors argue in the paper that COS loss scaled to GPP may be more accurate (e.g., GPP-scaled result is included in Table 4)... Recommendation: explicitly state in the abstract that your new results and those of others show that a simple scaling of NPP to derive COS loss to vegetation will result in an underestimate of this important sink. Furthermore, because of the influence of CO2 respiration in leaf-based measurements is small (but perhaps is poorly constrained), a global COS loss to vegetation is likely more accurately estimated from scaling GPP...(if indeed this is consistent with their thinking). The best estimate may be somewhere between those derived from scaling to NPP and GPP...(perhaps include both the NPP and GPP-derived estimates in Table 4, unless you truly believe the GPP based estimate is better)

p. 188, lines 20-25. Please be clear here on delineating observations from conclusions you have drawn as a result. For example, did you conclude that stomata did not close completely because some respiratory CO2 was still observed? What is a "physiological consumption"? Regarding COS, do you mean to say that the magnitude of the COS differences in the dark approached the analytical uncertainties associated with using 2 different cuvettes (reference and sample)?

p. 192, line 4-5. Is there a citation you could use to support your assertion that respiration in branches and leaves is small compared to total autotrophic respiration?

Perhaps include the estimate of Xu et al. (2002) in Table 4 (I wanted to know how their result compared, despite its known problems), but indicate with a note that the estimate may be biased high because of the influence of autotrophic and heterotrophic respiration.

p. 192, entire paragraph starting on line 24. Discussion here could be improved... Do we know that the magnitude of COS emissions from vegetation are insignificant? "as this number" what number? That for the global sink estimate when scaling loss to NPP? P. 193, line 1, "A better way" to do what?

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