Interactive comment on "A kinetic analysis of leaf uptake of COS and its relation to transpiration, photosynthesis and carbon isotope fractionation" *by* U. Seibt et al.

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Received and published: 21 December 2009

We thank Dr Harley for his encouraging and constructive review!

We agree that significant soil uptake would make the analysis more difficult. In this case, soil (or other) fluxes would have to be quantified before deriving and interpreting vegetation fluxes. However, the current assumption is that stomatal deposition indeed dominates the overall ecosystem uptake. In this context, using COS measurements to better understand other bi-directional fluxes from forest ecosystems is an interesting idea.

Similar to ecosystem studies, large-scale Ci/Ca estimates would be useful for studies

of carbon-water interactions, for example systematic changes in water use efficiency as a result of changing temperature or atmospheric moisture at large spatial scales.

Below are the replies to specific comments:

p. 9282, l. 21 Although you go into detail in Section 3 about the nature of R, it might be well to define it here as 'the ratio of diffusivities' > changed

p. 9283, l. 8 You introduce the term gi,COS here and say it accounts for the 'transfer of COS into the mesophyll'. Is it strictly speaking a true diffusion resistance or does it also incorporate biochemistry and the presumed rapid reaction with CA? > It also incorporates the biochemical reactions, particularly any limitations due to the reaction with CA. I have changed the sentence.

p. 9285, l. 11 I don't understand the need to 'calculate' values of ambient w, CO2 and COS. Aren't they measured directly in the sample chamber? > The measured values are often directly available only for the reference chamber (for example, in differential measurements, the sample chamber concentrations are used to calculate the fluxes but not recorded separately). Thus, the sample chamber values need to be (re)calculated from reference and flux values.

p. 9286, l. 10 by 'vapour fluxes' do you mean water vapour fluxes, and if so, why are they highly uncertain? Analytical constraints for COS lead to considerably uncertainty, but water vapour? > The water vapour fluxes are much less uncertain than the COS fluxes - I have changed the sentence accordingly - but the stomatal conductance estimates also had uncertainties due to the high relative humidity during the experiments.

p. 9286, l. 13 In my experience, deposition velocities are generally expressed as m s−1 (including Sandoval-Soto, 2005). You choose to normalize the fluxes by mole fraction, rather than concentration, resulting in units of mol m−2 s−1. Is there any advantage to this? > The normalized fluxes have the same units as conductances, which is an advantage for adding COS to models that already include CO2 and water, but there is

no general difference. In particular, the ratio of normalized uptake is identical to the ratio of deposition velocities. To avoid confusion, I am now using "normalized uptake" (instead of "deposition velocity") in the text.

p. 9287, l. 3 I found all the algebraic manipulations in the derivation straightforward until I reached the derivation of Eq (8); i.e., the step from the first to the second equality in Eq (8) requires a leap of faith. I finally convinced myself it was valid, but it would be a lot easier for the reader if they had a little help. That is, in order to get from the first to the second equality in Eq (8), one needs to use (1) the fact that R_{c-COS} is equal to $g_{sc}/g_{s,COS}$ which, although obvious, is not explicitly stated in the text until the following paragraph, and (2) the fact that $g_{COS}=(1/g_{s,COS}+1/g_{i,COS})-1$ which is buried in the text on l. 9, page 9283. > I have added the two equations that are used in Eq 8. I have also added a new short appendix to provide an alternative version of the derivations with mesophyll conductance directly included.

p. 9288, l. 10 ratio given in Table 2 is 2.6, rather than 2.4 > The ratio of 2.6 in Table 2 is the mean of data from three experiments, the ratio of 2.4 applies to the data set presented in Figure 1. I have added this to the sentence.

p. 9288, l. 23 doesn't Eq (4) imply an internal COS concentration of zero (as opposed to small)? > replaced small by negligible

p. 9289, l. 5 Please elaborate as to why C_i/C_a estimates at high humidities are less reliable > At high humidity, water fluxes may be very small and difficult to determine. I have added this to the sentence.

p. 9289, l. 6 'calculated' > changed

p. 9290, l. 12 Rather than 'taking into account possible variations', aren't you assuming a ratio of 0.1 in order to come up with your GPP weighted global mean? > The variations are taken into account when estimating the uncertainty of the global mean value. I have changed the sentence to clarify this.

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Fig. 3 caption: In the text, you use R_{c-COS}, not $R_{CO2-COS}$; suggest 'neglecting this component (i.e., assigning a value of 0.001). . .' > changed

Table 2 caption: Is it accurate to say (final line) that values could exceed (rather than simpy differ from) those in the table by 10-15%? i.e., is there a systematic underestimation? > I agree this usually leads to underestimation (unless, for example, the air supplied to the chambers has very different concentrations for the two gases). I have changed the sentence in the caption.