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Discussions

Interactive comment on "Dynamics of canopy stomatal conductance, transpiration, and evaporation in a temperate deciduous forest, validated by carbonyl sulfide uptake" *by* Richard Wehr et al.

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Response of Authors Wehr et al. (hereafter W) to Anonymous Referee #1 (hereafter AR1):

AR1: This is one of, if not the, first study to use carbonyl sulfide (COS) to investigate canopy- scale stomatal conductance and transpiration (and by difference from evapotranspiration, evaporation). The authors are modelling the various resistances to canopy COS exchange, which they infer from ecosystem-scale COS flux measurements and estimates of the soil exchange, to finally back out the stomatal re-

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sistance/conductance. These estimates are compared against estimates derived from a combination of sensible and latent heat flux measurements. The two approaches agree reasonably well, except for around dawn, when the authors suspect measurement artefacts. Two, random, land surface models do a poor job in simulating the magnitude, partitioning and seasonal evolution of the water fluxes, highlighting the value of the presented data for improving models. This is an excellent paper, well written and innovative. The comments below serve to further improve the manuscript.

W: We thank the referee for taking the time to improve our manuscript, and for these supportive comments.

AR1: Major comments: (1) In contrast to the authors I believe that their approach does not rely “minimally on modelling” (p. 1, l. 24), but rather that they use a complex model to simulate tremendously complex processes. In particular the vertical heterogeneity of the microclimate and associated patterns in exchange processes constitute a real challenge for up- scaling. I found the description of the modelling approaches sometimes hard to follow, in particular with regarding to keeping track what the different inputs are and by which approach these are used and what the assumptions are. In order to make this easier for the reader to follow I am suggesting a sort of a flow chart or similar that shows the inputs for the two approaches, lists the main equations and outputs and so makes it easier to follow what the differences between the approaches are.

W: We agree that “minimally” was a poor choice of word; we meant that partitioning ET based on empirical, EC-derived stomatal conductance is much more direct and constrained than partitioning based on a full-blown ecosystem model. That is probably not a key point to make, and so we will simplify the sentence to read: “Our method of ET partitioning avoids concerns about mismatched scales or measurement types because both ET and transpiration are derived from eddy covariance data.”

We like the idea of the flow chart; we have drafted one and attached it to this comment.

AR1: (2) I am surprised to see that what I would consider the standard approach for

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assessing the surface conductance to water vapour, the Penman-Monteith combination equation, was not used. This approach is used a lot in the flux measurement community and it would be nice to see how it compares to the other approaches. In doing so, and the same applies to the approach based on sensible and latent heat fluxes, the authors will need to deal with the energy imbalance (if it exists at this site, which I though presume).

W: Energy imbalance does exist at this site, as at most eddy flux sites, which is one reason that we do not use the Penman-Monteith (PM) equation. The sensible and latent heat flux equations that we use do not require energy balance. The PM equation, on the other hand, is derived from the sensible and latent heat flux equations by using energy balance to eliminate the heat flux as a variable (and by making a couple of approximations). When sensible heat flux measurements are available, as at every eddy flux site, there is no reason to eliminate the heat flux from the equations or make approximations. Moreover, it is not reasonable to use energy balance to eliminate the heat flux when the available energy flux measurements (heat, water, radiation) do not satisfy energy balance.

We will add the following paragraph on this topic to Section 2.5:

“Note that Eqs. (6,9-10) are the basis for the Penman-Monteith (PM) equation (Monteith et al., 1965), which is commonly used to estimate stomatal conductance. The PM equation was derived from them by using energy balance to eliminate the heat flux as a variable, and by some approximations. When sensible heat flux measurements are available, as at every eddy flux site, there is no reason to eliminate the heat flux from the equations. Moreover, it is inadvisable to use energy balance to eliminate the heat flux given that at most eddy covariance sites, including ours, the available energy flux measurements (heat, water, radiation) do not satisfy energy balance (Wilson et al., 2002). This energy balance closure problem has been repeatedly investigated but the culprit remains unclear and might vary between sites (Wilson et al., 2002; Foken, 2006; Lindroth et al., 2010). In any case, eddy covariance determines the sensible heat flux

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H at least as reliably as the water vapor flux E, and so there is no plausible scenario in which the PM equation would give a more accurate stomatal conductance than Eqs. (6,9-10). At our site, stomatal conductance obtained by inverting the PM equation was about 20% lower than that obtained from Eqs. (6,9-10)."

The cited references are:

Monteith, J. L., Szeicz, G., and Waggoner, P. E.: The Measurement and Control of Stomatal Resistance in the Field, J. Appl. Ecol., 2(2), 345-355, 1965.

Wilson, K., Goldstein, A., Falge, E., Aubinet, M., Baldocchi, D., Berbigier, P., Bernhofer, C., Ceulemans, R., Dolman, H., Field, C., Grelle, A., Ibrom, A., Law, B. E., Kowalski, A., Meyers, T., Moncrieff, J., Monson, R., Oechel, W., Tenhunen, J., Valentini, R., Verma, S.: Energy balance closure at FLUXNET sites, Agr. Forest Meteorol., 113, 223–243, 2002.

Foken, T.: The Energy Balance Closure Problem: An Overview, Ecol. Appl., 18(6), 1351–1367, 2008.

Lindroth, A., Mölder, M., and Lagergren, F.: Heat storage in forest biomass improves energy balance closure, Biogeosciences, 7, 301–313, 2010.

AR1: (3) In my view any paper should have a spelled-out statement of objectives and/or hypothesis and I am asking the authors to modify the introduction paragraph accordingly.

W: We will add the following sentence to the end of the first paragraph: "Our objective here is to test Wehr and Saleska (2015)'s method for estimating canopy stomatal conductance from the water vapor flux against a new, independent method based on carbonyl sulfide (OCS)âĂŤand then to use stomatal conductance to partition evapotranspiration."

AR1: Detailed comments: (1) p. 1, l. 32: and degree of opening

W: We will make this change.

AR1: (2) p. 2, l. 13: CO_2 assimilation depends both on the light AND dark reactions

W: We were not neglecting the dark reactions; we were merely making the point that the rate of CO_2 hydrolysis ultimately depends on light while the rate of OCS hydrolysis does not. To avoid confusion, we will alter the sentence to read: "…the net rate of CO_2 hydrolysis depends on downstream reactions involving light while the rate of OCS hydrolysis does not."

AR1: (3) p. 2, l. 16: stomata are generally the most influential component

W: It seems the referee means to soften the statement to allow for the possibility that stomata are not always the most influential component. Rather than "generally", which can be understood to make the statement even stronger, we will add the word "typically".

AR1: (4) p. 3, l. 31: that will work if both COS and CO_2 suffer the same attenuation; for CO_2 and H_2O we know that this does not work very well, H_2O being more strongly attenuated in the inlet tubes

W: That is a good point. We will add the sentence: "This approach assumes that OCS and CO_2 suffer the same attenuation, which would not be true for a 'sticky' molecule like H_2O."

AR1: (5) p. 4, l. 23-24: very often, canopy transport is dominated by large eddies which violate gradient-diffusion theory

W: Agreed. To address this point, we will add the following sentences: "The assumption that the eddy transport near the ground could be treated as gradient-driven is questionable, as turbulent transport in forest canopies is dominated by large, quasi-periodic eddy motions (Raupach et al., 1996). Nonetheless, the consistency of the calculated soil OCS uptake over the diel cycle and over the growing season (Section 3.1) suggests that the assumption is sufficient for our purposes. Moreover, the calculated soil OCS uptake varied by less than 0.2 pmol m-2 s-1 between low- and high-turbulence conditions as quantified by the friction velocity (results not shown).” The citation is: Raupach, M. R., Finnigan, J. J., and Brunet, Y.: Coherent eddies and turbulence in vegetation canopies: the mixing-layer analogy, Boundary-Layer Meteorology, 78, 351-382, 1996.

AR1: (6) p. 4, l. 29: I am confused by the sign convention here – does that mean that soil respiration has a negative sign?

W: Yes, that is the convention used, as stated after the equation: “where the F are fluxes into the ground (the CO2 flux is negative)…”. The same convention (uptake fluxes are positive) is used throughout the paper.

AR1: (7) P. 4, l. 34: Eq. (2) being based on concentrations, has a huge footprint and thus integrates a much larger area compared to chamber measurements

W: We are not convinced of that. The effective footprint of near-ground concentration measurements (the lower inlet being just 20cm away from the soil) is unclear and probably depends on the wind conditions and density of understory foliage. To acknowledge this issue, we have extended the sentence, “The flux tower itself, where the gradients were measured, was located about 20 m to the north of the chambers”, by adding the phrase, “and the effective footprint of the near-ground concentration measurements is unclear”.

AR1: (8) P. 5, l. 27: here an explanation/justification for the weighting with the light profile within the integral is in order

W: For the reader’s convenience, we will add the sentence: “The light absorption profile $\varphi(\zeta)$ is used here as a proxy for the heat source profile.” Further details underlying the equations in this section are explained in the cited reference, Wehr and Saleska (2015).

AR1: (9) P. 5, l. 30: Tl calculated this way is more commonly referred to as the aerodynamic temperature

W: We will add the following sentence: “Surface temperatures estimated based on heat flux in this way have been termed "aerodynamic temperatures" to contrast them with radiometric temperatures (Kustas et al., 2007)." The citation is: Kustas, W. P., Anderson, M. C., Norman, J. M., and Li, F.: Utility of radiometric–aerodynamic temperature relations for heat flux estimation, Boundary-Layer Meteorol., 122, 167–187, 2007.

AR1: (10) P. 6, l. 2, l. 9: how was the vertical averaging done?

W: We will clarify this point by adding the following: "The canopy airspace temperature Tn was derived from the above-canopy air temperature using a turbulent eddy conductance derived in turn from the corresponding CO2 gradient and NEE. The above-canopy CO2 value was measured at 29 m and the within-canopy CO2 value was the average of measurements at 12.7 m and 18.3 m (canopy top height is about 25 m). The 12.7 m and 18.3 m measurements were generally indistinguishable during the day due to efficient turbulent mixing."

AR1: (11) P. 6, l. 5: PAR is incident?

W: Yes; we will say "incident PAR" in our revised manuscript.

AR1: (12) P. 6, l. 24: wind direction-dependent

W: We will add the word "wind".

AR1: (13) P. 6, l. 33-35: this basically is the second method – should be described and placed more prominently

W: Yes, we should have been more clear about that. We will reorder this section and replace the sentence in question with the following: "Eqs. (8-9) represent the water-flux method for estimating stomatal conductance. To obtain stomatal conductance from our OCS measurements, we rearranged Eq. (1) to solve for gs and inserted the measured canopy OCS uptake F along with the boundary layer, mesophyll, and biochemical conductance estimates described in this section and in Section 2.6. As mentioned above (and see Section 3.2), the stomatal and biochemical conductances were limiting to OCS uptake at our site, and so the calculated stomatal conductance was not sensitive to the boundary layer and mesophyll conductances used."

AR1: (14) P. 7, section 2.6: this is how you achieve closure in your system of equations – should be spelt out more prominently

W: We will begin Section 2.6 with this: "The one term in Eq. (1) that is not constrained by measurements, empirical models, or established theory is the biochemical conductance associated with carbonic anhydrase activity, gCA. Apparent CA activity depends on the amount of CA enzyme and on where it is located relative to the intercellular air spaces, but little is presently known about either of those things (Berry et al., 2013). We therefore tested two simple assumptions for gCA that allow us to solve Eq. (1)."

AR1: (15) P. 8, l. 30-p. 9, l. 6: this is important and should be backed up with a graph showing COS and CO_2 eddy and storage fluxes, not necessarily in the main text, but at least in the supplement

W: We can show OCS and CO_2 flux data that illustrates the dawn peak in NEE and in OCS uptake that is described in the text. A draft figure showing such data is attached to this comment and could be included in a supplement or in the main text, at the editor's discretion. In our opinion, showing the eddy and storage flux data separately would clutter the plot without helping the argument (since the problem we are talking about in any case is inaccurate measurement of the storage flux).

AR1: (16) P. 9, l. 20: big leaf – see De Pury & Farquhar (1997; PCE)

W: We will add the citation of De Pury and Farquhar.

AR1: (17) P. 10, section 3.3: a strong point would be if the approach were able to pick up the differences between times when the canopy is wet after rain (larger evaporative fraction) and when the canopy is dry (mostly transpiration)

W: The approach can indeed pick that up, and we will include a new figure showing the water fluxes versus time since rain. A draft figure is attached to this comment.

AR1: (18) P. 10, l. 23-30: this section would be easier to follow based on the suggested flow chart

W: We hope that the attached flow chart helps.

AR1: (19) P. 10, l. 40: while you did not troubleshoot the models – but how did you determine the parameters?

W: The ED2 and SiB3 parameters were optimized for the Harvard Forest using standard data that included NEE and ET, but not our partitioned water fluxes or OCS measurements. We will state this in our revised manuscript.

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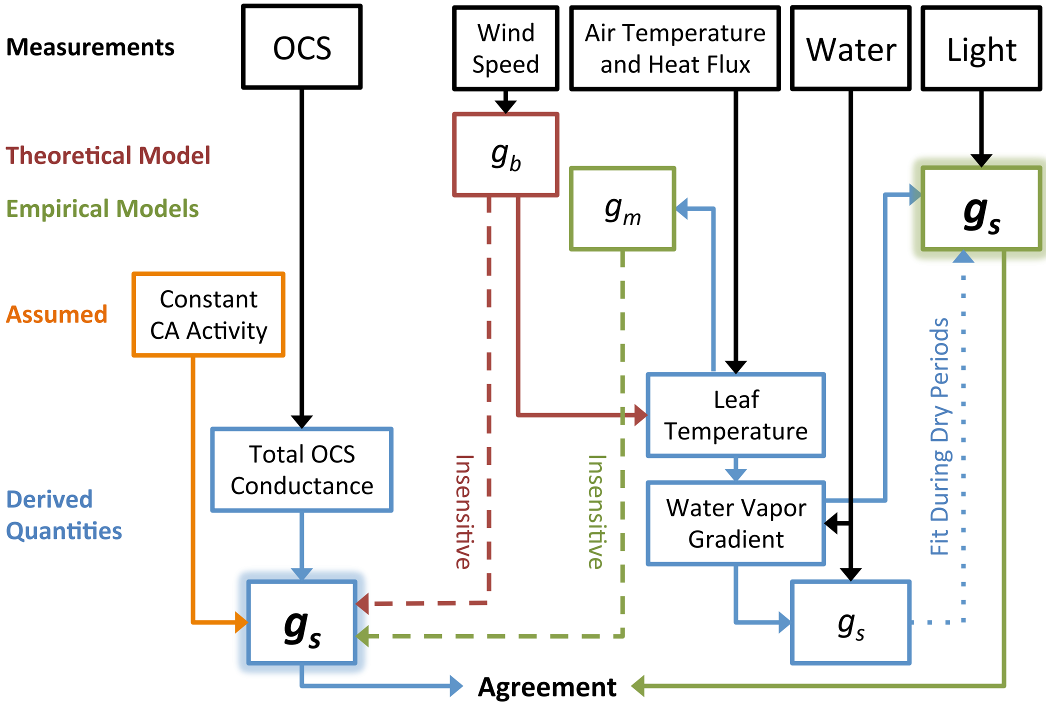

Fig. 1. Flowchart

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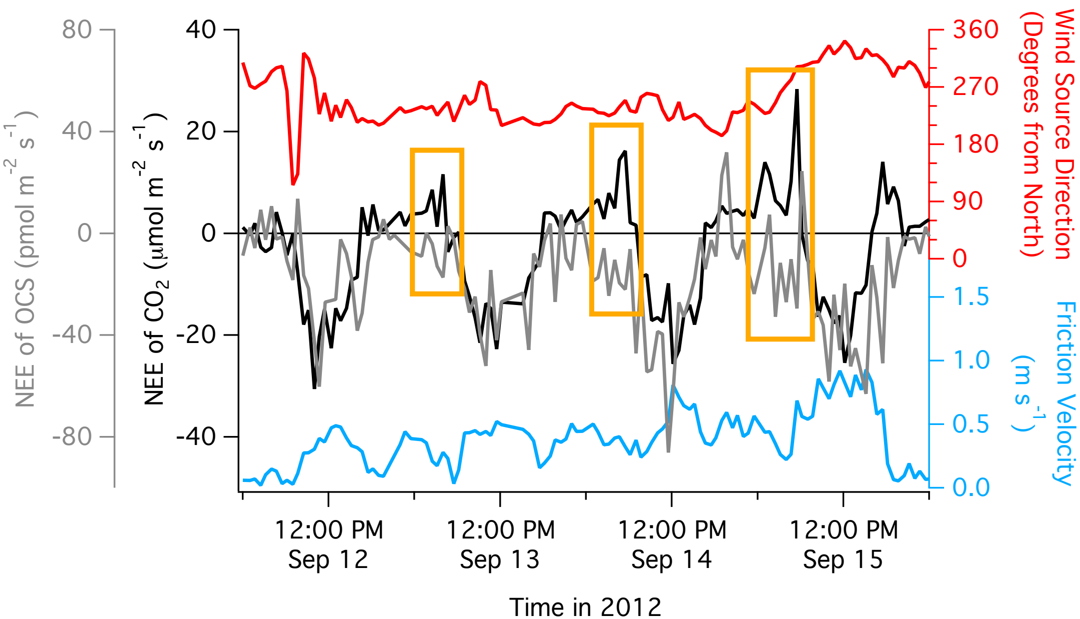

Fig. 2. Unmeasured storage example

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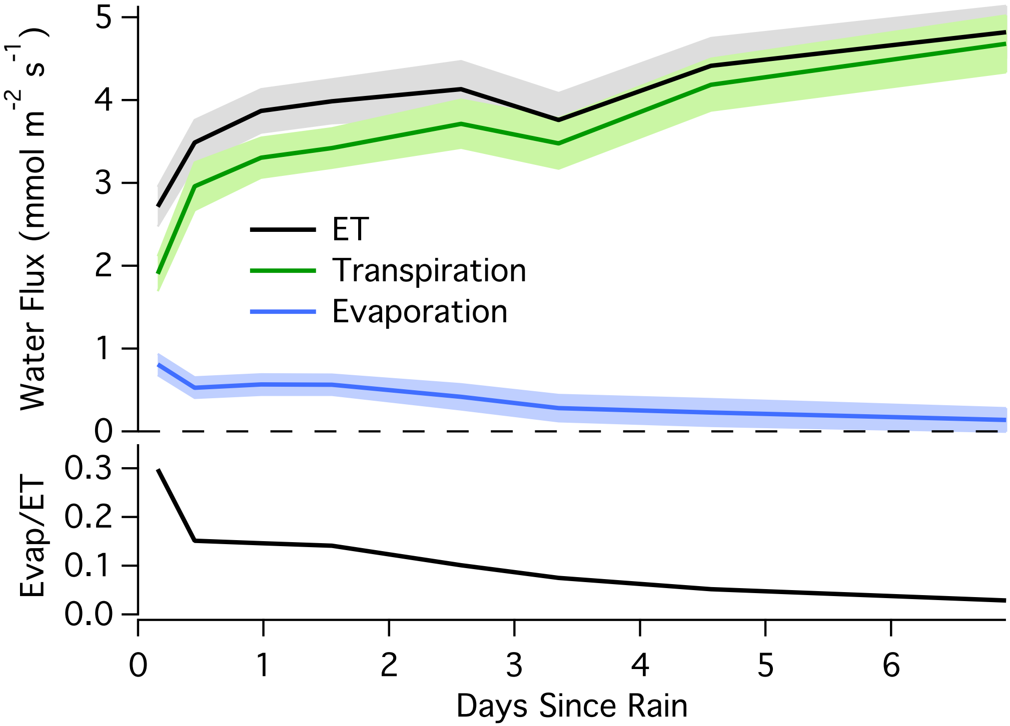

Fig. 3. Water fluxes vs time since rain