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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.

Anonymous Referee #1

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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 resistance/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.

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 upscaling. 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.

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

(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.

Detailed comments:

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

(2) p. 2, l. 13: CO2 assimilation depends both on the light AND dark reactions

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

(4) p. 3, l. 31: that will work if both COS and CO2 suffer the same attenuation; for CO2 and H2O we know that this does not work very well, H2O being more strongly attenuated in the inlet tubes

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

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

(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

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

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

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

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

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

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

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

(15) P. 8, l. 30-p. 9, l. 6: this is important and should be backed up with a graph showing COS and CO2 eddy and storage fluxes, not necessarily in the main text, but at least in the supplement

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

(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)

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

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

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