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Interactive comment on "Calculating Canopy Stomatal Conductance from Eddy Covariance Measurements, in Light of the Energy Budget Closure Problem" *by* Richard Wehr and Scott R. Saleska

Anonymous Referee #2

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Wehr and Saleska identify the implication of residuals in the energy balance at flux sites and its implications on the calculations of stomatal conductance. The authors are well posed to tackle this problem given their previous work on stomatal conductance modelling. While the issue is important, they falter in the motivation of the study and ignore very carefully laid out theory regarding movement of water between leaves and the atmosphere. The authors state that the Penman- Monteith equation was developed because sensible heat fluxes (H) were immeasurable at the time (while those of latent heat were not). This is not true, since both H and LE have previously been inferred

from flux-gradient approaches or Bowen- ratio based approaches, and were prone to similar errors. In the abstract the authors note "…even though H is measured at least as accurately as LE at every EC site while the rest of the energy budget almost never is". This is also needs to be rephrased. Components of net radiation (Rnet) are routinely measured, and in fact, this is generally a more reliable measurement than the eddy flux of latent and sensible heat, which is prone to well-known errors (e.g. poor turbulence). However, putting the issue of instrumentation aside, the differences between H + LE + G and Rnet is often due to differences in "fetch" or missing large eddies in tall forest canopies. 4 channel net radiometers are placed adjacent to the towers, and can be influenced by the tower itself, whereas measurements from IRGAs are highly contingent on the footprint. The authors note correctly that G (soil heat fluxes) are not universally measured, and areas sampled may not be representative of the average soil heat flux from the site.

Coming to the main argument of the paper, the Penman-Monteith equation accounts for the fact that water loss from landscapes is controlled by biotic and abiotic factors. Leaves can reduce the width of stomatal apertures to limit water loss. However, the total evaporation from terrestrial ecosystems, especially leaves, is also a function of available energy (Rnet). This framework was developed in a seminal paper by Paul Jarvis and Keith McNaughton (1986) where they proposed a decoupling coefficient that determines the extent to which transpiration is "stomatally imposed". By using a simple flux-gradient theory the authors imply that transpiration is totally stomatally imposed. This argument is likely to work in tall rough canopies where exchange of momentum (and therefore scalars) with the well-mixed air above is efficient. However, at sites where roughness is low and canopies are homogenous, this approach is more likely to lead to erroneous estimates of stomatal conductance, since it is the available energy that will dominate the amount of water vapour that is lost to the atmosphere. Of course, to an extent, this problem is mitigated by also inferring boundary layer conductance and sharing the limitation of water loss between stomata and resistance imposed by the boundary layer.

Penman-Monteith never intended to solve for stomatal conductance, rather to estimate water loss from vegetated canopies in a way that eliminated the need to know surface variables e.g. surface temperature. The method includes the parametrization of a “surface conductance” which really is somewhat of an emergent property since its accounts for a cumulative effect of “all stomata” but also canopy structure and coupling (much like a canopy scale stomatal conductance or gsv in the current manuscript).

I do really appreciate the careful analyses that the authors have performed and a nice example of this is highlighting the errors in the psychometric approximation, when FG and PM methods are considered equivalent.

Thus, in my opinion, the authors should need to reconcile errors due to flux-gradient approaches with Jarvis and McNoughton (1986). I would then consider this a very significant contribution to the literature and fit to be published in Biogeosciences.

A minor note on figures: I think Figures 1 and 2 are a little complicated and could be simplified. It might help to even show figure 3 first so readers can get a sense of the absolute differences between the various approaches and then dive in to the description of various errors.

Reference: Jarvis, P.G. and McNaughton, K.G., 1986. Stomatal control of transpiration: scaling up from leaf to region. In Advances in ecological research (Vol. 15, pp. 1-49). Academic Press.

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