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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

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Response by the authors (Wehr and Saleska, hereafter WS) to Anonymous Referee #2 (hereafter AR2):

AR2: 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.

WS: Thank you for reviewing our manuscript. We appreciate the perspective you have brought, which we believe to be complementary rather than contradictory, as we detail below.

AR2: 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.

WS: To be fair, we said "difficult to measure" (in contrast to eddy flux sites, where H is a routine measurement), not "immeasurable". But we see how this sentence can be misleading, and so we will rephrase to say: "The original (not inverted) Penman-Monteith equation was designed to estimate transpiration from the available energy (A), the vapor pressure deficit, and the stomatal and aerodynamic conductances. It was derived from simple flux-gradient relationships for LE and for the sensible heat flux (H) but was formulated in terms of A and LE rather than H and LE."

AR2: 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).

WS: We agree (and the manuscript states explicitly) that Rnet is ubiquitously measured. We meant this sentence to refer to the rest of the energy budget taken all together, including storage and ground heat flux. That entire "rest of the energy budget" is almost never measured. We will rephrase to say: "EC sites, in contrast, measure H and LE but rarely assess A in its entirety. True A is net radiation (Rn) minus heat flux to the deep soil (G), minus heat storage (S) in the shallow soil, canopy air, and biomass. . . ."

AR2: 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.

WS: Yes, we agree that the footprint mismatch between radiometers and the eddy fluxes is another reason why relying on energy balance (i.e. on the inverted PM equation) to estimate stomatal conductance is problematic at eddy flux sites, and we will mention it in our revised manuscript. We also agree that much evidence points to EC missing large, slow circulations, and our revised text will highlight this fact, e.g. by adding: "The other major contributor, which also impacts the iPM equation, is systematic underestimation of H + LE by the EC method, probably due to its failure to capture sub-mesoscale transport (Foken, 2008; Stoy et al., 2013; Charuchittipan et al., 2014; Gatzsche et al., 2018; Mauder et al., 2020)." Our revised manuscript will focus on the difference between the FG and inverted PM results without depending on a particular explanation for the energy budget gap.

AR2: 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.

WS: Thank you for the reminder of the important paper by Jarvis and McNaughton (hereafter JM86), which we will now use to better contextualize our revised manuscript, and which we hope makes clear that we are not, in fact, implying "that transpiration is totally stomatally imposed." The crux of Jarvis and McNaughton 1986 (hereafter JM86) is that stomata are only one segment of the transpiration pathway between the sub-stomatal cavity and the atmosphere (a point that goes back at least to Monteith 1965). The idea of JM86 can be concisely summed up in the flux-gradient framework by saying that the total resistance to transpiration consists of the stomatal resistance (r_s) plus an aerodynamic resistance (r_a) that includes all resistance between the surface of the leaf and the chosen atmospheric reference point, and which therefore increases relative to r_s as the spatial scale increases (because as spatial scale increases, the reference point moves from outside the leaf boundary layer to outside the canopy to outside the planetary boundary layer, progressively incorporating more aerodynamic resistance into r_a). The simple flux-gradient equation, transpiration = gradient/(r_s + r_a), tells us straightaway that the FG makes no particular a priori assumption about the relative importance of stomatal resistance: if r_s dominates over r_a, then transpiration will be sensitive to r_s, whereas if r_a dominates over r_s, then transpiration will be insensitive to r_s (in which case it is said to be limited by available energy, which is really just another way of saying it is limited by r_a rather than by r_s). That is the conclusion of JM86, paraphrased. The only assumption about the degree of stomatal limitation creeps into our analysis when we consider conditions under which the leaf boundary layer resistance takes on a typical forest value (with low decoupling coefficient) and neglect the rest of the aerodynamic resistance (according to the discussion on lines 138-143).

For our revised manuscript, we will discuss the issue of non-stomatal limitation and include simulations we have done showing that if the aerodynamic resistance increases (i.e. the decoupling coefficient increases), then the FG equations actually become more accurate while the inverted PM equation becomes less accurate. That is because decoupling (i.e. large aerodynamic resistance) impedes the exchange of heat and so causes the leaf temperature to increase, which causes the saturation vapor pressure inside the leaf to increase even faster (nonlinearly, according to the Clausius-Clapeyron relation). Thus transpiration actually increases and the Bowen ratio approaches 0 (so that underestimation of H becomes unimportant). The psychrometric approximation also becomes poorer in this situation because it is a linearization of the Clausius-Clapeyron relation, adding additional bias to the inverted PM equation. This "calm limit" and its misrepresentation by the PM equation is thoroughly discussed in a new paper that just came out (and which we now also cite): McColl, K. A. (2020). Practical and theoretical benefits of an alternative to the Penman-Monteith evapotranspiration equation. Water Resources Research, 56, e2020WR027106. https://doi.org/10.1029/2020WR027106. Of course both the FG and inverted PM equations rely on an estimate of the aerodynamic resistance, and they become increasingly sensitive to it as it becomes increasingly limiting.

AR2: 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).

WS: Indeed, the PM equation was developed to estimate transpiration, not to solve

for stomatal conductance. Nonetheless, use of the inverted PM equation to calculate stomatal conductance from transpiration is common in the scientific literature and goes back at least 25 years (to Grace et al., Glob. Change Biol., 1995). Our purpose in this paper is to address that common literature practice, and to show that if you are going to calculate stomatal conductance from transpiration, then you are better off using the flux-gradient (FG) equations than the inverted PM equation. The fundamental difficulty you raise applies similarly regardless of which equations are used: when transpiration is insensitive to stomatal conductance (i.e. when the decoupling coefficient is close to 1), there will naturally be very large error in the stomatal conductance you retrieve from transpiration. In our revised introduction, we will more clearly frame the scientific task under consideration, including this fundamental limitation, and we will distinguish the original PM equation and its purpose from the inverted PM equation commonly used to estimate stomatal conductance from transpiration.

Our revised first paragraph will begin: "Leaf stomata are a key coupling between the terrestrial carbon and water cycles. They are a gateway for carbon dioxide and transpired water and often limit both at the ecosystem scale (Jarvis and McNaughton, 1986)."

And our revised second paragraph will begin: "When the aerodynamic conductance to water vapor outside the leaf (gaV) is greater than gsV, the latter exerts a strong influence on transpiration, from which it can be inferred. The standard method is to calculate gsV from eddy covariance (EC) measurements of the latent heat flux (LE) via the inverted Penman-Monteith (iPM) equation (Monteith, 1965; Grace et al., 1995) âĂŤ but the EC method and the iPM equation make a strange pairing. The original (not inverted) Penman-Monteith equation was designed to estimate transpiration from the available energy (A), the vapor pressure deficit, and the stomatal and aerodynamic conductances. It was derived from simple flux-gradient relationships for LE and for the sensible heat flux (H) but was formulated in terms of A and LE rather than H and LE."

AR2: 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.

WS: Thank you; we're glad that point was of interest. You might be interested in the new paper we mentioned above (McColl, 2020), which is all about how the psychrometric approximation leads to significant error and incorrect limiting behavior in the (not inverted) PM equation.

AR2: 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.

WS: Hopefully our responses above have convinced you that the flux-gradient framework is entirely consistent with JM86, and that it does not invoke any additional error compared to the inverted PM equation.

AR2: 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.

WS: We have created new, simplified versions of our figures, in which only two flux correction scenarios are included. We have also created a small figure that aids with interpretation of Fig. 1 by visually highlighting the contribution of each source of bias to the inverted PM equation.

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