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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

**Richard Wehr and Scott R. Saleska**

rawehr@email.arizona.edu

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Response by the authors (Wehr and Saleska, hereafter WS) to Anonymous Referee #1 (hereafter AR1):

AR1: Overall, this is an innovative study and should be considered for publication in the Biogeosciences after some revision. I have a few general comments and suggestions.

WS: Thank you for taking the time to review our manuscript, and for the helpful suggestions, to which we respond below.



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Discussion paper

AR1: The readability of this paper could be improved. I found that Methods, Results, and Discussion are difficult to follow. I had to go back and forth several times to find the necessary details and information. I understand this type of paper might not necessarily follow the same structure of a typical research paper, but I suggest the authors should clearly ad structurally lay out the data used and steps taken upfront. Potentially, an overview paragraph summarizing the study design, a table listing the different simulation scenarios, and/or a more explicit subtitle might also help readers. Figure legends, especially Figure 1 & 2, should be more self-explanatory.

WS: We apologize for the lack of clarity in the original manuscript. We have prepared a revised version that includes substantial changes aimed at making the presentation more straightforward and clear, including: (i) revised and additional text in all sections, (ii) a brief summary of the study design at the end of the introduction, (iii) reorganized Methods and Results sections with separate subsections for the simulations and the real time series analysis, and (iv) simplified figures. We believe the new version is much easier to follow.

AR1: Several recent studies suggested that the energy imbalance issue was likely caused by mesoscale or secondary circulations instead of instrumental or other local sources, and H and LE might be influenced disproportionally (Mauder et al., 2020; Xu et al., 2020). It might still be an open question, but I suggest the authors taking that into consideration.

WS: This is a very good point. Whether the Bowen ratio is preserved by EC measurement error is an open question. Our new manuscript version includes new text in the introduction, methods, and results sections dealing with this point, and backs away from recommending a specific flux correction method. Instead, the new manuscript focuses on comparison between the flux-gradient and inverted Penman-Monteith equations, and explores the impact of ideal and flawed corrections. It thereby motivates future work on the question of how to best correct the eddy fluxes.





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Discussion paper

AR1: It's a bit puzzling to me about one of the key arguments – the preferred use of flux gradient equations instead of Penman-Monteith equation. I think ultimately the main difference resulted from how the energy imbalance was treated and/or how the total available energy was partitioned. The psychrometric approximation should have only marginal influence, right? Or, do the authors imply anything additionally? For example, some studies used available energy (LE+H) or adjusted total energy in the Penman-Monteith equation. Would it be sufficient enough?

WS: Our new version with revised introductory context, framing, and figures should help clarify this issue, which is central to the paper. The main practical difference between the FG and inverted PM equations is that the former uses measurements of H and LE while the latter uses measurements of A and LE, and then implicitly infers H as the difference between the two. Thus the FG equations involve underestimated H and LE, which bias stomatal conductance in opposite directions, whereas the inverted PM equation involves underestimated LE and overestimated H, which both bias stomatal conductance in the same direction. The psychrometric approximation is less important but not negligible (bias $\sim$ 5%).

When people use the EC-derived available energy (H + LE) in the "Penman-Monteith equation", that is not the Penman-Monteith equation anymore; it is an undoing of the Penman-Monteith equation that moves back towards the FG equations on which it was based (which we are recommending use of here). The distinguishing feature of the PM equation is its elimination of H and surface (i.e. leaf) temperature.

All these points are explained more clearly in our new manuscript version. For example, our new introduction includes:

"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.”

And our new Results and Discussion section includes:

“. . .the FG formulation (solid black lines) is always more accurate than the iPM formulation (solid red lines) because regardless of whether the gap is due to negative measurement bias in A or in H + LE, the iPM equation implicitly overestimates H (as the residual of the other fluxes) and therefore the leaf temperature and therefore the water vapor gradient, which exacerbates underestimation of the conductance. In other words, it is better to have both LE and H underestimated (as in the FG equations) than to have LE underestimated and H overestimated (as in the iPM equation).”

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