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Discussions

Interactive comment on "Examining the evidence for sustained transpiration during heat extremes" *by* Martin G. De Kauwe et al.

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I thank the authors for their careful replies, which nonetheless make it clear that there are issues upon which we simply disagree. Although they have not convinced me, I see little point in repeating certain arguments and prefer to leave their resolution to the discretion of the editor. Nonetheless, I wish to rebut certain points made by the authors in their response.

I find their responses to my points 1 and 2 to be essentially acceptable, although I maintain that the explicit delineation of "Conclusions" would enhance their presentation.

Regarding the issue of dimensional analysis (point 3), I feel obliged to justify my use

of the strong term "absurd", and not to back down regarding its appropriateness here. Dimensional analysis of eq. (6) of Medlyn et al. (2011) reveals that the function of incident light, f(I), must have the same units as assimilation (A), since the CO2 concentration defined is a dimensionless mole fraction. In eq. (7) of Medlyn et al. (2011), the optimal stomatal conductance has the same units as f(I), such that the term in parentheses must be dimensionless. Since both the atmospheric CO2 concentration (Ca) and the variable lambda are dimensionless (each expressing a mole/mole ratio), the only way that the expression under the square root sign could be dimensionless and comparable with 1 is if the constant 1.6 were to have units of m s2 kg-1 (the inverse of pressure). The authors have put this equation forth as a definition of "the theoretical expectation", and for this to be so it would be necessary that 1.6 m s2 kg-1 be some sort of universal constant, but I can find no evidence for this. Rather, from the Hari et al. (1986) paper upon which Medlyn et al. (2011) base their analyses, it appears that the factor 1.6 is simply the dimensionless ratio of the diffusivities of water vapour to CO2, which derives from Graham's law (inversely proportional to the square root of the ratio of their molecular masses). Based on these analyses, I maintain my position that this "equation" is dimensionally incorrect and therefore nonsensical.

In their reply (at line 100), the authors suggest a different equation which they would add to their manuscript. Given the definitions provided in the reply, this equation is indeed dimensionally correct. However, the use of two variables with the same symbol (albeit different subscripts) and different units is unfortunate, and breaks with tradition in scientific notation. If g_s is the stomatal conductance (mol m-2 s-1), then it is logical for all g variables refer to conductances with the same units (e.g., mesophyll conductance, boundary layer conductance, etc.). In equation (2) of Medlyn et al. (2011), we find g_s and g_0 with units of mol m-2 s-1, but g_1 with some different units. If the units of this "key parameter" are kPaˆ0.5, as the authors propose, then eq. (2) of Medlyn et al. (2011) is also dimensionally incorrect. In short, I find the entire framework of equations to be dimensionally inconsistent (hence, "absurd"), and in any event, I suggest that a different symbol be chosen for the fitted parameter with units of kPaˆ0.5, rather than g_1.

I do believe the only acceptable justification for excluding the units on variable axes when data are being plotted is that the variable be non-dimensional variable.

Regarding point 4, in addition to clarifying the effect of normalization on a probability function, the authors should take care to distinguish between a density (mass/volume) and a probability density. Similarly, if the probability mass function were to be presented, it would be inappropriate to label the axis with simply "mass".

Finally, I do not wish to belabor point 5, as is evidenced by the parenthetic remark with which I introduced it, and I agree that experimental evidence will likely be required to change minds and bring about a paradigm shift. However, there are some points with which I strongly disagree with the authors replies. First, the relative newness of a publication is no justification for ignoring it. Second, my paper does make specific predictions for behaviour under heatwave conditions: "Consistent with the determinants of q, as the temperature of a (saturated) stomatal environment increases, even for a constant stomatal aperture, the WUE is reduced, wresting some control over gas exchange rates from the plant." (The variable q is previously defined as the specific humidity.) Finally, the authors encourage me to develop my "theory". My paper does not present a "theory" but rather a simple derivation grounded in the basic laws of physics. It might be better described as a proof, and neither is it novel since it was derived long ago by one of the giants of classical physics (Josef Stefan).

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