Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-363-RC2, 2016
© Author(s) 2016. CC-BY 3.0 License.

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

Interactive comment on "Leaf-scale experiments reveal important omission in the Penman-Monteith equation" *by* Stanislaus J. Schymanski and Dani Or

Anonymous Referee #2

Received and published: 20 October 2016

Comments on "Leaf scale experiments reveal important omission in the Penman-Monteith equation" by Schymanski and Or

My first feeling is that the title of the paper does not reflect its real content and that its content is not really appropriate for a hydrology journal such as HESS.

Despite an important theoretical development with 83 equations (27 in the main text + 56 in appendices), the title seems to induce that the "supposed" omission in the Penman-Monteith equation was revealed through experimental data. It is not true. The authors, in fact, derive an equation for the evaporation from a single leaf (Eq. 22) using a lot of mathematical details and then test the equation by means of an experimental setup. The theory precedes the experiment and justifies the experiment. Additionally,

Printer-friendly version

Discussion paper

CC BY

the Penman-Monteith (PM) equation commonly refers to canopy evaporation and not to single leaf evaporation (e.g., ET0 in FAO Irrigation and Drainage Paper 56). The PM equation represents a particular form of the so-called combination equation, first derived by Penman (1948) for open water and then extended by Penman and Monteith to any evaporating surface (bare soil, leaf, canopy, etc ..). Speaking of PM equation at leaf scale can be somewhat misleading from my standpoint; it would be more appropriate to speak of combination equation.

Many aspects of the theoretical development, however, are not new and can be found in many textbooks or previous articles. The question of single leaf evaporation in relation with stomata distribution is an old issue. It has been addressed by many authors other than Monteith and Unsworth (for instance: Jarvis and McNaughton, 1986; Verhoef and Allen, 2000; Lhomme et al., 2012) and the question should be considered as closed from my standpoint. Assuming that the content of the paper is novel and relevant, HESS is certainly not the appropriate journal for such a topic. Plant, Cell and Environment or Journal of Experimental Botany should be more suitable. I should recognize, however, that the authors made a remarkable experiment in a wind tunnel with artificial leaves connected to a water supply, performing laser perforations and measuring all the components of the energy balance.

As far as I understand, the main point of the theory is the derivation of Eq. 22, which gives the evaporation from a single leaf (amphistomatous or hypostomatous) in the form of a combination equation (combining surface energy balance and convective transfers with the surrounding air). It is opposed to the so-called MU equation (Eq. 21), previously derived by Monteith and Unsworth in their reference book (Principles of Environmental Physics).The authors' equation (Eq. 22) appears to be correct, provided resistances ra and rs are defined as one-sided leaf resistances (this point, however, is not clear in the text: see P7 Line 156, where we could understand they are defined as two-sided). The authors claim that the MU equation, correct for amphistomatous leaves, is not correct for hypostomatous leaves because of a factor 2 missing in the

Printer-friendly version

Discussion paper

CC BY

definition of the resistance ra in the nominator. I have checked Eq. 21 in the reference book of Monteith and Unsworth (P188 of the second edition 1990). Their demonstration is not perfectly clear because they do not give the complete combination equation for a single leaf; they only specify the change (their equation 11.30) in the denominator of the equation. One may suppose, nevertheless, that their equation is valid for amphistomatous leaves, but not for hypostomatous leaves. I must emphasize that by no means, the point mentioned above should be considered as an "important omission in the Penman-Monteith equation": first, because it has been correctly addressed in previous articles (those mentioned above), second and more importantly, because the authors do not assess the possible impact this "new" leaf formulation (and the small error supposedly encountered in the combination equation) can generate on the PM equation at canopy scale (the relevant scale for the hydrological community). It is the main problem of the paper.

I should add, as minor comment, that the beginning of the discussion section (leaf temperature and wind speed) is not clear and quite confusing insofar as it deals with "observations not presented here" (I quote).

References

Jarvis, A. J. and McNaughton, K. G.: Stomatal Control of Transpiration: Scaling Up from Leaf to Region, Advances in Ecological Research, 15, 1–49, 1986.

Verhoef, A. and Allen, S.J.: A SVAT scheme describing energy and CO2 fluxes for multi-component vegetation: calibration and test for a Sahelian savannah. Ecol. Model. 127, 245–267, 2000.

Lhomme, J.P., Montes, C., Jacob, F., Prévot, L.: Evaporation from heterogeneous and sparse canopies: on the formulations related to multi-source representations. Boundary-Layer Meteorology 144, 243-262, 2012.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-363, 2016.

CC BY