

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

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**We thank the reviewer for clearly formulating his/her concerns about our manuscript and for giving us the opportunity to clarify a few issues that could be misleading. Below, we respond to the comments one-by-one.**

i) 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.

**We are confident that our responses to the detailed comments below illustrate the adequacy of the title and the appropriateness of the manuscript for HESS.**

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ii) 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.

**Although we do not see the relevance of the order of events (after all, this is not a mystery novel), we wonder about the basis for the reviewer’s assertion that “it is not true”. In fact, we confirm that the omission in the Penman-Monteith (PM) equation was revealed experimentally first. The experimental setup was devised to test the detailed leaf energy balance model we used in previous publications (Schymanski et al., 2013; Schymanski and Or, 2015, 2016), and the idea to compare results with the PM equation arose during the evaluation of the experimental data. Therefore, we believe that it is appropriate to state in the title that the experimental evidence revealed a problem, which was subsequently identified in the derivations.**

iii) Additionally, the Penman-Monteith (PM) equation commonly refers to canopy evaporation and not to single leaf evaporation (e.g.,  $E_{T0}$  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.

**The notion that the PM equation “refers to canopy evaporation and not to single leaf evaporation“ is a common confusion in the scientific community. On Lines 22–28 in our manuscript, we suggest that the use of the PM equation at the**

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canopy scale may be the reason for employing various empirical corrections rather than testing the adequacy of the equation itself. In the revised manuscript, we will point out more clearly that Monteith (1965) referred to a single leaf when deriving the PM equation, as evident in the abstract of his paper and from Page 208 onwards. Use of the PM equation at canopy scale is commonly motivated in the context of a big leaf analogy, implying that the physics valid for a leaf are also valid for a canopy (e.g. Lhomme et al., 2012, cited by the referee below). Our point is that a physically-based equation should at least represent all relevant processes adequately at the scale of derivation before being upscaled. Therefore, the failure of the PM equation to reproduce fluxes at the leaf scale has to be considered potentially relevant for its performance at the canopy scale. Note that use of the term “combination equation”, as proposed by the referee, would equally refer to the Penman and the Penman-Monteith equation, whereas we specifically focus on the PM equation, which was formulated for a leaf, as opposed to a wet soil surface, and has added consideration of stomatal resistance.

iv) 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.

**In the present manuscript, we aimed to provide consistent and complete derivations, focusing on the key papers related to the original derivations of the PM equation and the interpretation considered as “textbook knowledge”, i.e. the book by Monteith and Unsworth (2013). The referee is correct that Jarvis and McNaughton (1986) included a correction in the PM equation for amphistoma-**

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tous leaves in the appendix, Eq. A9. However, they mistakenly explained the difference “as a result of our use of conductances defined on a single surface area basis”, and did not alert the reader to the more fundamental issue we found, namely the missing half of sensible heat exchange. This is explained in our manuscript in Lines 170–174. We cannot see why the referee cites Verhoef and Allen (2000) and Lhomme et al. (2012) as examples for the treatment of single leaf evaporation. The former focuses solely on canopy evaporation and the latter just re-uses the formulation by Monteith and Unsworth (2013). To our knowledge, a general formulation as presented in our manuscript and its test for a single leaf has not been presented in the literature and hence we do not agree that the question should be considered as closed. On the contrary, our experimental evidence suggests that the question of the physical basis of the PM equation should be re-opened and re-evaluated not only at the leaf scale, but also at canopy scale.

v) 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.

**This assessment by Referee 2 is in contrast to the assessment by Referee 1, and likely based on the misunderstanding that the PM equation was derived at canopy scale, while our correction is merely relevant to its application at the leaf scale. Closer study of the literature reveals that the PM equation is actually rarely used at the leaf scale, in favour of explicit solution of the leaf energy balance, e.g. Ball et al. (1988). In the revised manuscript, we will emphasise more clearly that the paper focuses on the physical fundamentals of a formula-**

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tion central to hydrology, from the leaf to the canopy and continents, and that there is no additional physics involved in the scaling up of the PM equation from leaf to canopy other than ad-hoc upscaling from leaf to canopy resistance. Following up on the reviewer's statement at the beginning of the review, that the PM equation is mainly used at the canopy scale, we conclude that its physical basis is therefore of great relevance to Hydrology and Earth System Sciences.

(vi) 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  $r_a$  and  $r_s$  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).

**Actually, we consider Eqs. 25–27 as the main point of our theory, delivering general analytical solutions for latent heat flux, sensible heat flux and leaf temperature, while also considering the longwave radiative feedback. However, we acknowledge the referee's comment that a skimming reader might be confused by Lines 151–159, where we explore various alternative assumptions about the resistances to show that neither of them makes the PM equation physically consistent for a planar leaf. To clarify this, we will introduce this paragraph with the following sentence:**

**“To test whether Eq. 19 is physically consistent for a planar leaf, we will attempt to deduce it from our generally valid Eq. 10, using any suitable definitions for  $c_E$ ,  $c_H$ ,  $r_a$  and  $r_s$ .”**

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vii) 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 definition of the resistance  $r_a$  in the nominator. I have checked Eq. 21 in the reference book of Monteith and Unsworth (P188 of the second edition 1990). Their demonstra- tion 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

**We do not agree with the referee that the MU equation (Eq. 21 in our manuscript) is valid for amphistomatous leaves. In Line 171, we clarified that the MU equation is missing a factor of 2 in the denominator, representing two-sided exchange of sensible heat. This factor is independent of stomata being present on one or both sides of the leaf and hence makes the MU equation *invalid for any planar leaf* in a free air stream. As mentioned above and in our manuscript, some corrections can be found in the literature, but a systematic and explicit general derivation of the correct equations, as presented here, has been missing.**

viii) 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.

**Here, the referee seems to dispute that the omission we identified persists in the literature and additionally questions its relevance for canopy-scale process**

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representation. We hope that we have established clearly that the PM and MU equations indeed omit an energy balance term (that cannot be recovered by redefining resistances) and that this omission has not been rectified in the literature, as claimed by the referee. Given that the PM equation is commonly scaled up to the canopy by considering the canopy as a “big leaf”, the effects of the omission seen at the leaf scale are likely similarly important in canopy-scale models. We have not yet extended the analysis to canopy scale response and thus cannot comment on the propagation of the omission to larger scales and how much of the effect of the “small error” may be eliminated or obscured by canopy-scale parameterisation. However, we are confident that the reviewer is not suggesting to overlook simple errors in the basic representation, in the hope these will not affect larger scales? Moreover, we have shown that the sensitivity of ET to future temperature changes is likely represented incorrectly by the PM equation, as its derivative remains anchored in the erroneous leaf level representation.

ix) 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).

**Thank you for pointing this out. The discussion of the leaf temperature feedback on Lines 291–297 actually detracts from our main point and will be replaced by the following:**

**“This results in a strong under-estimation of latent heat flux by the PM equation in our experiments, where sensible heat flux is the main source of energy for evaporation (in the absence of shortwave radiation). ”**

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