Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-363-AC2, 2016

Hydrology and Earth System Sciences
Open Access
Discussions

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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Received and published: 23 September 2016

First, we would like to thank Stefan Dekker for his kind and motivating words, as well as for his insightful questions and helpful suggestions. Below, we respond to the various comments one-by-one.

i) Abstract: L9: you report two errors in the PM, without telling which ones (leaf exchanges of sensible heat and longwave radiation). Please add them, a reader would like to know what is missing.

ii) Also in the abstract I would like to see a statement that the error can be enormous.

For instance, in Fig 7,8 the authors report differences in modelled vs observed ET and H of up to 100 W/m2

Good points, the sentence in the abstract will be modified to:

> **"Detailed analysis of the derivation by Monteith (1965) and later amendments revealed two errors, one in neglecting two-sided exchange of sensible heat by a planar leaf, and the other related to the representation of hypostomatous leaves, which are very common in temperate climates. Furthermore, we found that the neglect of feedbacks between leaf temperature and radiative energy exchange also contributes to bias in simulated latent heat flux by the PM equation, which was as high as 50% of the observed flux in some experiments."**

iii) Minor point: Figure 1, you write the Eq Rs=El+Hl+Rll, while in the text you write Rs=Rll+Hl+El

Thanks, we will make this more consistent.

iv) There are some inconsistencies in reporting units in the main text. I mean all units are correct but are not reported. I would be in favour that all variables if introduced for the first time should have a unit. For instance, no unit is given for Rs, while El (L71) does have a unit.

Very good point, we will add units everywhere.

v) L. 80-84, This part is introduced to fast. I do not understand this sentence. . . you introduce hc here (convective transport coefficient) What do you mean with leaf boundary conductance (the leaf boundary layer conductance to sensible heat is gsw (ant not hc)

We apologise for the confusion caused by inconsistent terminology. We called h_c the "convective heat transfer coefficient" above and here we called it the "leaf boundary layer conductance to sensible heat". We will make terminology more consistent and clarify that both h_c and g_{bw} relate to the same physical principles of diffusion and boundary layer dynamics.

vi) I think it would be good to end chapter 2 with a new section (2.5) on the comparison of the different models (Rlin, MuC,PM, MU) and that you finally want to compare numerical solutions with PM and your new derived analytical solution (Rlin). This means that the part from the results L258-264, which are actually methods, should be incorporated in this new section (2.5). It will help the reader what he/she can expect.

We will add a section 2.5, "Comparisons of numerical and analytical models with observations", leading into the description of the experimental setup. However, we will keep a condensed version of L258-264 at the beginning of the results section, as these sentences are meant to introduce the reader to the plots that follow.

vii) Make new section 4.1: I think that the results should be divided in two parts. First 4.1 in which the data are compared to numerical solutions. In that sense I also would like to incorporate the data wind speed against H and ET measurements and numerical solutions. In this section it would be good to link back to Figure 1, were they show that H, ET are dependent on wind speed and Rs and Rll not.

We will follow the above suggestion and in addition to Fig. 6, we will add experimental and numerical model results of the response of LE, H and Rnet to wind speed. We discuss this in more detail in the upcoming technical note, but the reviewer is right that this would be helpful in the present paper already.

viii) Make new section 4.2: In section 4.2 you then can compare the different models. If possible I would like to see 4 graphs (35 pores/mm2 against wind speed, 7 pores/mm2 against wind speed, 35 pores/mm2 against VPD, 7 pores/mm2 against VPD). If they are not measured then only the 35 and 7 against VPD and 35 against wind speed. In this section I hope that the authors can tell a little bit more on the general behaviour of the models and why there is a clear order of over/under estimation depending on the models used. The authors could do that by also showing the resulted Rll ((Eq2) versus Eq. (23) and as a result also the difference in Tl for the different models. Then I hope it becomes more clear why the PM, MUc,MU over and/or underestimate.

We will follow the above suggestion and add a plot of 35 pores/mm2 against vapour pressure for comparison with 7 pores/mm2 against vapour pressure. Unfortunately, we have not conducted experiments with 7 pores/mm2 against wind speed in darkness, but we could add plots of 1.8 pores/mm2 or without a perforated foil, i.e. infinite conductance, if this is deemed necessary to increase confidence in the results. We tried to minimise the number of figures per amount of information, and with the added figure in Section 4.1. and one more in 4.2, we will be already at 10 figures in the main document. All additional plots will be included in the technical note. A plot of Eq. 2 against Eq. 23 for R_{ll} was presented in Fig. A3. The effect of neglecting the R_{ll}-T_l feedback is expressed in the difference between the Rlin and MUc results, while the PM result shows the result of added neglect of two-sided sensible heat flux and the even stronger under-estimation of LE by the MU equation results from further reducing latent heat flux in an attempt to account for one-sided transpiration on top of the erronously one-sided sensible heat flux. We will clarify this further in the revised manuscript.

ix) Minor point: Results (L257). Here we only report two experiments under varying vapour pressure. This is true for Figure 6, but not for Figure 7 as here you also report an effect of different wind speeds. Please add that.

Thank you for pointing out this inconsistency. We will adapt the text to reflect the plots presented.

x) Minor point: I am not in favour of saying that most of the results will be presented in a technical note

Good point, we will reformulate to state:

"The ranges of stomatal geomtetries and deduced conductances for the two different leaves presented here are given in Table 1. A more detailed analysis of correspondence between experimental results obtained for a larger variety of artificial leaves and the numerical model will be presented in a technical note (Schymanski and Or, in prep.). Here, we only present selected experiments that highlight systematic differences between the various analytical solutions, the numerical model and observations."

xi) L 288, again two errors without clearly telling which ones.

xii) L 292: The discussion directly continues in observations which are not shown, while I think that the authors could discuss the observations which are presented. Especially why some models over/under estimate the results is important to address and important for the readers of HESS

Lines 288-297 will be modified to:

"In our mathematical analysis, we found two errors in the PM equation and in the "corrected" MU-formulation by Monteith and Unsworth (2013). Both formulations are based on evaporation from a soil surface, which exchanges sensible and radiative heat only on one side, whereas planar leaves have two sides

exposed to the surrounding air. Failure to recognise this error led to a second error in the MU formulation, where an additional reduction to transpiration was introduced to represent leaves that exchange water vapour only on one side. For a leaf, the energy for transpiration in darkness is mainly supplied by sensible heat flux (on both sides), which increases with increasing wind speed. In contrast, the energy for evaporation from a soil surface in darkness is supplied by sensible heat on the evaporating surface only, (and by soil heat flux from below). The neglect of the additional uptake of sensible heat on the second side of the leaf in the PM and MU models led to significant under-estimation of the observed transpiration rates in our experiments. Note, however, that the bias is not constant and not always negative. As illustrated in Fig. 8, the negative bias decreases with increasing irradiance or air temperature, goes to 0 at a certain combination of temperature and irradiance and then becomes positive at higher values of irradiance and/or temperature. This is because under conditions when the leaf temperature is lower than ambient, sensible heat flux is a source of energy for transpiration, whereas under conditions when the leaf is warmer than the air, sensible heat flux competes with transpiration for energy. The omission of sensible heat exchange by the second leaf surface has therefore most drastic effects when leaf temperature most strongly deviates from air temperature. It may also be noteworthy in this context that the expression for aerodynamic resistance..."

Appendix: It is a real pleasure that, as far I can see, all equations and units are correct. One minor detail, the alphabetic order of variables in table A2 is not completed, for instance Reynolds numbers and Prandl number. Therefore difficult to find.

Wow, thank you! This was due to different working names for the dimensionless numbers, which were used for sorting. This will be corrected. We will also make all the code and data available online, for easy verification and further use by the

reader. The preparation of the data and worksheets is in progress and can be accessed here: https://github.com/schymans/Schymanski_leaf-scale_2016

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