Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-160-RC1, 2019

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Uncertainty caused by resistances in evapotranspiration" *by* Wen Li Zhao et al.

Anonymous Referee #1

Received and published: 17 June 2019

Zhao et al. compare simulations of evapotranspiration (ET) from a big-leaf (one-source) Penman-Monteith model with simulations from a two-source model and the three temperature (3T) model. Simulated ET is compared to measured fluxes from a number of eddy covariance sites in an oasis in Northwest China. Focus is given to uncertainties caused by the parameterization of resistances (surface and aerodynamic) in these formulations.

The study investigates a relevant scientific question, which is nicely introduced in the introduction section. However, I cannot recommend this manuscript to be published due to the following shortcomings:

- the comparison of the different approaches is reduced to a simple sensitivity analysis, resulting in very little scientific progress. The key message from Figures 8-12 is that

different models or a different parameterization of the same model give different results, which will be of little interest to the readers.

- Likewise, the discussion section does not provide a reasonable scientific contribution. For example, it is concluded (page 15, l.10f.) that the use of calibrated values would improve model performance. This statement is certainly true, but also obvious. I encourage the authors to repeat the analysis and focus on more relevant scientific questions, for example: - more complex/parameter-rich models are more likely to give accurate results, but are also more difficult to parameterize and to apply across sites. In that sense, is the use of more complex models justified in that case? Does better model performance outweigh the difficulty of finding the right parameter values? This is already discussed in the manuscript, but not clearly presented and not quantified. - what is a reasonable approach to estimate resistance values required in ET models? Which approach is most applicable here? Do the parameters have a mechanistic meaning, i.e. can the model be applied to other sites as well if some key biophysical properties are available? - are uncertainties mostly caused by surface resistance or aerodynamic resistance? What role do these individual resistances play under different environmental conditions?

- the presentation of the results is weak. Most of the figures show irrelevant or redundant information. For example, Figure 5 shows Rˆ2 values of individual flux sites that will not be meaningful for most readers who do not know the sites. Figure 6 is supposed to show values of surface temperature but axes labels and units denote LE values in W m-2. Tables 4-6 show results of individual flux towers at a single point in time, which is of little interest to the readers.

specific comments:

Introduction

- page 2, l. 19: "Directly measuring surface or canopy resistance can also be difficult" is rather optimistic. I am not aware of any approaches capable of measuring canopy resistance directly. - page 2, l. 20: there is nothing wrong with defining stomatal conductance as Cs, but I would strongly recommend to stick to the most common notation of gs, as it is simpler to read.

Methods

section 2.1.2: it needs become clearer why the formulation of Jarvis 1976 was chosen here instead of more recent stomatal conductance models such as Medlyn et al. 2011. I would not persist on taking the Medlyn model here, but it would be helpful for the reader to understand why the Jarvis model is taken, despite the fact that Medlyn is more often used in e.g. land surface models, and easier to parameterize (only 2-3 parameters)

- Eq. 3: change label to Eq. 3a,3b,3c,3d, which makes it easier to refer to

- Eq. 6: why are effects of atmospheric turbulence ("stability correction") not included here?

- p. 5, l. 17: since ra is calculated according to Eq. 6 in the Jarvis approach as well, I would show Eq. 6 directly after Eq. 3

- I wonder if rs in the Jarvis and KP parameterization is conceptually the same thing? rs in the Jarvis model is surface resistance only, whereas rs in the KP method includes both surface and aerodynamic resistances (Eq. 4). This could also contribute to the fact that rs as simulated by KP is much higher than the one given by Jarvis (Figure 8). In any case, differences between rs in Eq. 2 and 4 must be clarified, or a different notation used.

- throughout the manuscript, a clearer distinction between surface and aerodynamic resistance would be appropriate. In general, little attention is given to the aerodynamic part. I am sure many readers would be interested in the contribution of aerodynamic and surface resistances under different environmental conditions.

Results

- Figure 3: I do not think that the comparison of absolute LE fluxes is the best way of presenting the results. As indicated on page 9 l.7f., fluxes were Bowen ratio adjusted if energy balance closure is less than 80%. A critical discussion of the implications of this adjustment would be adequate.

- what is the justification for the sensitivity analysis as shown in Figure 10? In particular, what is the justification to assume an optimal temperature of 10degC for a C4 plant? Likewise, where do the different numbers of the rsmin come from? Why are previous estimates of rsmin so different (page 13 l. 25ff.)? These parameters will of course critically affect simulations of LE, but rather than just telling the reader what these different values would give in terms of LE, it would be be much more meaningful to discuss approaches to parameterize the models. E.g. how should we parameterize rsmin? This is a physiological parameter that can and should be measured, rather than optimized.

- Table 2: ordering the Table according to the site ID would make the Table easier to screen. Please also add the surface type as additional column. Please also remind the reader what year the column 'observation duration' is referring to.

- Figure 1: it would be clearer to show the extent of Fig. 1b in Fig. 1a as it is done for Fig. 1c and 1b.

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