Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-160-AC1, 2019

Hydrology and Earth System Sciences
Open Access
Discussions

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

Wen Li Zhao et al.

xiongyuj@mail.sysu.edu.cn

Received and published: 10 July 2019

Dear Reviewer:

Thank you for carefully reading the manuscript and providing constructive suggestions and comments. We address all of the comments point-by-point below. All of the revisions are highlighted in the new version of the manuscript.

NOTE: This revision of the manuscript (attached as supplement file) is based on comments from Reviewer 1 (because comments from Reviewer 2 were received on July 7, 2019); the newly added content is shown in blue, and any revised content is shown in red. Our answers to every question/comment are provided below.

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, shortcomings existed in the manuscript and I cannot recommend publication.

Responses:

Thank you for your positive evaluation, especially the comment on the introduction section. We attempted to revise the manuscript with the help of your comments and suggestions, which hopefully has improved the quality of the manuscript.

Comment 1: 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.

Responses:

Thank you.

As described in the introduction section, the topic of simulation uncertainty has high academic and practical relevance. In fact, our manuscript has received attention from the research community with more than > 390 views in last 50 days (please see the statistics at https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-160/#discussion).

The main goal of our manuscript is to identify the sources of uncertainty in resistance parameterizations in ET estimates. We quantified these uncertainties by comparing resistance parameterizations with different complexities with the one- and two-source Penman-Monteith (PM) equations. As such, we can investigate how the model structure and the parameterization process will affect the ET estimates. The method used in our study is simple, but the results are intuitive and straightforward. Scientific research does not necessarily require complicated methods. In other words, the figures mentioned above were quantitative explanations that show how the model structure and the parameterization process will affect the ET estimates and thus, improve our understanding of the uncertainty caused by resistance when estimating ET. Our reply to specific comment 10 shows an example that due to difficulty in observing physiological parameters, these values may exhibit substantial difference and cause uncertainty in ET estimation.

Nonetheless, in this revision, we reorganized the results and discussion sections based on your comments, and Figures 8-12 were revised accordingly. For example, the original Figs. 8 and 11 were merged to avoid redundant information. Please see the revised manuscript for details.

Comment 2: 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?

Responses:

Thank you for this detailed comment.

As you mentioned, because the section already discussed problems related to the impact of models with different complexities on ET estimation, we reorganized the discussion section and focused more on these scientific questions. However, several questions are currently beyond our analysis. For example, we analyzed the impact of a change in wind speed on the aerodynamic resistance, but aerodynamic resistance is difficult to determine, and the uncertainty from aerodynamic resistance is not a major topic in this study. Hence, we could not identify which factor (surface resistance or aerodynamic resistance) contributes more to the uncertainty in ET estimation.

In this revision of the manuscript, we revised the discussion section. In the beginning (Section 5.1 Uncertainty caused by resistance in ET estimates), we discuss the general influence of different model complexities and parameterization processes on the ET estimates and clearly presented the results. Section 5.2 still discusses the uncertainty in canopy resistance estimation but was also revised, because most of the discussed biophysical variables are commonly used in canopy resistance estimation, and the problems discussed in this section are faced by the research community. Similarly, Section 5.3 (Possible solution for reducing uncertainty in resistance parameterization) was kept and revised. We believe that the discussed biophysical boundaries, which were deduced from site experiments (observations) rather than from an understanding of their mechanistic theory, can cause substantial uncertainty in ET estimation. Therefore, calibrations of such biophysical boundaries are necessary. However, the calibrations are performed by trial and error and require additional observation datasets, which may be impossible to obtain in most applications. With these discussions, we hope to provide insightful information for interested readers. Please see the revised manuscript for details.

Comment 3: The presentation of the results is weak. Most of the figures show irrelevant or redundant information. For example, Figure 5 shows RËĘ2 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.

Responses:

Thank you for the detailed comment.

Figure 5 was intended (figure 6 in this revision) to evaluate models with different complexities and parameterizations at different maize sites because the study area was a maize-dominated heterogeneous oasis, and the maize fields exhibited varied plant biophysics and soil moisture content conditions. We redrew the figure by showing the model performance (MAPE) in the LAI-soil moisture space, which may provide intuitive information to readers even they do not know the sites, as follows:

[insert figure 6 here]

Figure 6 (figure 5 in this revision) was used to show the differences in LE estimates when using different temperature parameterizations. To prevent misunderstandings, we revised the notes in the figure, i.e., changing “Ts (Fluke)” to “LE_3T: LST (LE_3T: Fluke)” , as follows:

[insert figure 5 here]

Tables 3-6 were used as examples in several places in the manuscript. We believe they are necessary, but we moved them to a supplemental file.

In this revision, we reorganized and revised (enhanced) the results section. First, we present the ET estimates from models with different complexities and parameterizations (Section 4.1 Characteristics of the LE estimates). We then show the differences between the estimates (Section 4.2 Differences among the LE estimates and model performance), and we finally presented the sources of the differences between the estimates (4.3 Sources of difference among the LE estimates and model performance). Please see the revised manuscript for details.

Specific comments:

Comment 1: 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.

Responses:

Thank you. Based on this comment, we revised this sentence as follows:

Surface or canopy resistance cannot be measured directly, and these terms are often estimated and scaled from the leaf stomatal resistance or its inverse, the leaf stomatal conductance (gs), using the leaf area index (LAI) (Wang and Dickinson, 2012; Schymanski and Or, 2017) or estimated from flux-tower-based meteorological observations and the inversed one-source PM equation.

Comment 2: Introduction. 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.

Responses:

Thank you. Based on this comment, we changed Cs to gs (an example is shown in our reply to specific comment 1). Please see the revised manuscript for details.

Comment 3: 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).

Responses:

Thank you. Based on this comment, we added several sentences to Section 2.1 to explain the reason for using the formulation of Jarvis (1976) as follows:

For the one-source PM equation (Eq. 1), rs was calculated using two classical methods: the method of Jarvis (JA) (1976) and the method of Katerji and Perrier (KP) (1983) (hereby abbreviated as PM_JA and PM_KP, respectively). The empirical KP method requires fewer inputs than the JA method and can be used easily in practical applications, but the variables in the JA method have clear physical meanings that may better represent actual conditions. Therefore, we can address questions such as if more accurate results are likely to be obtained by more complex models. If the answer is yes, is a more complex model more difficult to parameterize and apply? Furthermore, algorithms with substantial differences are used to parameterize a given variable in the JA method; thus, we can investigate the uncertainty generated by the nonunique parameterizations. In addition, to accurately describe heat or water vapor transfer, the PM equation can be extended to include two or more sources depending on the configuration of the resistance networks. Therefore, a modified two-source PM equation that was proposed for RS applications (Mu et al. 2011) (abbreviated as PM_Mu) was also used to estimate ET as well as discuss the problems described above (see Section 2.2).

Comment 4: Methods. Eq. 3: change label to Eq. 3a, 3b, 3c, 3d, which makes it easier to refer to.

Responses:

Thank you. Based on this comment, we labeled the equations separately as suggested. Please see the revised manuscript for details.

Comment 5: Methods. Eq. 6: why are effects of atmospheric turbulence ("stability correction") not included here?

Responses:

Thank you. In this study, we mainly focused on the uncertainty caused by rs. Because

the calculation of ra depends on several parameters that are difficult to obtain accurately (e.g., atmospheric stratification), ra was estimated without considering the effect of atmospheric turbulence to reduce the uncertainty in ra estimation. Based on this comment, we added such information to Section 2.1 as follows:

2 Description of ET models

2.1 One-source PM equation and its parameterization

The PM equation is based on a single big-leaf assumption (one-source) and the energy budget closure, as follows (Monteith, 1965):

[insert Eq. (1) here]

where Rn is the net radiation; . . .; rs is the surface resistance and ra is the aerodynamic resistance.

In this study, we focus on the uncertainty caused by rs rather than ra. To reduce the uncertainty in ra estimation (e.g., lack of detailed atmospheric stratification data), ra was calculated without considering the effect of atmospheric turbulence in the one-source PM equation using Equation (2) (Brutsaert and Stricker, 1979; Irmak et al., 2008).

[insert Eq. (2) here]

Comment 6: Methods. 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.

Responses:

Thank you. Based on this comment, we adjusted the location of Eq. 6 and placed directly after Eq. 1 when introducing the PM equation (please see our reply to comment 5 for details). The orders of the related equations was also revised (please see the revised manuscript for details).

Comment 7: Methods. 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.

Responses:

Thank you. The parameterization method for rs differs significantly different between the Jarvis and KP methods. In the KP method, it uses the aerodynamic resistance as an input. Nonetheless, we believe that the purpose of the two methods is to calculate rs for the one-source PM equation.

Comment 8: Methods. 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.

Responses:

Thank you. This problem requires further study. However, we neglected the uncertainty from aerodynamic resistance in this study because is difficult to determine and its calculation depends on several parameters that are difficult to obtain accurately, such as the roughness height and atmospheric stratification.

Comment 9: 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.

Responses:

Thank you. In this study, the observed LE fluxes are corrected using the Bowen ratio closure method if the closure rate (represented by (LE+H)/(Rn-G)) is less than 0.8. Under such conditions, some of the LE estimates are not completely independent from the observations corrected using the Bowen ratio closure method, as raised in the comment. The comparison method is commonly adopted to validate modeled ETs (e.g., Ershadi et al., 2014, Agricultural and Forest Meteorology, 187, 46–61; Jiang and Ryu, 2016, Remote Sensing of Environment, 186, 528–547), including those retrieved from methods that contain the "Rn-G" term, such as the Penman-Monteith equation and the Priestley-Taylor method.

Based on this comment, we added text to discuss the problems in the last paragraph in Section 4.2 as follows:

As shown in Figs. 2 and 7, the investigation data covered different phenological stages and weather conditions during the 2012 growing season. For example, the daytime (7:00-19:00 GMT+8) solar radiation varied from 0 to 1055 W m^{-2}, with a mean value of 464 W m^{-2} and an SD of 307 W m^{-2} (Fig. 7a). The mean wind speed was 1.5 m s^{-1} with maximum, minimum, and SD values of 5.8, 0.2, and 1.1 m s^{-1}, respectively (Fig. 7b). The average temperature was 23.1 °C, with maximum, minimum, and SD values of 33.1, 8.4, and 4.6 °C, respectively (Fig. 7c). Although approximately half of the EC-observed LE values were adjusted with the Bowen ratio to achieve energy balance closure, which may make the ET estimates incompletely independent from the corrected observations, because the "Rn-G" term was used in both the adjustment and the model estimation, the validation results at a half-hourly scale, as shown in the previous sections, were recorded at different phenological stages and under various atmospheric conditions during the growing season, indicating a meaningful comparison.

Comment 10: Results. 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 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.

Responses:

Thank you. The value of minimum resistance for a leaf (rsmin) can be measured. However, measured values for plant species under different water and climatic combinations are rare. In this study, such data were unavailable; therefore, we cited these physiological values published values in a maize field that was adjacent to our study area.

In our calculation, the rsmin was set to 20 s m^{-1} according to Li et al. (Journal of Hydrology, 2013, 489, 124–134) (also Mu et al., Remote Sensing of Environment, 2011, 115, 1781–1800). The different value, i.e., rsmin=150 s m^{-1}, was used to perform the sensitive analysis, and this value was cited in another study from the same research group (Li et al., Agricultural Water Management, 2016, 178, 314–324). Similarly, the optimal temperature values used in this study (i.e., 35 and 10 °C) were applied in the arid Heihe River Basin (or adjacent areas). We used 35 °C for the optimal temperature according to Hu and Jia (Remote Sensing, 2015, 7, 3056−3087), but 10 °C was adopted for a sensitivity comparison. The value of 10 °C for maize was optimized based on field observations by Li et al. (Journal of Hydrology, 2013, 489, 124–134).

I communicated and discussed with Dr. Li personally. The reason he used an optimal value, rather than the observed one, is that although the observed values covering 4 years, the observation could not perform every day and year around (not consecutive), but the rsmin (or the optimal temperature) value varied in different phenological stages. In addition, one could not guarantee the measured physiological parameter, e.g., the

rsmin, is the minimum value.

Considering difficulties in field measurement and substantial difference exited in the same physiological parameter optimized from difference observational data set, we believe the sensitivity analysis in our study was necessary and may provide readers with useful information.

Comment 11: Results. 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.

Responses:

Thank you. We revised Table 2 as follows:

[insert Table 2 here]

Comment 12: Results. 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.

Responses:

Thank you. We revised the figure as follows:

[insert figure 1 here]

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

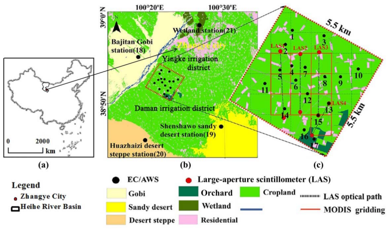

Figure 1: Study area: (a) locations of the Zhangye Oasis and the Heihe River Basin in China, (b) 21 EC flux towers in HiWATER, and (c) detailed locations of the 17 EC systems within the key experimental area in the Zhangye Oasis. (b) and (c) were revised from Ma et al. (2015); AWS and LAS are abbreviations for automatic weather station and large-aperture scintillometer, respectively.

Fig. 1.

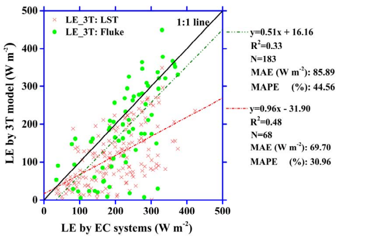

Figure 5: Comparison of the half-hourly ET estimates from the 3T model and measurements at a residential flux site. MAE and MAPE represent the mean absolute error and absolute percent error, respectively. The red crosses represent the estimated LE from the 3T model (LE_3T: LST) using the reference temperatures from the observed land surface temperatures (LSTs) at the 19th EC system, whereas the green dots are the LEs estimated by the 3T model (LE_3T: Fluke) using Fluke thermal-image-based temperatures as the reference temperatures.

Fig. 2.

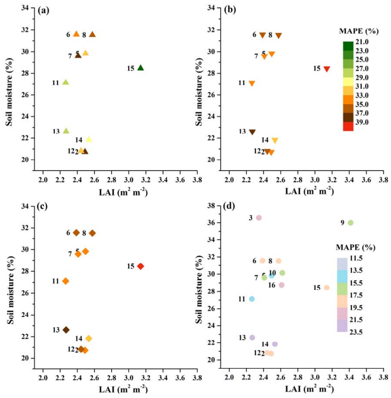

Figure 6: Performance of different models at different maize sites with different leaf area index (LAI) and soil moisture conditions in terms of mean absolute percent error (MAPE): (a) PM_JA method, (b) PM_KP method, (c) PM_Mu method, and (d) the 3T model. Note: the number represents the EC system ID in Figure 1c; the data are from Figure 4; panels a, b, and c have the same color legend.

Fig. 3.

Table 2. Details of the 17 eddy covariance (EC) systems in the key experimental area shown in Fig. 1c.

EC ID	Sensor type & manufactures	Sensor height (m)	Observation duration in 2012	Surface condition	LAI (m^2 m^{-2})	Soil moisture (%)
1	Gill/Li7500A, Gill, UK/Li-cor, USA	3.8	Jun. 16 to Sep. 17	vegetable field	1.83	36.04
2	CSAT3/Li7500, Campbell/Li-cor, USA	3.7	Jun. 15 to Sep. 21	maize	2.49	20.73
3	Gill/Li7500A, Gill, UK/Li-cor, USA	3.8	Jun. 25 to Sep. 18	maize	2.35	36.57
4	CSAT3/Li7500A, Campbell/Li-cor, USA	4.2 (6.2 after Aug.19)	May 31 to Sep. 17	residential area	–	18.65
5		3	Jun. 3 to Sep. 18	maize	2.50	29.83
6		4.6	May 28 to Sep. 21	maize	2.39	31.57
7		3.8	May 29 to Sep. 18	maize	2.41	29.59
8		3.2	May 28 to Sep. 21	maize	2.58	31.53
9	Gill/Li7500A, Gill, UK/Li-cor, USA	3.9	Jun. 25 to Sep. 17	maize	3.42	35.99
10	CSAT3/Li7500, Campbell/Li-cor, USA	4.8	Jul. 5 to Sep. 17	maize	2.62	30.14
11		3.5	May 29 to Sep. 18	maize	2.26	27.12
12		3.5	May 28 to Sep. 21	maize	2.44	20.82
13		5	May 27 to Sep. 20	maize	2.27	22.62
14		4.6	May 30 to Sep. 17	maize	2.53	21.83
15		4.5	May 25 to Dec. 30	maize	3.14	28.45
16	Gill/Li7500, Gill, UK/Li-cor, USA	4.9	Jul. 2 to Sep. 17	maize	2.61	28.76
17	CSAT3/EC150, Campbell, USA	7	May 31 to Sep. 17	orchard	1.65	28.90

Note: 1. All of the sensor types are open-path, and related information was cited from Liu et al. (2016);
2. The sampling frequency of the EC systems was 10 Hz, and the EC data were post processed, quality controlled, recorded every 30 min on average by Liu et al. (2016) and distributed by the HiWATER project;
3. LAI and soil moisture values were averaged using corresponding data provided by the HiWATER project.

Fig. 4.