https://doi.org/10.5194/hess-2020-310
Preprint. Discussion started: 20 July 2020

Hydrology and Earth System Sciences
Discussions

Comment on: "A review of the complementary principle of evaporation: From the original linear relationship to generalized nonlinear functions" by S. Han and F. Tian

Richard D. Crago[1], Jozsef Szilagyi[2], Russell Qualls[3]

[1] Department of Civil and Environmental Engineering, Bucknell University, Lewisburg, PA, USA

[2] Department of Hydraulic and Water Resources Engineering, Budapest University of Technology and Economics, Budapest, Hungary; also at School of Natural Resources, University of Nebraska, Lincoln, Lincoln, NE, USA

[3] Department of Biological Engineering, University of Idaho, Moscow, ID, USA

Abstract

The paper by Han and Tian reviews the history of developments in the complementary relationship (CR) between actual and potential evaporation and introduces the generalized complementary principle (GCP) developed by the authors. This comment assesses whether the GCP: 1) Can give reasonable results from a wide range of surfaces worldwide; 2) is supported by experimental data that verify the three-stages of evaporation implicit in the GCP, particularly in the wet-surface limit; 3) has been proven to be correct by the authors in a previous paper; and 4) is supported by model studies showing that wet surfaces occur predominantly during periods of

large-scale moisture convergence. The assessment finds that arguments in favor of the GCP deserve to be taken seriously, but ultimately remain unconvincing.

1. Introduction

Han and Tian (2020) (hereafter HT20) provide important insights into the growing body of literature regarding the Complementary Relationship (CR) of evaporation, and serves well as an accessible review of the literature. The sigmoid formulation (their equation 13), a key feature of their Generalized Complementary Principle (GCP) (Han and Tian, 2018; hereafter HT18) is presented and defended in their paper.

Two of the present authors (Szilagyi and Crago, 2019, hereafter SC19) wrote an earlier comment critiquing the sigmoid function for violating established physical principles (see also the reply by Han and Tian, 2019a). After further consideration, the present authors recognize that the Priestley and Taylor (1972) line at $x_H = E_{rad}/E_{Pen}=1/\alpha=1/1.26$ that appears in HT20 (their Figure 3), could be intended by HT18 and HT20 to mark a reference point on the graph, rather than to establish a limiting value that cannot be crossed. Unless otherwise noted, all notation herein follows that of HT20—see also Tables I and II for notation and variable names. Also, the role of a related (but different) adjustable parameter (also named α) seems to be used in the formulation primarily to adjust the shape of the sigmoid curve, rather than to set a limit on wet surface evaporation.

Table I Variables used

b	A GCP model parameter that adjusts the shape of the sigmoid function
E	Actual regional evaporation rate
E_{aero}	The second term of Penman's (1948) equation, related to the drying power of the air.
$E^{\mathrm{max}}_{\mathrm{MT}}$	Hypothetical maximum value of E that would occur from a wet patch in an otherwise completely desiccated region
E_{Pen}	Evaporation rate from Penman's (1948) equation
E_{PT}	αE_{rad} proposed by Priestley and Taylor (1972) for a wet regional surface with minimal advection
E_{rad}	The first term of Penman's (1948) equation, equivalent to the equilibrium evaporation rate of Slatyer and McIlroy (1961)
$\mathrm{E}^{\mathrm{Tws}}_{\mathrm{PT}}$	Value of EPT found if the slope of the saturation vapor pressure curve is estimated at the wet surface temperature, T_{ws} (see Szilagyi et al., 2016)
$f(E_{\mathrm{rad}}/E_{\mathrm{Pen}})$	A hypothesized function of $E_{\mathrm{rad}}/E_{\mathrm{Pen}}$
x_{H}	$E_{\mathrm{rad}} / E_{\mathrm{Pen}}$
x_{m}	$E^{Tws}_{PT} / E^{\mathrm{max}}_{\mathrm{MT}}$ the value of $E^{\mathrm{Tws}}_{\mathrm{PT}} / E_{\mathrm{Pen}}$ at which E goes to zero in the rescaled CR (Crago et al, 2016)
x_{max}	Parameter that sets the maximum value x_{H} can reach
x_{min}	Parameter that sets the value of x_{H} at which $y_{\mathrm{H}} \rightarrow 0$
y_{H}	E / E_{Pen}
α	The Priestley & Taylor (1972) parameter

https://doi.org/10.5194/hess-2020-310
Preprint. Discussion started: 20 July 2020

Table II. Abbreviations

BC4	Boundary condition 4: $d(E/E_{pen})/d(E_{rad}/E_{pen}) = dy_H/dx_H \rightarrow 0$ as as $y_H \rightarrow 1$
CR	Complementary Relationship (between actual and potential evaporation) proposed by Bouchet (1963)
GCP	Han and Tian's (2020) Generalized Complementary Principle
HT18	Han and Tian (2018)
HT20	Han and Tian (2020)
SC19	Szilagyi and Crago (2019)

The most controversial feature of the sigmoid function is the slope of the curve at the wet-surface limit. Namely, it requires that $d(E/E_{pen})/d(E_{rad}/E_{pen}) = dy_H/dx_H \rightarrow 0$ as as $y_H \rightarrow 1$ (hereafter, this boundary condition will be denoted "BC4"). That is, rather than a complementary relationship, BC4 requires that E and E_{Pen} are equal and that E exactly follows any variability by E_{Pen} in the wet surface limit.

BC4 deserves careful attention. A major purpose of this comment is to show that there are some indications such behavior can occur, but when it does it is a consequence of large-scale processes that disconnect the regional land surface from the overlying atmosphere, thus violating the basic assumptions behind the CR (namely, that atmospheric and surface conditions are tightly linked through surface fluxes). In light of this, corrections to the CR attempting to account for these exceptional cases will inevitably result in a formulation that does not accurately represent ordinary (minimally-advective) conditions.

https://doi.org/10.5194/hess-2020-310
Preprint. Discussion started: 20 July 2020

This comment will consider the evidence for the following four claims made by HT18 and HT20 in support of the sigmoid function and BC4: First, that the function works reasonably well to model evaporation from sites around the world; second, that data from these sites support a three-stage evaporation process and BC4, both of which are required by the sigmoid function; third, that HT2018 have provided a rigorous proof of the boundary conditions underlying the formulation; and fourth, that a partial explanation of BC4 has been provided by the study of Lintner et al. (2015).

2. Claim regarding modeling results

First, it is clear that the sigmoid function has been used successfully to model evaporation from flux stations around the world (see HT18). It is quite a flexible formulation that can match a wide range of data patterns on an (x_H, y_H) graph. Calibrated values of α and b published in HT18 (their Table 5) range from about 1.01 to 1.49 and from 0.59 to 17, respectively. Figure 1 shows the sigmoid function for the four combinations of these extreme parameter values (with x_{min}=0 and x_{max}=1). These show the wide range of possible curve shapes; allowing x_{min} and x_{max} to take other fixed values further increases the flexibility. Such an equation is likely to fit many datasets well, if tuning is permitted. Of course, any CR formulation must ultimately work well without requiring local calibration of parameters.

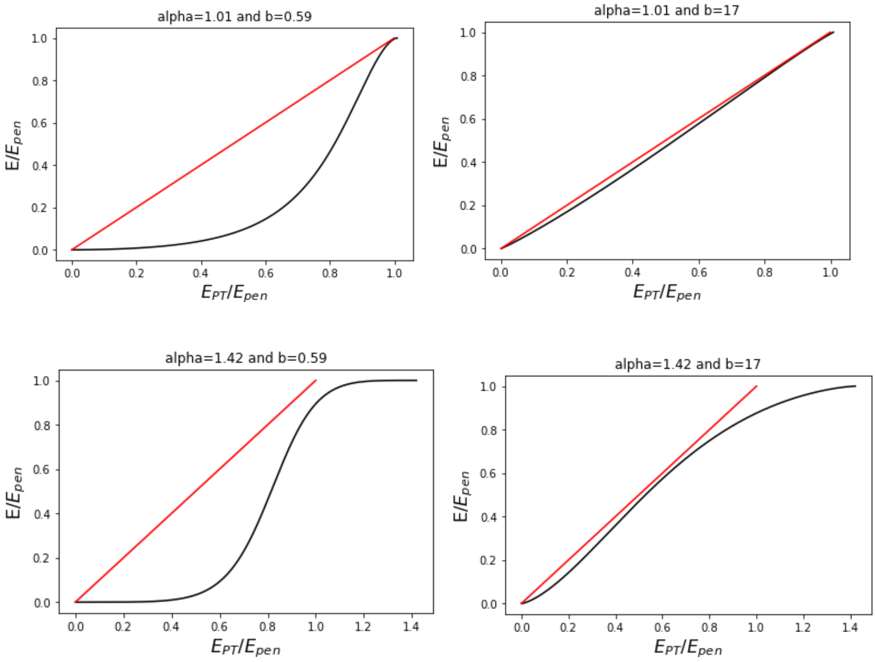

Figure 1. The sigmoid function (black curves) and the Priestley-Taylor line (α=1.26, straight line in red) for the most extreme parameter values documented in HT18. The scales of the horizontal axes differ.

3. Claim regarding empirical support for three evaporation stages and for BC4

Second, there does seem to be some empirical support for different slopes at different positions on (x_H,y_H) graphs (HT18, their Table 3). However, the curve proposed by Brutsaert (2015) also proposes a shallow slope for small y_H,(stage 1) a steep slope in the middle (stage 3), and a less steep slope near y_H=1 (stage 3). Similar behavior is also possible with the rescaled models of the present authors. The stage 3 slopes at large y_H values (HT18, Table 3) would be near zero according to BC4, but are generally near 1 instead. HT18 directly address BC4 with data in their Figure 6, which plots empirical data along with red curves resulting from the sigmoid function

https://doi.org/10.5194/hess-2020-310
Preprint. Discussion started: 20 July 2020

relating E/E_{PT} to E/E_{Pen}. The sigmoid function curves show E/E_{PT} increasing as E/E_{Pen} increases, until E/E_{PT} reaches a peak and then begins to decrease with further increases in E/E_{Pen}. Correlational evidence for this downturn is given by HT18, but the actual data plotted do not visibly follow the downturn in E/E_{PT} in either panel of Figure 6; the dramatic downturn in the red curve Figure 6(a) (the left panel) certainly is not matched by the data.

4. Claim regarding the derivation by HT18

Third, the derivation by HT18 is inconclusive. The derivation begins [HT18, their Eq. (8)]:

$$E = (E_{pen}) * f(E_{rad} / E_{pen}), \text{ where } E_{pen} = E_{rad} + E_{aero} \qquad (1)$$

where f is a function of (E_{rad}/E_{pen}). Partial derivatives of E were taken from Eq. (1) with respect to E_{rad} and E_{aero}. Further manipulations of these derivatives resulted in the four boundary conditions corresponding to the sigmoid curve (HT18). The function $f(E_{rad}/E_{pen})$ in Eq. (1) could include constants or parameters (for instance α, x_{min}, and x_{max}), whose "correct" values can be found by calibration, after which they must be treated as constants. This means that, once the parameters are determined, the shape of $f(E_{rad}/E_{pen})$ is also determined.

Unfortunately, this leads to two problems. First, the present authors' work with the "rescaled" CR (Crago et al., 2017, Szilagyi et al., 2017, Crago and Qualls, 2018) gives evidence that the variable $x_m = \mathrm{E}^{Tws}{}_{PT} / \mathrm{E}^{max}{}_{MT}$, ($x_m$ is our own notation) related to the value of $E^{Tws}{}_{PT} / E_{Pen}$ at which E goes to zero, is in fact a variable, not a constant. It must be calculated for each individual data point, and it results in a significant re-arrangement of the data. It could have been included in Eq. (1) by writing Eq. (1) as: $y_H = f(x_H, x_m)$. By taking derivatives without including the impact that a variable x_m might have, HT18 assumed from the beginning that E/E_{pen} does not vary with x_m, so a variable x_m boundary condition could not possibly arise from this derivation. On the other hand, if x_m is in fact a significant variable (as the papers cited above suggest), it could impact the entire derivation, but particularly the two dry-limit boundary conditions.

The parameter x_{max} is the maximum value x_H can reach, and is usually taken by HT18 and HT20 to be 1.26^{-1}, where 1.26 is the commonly-accepted value for the Priestley and Taylor parameter α. To prove that $dy_H/dx_H \rightarrow 0$ as $y_H \rightarrow 1$ (the most controversial finding of the derivation), HT18 had to show that $\partial x_{max}/\partial E_{rad}$ evaluated at y=1 cannot be 0 (see the paragraph starting at the bottom of page 5054 and ending at the top of page 5055 of HT18). But if Eq. (1) is true, x_{max} has to be treated as a constant, so the partial derivative must be 0. It is impossible for x_{max} to be a constant for the purpose of taking derivatives of Eq. (1), but a variable when evaluating $\partial x_{max}/\partial E_{rad}$. Thus, there is a logical inconsistency hidden in this derivation. SC19 showed that, if the Priestley-Taylor α (equivalent here to $1/x_{max}$) is actually a constant, HT18's derivation does not result in a specific required value for dy_H/dx_H at y=1. Thus, the boundary condition $dy_H/dx_H \rightarrow 0$ as $y_H \rightarrow 1$ does not follow from (1).

To sum up consideration of the derivation, three of the four boundary conditions (slope and intercept at the point where $y_H \rightarrow 0$, and slope as $y_H \rightarrow 1$) are doubtful due to the assumptions made when (1) was used as the definition of E.

5. Claim regarding support from the modeling study of Lintner (2015)

HT18 cite the modeling results of Lintner et al. (2015) in support of BC4. This study used a steady-state model that captured the key physical processes affecting evaporation. Model results show decreases in both E_{Pen} and E as soil moisture approaches saturation, similar to the behavior required by BC4. According to Lintner (see also HT18), large-scale horizontal moisture convergence decreases E_{Pen} by increasing atmospheric humidity, and at the same time it increases precipitation and thus soil moisture content. Near the wet limit, water availability matters less than E_{Pen} in determining E, so E and E_{Pen} decrease at the same rate. Thus, at the point of saturation, $E=E_{\mathrm{Pen}}$, and $\mathrm{d}(E/E_{\mathrm{Pen}})/\mathrm{d}(E_{\mathrm{PT}}/E_{\mathrm{Pen}}) = 0$, apparently satisfying BC4.

But note that the normal (i.e., minimal moisture convergence, divergence, or advection) behavior for a wet surface is $E=E_{\mathrm{Pen}}=E_{\mathrm{PT}}$ (e.g., Brutsaert, 2015). The only way to get BC4-type behavior is to impose a large-scale process that causes E_{Pen} to differ from this value. That is, BC4 is not describing the drying process and the CR at all; rather, it is describing what happens when the CR simply does not apply. The scenario described by Lintner et al. (2015) requires a clear disconnect between the land surface processes and the overlying atmospheric conditions, violating the central assumption of the CR (e.g., Brutsaert, 1982, 2005).

It need not be the case that nearly-saturated surfaces coincide with moisture convergence outside of steady-state models. Nearly-saturated surface conditions can exist under a range of large-scale patterns, including positive, negative or negligible moisture convergence or advection. This is the case because soil moisture content varies at larger time scales than most other components of the surface water and energy budgets (e.g., Sellers et al, 1992), so nearly-saturated surface conditions can persist after a period of moisture convergence has ended. Furthermore, saturated surfaces can occur from other processes, such as thunderstorms driven by surface heating.

6. Conclusions

HT18 and HT20 have martialed several empirical and theoretical arguments in support of their proposed sigmoid formulation of the CR. The range of arguments and data sources used is impressive, and the present authors only recently recognized the specific nature and the impact of this challenge to other CR formulations. There is little doubt that some aspects of their argument are true, including the ability of their formulation to match numerous experimental datasets. Nevertheless, the specific boundary conditions leading to the sigmoid function are not well-supported by empirical data; the derivation of the boundary conditions by HT18 was inconsistent regarding which model values are constants and which are variables; and the argument that large-scale processes require adoption of BC4 fails because it essentially makes the exception (large-scale processes dominating land surface processes in determining near-surface atmospheric conditions) into the rule, and in doing so, it violates the assumptions of the CR. The CR should ideally only be used under circumstances where advection is minimal. Attempts to adjust for other conditions (e.g., Parlange and Katul, 1992) are possible, but should not over-ride consideration of the basic CR concept. This may require developing specific conditions for screening data.

There does not seem to be consensus in the research community on any of the boundary conditions of the CR except for x_H=1 when y_H=1. The current authors find the evidence for a variable x_m to be strong. This value can be calculated separately for each data point and it leads to a rescaling of the x_H-axis, and a resulting reduction in the scatter of the data points (Crago and Qualls, 2018).

While the sigmoid formulation is clearly the result of a serious and substantial research program, the difficulties with it described here are serious enough that we cannot see it as an improvement over other recent CR formulations.

References

Bouchet, R.: Epototranspiration reelle et potentielle, signification climatique, Int. Assoc. Hydrolog. Sci. Publ., 62, 134-142, 1963.

Crago, R. D., Qualls, R. J.: Evaluation of the generalized and rescaled complementary evaporation relationships, Water Resour. Res., 54, 8086-8102, 2018.

Crago R., Szilagyi, J., Qualls. R., and Huntington, J.: Rescaling the complementary relationship for land surface evaporation, Water Resour. Res., 52, 8461-8471, doi: 10.1002/2016WR019753, 2017.

Han, S. and F. Tian: Derivation of a sigmoid generalized complementary function for evaporation with physical constraints, Water Resour. Res. 54, 5050-5068, doi: 10.1029/2017WR021755, 2018.

Han, S. and F. Tian: Reply to comment by J. Szilagyi and R. Crago on “Derivation of a sigmoid generalized complementary function for evaporation with physical constraints,” Water Resour. Res., 55, 1734-1736, doi:10.1029/2018WR023844, 2019a.

Han, S. and Tian, F.: A review of the complementary principle of evaporation: From original linear relationship to generalized nonlinear functions, Hydrol. Earth Syst. Sci., 24, 2269–2285, https://doi.org/10.5194/hess-24-2269-2020, 2020.

Lintner, B. R., Gentine, P., Findell, K. L., and Salvucci, G. D.: The Budyko and complementary relationships in an idealized model of large-scale land-atmosphere coupling, Hydrol. Earth Syst. Sci., 19, 2119-2131, 2015.

Parlange, M. B., Katul, G. G.: An advection-aridity evaporation model, Water Resour. Res, 28, 127-132, doi:10.1029/91WR02482, 1992.

Penman, H. L.: Natural evaporation from open water, bare soil, and grass, P. Roy. Soc. Lond. A, 193, 120-145, 1948.

Priestley, C. H. , and Taylor, R. J.: On the assessment of surface heat flux and evaporation using large-scale parameters, Mon. Weather Rev., 100, 81-92, 1972.

Sellers, P.J., Hall, F. G., Asrar, G., Strebel, D. E., and Murphy, R. E.: An overview of the First International Satellite Land Surface Climatology Project (ISLSCP) Field Experiment (FIFE), J. Geophys. Res., 97 No D17, 18345-18371, 1992.

Slatyer, R. O. and McIlroy, I. C.: Practical Microclimatology, Melbourne, Australia: CSIRO, 1961.

Szilagyi, J. and Crago, R.: Comment on “Derivation of a sigmoid generalized complementary function for evaporation with physical constraints” by S. Han and F. Tian, Water Resour. Res., 55, 868-869, doi: 10.1029/2018WR023502, 2019.

Szilagyi, J. Crago, R., and Qualls, R.: Testing the generalized complementary relationship of evaporation with continental-scale long-term water-balance data. J. Hydrol., 540, 914-922, doi: 10.1016/j.hydrol.2016.07.001., 2016.