

Interactive comment on “Estimating actual, potential, reference crop and pan evaporation using standard meteorological data: a pragmatic synthesis” by T. A. McMahon et al.

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1 Summary

The manuscript and accompanying supplementary material comprises a very extensive and useful collection and documentation of existing approaches to estimation of evaporation. It has the potential to become a widely used reference, and facilitate further synthesis and research in the field of evaporation from lakes and land. The manuscript is generally well written and the supporting material a very valuable re-

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source. However, as it stands, the manuscript is not very helpful for understanding the underlying processes, and the reader risks to get lost in the fine differences between different approaches. I believe that the manuscript would benefit tremendously if the authors added a general discussion of the underlying processes and related all the different approaches back to these processes. I also found a number of other issues that need revision, but I am confident that the manuscript will become a very important contribution to hydrology and earth system science.

2 General comments

The paper entitled “Estimating actual, potential, reference crop and pan evaporation using standard meteorological data: a pragmatic synthesis” by McMahon *et al.* gives a very useful overview of the literature and different approaches relating to the estimation of potential and actual evapo(trans)piration from water bodies, evaporation pans and vegetated land surfaces. The paper was quite eye-opening to me with regards to the wealth of literature dedicated to this topic. It is well written and accompanied by extensive and carefully prepared supplementary material, which is of similar importance to the paper itself. I was only able to consult a couple of sections in the supplementary material and was not able to review it as a whole. I believe that it can be very helpful but it would be even more helpful if the authors referred more explicitly to the original equations in the main paper and if they used the same notation.

I agree with Jozsef Szilagyi, that this work is likely to become a widely used reference for years to come. However, this also imposes an elevated responsibility on the authors to inform the reader in a complete and verifiable way. To this effect, I found a few shortcomings that I hope the authors will be able to address in a revised manuscript.

As it stands, the paper is a very useful and well referenced catalogue of approaches to estimate evaporation at local to regional scales and seasonal to decadal time scales.

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What it still lacks a bit, is the synthesis component. The reader is confronted with a lot of puzzle pieces involving radiative and aerodynamic components, effects of wind, surface temperatures, resistances and upwind-downwind effects, and a large variety of empirical or calibrated constants to replace missing data or knowledge. Unfortunately, without considerable prior knowledge, the reader is likely to have trouble distinguishing between the different approaches and equations and make an informed decision about the most appropriate approach for a given case, while keeping in mind associated assumptions and uncertainties. An example for this shortcoming is the introduction of the complementary relationship in Section 2.5.1. The reader is confronted with the equation and a conceptual plot of the “theoretical form”, without being given the motivations behind it or justification for it. I would find it very helpful if the authors gave a general introduction to the evaporation process up front and described all the environmental factors that may have an effect on it. Then, they could link back all of the different approaches to this general description and help the reader understand which aspects are considered, which are ignored and which are replaced by empirical parametrisations.

Another area requiring improvement is a consistent set of units. Despite their best efforts and intentions to convert empirical constants to a consistent set of units, the units are still a mess in some parts. In the Specific comments below, I suggest the use of SI units throughout to avoid confusion and point out a number of equations where the units do not match. I believe strongly that the field as a whole would benefit from the consistent use of SI units and this paper is a great opportunity to motivate a move in this direction.

Section 3.9 contains recommendations for the choice of appropriate approaches for estimating evaporation under different scenarios. The recommendations given here are not justified in a verifiable way. They are “based on the information summarised in the paper and in the supplementary materials along with the authors’ personal experiences”. I am a bit concerned that the “preferred” stamp given in Table 4 could

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be viewed by some readers as a justification to use a given approach off the shelf without considering its particular assumptions and shortcomings. Therefore, I would strongly recommend to use a consistent set of criteria for assigning the different tags (preferred, acceptable, not preferred, not recommended) and to clearly communicate these criteria. Criteria that are mentioned include theoretical background, extent of testing, consideration of particular effects (e.g. heat storage), need for calibration, potential to obtain very wrong estimates (e.g. negative values) and the degree of adoption by the community. Since the authors did not elaborate on what they consider the fundamental theoretical background for estimating evaporation, it is not clear what they consider an acceptable level of theoretical background in a given model. It is also not specified what the authors consider adequate testing and how they weigh up the need for calibration against inclusion of theoretical background. Therefore, I see Table 4 as an expression of the authors' opinion, which is undoubtedly based on extensive experience, but not obviously on verifiable evidence.

3 Specific comments

1. P11830L21: The term “hard-wired evaporation estimates” is not clear at this stage. It may be helpful to explain that you mean automatic calculation of evaporation in commercial weather stations.
2. P11833L12–17: A logical and useful nomenclature would be e.g. to refer to transpiration for vapour flux through stomata and evaporation for evaporation of interception and soil evaporation. Evapotranspiration could be reserved for the sum of all these fluxes and one could then separate out the different components by referring explicitly to soil evaporation, evaporation of intercepted water and transpiration. It does not appear useful to use the same terms as the different publications (e.g. evapotranspiration), unless the different definitions are clarified

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- for each case.
3. P11833L27: What does it mean that “effects of upwind advections are negligible”? Does it mean that advected energy is negligible because it is small in comparison to the total latent heat flux? This would probably be a lot clearer if the evaporation processes were introduced in a control volume framework up front.
 4. P11834L1–: Why are seasonal heat storage changes in *shallow* lakes insignificant? Is this because of their low heat capacity and hence little heat storage capacity? The sentence seems a bit counter-intuitive, as I would expect relative heat storage changes to be much larger in shallow lakes than in deep lakes. Also, why are changes in heat storage considered unimportant at the annual scale even for deep lakes? Please explain/clarify.
 5. P11834L14–16: Very good point about adjusting the empirical constants to a consistent set of units. However, I would propose the International System of Units (SI), which was specifically designed to be consistent and is widely adopted across disciplines. Except for time, I see no reason not to use SI-units throughout this paper. Accordingly, evaporation could be expressed in kg m^{-2} per unit time, pressure in Pa and radiation in J m^{-2} per unit time. As a result, the following statement would be that evaporation of 1 kg m^{-2} (instead of 1 mm) requires $2.45 \times 10^6 \text{ J m}^{-2}$ energy at 20°C , given that the latent heat of vaporisation is $2.45 \times 10^6 \text{ J kg}^{-1}$. For more efficient notation, one could also use $2.45\text{e}6 \text{ J kg}^{-1}$ after appropriate introduction. In the current notation, if the units of evaporation are in mm, the units in Eqs. 4, 5, 6 etc. do not match, unless the latent heat of vaporisation (λ) is re-defined in units of MJ m^{-3} instead of MJ kg^{-1} and the result multiplied by 1000 mm m^{-1} . Since evaporation is usually considered a mass flux and not a volume flux, the appropriate units are kg m^{-2} per time, while the reader should have no trouble remembering that 1 kg m^{-2} roughly represents a water column of 1 mm.

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6. P11836L18: If EP2 is the lower limit of actual evaporation from a wet surface, why is it then $EP2 \geq E_{Act}$ in Eq. 3?
7. P11836L26–: This is confusing and has nothing to do with different processes and directions. Any flux process can be expressed as a function of a driving force (directional) and a resistance (1/conductance, non-directional). In this context, potential evaporation refers to a specific combination of demand and resistance, not to demand only, as implied in this sentence. Please clarify.
8. P11837L4–5: What does “without advection or heating effects” refer to? No negative sensible heat flux? What is the difference to reference crop evaporation? What is the difference between “growing vegetation” (here) and “reference vegetated surface” (below)?
9. P11837L22: E_a seems to be an important part of the equation, so the description is not complete without specifying what it represents and how it is estimated.
10. P11838L2: What does “no water-advected energy” mean?
11. P11838L13–14: On P11837L14–15, you stated that Penman eliminated the surface temperature variable. Why do you state now that both Penman’s equation and the Penman-Monteith model depend on surface temperature?
12. P11838L19: The main difference between Eq. 5 and Eq. 4 is that Eq. 5 does not assume $G=0$ and it refers to surface and atmospheric resistances, whereas Eq. 4 does not. In the text, this is not mentioned at all, but instead this equation is presented as the result of eliminating surface temperature. The description given here does not help the reader to understand differences and common grounds between Penman and Penman-Monteith.
13. P11839L8: If the 2.45 in Eq. 6 refers to the latent heat of vaporisation, please denote it as such and give its correct units. If it is unitless, as implied in this

- equation, the units do not balance.
14. P11839L17: What kind of wet surfaces were considered here? Bare soil, open water, short crop, forest?
 15. P11840L7–: This is not easy to understand conceptually. If there is no exchange between the “air passing over a saturated surface” and the overlying air masses, then I would imagine that it would indeed become fully saturated as the distance it passes over goes to infinity. What would the equilibrium rate of evaporation be per unit area? Is the unit area infinite, then? The “surface temperature of the evaporating surface at which the net rate of heat exchange is zero” would be the dew point temperature for latent heat exchange. What heat is meant here? Sensible heat, latent heat or the sum of both?
 16. P11840L20: The term “closed evaporating system” sounds like an oxymoron, as the definition of a closed system is that there is no mass exchange across its boundaries. Could you explain what it means? A closed system within which both evaporation and condensation happen?
 17. P11841L4: I believe that the Thornthwaite equation should be discussed here, as it is widely used and referred to multiple times later on in this paper.
 18. P11841L13: The units do not match in Eq. 8. It should be possible to separate the physically based parts from the empirical/calibrated parts to help the reader understand its meaning. For example, what does $\gamma \frac{900}{T_a + 273}$ or $\gamma(1 + 0.34u_2)$ represent?
 19. P11844L18: Please explain how the reader can verify the applicability of this assumption for a particular case. Does this imply that the open water body should not exceed a certain size?

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20. P11845L4: Eq. 12 is very similar to Eq. 4. The descriptions should be merged with those related to Eq. 4 (e.g. the description of E_a) and the reader could be referred to Eq. 4 for details, while here the authors could just discuss how it is applied to estimate open-surface water evaporation.
21. P11846L11: Again, the assumption of “no advected energy and, hence, the actual evaporation does not affect the overpassing air” is not clear to me. By advected energy, I imagine the heat content of the incoming air, that can be extracted for evaporation by absorption of sensible heat flux and cooling of the outgoing air. In contrast, actual evaporation affecting the overpassing air, would imply to me that it modifies its vapour pressure, which would be an entirely separate assumption. How can the applicability of these assumptions be assessed for a particular case?
22. P11846L14: Are the same parameter values of the wind equation applicable at different time scales?
23. P11846L20: Why is the boundary layer resistance not taken care of by the wind equation? What else is the wind equation for? This should become clear if the authors give a general introduction to the evaporation process and relate all of the different approaches to it.
24. P11847L7: The saturated vapour pressure at the water surface cannot be calculated without knowing the temperature at the water surface. Please specify how this can be obtained for using Eq. 14. Or is the approach based on the assumption that the temperature at the water surface equals air temperature?
25. P11847L11–: Please provide the motivation and justification for the complementary relationship. Neither the equation nor the figure are self-explanatory enough to help understand its meaning and applicability.

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26. P11848L20–: Please provide a brief explanation why there are three different models (CRAE, CRWE and CRLE) and what they are used for. Without such a brief introduction, the respective sections about the models are very confusing, and it was not clear to me what their differences are and what each of them is used for.
27. P11849L1: How does the complementary relationship take into account the modification of air passing from land to a lake environment? This is not clear from the description of the complementary relationship.
28. P11849L7: What is “the Morton methodology”?
29. P11849L15–: The units do not match in Eqs 16 and 17. $\gamma p f_v (T_e - T_a)$ would give units of $\text{mbar K}^{-1} \times \text{mbar} \times \text{W m}^{-2} \text{mbar}^{-1} \times \text{K} = \text{mbar W m}^{-2}$, which is not the same as the units of R_n . Despite the promise on P. 11834 to use a consistent set of units (e.g. pressure in kPa), here pressure is expressed in mbar. Further, the units for the latent heat of vaporisation (λ) are given in “W day kg^{-1} ”, which can be expanded to $\text{J s}^{-1} \text{day kg}^{-1} = 3600 \times 24 \text{s day}^{-1} \text{J s}^{-1} \text{day kg}^{-1} = 86400 \text{J kg}^{-1}$. I hope you agree that this is confusing. What is the value of f_v and what does it depend on? I was unable to find Eqs. 16 and 17 in the supplementary material S21. Regardless of the supplementary material, it would be helpful to also provide the values of constants used in the equations, e.g. λ , γ and σ , in their respective units.
30. The meaning and calculation of the term b_1 was not discussed in Sect. 2.1.3. Please explain.
31. P11856L14: The term “water advected energy” is not clear. Do you mean the water-equivalent of the advected energy, i.e. the energy divided by λ ?
32. P11858L13: It is important to point out that the coefficients can vary between

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- 0.47 and 2.19 seasonally and between 0.66 and 1.00 between lakes at the annual scale.
33. P11858L19: Is it just about advected energy or also about the effect of lake evaporation on air humidity? A general discussion based on a control volume approach in the introduction would have helped.
 34. P11859L21: Increased evaporation in comparison to large lakes? Is this due to greater importance of heat absorbed from the overpassing air and/or reduced importance of humidifying the overpassing air by evaporation?
 35. P11860L15: Is $\Delta S = 0$ at annual time step justified? How can it be verified for a particular case?
 36. P11860L7: Can you provide a reference for the “simple Thornthwaite soil moisture model”?
 37. P11863L21: Is this due to the absorption of sensible heat from the overpassing air? The reasons were not explained in Sect. 3.2, either.
 38. P11866L27: Why is the Matt-Shuttleworth model considered acceptable for specific crops in windy semi-arid regions? What crops, how windy?
 39. P11867L17: How was the “strength of the theoretical basis” assessed?
 40. P11868L20: If wind is indeed important, this can only mean that the wind effect is hard-wired in Morton. What additional assumptions have to be satisfied?
 41. P11868L22: What is considered a successful application? Could you specify?
 42. P11869L3: Why would neglecting a heat source lead to *over*-estimation of evaporation?

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43. P11870L1: What is the relevance of these numbers? Depending on local climate patterns, the error could be a lot more. Imagine e.g. taking atmospheric forcing from a site in Northern Italy to estimate lake evaporation from Lake Zurich. This would also be just a 110 km distance.
44. P11870L11: Should this be -3.19 mm yr^{-2} (negative trend)?
45. P11871L16: This sentence is unclear.
46. P11872L2: What does it mean to calibrate a model with potential evaporation inputs? Do you mean calibration of a potential evaporation-forced model to runoff data? What is the relevance of this finding, then? The calibration procedure could compensate errors in the forcing.
47. P11876L13: Again, what does this mean and when is it the case?
48. P11877L3: This statement does not say much about the appropriateness of the estimation methods. If there is enough freedom in the calibration, the model may equally reproduce runoff if potential evaporation was replaced by the time series of e.g. net radiation.
49. Table 1: According to Eq. 6, air temperature, net radiation and ground heat flux are needed for the Priestley-Taylor approach. Why does Table 1 imply that only sunshine hours are needed?
50. Table 4: The last sentence in the caption is misleading, as it implies that this table does not contain empirically-based techniques, whereas most, if not all, of the models in the table are empirically based to various degrees.
51. Table 5: The information here is very helpful, but the table is very difficult to read. I would recommend putting the descriptions in footnotes and also explaining what the numbers mean. Table 6 seems to contain the same information again, so

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perhaps, the descriptions could simply be moved to footnotes in Table 6 and Table 5 could be removed.

52. Figure 1: The caption needs a lot more explanation to make this figure useful.
53. Figure 2: Likewise, a brief description of the meaning of this figure would be helpful.

4 Technical corrections

- P11832L5: “a historical”
- P11832L6: “including for” sounds unusual. Maybe better: “many practical needs for daily or monthly actual evaporation estimates, including deep lakes or post-mining voids...”?
- P11832L24: “should note that there are”
- P11832L27: “indicated”
- P11841L7–: This sentence is incomplete (should be “is defined as follows”?).
- P11855L23: Repetition of “needs to be considered”. It may be better to delete the first sentence and write: “Second, if the inflows to a lake are...”
- P11867L10: Priestley instead of Priestly (twice)

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