Authors responses (in red) to short comments and referees comments (in black)

J. Szilagyi (Short comment)

First of all I would like to commend the authors on the breath of work they have taken up. I think their study will become an instant work of reference people will consult many-many times in the coming years.
The authors thank Professor Szilagyi for his supportive comments.

I would like to point out one minor typo in their Table S13: The ET and precipitation values are for a year, not for a day, as the unit in the title of the Table suggests.
DONE: Corrected in revised manuscript.

I would also like to submit a comment concerning the Budyko approach that relates the mean annual value of E / P to Ew / P (E is actual, Ew is a certain potential evaporation rate and P is precipitation), in a functional form. Szilagyi and Jozsa (2009) realized that the sought for functional relationship can be expressed by the Complementary Relationship (CR) of Eq. (15) of McMahon et al.
E = 2Ew –Epot (1)
by dividing both sides by P and factoring the Ew / P term out to obtain
E / P = (2 – Ep/Ew) Ew / P . (2)
In the CR Ew is the wet environment (potential) evaporation rate, while Epot is the Penman potential evaporation rate. As Morton argues, the main difference between the two types of potential evaporation terms is due to their differing size: the former has a regional extent (Ew) while the latter is the size of a pond (Epot). So this way the CR tells us how the function in the Budyko equation can be obtained: i.e., by the ratio of two types of potential evaporation rates, both valid under energy limited conditions, but one is affected by advection of energy due to its small size. I think this is an important issue, most people are not aware of. Note that the Ep/Ew ratio can incorporate the changes of environment, as Donohue et al. and Zhang and Chiew note, referenced in McMahon et al., about the Budyko approach. So this way two different pieces of the evaporation puzzle are connected and it still takes time to figure out the ensuing implications. Reference: Szilagyi, J. and Jozsa, J. 2009. Complementary relationship of evaporation and the mean annual water-energy balance, Water Resources Research 45, W09201 doi:10.1029/2009WR008129.
DONE: We thank Professor Szilagyi for this very good comment. At the end of the paragraph discussing Budyko's steady-state hydroclimatological framework we added one sentence noting that several researchers, including Szilagyi and Jozsa (2009), are extending Budyko's framework in a varierty of ways to better account for sub-annual transitory processes such as: (i) rooting depth dynamics; (ii) soil store dynamics; and (iii) groundwater dynamics.

S.J. Schymanski (Referee)

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 resource.
The authors thank Dr Schymanski for his detailed incisive comments and excellent review. We trust our amendments and corrections in response to his extensive comments make the paper more readable and useful.

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.
DONE: As suggested, we have added (nearly 700 words to Section 1) a discussion of the underlying evaporative processes and related issues. For the new material please see paragraph beginning at P11833L12.

2. General comments (We have number the general comments by letter a, b,...)
a) 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.
Once again, thank you for your generous comment.

b) 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.
We have considered all the points raised by Dr Schymanski and outline below our position on each.

c) 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. 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.
DONE: We expanded our discussion of the Complementary Relationship (CR), please see Section 2.5.1 in our revised manuscript.

d) 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 parameterisations.
DONE: We added to the Introduction an explanation of the evaporation processes and related issues (this increased the length of the text by nearly 5%). Where appropriate we linked the different approaches back to this description.

e) 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.
DONE: Except for the Morton equations we do use a consistent set of units throughout the paper and supplementary material. Regarding Morton, we elected not to amend his equations to be consistent with the rest of the paper especially as we have included a Fortran 90 listing of his WREVAP program. The original units were adopted to ensure that an error was not injected into the values of the empirical constants through a change in units; there would have been quite a few constants to be adjusted. As this paper is partly directed at practicing hydrologists and consulting engineers, we deliberately chose not to use SI units but rather use consistent units that are common in the hydrology literature. We propose to retain the current set of units.

f) 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 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.
DONE: We have provided greater justification in the revised manuscript for our recommendations contained in the table. In assessing a procedure, we gave more credence to a theoretically based approach compared to an empirical method.

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.
DONE: The text has been reworded appropriately.

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.
DONE: We deleted the paragraph in question and revised our definitions of transpiration and evapotranspiration.
It does not appear useful to use the same terms as the different publications (e.g. evapotranspiration), unless the different definitions are clarified for each case.

DONE: We checked each occurrence of 'evapotranspiration' to ensure this usage conforms with the revised definitions.

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.
DONE: In the revised manuscript we have expanded the sentence to indicate the effect of low water vapour concentration advected over the lake is quickly dissipated.

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.
As stated in the manuscript for shallow lakes the sub-surface heat exchange is small relative to the other heat fluxes (Morton, 1983a, page 82). No change to text.

Also, why are changes in heat storage considered unimportant at the annual scale even for deep lakes? Please explain/clarify.
DONE: Over an annual cycle there is no net change in the heat storage (there is a phase shift depending on the depth of the lake), so that the sum of the seasonal lake evaporation equals the annual estimate. The text has been modified at P11852L9.

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 x 10^{6} J m^{-2} energy at 20oC, given that the latent heat of vaporisation is 2.45 x 10^{6} J kg^{-1}. For more efficient notation, one could also use 2.45e6 J kg^{-1} after appropriate introduction.
As explained earlier we do not believe it is appropriate to change the units from those we have adopted. Furthermore, to implement this change throughout the paper and the supplementary material opens the way for introducing numerical errors.

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.
DONE: We have remedied this inadequacy by inserted two sentences at P11834L23 indicating the relationship between kg m^{-2} day^{-1} and mm day^{-1}. At each equation of relevance, we note this equivalence.

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?
DONE: But E_{Act} could be evaporation from a non-saturated surface. Text modified appropriately.

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.
DONE: We agree. The sentence and reference have been deleted from the manuscript.

8. P11837L4–5: What does "without advection or heating effects" refer to? No negative sensible heat flux?
DONE: An additional sentence expanding the definition has been added in the revised manuscript after the phrase "without advection or heating effects".

What is the difference to reference crop evaporation? What is the difference between "growing vegetation" (here) and "reference vegetated surface" (below)?
DONE: The definition of reference crop evapotranspiration has been amended

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.
DONE: An additional sentence has been added here to alert readers where further details can be found.

10. P11838L2: What does "no water-advected energy" mean?
DONE: No inflow if a lake; text has been improved.

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?
DONE: We have deleted the reference to Penman's equation and replaced the Monteith reference with the Raupach (2001) reference as well as slightly modifying the equation in the revised manuscript.

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.
DONE: We have added to the revised manuscript two sentences at the end of the section dealing with 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.
DONE: 2.45 is the latent heat of vaporisation, the equation has been corrected; thanks for your detailed review.

14. P11839L17: What kind of wet surfaces were considered here? Bare soil, open water, short crop, forest?
DONE: Irrigated bare soil; text improved accordingly.

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?
These are interesting questions to ponder but their explanation is well beyond the scope of this paper. No change to text.

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?
DONE: Includes all processes at the evaporating surface; text improved accordingly.

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?
DONE: A closed system is one in which there is no mass exchange with the external environment. This explanation has been added to the text.

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.
Although the Thornthwaite procedure is referred to several times in the manuscript and it is historically important in the evolution of evaporation techniques, we note from the literature in Table 5 that over the past 10 years the method is applied less often than Makkink, Turc or Hargreaves-Samani. Space precludes details of any of these procedures to be incorporated in the manuscript. Details are included in the supplementary material.

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?
1. This paper is not written to provide detailed developments of each equation. There are appropriate texts, manuals and papers that deal with those details. We are attempting to provide sufficient information to inform our readers about key procedures and related issues and identify where the important information can be found. For example, in this case to provide adequate detail of the steps from Equation (5) to Equation (8) would require at least an additional half a page of text plus several equations. If this approach had been adopted, the practical nature of the paper would be lost. As stated in our text, “A detailed explanation of the theory of reference crop evapotranspiration is presented by McVicar et al. (2005, Section 2).” The material is freely available from the URL that is provided in the reference list.
2. The numerical coefficients of 900 and 0.34 in Equation (8) incorporate conversion factors to ensure the output units of evaporation are consistent with the input units of radiation, temperature, vapour pressure deficit, and wind speed. **DONE:** We have reworded the text to clarify this issue.

19. P11844L18: Please explain how the reader can verify the applicability of this assumption for a particular case.
DONE: Our statement was confusing. We have deleted the last part of the sentence that refers to Dingman. For an open water body, water is always freely available. Thus there is no need to further modify the text.
Does this imply that the open water body should not exceed a certain size?
No. The practical issues of lake size are discussed in Section 3.2. **ALREADY DONE:**

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.
We provide Equation (4) as the Penman general case of evaporation from a saturated surface. While we appreciate there is little difference between Equations (4) and (12), it is more appropriate to deal

with the practical issues of albedo and Ea in a section on open-water with slightly modified equation set out at the beginning of the discussion. We made no change to the manuscript.

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?
DONE: We have amended the sentence in the revised manuscript to reflect Dingman's comment more accurately. The practical issues relating to these assumptions are dealt with in Section 3.

22. P11846L14: Are the same parameter values of the wind equation applicable at different time scales?
No. The only change is in the average wind speed over the time-step used in the computation. We made no change to the manuscript.

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.
The boundary layer resistance is taken care of by the wind equation. However, some researchers have incorporated values of aerodynamic resistance directly into the modified Penman equation rather than using the original Penman equation or have introduced the area of a lake area (in computing lake evaporation) into the aerodynamic resistance. See Appendix S4. We made no change to the manuscript.

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?
Water temperature would be measured by the standard procedure. We made no change to the manuscript.

25. P11847L11–: Please provide the motivation and justification for the complementary relationship.
DONE: In the revised manuscript we made considerable changes to Section 2.5.1. Morton's models for estimating actual evapotranspiration from a landscape environment and for estimating lake evaporation are based on the Complementary Relationship (CR) as are the Aridity-Advection models of Brutsaert-Strickler and Szilagyi-Jozsa models for estimating actual landscape evaporation. As noted in the manuscript P11848L6, credible evidence is building that supports the CR hypothesis.

Neither the equation nor the figure are self-explanatory enough to help understand its meaning and applicability.
DONE: We added an additional sentence in the caption to Figure 1 and included a broader description of Figure 1 in the revised text.

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.
DONE: An additional sentence is added at P11848L25 clarifying the application of each model.

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.
DONE: We introduced another figure into the revised manuscript to illustrate the CR relationship for a water body and amended the manuscript at P11849L3.

28. P11849L7: What is “the Morton methodology”?
DONE: Sentence deleted.

29. P11849L15–: The units do not match in Eqs 16 and 17. $\gamma p f_v \; (T_e - T_a)$ would give units of mbar K^{-1} x mbar x W m^{-2} $mbar^{-1}$ x K = 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 J s^{-1} day kg^{-1} = 3600 x 24 s day^{-1} J s^{-1} day kg^{-1} = 86400 J kg^{-1}.
DONE: The text has been corrected accordingly. This was our error; thanks for detailed review. We specified the symbol γ as the psychrometric constant which is incorrect. Morton adopted γp as a constant with units mbar $°C^{-1}$.

I hope you agree that this is confusing.
We do agree this is confusing to a reader who is attempting to understand the details of Morton without referring to the supplementary material and his original two papers (Morton, 1983a, b). The supplementary Appendices S7 and S20 and the worked example S21 have been included to support a reader who may wish to understand the intricacies of the Morton methodology and the many assumptions and calibrated constants he introduced.

What is the value of f_v and what does it depend on?
DONE: The manuscript has been amended appropriately. f_v is a function of atmospheric stability. The equation to estimate f_v is complex. For details, readers are referred to Appendix S7 or to Morton (1983a).

I was unable to find Eqs. 16 and 17 in the supplementary material S21.
Equation 16 is Equation S21.79, and Equation 17 is embedded in Equation S21.75 (by equating Equations 16 and 17), which is used in the iterative process to find the equilibrium temperature T_e. We have made no change to the manuscript.

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.
This seems unnecessary detail to be included in the manuscript unless we were to provide much more detail about the Morton equations and their related detail. We have sought to balance detail with length of our manuscript; noting that many details are given in the supplementary Appendix S21. We have made no change to the manuscript.

30. The meaning and calculation of the term b_1 was not discussed in Sect. 2.1.3. Please explain.
Section 2.1.3 deals with Priestley-Taylor and b_1 is not part of the Priestley-Taylor equation. As explained in the manuscript P11856L8, b_1 is a term added by Morton. **DONE:** We have modified the reference to section 2.1.3 to make it clear that it refers to Priestley-Taylor.

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?

No. Water advected energy refers to precipitation, streamflow and groundwater flow into the lake. See details in Appendix S10 and the Worked Example 17. **DONE:** The manuscript has been amended at P11856L7.

32. P11858L13: It is important to point out that the coefficients can vary between 0.47 and 2.19 seasonally and between 0.66 and 1.00 between lakes at the annual scale.
DONE: The manuscript has been amended to include these values.

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.
In this paragraph, we are assuming a large lake and, therefore, it follows that the effect of the upwind transition from the land to the lake has a negligible effect on the overall evaporation of the lake. We also assume the lake is shallow and therefore the seasonal heat storage is minimal and can be ignored. We do not see the need to amend the text.

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?
Morton (1978, page 78) commented on this in relation to examining evaporation as one moves from a dry landscape to an irrigated area. He noted that the decreased evaporation from the transition into the irrigated area (analogous to a lake) was associated with decreased air temperature and increased humidity. **DONE:** The manuscript has been amended to include an appropriate comment with reference to Morton's observation.

35. P11860L15: Is $\Delta S = 0$ at annual time step justified? How can it be verified for a particular case?
This is one of the most common assumptions in mean annual (or steady-state) water balance studies of catchments. Although ΔS may vary considerably from year to year, over several years because ΔS is not accumulating, the error in ΔS is small relative to mean P and Q (see Wilson, E.M., 1990. *Engineering Hydrology* Fourth Edition (Macmillan) page 44). As noted in our response to J. Szilagyi (above), recent research has addressed the need of better accounting for sub-annual transitory processes such as: (i) rooting depth dynamics; (ii) soil store dynamics; and (iii) groundwater dynamics. We have added a sentence mentioning this point.

36. P11861L7: Can you provide a reference for the "simple Thornthwaite soil moisture model"?
DONE: Our sentence was incorrect and has been corrected with a reference to Doyle (1990, Figure 1).

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.
DONE: In the revised text, the reference to Section 3.2 is deleted. We have added a sentence explaining that some of the sensible heat is advected into the irrigation area downwind.

38. P11866L27: Why is the Matt-Shuttleworth model considered acceptable for specific crops in windy semi-arid regions? What crops, how windy?
DONE: The sentence has been modified in the revised text. The method is not restricted to specific crops.

39. P11867L17: How was the "strength of the theoretical basis" assessed?
See response to the same question under General Comments f) above.

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?
We are not clear what is being asked here. Many procedures for estimating evaporation (Penman, FAO-56 Reference Crop, PenPan, Advection-Aridity) require wind as an input variable. However, Morton (1983b, page 95) argued that using routinely observed wind speeds in estimating lake evaporation do not significantly reduce error in evaporation estimates. Based on three arguments: (1) the vapour transfer coefficient (f_v) increases with increases in both surface roughness and wind speed yet wind speed tends to be lower in rough areas than in smooth areas; (2) f_v increases with atmospheric instability and is more pronounced at low than at high wind speeds; and (3) errors in wind measurements, Morton assumed that the vapour transfer coefficient is independent of wind speed (Morton 1983a, page 25). **DONE:** We added a referral to Appendix S7 in the revised manuscript, and added a paragraph similar to above in the appendix.

41. P11868L22: What is considered a successful application? Could you specify?
DONE: We added a phrase 'using independent lake level data' and replaced "applied" with 'tested' in the revised manuscript.

42. P11869L3 (We think this is meant to be L23): Why would neglecting a heat source lead to over - estimation of evaporation?
Rosenberry et al. (1993) are silent about this. One would assume the heat losses are into the bottom sediments. No change to the manuscript.

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.
DONE: We have amended the text to make it clear that this is a local climate effect.

44. P11870L11: Should this be -3.19 mm yr^{-2} (negative trend)?
DONE: Text has been corrected.

45. P11871L16: This sentence is unclear.
DONE: In the revised manuscript we have deleted the reference to the Complementary Relationship which is unhelpful. The second sentence expands the comment about the supply and demand for water.

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.
DONE: We restructured the sentence to ensure our explanation is clear.

47. P11876L13: Again, what does this mean and when is it the case?
DONE: In the revised manuscript, the sentence had been modified to be consistent with the amended sentence in Section 2.4.1.

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.
We believe it implies that many of the simple rainfall-runoff models do not mimic the hydrologic processes very well. We have made no change to the manuscript.

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?
DONE: Table 1 was incorrect. We have corrected this table in our revised manuscript; thanks for your detailed review.

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.
DONE: We have amended the caption in Table 4.

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 perhaps, the descriptions could simply be moved to footnotes in Table 6 and Table 5 could be removed.
Because there is additional information in Table 5 compared with Tables 6 and 7 we have elected to retain all three tables. **DONE:** Table 5 is restructured with the descriptions moved to footnotes.

52. Figure 1: The caption needs a lot more explanation to make this figure useful.
DONE: An additional sentence is added to the caption and additional explanation is provided in the Section 2.5.1 in the manuscript.

53. Figure 2: Likewise, a brief description of the meaning of this figure would be helpful.
DONE: An additional sentence is added to the caption (now Figure 3 in revised manuscript) and a sentence is added at P11863L24.

Technical corrections
• P11832L5: "a historical"
DONE: Corrected in revised manuscript.

• P11832L6: "including for" sounds unusual. Maybe better: "many practical needs for daily or monthly actual evaporation estimates, including deep lakes or postmining voids..."?
DONE: Sentence is modified.

• P11832L24: "should note that there are"
DONE: Corrected in revised manuscript.

• P11832L27: "indicated"
DONE: Corrected in revised manuscript

• P11841L7–: This sentence is incomplete (should be "is defined as follows"?).
DONE: Change made as requested.

• 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..."
DONE: Replaced with 'taken into account'.

• P11867L10: Priestley instead of Priestly (twice)
DONE: Corrected in revised manuscript.

Anonymous Referee #2

Congratulations: assembling this synthesis is a great idea, and this paper is potentially extremely helpful to clarify the concepts.
The authors thank the referee for their support.

However, I would like to ask the authors to try to be still more pedagogical in their presentation. I think that the key to all the paper is section 2.1, where you present the different concepts. I would like to ask the authors to extend it a little further to clarify the differences between definitions. For example, I think you should introduce the concept of ET wet from the beginning, and not wait for the presentation of Morton's work.
DONE: We have addressed these issues in two locations. Firstly, in a revised and expanded Section 1.1 we provide a detailed explanation of evaporation processes. Secondly, in Section 2.1 we have expanded the definitions of potential evaporation, including wet environment evaporation, as discussed by Granger (1989a).

I also remember a discussion by Perrier on the difference between "Maximal" and "Potential" Evapotranspiration that would be interesting here.
DONE: In the revised manuscript (P11837, L11) we now refer readers to Katerji and Rana (2011) who discuss Perrier (1984); which is published in French.

Last, I would like to see a clear definition of the role of the reference crop here: when are crops sometimes seen as a factor only active in the PET - > AET transformation ("stomatic resistance"), and when are they seen as a factor increasing PET. There has always been a lot of confusion in the literature, and for paractitionners, it would be extremely useful to address these questions, most of them lack a clear understanding of the differences.
Thank you for this comment. This is a complex area and is not relevant to the practitioners we are addressing in this paper. It is outside the scope of the paper. A paper by Shuttleworth and Gurney (1990) deals with the issue in detail. **DONE:** We added a sentence in Supplementary Appendix 5 where we alert readers to this reference and an earlier one by Monteith (1965).

Among the other debates that I wish would be dealt here is that of interception: does it represent a double count or not?
DONE: We thank the referee for pointing out this omission. We have added an additional paragraph and two references in the Supplementary Appendix S14 that discusses double counting in interception.

Miscellaneous comments
I was wondering whether Eq. 1a should not include a leakage term L, to account for flows which may leave the analysed system (e.g. a catchment) moving to a neighboring catchment or a regional aquifer. (you do it in Eq. 24).
DONE: An additional phrase (P11835, L17) has been added to the text noting this point.

P11837L5: Please define advection in simple terms
DONE: An additional sentence (P11837, L5) has been added in the revised manuscript expanding the definition.

P11847, when you introduce the complementary relationship, you introduce ETwet. Why didn't you do it earlier in section 2.1?
We did not introduce the ETwet in Section 2.1 because the term is unique to Morton's description of the Complementary Relationship. Introducing the term in Section 2.1 without explaining its relationship to ETpot in the CR relationship would potentially confuse the reader. We have not amended the manuscript.

Morton complementary relationship: when you describe Morton's attempt to validate the relationship, you could perhaps discuss Oudin's unsuccesful attempt to introduce it in a hydrologic model.

References Oudin, L., C. Michel, V. Andréassian, F. Anctil, & C. Loumagne, 2005. Should Bouchet's hypothesis be taken into account for estimating evapotranspiration in rainfall-runoff modeling? An assessment over 308 catchments. Hydrological Processes, 19: 4093-4106.

This paper is not evaluating Morton's application of the Complementary Relationship but rather an application of Morton's and Brutsaert and Strickler's estimate of actual ET embedded in a rainfall-runoff model. For this reason we did not include a comment in the original manuscript. We see no reason to do otherwise.

J. Dracup (Referee)

This is an outstanding paper and should be required reading for all students and practitioners of hydrology. Professor McMahon and his colleagues have meticulously examined every evaporation equation and numerous applications with a critical eye. Their discussion and conclusion is not a review but a "pragmatic synthesis" and a "summary of techniques". Their discussion on the uncertainties in evaporation estimates and model performance, Section 4.6 is outstanding. I personally know that the authors spent several years developing this comprehensive work. Their effort is reflected in 32 papers of references, all of which they have read and thoughtfully analyzed. The result is this outstanding paper...

The authors thank Professor Dracup for his supporting comments. We hope the paper lives up to his high expectations.