

Dear Anonymous referee, editor,

We are glad to see this discussion being reopened despite the delay, and we appreciate this new criticism. Please allow us to reply to these comments below (see blue fonts).

The manuscript titled 'Potential evaporation at eddy-covariance sites across the globe' is a surprising piece of work to read through. The reasons are as follows:

(1) The title of the paper is inappropriate in my view. What the authors have done is they selected the events of unlimited soil moisture and/or high EF events and used a host of predictive Potential evaporation models to calculate the statistical errors of the models, based on which the appropriateness of the models are highlighted. The title of the paper should be 'Unstressed evaporation modeling at eddy covariance sites across the globe'.

There are multiple definitions of potential evaporation (E_p) in literature. The evaporation occurring under (hypothetical or actual) unstressed conditions is in fact one of them. This is the definition of E_p we uptake here, as the reviewer mentions, and as it is clearly stated in the manuscript. This agrees with multiple previous articles – just to cite some of them, see Douglas *et al.* (2009), Pereira and Pruitt (2004), Katerji and Rana (2011), Li *et al.* (2016), Jacobs *et al.* (2004), Fisher *et al.* (2011).

(2) It is obvious that under unlimited soil moisture, radiation explained maximum variability in evaporation. Such results have been published in many literatures and not new to the community.

We appreciate the comment. Indeed, there are plenty of articles that sustain that radiation is the main driver of evaporation under potential rates. However, while for the referee it appears to be an obviousity, many authors – like e.g. Thornthwaite or Odin – have proposed methods based on a rationale that contradicts this expectation. While we believe that it is important to highlight the role of radiation as dominant driver, we certainly do not claim this is a novel result: we cite plenty of articles that agree with this finding in the 'References' section. If we have missed any relevant one, we invite the reviewer to suggest their preferred ones and we will incorporate them to the revised version.

However, flagging it as potential evaporation is misleading.
We disagree. See first response.

It should be seen as actual evaporation under unlimited soil moisture which is driven by radiation only. The fact that E_p is driven by radiation (not 'only', but mainly) is concluded from our findings that radiation-driven formulations perform better at estimating E_p when it happens. And yes, for us, 'actual evaporation under no stress' is in fact E_p . We refer to the above-mentioned articles again and our chosen definition of E_p stated in the first response.

The authors should realize that potential evaporation is a notional term?

Yes, we should... and in fact we do. We certainly understand it is a notional or idealised concept, as it is extensively discussed in the text. Our estimates of E_p in days with stressed conditions correspond to the evaporation that would take place under no stress, bearing in mind the uncertainty due to possible feedbacks as extensively discussed. Again, if the reviewer finds incomplete any of the discussions we invite them to clarify what exactly should be expanded or amended.

What happens in desert where high radiation load is accompanied by extremely high VPD?

Evaporation under sufficient soil water availability would be larger for the same net radiation if VPD were lower. The reviewer certainly knows this... Maybe we are misinterpreting the question.

If we plot an image of global E_p distribution, we will see the deserts to have the maximum E_p values. This depends again on the method used to estimate E_p , which happens to be a notional term. The referee is probably not acknowledging that the high albedo and land surface temperature in the deserts lead to a reduced net radiation, despite the high VPD. The result is that the E_p estimated via radiation-driven approaches will show remarkably low values in the desert. This may come across as surprising to the reviewer, so we refer to e.g. Fisher *et al.* (2011) Figure 3. This is to highlight again the differences among the definitions – and subsequent formulations – of E_p in current research. Nonetheless, as the reviewer also knows, there are no desert sites in the database: we did not apply the method to any arid climate (where the value of accurate potential transpiration estimates has arguably no value anyways).

Then how would one can pick potential evaporation events based on EF or soil moisture saturation. See again our first response for our definition of E_p , and Section 2.4 in the article, and the literature referred here. This is common practice and is consistent with the definition of potential evaporation considered here.

Although the authors have hinted (in Page 3, L15) that E_p is the potential evaporative demand, but finally inclined to wettest events instead of looking at the evaporative demand.

We are interested in providing estimates of E_p for wet and dry times. Our use of 'evaporative demand' here is as synonym to E_p , and consistent with our definition: the evaporation that would occur under unstressed conditions. We do not mean vapour pressure deficit, if this is what the referee means.

(3) The estimation r_{AH} is extremely outdated and the no attempt is made to demonstrate how sensitive is the PM and Penman equation to r_{AH} parameterization, which in my opinion should carry a section of results, instead of concluding biome specific PT is consistently better than any other models.

We strongly disagree with the reviewer if it is implied that the logarithmic wind profile method and the Monin-Obukhov theory are 'extremely outdated'. To date, these are the standard and optimal methods for calculating aerodynamic resistance (r_{aH}). In fact, we incorporated several aspects in the paper to ensure our calculation of r_{aH} is the best available, specifically to avoid Penman methods to be disadvantaged. We estimated the vegetation height using a recent new method by Pennypacker and Baldocchi (2016), incorporated stability functions using the best available methods (Garratt, 1992; Brutsaert, 2005), and incorporated a parameterisation of the Stanton number based on recent insights by Li *et al.* (2017). Prior to submission, this paper was sent to all the principal investigators of all 107 flux towers, many of which are very well acquainted with the difficulties of estimating r_{aH} . Many of them replied and none of them had a complain about this methodology.

Of course, we would still appreciate it if the reviewer pointed us to the newer formulations that they are referring to. This would make this comment (and others!), in fact, constructive and helpful. Meanwhile, we trust the references highlighting that our r_{aH} calculation is the state-of-the art. See from the last couple of months only: Valayamkunnath *et al.* (2018), Lin *et al.* (2018), Nyman *et al.* (2018), Srivastava *et al.* (2018), Yan *et al.* (2018).

Yes, when the evaporation is driven by radiation only, it is no wonder that PT will do a good job. However the calibration of PT was still needed to adjust the hidden VPD and r_{AH} related variability in the 'alpha' parameter. Absolutely right. We are glad the referee agrees with the discussion in the article! Then, of course if radiation was not the main driver, with a constant alpha per biome type PT would not outperform more complex methods.

(4) How the residual ET error in PM and Penman was related to r_{AH} ? It is now becoming prominent to the ET community that r_{AH} parameterizations are ambiguous and this needs to be resolved in surface energy balance

modelling. Some recent studies have highlighted the importance of analytical estimation of aerodynamic conductance to overcome the uncertainties in ET modelling, which authors are expected to be aware of. Unfortunately, the reviewer failed again to add the references to these articles. We therefore choose to believe the findings from the above-mentioned recent literature.

(5) What about the feedback that rAH provides to the evaporation? Without considering those feedbacks, it would be unjustified to come to conclusion about PM or Penman equations (as mentioned in section 4.3).

As mentioned in the response documents addressing the constructive feedback from previous referees, the consideration of atmospheric feedbacks will be further discussed in the revised manuscript.

(6) Estimation of g_{c_ref} is purely climatological and as a result the differences in g_{c_ref} between the biomes are marginally significant.

The estimation of g_{c_ref} is done by considering it as the residual term in the PM equation. It is therefore not purely climatological but reflects e.g. ecosystem roughness, species-based genetic differences in maximum stomatal conductance, etc. We believe this is clearly discussed in the article but will be further clarified in the revised version.

(7) A Table of symbols and variables would be very helpful to the readers.

This is indeed a constructive comment. We will incorporate it to the revised version.

(8) section 4.3: A complete change of description is needed. The conclusion of Michel *et al.*, 2016 and Ershadi *et al.*, 2014 was an outcome of outdated conductance parameterization (despite they were published) and should not be used to as a justification in the discussion.

See above response. To our understanding, both articles mentioned here were highly welcomed by the scientific community and have been highly cited. Could the reviewer perhaps share with us the list of articles he/she considers as 'state-of-the art', and the one containing the articles that should be blacklisted? That, together with a description of the reason we should not believe those articles, would make all these comments constructive.

(9) section 4.3: It is important to highlight the fact that the conductances (both g_{AH} and g_c) in the PM equation provides feedback in evaporation that changes the aerodynamic vapor pressure and temperatures. This study used empirical g_{AH} model to obtain evaporation estimates from PM and Penman. In addition the authors made an effort to show g_c -VPD known curve to justify the results. In the present case, justification on why PM and Penman equation is complex should come from analysis of g_{AH} and linking the model errors with empirical uncertainties in g_{AH} .

In section 4.3, we demonstrate that the flaw of using the Penman-Monteith based approaches for estimating E_p is the assumption in these approaches that g_{c_ref} should be constant under unstressed surface conditions. This is not the case, and is the main reason why the PM approaches perform poorly when aiming to calculate E_p . We do not claim that the Penman-Monteith method as such is inferior in any way, yet the highly dynamic nature of the aerodynamic and surface conductance makes it difficult to apply reliably. As mentioned above, we tried to provide the best available method to estimate r_{AH} , and invite the reviewer to provide any improvement. However, this would not affect the underlying problem of the constant g_{c_ref} of the PM-based E_p methods.

(10) Also, the authors did not mention if they took care of the sky conditions. Ideally the study should select clear sky cases.

We would like to sample all-sky conditions, since E_p also matters when it is cloudy.

Finally, I would like to thank the authors for the honest effort to use large fluxnet dataset and untap the events of unstressed evaporation. But this should not be seen as potential evaporation. A detailed analysis of the role of

gAH in PM, additional role of VPD in creating the differences in evaporation between PTb, PM, and Penman would make the study worthy of publication.

See all responses above. Again, pointing us to the mentioned literature would make this a much more valuable exercise.

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