Response to Reviewers' comments

We greatly appreciate the editor and the two referees for providing constructive comments that are of great help for us to improve the manuscript. We have fully considered the comments and the point-to-point responses are listed below. The manuscript and the supplementary material have been revised to accommodate the changes (changes are marked in blue color). In the following the reviewer comments are black font and our responses are blue and to assist with navigation we use codes, such as R1C2 (Reviewer 1 Comment 2)

To reviewer #2

R2C1: Overall, I greatly appreciate how the authors dealt with my comments (and the other reviewer's comments), with clear detailed answers and new analyses when necessary. I also appreciate how they modified the manuscript accordingly.

I think the paper is almost ready for publication, except for the few (but important) comments below, and will be an excellent contribution to the field.

Reply: Thanks for your positive evaluation of our revised manuscript. Your individual comments are replied below.

R2C2: I am not really satisfied by the response to my comment R2C3 in the previous review: My question was: Why is the actual evaporation equal to the maximum evaporation? The authors answered "We do not understand the comment, "An infinity of pairs of (evaporation, Ts) values are compatible with the authors' model.", as there is only one maximum evaporation and one corresponding Ts along the entire Ts range."

I was indeed not clear: I should have said: "An infinity of pairs of (evaporation, Ts) values are compatible with Equation (3) (in the new version of the manuscript)". And Equation (3) is not sufficient to estimate actual evaporation in the authors' framework. To estimate actual evaporation, the authors also have to make the additional assumption that actual evaporation is equal to maximum evaporation.

The model proposed by the authors to estimate evaporation actually needs two "ingredients":

(i) Equation (3) that describes how evaporation evolves with Ts, and shows the existence of a maximum evaporation at a given Ts

AND

(ii) The assumption that actual evaporation is equal to this maximum evaporation

To my question: “Why is the actual evaporation equal to the maximum evaporation?”, basically the authors answered: if we look at observations, it works. I agree, but this does not explain why.

Their model is therefore empirical to an important extent, which is somewhat less satisfying from an intellectual point of view. It should be acknowledged.

It is not that important for the model proposed by the authors in practice, as it is true that “it works” according to observations, but I think it should be made clear that the model relies both on equation (3) and on assumption (ii) above, and that assumption (ii) is only justified empirically in this work.

Reply: We now understand this point better. However, we did not invoke the assumption that the maximum evaporation is equal to actual evaporation in the method. We did not know whether and to what extent the maximum evaporation corresponds to actual evaporation. In fact, we did not even know what this maximum evaporation means at the beginning. The maximum evaporation emerges naturally from the intrinsic interactions between radiation, surface temperature and evaporation. The fact that this maximum evaporation corresponds to the actual evaporation indicates that the maximum evaporation is a natural attribute of extensive wet surfaces. So a better summary of our previous study on ocean surfaces is not “developed a new evaporation model” but “identified this natural attribute of global ocean surfaces”. In the current study, we further demonstrated that this is also a natural attribute of wet surfaces over land (with vegetation). These findings combined lead to an important conclusion that the maximum evaporation is a natural attribute of an extensive wet surface, regardless of the surface is covered by water, soil or vegetation.

So a clear answer to your question “Why is the actual evaporation equal to the maximum evaporation?” is “the maximum evaporation is a natural attribute of an extensive wet surface”.

R2C3: L161-162. “the key processes governing the interactions between incoming and outgoing longwave radiations are essentially the same for ocean and land (mainly greenhouse gas and aerosol effects)”.

It is not true: the influence of most aerosols on longwave radiation is very weak. And the influence of clouds and of the vertical temperature structure of the atmosphere are crucial for longwave radiation.

Reply: We agree with this reviewer. However, water vapor (the largest greenhouse gas in the atmosphere) and aerosols are the key foundations of clouds, and the concentration of greenhouse gases also largely determines the vertical temperature structure in the atmosphere. To avoid potential misunderstandings, we have clarified this point by revising the statement as “mainly greenhouse gases that affect the vertical temperature structure of the atmosphere, and water vapor and aerosols that affect the formation of clouds) (Line 162-164).

R2C4: L294-308. “However, the shortwave atmospheric transmissivity is primarily affected by aerosols while the longwave transmissivity is mainly affected by the concentration of greenhouse gases”

And what about clouds? Generally, the impact of aerosols on shortwave radiation is not greater than the one of clouds. And clouds may have very different impacts with regards to the shortwave and longwave radiation budgets. For example, high clouds have a strong impact on longwave radiation compared to low clouds, but the same cannot be said for shortwave radiation.

Reply: We agree with this reviewer and the different impacts of clouds on shortwave and longwave are accounted for in the scaling relationship between ΔT and τ.

R2C5: L310. Thanks for the very detailed answer to my previous comment on this point: it is very interesting.

I still think it is important to say in the paper that there may be some issues at longer time-scales, in the climate change context, as the relationship between shortwave transmitivity and longwave radiation is expected to evolve with anthropogenic climate change.

Reply: Done. We have added a sentence that reads “For even longer periods, especially for assessing the impacts of climate change, the relationship between shortwave and longwave radiations used herein may be also invalid, as we expect this relationship to evolve with anthropogenic climate change.” (Line 317-319). Thanks for the suggestion.

To reviewer #3

R3C1: With great interest I read the revised manuscript and the author's response. The authors apply their previously found model to estimate 'maximum evaporation' over ocean to saturated land evaporation. The proposed method is in my view very elegant due to its simplicity. Equation 3 is the core equation from where it can be easily seen that Ts has positive and negative feedbacks on LE (first term positive, 2nd one negatively correlated to LE). In that sense I think that maybe 'equilibrium evaporation' might be a better term than 'maximum evaporation', although I can see from an optimisation point of view that it's indeed the max. Using the term equilibrium might also help to understand why LEmax corresponds to actual evaporation under saturated conditions.

Reply: Thanks for your encouraging comments on our revised manuscript. However, we feel that “equilibrium evaporation” is not an appropriate term, as there has already been a specific definition of “equilibrium evaporation” – evaporation from a saturated surface into a saturated atmosphere (Schmidt, 1915; Slatyer and McIlroy, 1961; Philip, 1987). This is different from our maximum evaporation, where we do not assume a saturated atmosphere. The maximum evaporation emerges naturally from the intrinsic interactions between radiation, surface temperature and evaporation. The fact that this maximum evaporation corresponds to the actual evaporation indicates that the maximum evaporation is a natural attribute of extensive wet surfaces. Therefore, we use “maximum evaporation” to highlight this is a natural attribute of extensive wet surfaces.

R3C2: Only surprising point for me remains the low sensitivity of all energy balance fluxes to Ts (Figure 3), especially, in the domain of interest: 280<Ts<310 (although already quite elaborately discussed in the manuscript and author-response: low sensitivity of the bowen ratio for Ts more most observation points). The results for LE and Rnet are excellent (figure 4 and 6), those for Ts are moderate/good (figure 5). Leaving a comparison on H open. How are these results? Might it be possible that all 'errors' are allocated to H? Also partly related to the way how you dealt with the energy balance gap in your EC-data.

Reply: The low sensitivity of evaporation to Ts is actually somewhat expected and implied in many previous studies. This is because that the incoming longwave radiation increases concurrently with the outgoing longwave radiation (due to the increase of Ts), leaving changes in net radiation very small. This also partly explains the overall low hydrological sensitivity to climate change (1.5% - 2% increases in the strength of the global hydrological cycle with a 1°C increase of temperature). As for the sensible heat, it is much more difficult to correctly capture the sensible heat than for the latent heat, because under saturated conditions, the sensible heat is usually very small. Following your suggestion, we had a quick look at the sensible heat estimates and found an overall RMSE of 8.3 ~ 9.7 W m^{-2} and a mean bias of 0 ~ 0.8 W m^{-2} (the range is due to using different methods to close the EC-tower energy balance and using the generic or biome-specific β-Ts relationships). This is not bad at all.

R3C3: Nonetheless, I highly appreciated to read this manuscript and looking forward to the next step: how does it work under unsaturated conditions?

Reply: Very good point. We are actually working on this.

Reference:

Schmidt, W. (1915), Strdhlung und Verdunstung an freien WasserflAchen; ein Beitrag zum Warmehaushalt des Weltmeers und zum Wasserhaushalt der Erde (Radiation and evaporation over open water surfaces; a contribution to the heat budget of the world ocean and to the water budget of the earth), Annalen der Hygrographir und Maritimen Meteorologie, 43, 111-124.

Slatyer, R.O. and McIlroy, I.C. (1961) Practical Microclimatology. Melbourne: Commonwealth Scientific and Industrial Research Organisation.

Philip, J.R. (1987) A physical bound on the Bowen ratio. Journal of Climate and Applied Meteorology, 26, 1043–1045.