**Response to Reviewers' comments**

We greatly appreciate the anonymous referees for providing valuable and constructive comments that are of great help for us to improve the quality of the manuscript. We have fully considered the comments and will revise the manuscript accordingly. The point-to-point responses to the comments and our plans for revision are listed below. 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 #1**

**R1C1:** The authors have validated the maximum evaporation theory originally developed for oceans over global saturated land surfaces. I think this paper is a good extension of Yang et al. (2019) and is of great importance for land potential evaporation estimation.

**Response:** Thanks for your positive evaluation and encouraging comments on our manuscript. Your individual comments are replied below.

**R1C2:** In the last paragraph of introduction, the author intended to test their ocean research directly over saturated lands without any comparison between two different surfaces. I suggest to add some discussions on comparison (vegetation effect?) between ocean and land surface, which was mentioned in discussion. I think this kind of comparison can highlight the importance of this research and also can help authors to propose scientific hypothesis.

**Response:** Thanks for the suggestion. We will add some discussion in the introduction as suggested in the revised manuscript.

**R1C3:** In introduction, the authors have pointed out the limitations of Penman and Priestley-Taylor model. So please simulating evaporation with this two models at select site-days, and then compare simulations to maximum evaporation method results. To see if maximum evaporation method show higher performance than the two classical models.

**Response:** As suggested, we compared the maximum approach, the Priestley-Taylor model and the open-water-Penman model at the selected site-days (Figure R1). We will add this comparison in the revised manuscript. It shows that the Priestley-Taylor model performs best in estimating *LE,* the maximum approach performs slightly worse (but still very good) than the Priestley-Taylor model and the Penman model performs worst. It is not surprising that the Priestley-Taylor model performs better than the maximum evaporation approach since the Priestley-Taylor model uses the observed net radiation and surface temperature while the maximum approach uses the estimated net radiation and surface temperature (the performance of surface temperature/net radiation estimations are shown in Figure 5 and 6). However, as demonstrated in Yang and Roderick (2019), the underlying interactions between radiation, surface temperature and evaporation in the Priestley-Taylor model are incorrect, which means that the Priestley-Taylor model gets a right answer with a wrong approach. The weakness of the Priestley-Taylor model would not be apparent under wet conditions (as focused here) but would become more evident when the surface becomes drier, since the observed net radiation and surface temperature under dry conditions can be very different from those if the surface were wet (the idea of potential evaporation).

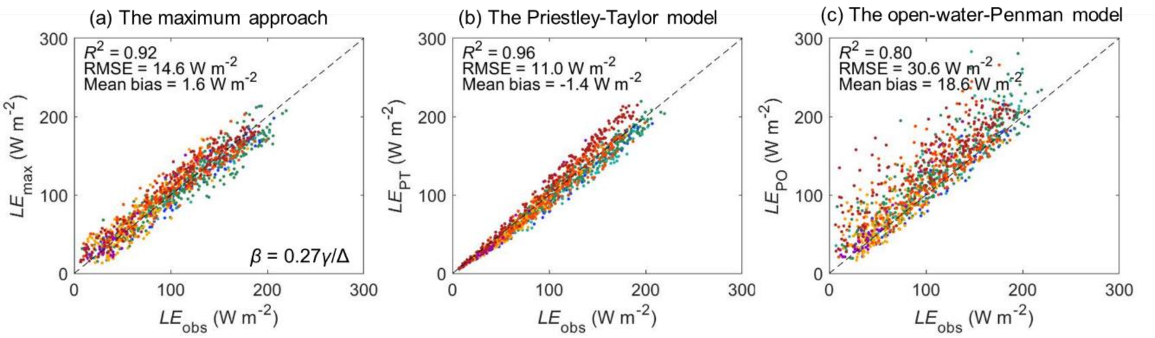



**Figure R1.** Performance of the maximum evaporation approach, the Priestley-Taylor model and the open-water-Penman model in estimating evaporation at selected site-days.

Since the main purpose of this study is to test the maximum evaporation approach over wet lands, we do not plan to include this comparison in the main text. In addition, a previous study by Maes et al. (2019) has already demonstrated that the energy balance-based approaches generally perform better than other approaches (including Penman-Monteith) at flux sites when the surface is wet. We will refer to that study and add relevant discussions in the revised manuscript. Figure R1 will also be included as supplementary material.

**R1C4:** In section 2.1, the residual approach was used to force energy balance for EC flux data. The method will decrease Bowen ratio because latent heat flux usually increase after adjustment due to lack of energy balance for EC method, while sensible heat keep the same. The residual method not only changed latent heat flux, but also changed the Bowen ratio. And Bowen ratio is a very important variable in your research. I think the residual approach is not the optimal one here. You can try the method proposed by Twine et al. (2000). Twine method assumes that even though the EC latent and sensible heat fluxes are not measured accurately, the resulting Bowen ratio is accurate. Then turbulent fluxes are adjusted without changing the Bowen ratio.
**Response:** Following this comment, we used the Bowen ratio approach noted by the

reviewer to close the energy balance and repeated the calculations. We find that using different approaches to close the energy balance results in similar model performance in estimating *LE* and $T_s$ (Figure R2). This is not surprising, as over saturated surfaces, sensible heat is usually very small. We will add these new results to the supplementary material in the revised manuscript to demonstrate that different approaches to closing the flux site energy balance do not change our conclusion.

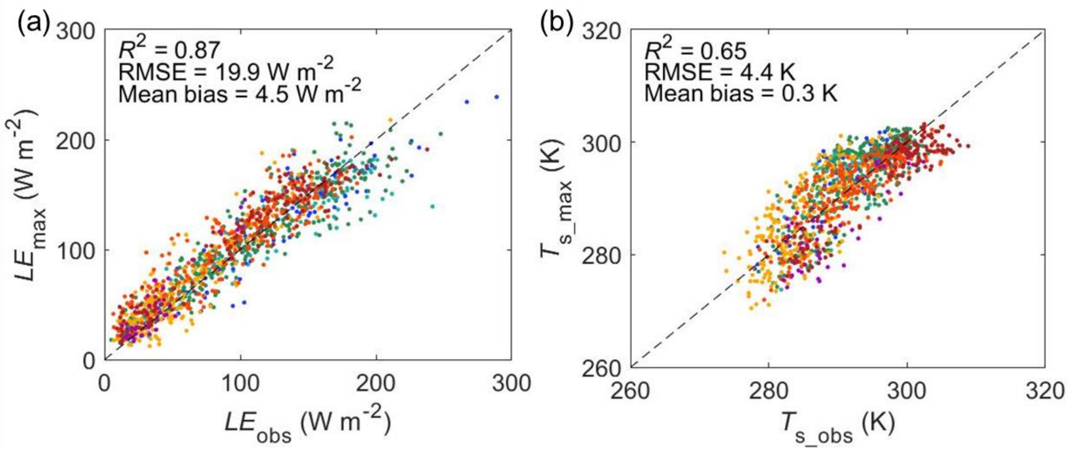



**Figure R2** Validation of *LE* and $T_s$ estimated using the maximum evaporation approach at the selected site-days where the energy balance closure of the flux site measurements is achieved by using the Bowen ratio approach.

**R1C5:** Equation (2). Please describe obtaining surface emissivity value with MOD11A1 products in more detail, such as time scale (different emissivity value for different day?), spatial scale (the matching between site location and MODIS pixel) and missing data problem (how to deal with conditions with no MOD11A1 for some site-day).

**Response:** The MOD11A1 surface emissivity has a daily temporal resolution and a 1 km spatial resolution. To obtain the emissivity for each EC flux site, we center on the pixel where the site is located and take the mean value of the 81 neighboring pixels (9×9 pixels) as the emissivity value of the site. For the conditions with no MOD11A1 data, we deleted these site-days. We will add these details in section 2.1 in the revised manuscript.

**R1C6:** Around line 110. The data with negative sensible heat flux with advection (maybe caused by mesoscale circulation or synoptic system) were removed in the research. So maximum evaporation theory can be not used under advections. This is one of difference between relative homogeneous ocean surface and complicated land patches. Please add some discussions on this topic in your discussion part, especially the cautions of applying maximum evaporation theory (limitations?) over saturated land surface.

**Response:** We removed the negative values for sensible heat to guarantee the data quality. These negative values may be caused by strong advection, interception and condensation when accurate measurements are not guaranteed (Mizutani et al., 1997; Maes et al., 2019).

As for the maximum evaporation approach, the basic principles should also hold under the condition of advection except that the Bowen ratio – $T_s$ relationship would be different. We will add some discussion on this point in the revised manuscript.

**R1C7:** Around line 125. The calculation of $\tau$ here is same to clearness index. So atmospheric transmissivity here is identical to clearness index?
**Response:** Yes, this reviewer was correct. The shortwave atmospheric transmissivity used here is identical to clearness index.

**R1C8:** Around line 135. "the key processes governing the interactions between incoming and outgoing longwave radiations are essentially the same for ocean and land (mainly greenhouse gas effect)". Firstly, what is the interaction between incoming and outgoing longwave radiation? Secondly, I think the longwave effect process caused by well-mixed GHGs is similar for ocean and land. But clouds and aerosols are different between ocean and land, both two have great effect on longwave radiation.
**Response:** The interaction between incoming and outgoing longwave radiation is that the outgoing longwave radiation would impact the amount of incoming longwave radiation, and vice versa. In our formulation, this interaction is quantified by the temperature difference between the surface and the effective radiating height of the atmosphere.
We agree with this reviewer that besides the GHG effect, aerosols also affect longwave radiation. In the maximum evaporation approach, the aerosol effect is implicitly considered in the atmospheric transmissivity. We will add the aerosol effect in the revised manuscript.

**R1C9:** Equation (7). You indicated that latent heat of vaporization is a weak function of temperature, so please state this with words and show the calculation formula.
**Response:** The latent heat of vaporization is a weak function of temperature:

$$L(T_s)=2.51\times10^3-2.32\times(T_s-273.15) \tag{R1}$$

We will add this equation in the revised manuscript.

**R1C10:** Around line 155. You explained why Bowen ratio over land is larger than ocean value in discussion section from stoma resistance. If stoma resistance is the main reason, Bowen ratio of sparse vegetated land should be close to ocean value, and dense vegetated land should be much higher than ocean value. Can this inference be reflected in Figure2? In addition, aerodynamic resistance for sensible and latent heat flux is thought to decrease with roughness (Zhao et al., 2014). So roughness difference between land and ocean can be used to explain the Bowen ratio difference? Please add some discussion on roughness effect.

**Response:** This reviewer was correct that for a single leaf layer, the stomatal resistance should be higher for dense vegetation than sparse vegetation. However, over densely vegetated land, there are always multiple leaf layers and the stomal resistance for each leaf layer is connected in parallel so the overall canopy resistance is often smaller for dense vegetation than sparse vegetation. As a consequence, the Bowen ratio is usually smaller over densely vegetated lands than over sparsely vegetated lands, when all else is equal. This is also supported by the data showing that croplands and forests have a smaller Bowen ratio than savanna and shrublands for the same surface temperature (Figure 2).

We agree with this reviewer that the roughness difference can be another reason for the Bowen ratio difference between land and ocean. We will add some discussions in the revised manuscript. Thanks.

**R1C11:** "since $T_s$ is very sensitive to changes in *LE* (Figure 3)" I think it should be "*LE* is very sensitive to changes in $T_s$" here.

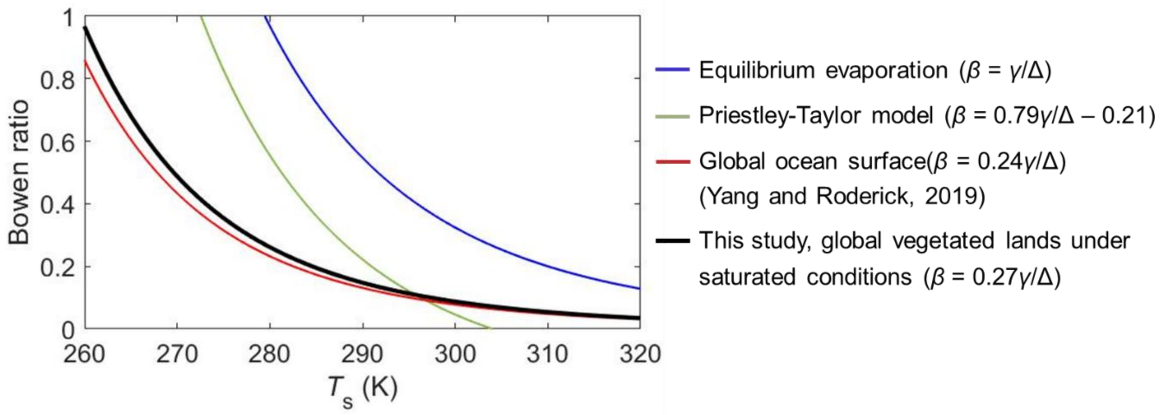
**Response:** It is "$T_s$ is very sensitive to changes in *LE*". As shown in Figure 3, the curve relating *LE* and $T_s$ is very flat near the maximum evaporation point (where actual evaporation occurs). This means that *LE* is only a weak function of $T_s$ but a small change in *LE* can lead to a large change in $T_s$.

**R1C12:** Around line 265. I think the maximum evaporation approach need both incoming solar radiation and reflected solar radiation. If so, using "incoming and reflected solar radiation" is more accurate than "ultimate external forcing".

**Response:** This reviewer was correct. We will revise relevant statement as suggested.

**R1C13:** Symbols and lines are hard to be distinguished in Figure 2. Please improve it. Please add the line of Priestley-Taylor model in Figure 2, which can give some implications for PT model applicability for different land surface.

**Response:** Will do. Comparison of the $\beta - T_s$ relationship over wet lands with those for the PT model, the equilibrium evaporation and over ocean surfaces are illustrated in Supplementary Figure S2. Thanks for your suggestion.




**Supplementary Figure S2.** Relationships between the Bowen ratio ($\beta$) and surface temperature ($T_s$).

**R1C14:** "Our results found this held over saturated lands but with considerable scatter (Figure 3)" It should be Figure 2 here.

**Response:** Oops! Thanks for pointing out this typo, which will be corrected in the revised manuscript.

Reference:


*Maes, W. H., Gentine, P., Verhoest, N. E., and Miralles, D. G.: Potential evaporation at eddy-covariance sites across the globe, Hydrol. Earth. Syst. Sci., 23, 925–948, https://doi.org/10.5194/hess-23-925-2019, 2019.*

*Mizutani, K., Yamanoi, K., Ikeda, T., and Watanabe, T.: Applicability of the eddy correlation method to measure sensible heat transfer to forest under rainfall conditions, Agr. Forest Meteorol., 86, 193–203, https://doi.org/10.1016/S0168-1923(97)00012-9, 1997.*

*Yang, Y., and Roderick, M. L.: Radiation, surface temperature and evaporation over wet surfaces. Q. J. R. Meteorol. Soc., 145(720), 1118–1129, https://doi.org/10.1002/qj.3481, 2019.*

**To reviewer #2**

**R2C1:**This is an interesting paper, which presents a new framework (in the context of continental surfaces) that could, according to the authors, allow to estimate potential evaporation. I find their approach very "elegant", and the results of this study could be important. However, there are important points that need to be clarified.

The approach ("maximum evaporation theory") is in fact not really new, as its most interesting developments have already been described by the authors in a previous paper, focused on evaporation over ocean ("ocean paper" hereafter). As said by the authors themselves, there is no major reason to expect strong differences between ocean and saturated land. Therefore, the main interest and the main novelty of the paper lie in the evaluation of this approach over land, thanks to a comparison with data from FLUXNET.

It is very difficult to understand the methodology in this paper correctly without carefully reading the ocean paper at the same time, as the authors don't properly justify and discuss the theoretical framework, the assumptions behind their approach, in the submitted paper. They often cite many papers to support their assumptions, but often many of them are not immediately relevant, and the best option for the reader is clearly to directly go to the "ocean paper".

Without explaining everything again in this paper, I think the paper would be much nicer and easier to understand if the authors better explained and justified the main assumptions, limitations etc. of their approach in this paper. It can be done concisely and, in any case, it should not be an issue as the paper is very short (it seems to have been written as a letter). I also think that a few additional analyses should be done. Additionally, important points need to be clarified (see below).

I therefore think that major revisions are needed before the paper could be published.

**Response:** Thanks for your positive evaluation and constructive comments. We will try our best to incorporate them during revision. We will add a new section titled "Overview of the maximum evaporation approach" in the revised manuscript to help the readers better understand the approach. Your individual comments are replied below.

**R2C2:** The new method to calculate potential evaporation proposed by the authors in this paper lies on several strong assumptions, not always well justified.

First, the authors hypothesize that "the Bowen ratio is a decreasing function of temperature". The authors cite some theoretical studies that make that point (sometimes indirectly and not very clearly). But I'm quite confused as, as noted in the discussion by the authors themselves, there is a major spread in the observed relationship between the Bowen ratio and Ts (Figure 2). The fit proposed by the authors is quite poor and the explained variance is small.

One could say that based on data shown by the authors, the Bowen ratio is in fact quite poorly controlled by Ts, while in the approach proposed by the authors the Bowen ratio is supposed to be a simple function of Ts.

It seems that either the theoretical arguments are wrong, or H and LE estimates and therefore Bowen ratio estimates from FLUXNET are far from accurate. The authors somewhat acknowledge the issue I stress here in the discussion section, but they seem quite embarrassed by it and to not really know how to deal with it: they don't provide a real conclusion to the discussion of this issue. This should be improved.

**Response:** The decreasing of Bowen ratio with surface temperature under wet conditions has long been tested and validated in numerous previous studies (Andreas et al., 2013, their Figure 1 and Figures 4-6; Guo et al., 2015, their Eq. 4; Philip, 1987, his Figure 1; Priestley and Taylor, 1972, green curve in Figure S3 below; Slatyer and McIlroy, 1961, blue curve in Figure R3 below; Yang and Roderick, 2019, black curve in Figure R3 below). This is also the basis of many other energy balance-based evaporation models, such as the Priestley-Taylor model and the equilibrium evaporation model (see Figure R3 below). This figure is included in the manuscript as Supplementary Figure S2.

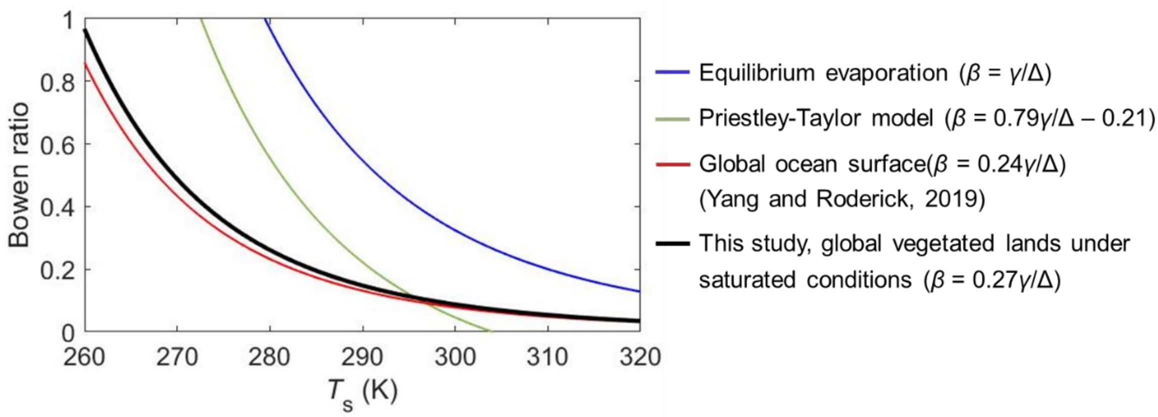



**Figure R3.** Relationships between the Bowen ratio ($\beta$) and surface temperature ($T_s$).

According to its definition, the Bowen ratio of equilibrium evaporation ($\beta_e$) can be written as,

$$\beta_e = \gamma \frac{T_s - T_a}{e_s(T_s) - e_s(T_a)} = \gamma \frac{\partial T}{\partial e_s} = \frac{\gamma}{\Delta}$$

where $\gamma$ is the psychrometric constant, $T$ and $e_s$ are temperature and saturated vapor pressure and subscripts $s$ and $a$ stand for surface and near-surface atmosphere, respectively. $\Delta$ is the slope of the saturation vapor pressure – temperature relationship. Since $\gamma$ is a very weak function of temperature and $\Delta$ increases with temperature, so the ratio $\gamma/\Delta$ decreases with temperature. A subsequent study by Priestley and Taylor accounted the fact that the real atmosphere is generally not saturated and modified $\beta_e$

as $\beta_{PT} = 0.79\gamma/\Delta - 0.21$. In our ocean paper, we fitted a Bowen ratio as $\beta_{ocean} = 0.24\gamma/\Delta$ and here we find that $\beta_{wet\ land} = 0.27\gamma/\Delta$. The difference between ocean and wet lands is mainly caused by stomatal resistance of vegetation over land as well as different surface roughness between ocean and land. This difference is discussed in the manuscript.

The spread of the data points can be caused by many reasons. First, the observations by EC towers can be a source of uncertainty. This is three-fold: (1) the quality of the observations, (2) the footprint within each EC tower may be heterogeneous and (3) whether the selected days are truly non-water-limited still contains uncertainties (however, please see our reply to R2C9). Second, as is seen in our Figure 2, different biome types exhibit different $\beta - T_s$ relationships. This can be caused by different surface resistance and roughness between biome types and even between sites. However, we are not able to parameterize $\beta$ for individual sites due to data limitation. Nevertheless, this limitation only has limited impacts on the model performance, as similar performance is obtained using both the generic $\beta - T_s$ relationship (i.e., $\beta = 0.27\gamma/\Delta$) and biome-specific $\beta - T_s$ relationships (Figure 4). Third, wind speed could be another factor that leads to the spread of the data points. For the same surface roughness, different wind speed lead to different aerodynamic resistance and therefore different Bowen ratio. However, this effect is usually very small, as demonstrated by the long-standing similarity theory (the transfer of mass and heat share the same aerodynamic process in the lower atmospheric boundary layer).

Despite all these effects, we do not intend to incorporate all of them in the calculation of $\beta$ to retain the simplicity (and so the practical application) of the method. On the other hand, incorporating all other effects (or a better model of estimating $\beta$) would not materially affect the model performance, as the sensible heat is generally very small over saturated surfaces.

We will improve the discussion about this important point in the revised manuscript.

**R2C3:** Second, if we accept the assumptions made in the paper, I agree that there exists a maximum evaporation along the Ts gradient. However, I don’t understand why the actual evaporation should be equal to this maximum evaporation given by their model. An infinity of pairs of (evaporation, Ts) values are compatible with the authors’ model. The authors do not discuss this point at all. Maybe I am missing something obvious.

I agree that the analysis of observations suggests that the maximum evaporation calculated with the authors’ approach is close to the observed evaporation (when there is no water limitation) but could the authors justify, based on physical arguments, why the actual evaporation should be equal to the maximum evaporation given by their model?

**Response:** We do not understand the comment, "An infinity of pairs of (evaporation, $T_s$) values are compatible with the authors' model.", as there is only one maximum evaporation and one corresponding $T_s$ along the entire $T_s$ range. More importantly, we did not invoke any maximization (or minimization) assumption in the development of the method, the maximum evaporation emerges naturally from the trade-off between decreased net radiation and increased evaporative fraction as $T_s$ increases. Compared with observations (over both ocean and wet land surface), this maximum evaporation corresponds to actual evaporation and the $T_s$ at which the maximum evaporation occurs also corresponds to the observed $T_s$. This means that the method correctly captures the interactions between radiation, surface temperature and evaporation. This also explains why the maximum evaporation corresponds to the actual evaporation, because the method simultaneously recovers the observed $T_s$. We believe that this reviewer would accept this more easily if we used the observed $T_s$ to locate evaporation on the evaporation – $T_s$ curve (that will be the maximum evaporation or somewhere near the maximum point). The fact that we do not rely on observed $T_s$ again demonstrates the intrinsic interdependence between radiation, surface temperature and evaporation is correctly captured by the method. Our results also suggest that the maximum evaporation is a natural attribute of saturated surfaces, which results from the trade-off between decreased net radiation and increased evaporative fraction with the increase of $T_s$, as explicitly shown in Yang and Roderick (2019) and in the current study. Following your suggestion, we will add an "Overview of the maximum evaporation approach" in the revised manuscript to help the readers better understand the approach.

**R2C4:** Third, Delta T in equation (4), and therefore net longwave radiation at surface, is computed thanks to the atmospheric transmissivity for shortwave radiation. It is a huge assumption and it should be discussed.

For example, I don't see how this approach can deal correctly with the impact of aerosols or greenhouse gas (the former having generally an effect on shortwave radiation but not on longwave and conversely for the later). Their approach cannot deal with climate change, right? It should be said. Even for clouds, this assumption is problematic, as some clouds have a strong impact on shortwave radiation, but a weak one on longwave radiation, and conversely.

The authors should discuss this assumption and its limits, and demonstrate that it is reasonable, over land, that they can recover correctly net longwave radiation at surface in a wide range of conditions based on this approach etc.

**Response:** As suggested by this reviewer, we evaluate the estimates of longwave radiation against observations and other global products, and also compared our estimates with other two semi-empirical models. The overall conclusion is that the method used is able to capture net longwave radiation at the surface reasonably well and similarly (or even slightly better) with the other two semi-empirical models across a wide of conditions **when the surface is wet**.

Specifically, Figure R4 (this is Figure 6 in the manuscript) below shows a comparison of estimated net radiation with observed net radiation at the flux sites (across all selected site-days under wet conditions). Since we adopt observed net shortwave radiation, this comparison is essentially the validation of estimated net longwave radiation. It shows that the estimated net radiation corresponds well to the observed ones.

Figure R5 shows a comparison of three models in estimating monthly incoming longwave radiation against global products under wet conditions across the globe (the wet conditions are determined following Milly and Dunne, 2016). The three models include (i) the one used in our study (maximum evaporation model), (ii) the Brutsaert model (1975) and the (iii) Shakespeare-Roderick model (2021). Four global radiation products are used, including (i) ERA5, (ii) CERES, (iii) the Princeton forcing and (iv) the GLDAS forcing. We evaluate incoming longwave radiation here for two reasons: (i) some of the global products do not contain outgoing longwave radiation, and (ii) the outgoing longwave radiation is estimated based on the Stefan–Boltzmann law, so the real concern lies in the estimation of incoming longwave radiation. Our results show that the maximum evaporation model performs well in estimating incoming longwave radiation across global terrestrial environments when the surface is wet, with a typical RMSE of 20 W m$^{-2}$ and a typical mean bias within ±5 W m$^{-2}$. Compared with the other two methods, the longwave formulation embedded within the maximum evaporation model performs similarly in estimating incoming longwave radiation in terms of RMSE and better than the other two methods in terms of mean bias (Figure R5).

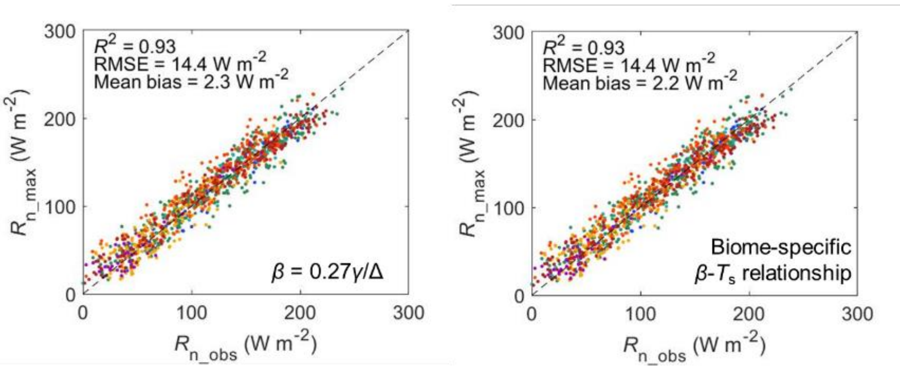



**Figure R4.** Comparison of estimated net radiation ($R_{n\_max}$) with flux site observations ($R_{n\_obs}$).

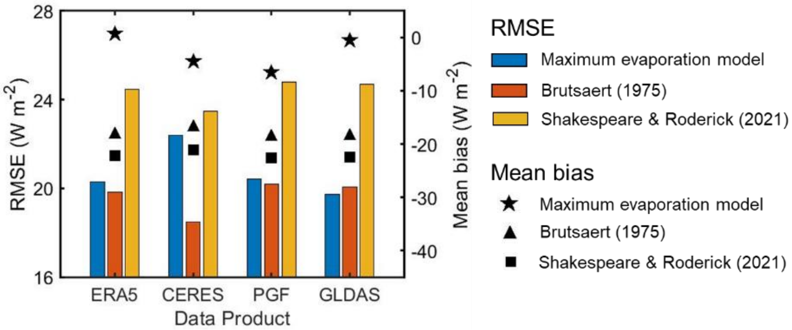


**Figure R5.** Comparison of model performance in estimating incoming longwave radiation validated against four global products. The three compared models include the maximum evaporation model in this study, the Brutsaert model (1975) and the Shakespeare and Roderick model (2021). The four global products include ERA5 (1979-2019; Hersbach et al., 2019), CERES (2001-2016; Kato et al., 2018), the Princeton global forcing dataset (PGF, 1979-2010; Sheffield et al., 2006) and the GLDAS global forcing dataset (1979-2014; Rodell et al., 2004).

We agree with this reviewer that the greenhouse gases and aerosols impact on shortwave and longwave differently. On the basis of a simple formula for practical applications, our justification for this overall good model performance is that we only deal with wet conditions. When the surface is wet, relative humidity of the atmosphere is also relatively high. When the atmospheric moisture is sufficient, more aerosols tend to favor the development of more clouds that simultaneously affect both shortwave and longwave radiation. This is different from the conditions such as high aerosol concentrations in dry environments (e.g., deserts), under which the method used herein may fail. However, this is beyond the scope of this study. With that we will add more discussion and evaluation of results in the revised manuscript regarding this point.

As for the concern on climate change, we did test it (but did not and do not plan to include it in the current manuscript) by incorporating the greenhouse gas effect (primarily the $CO_2$ concentration) in the formulation of $\Delta T$. The sensitivity of incoming longwave radiation on $CO_2$ concentration is determined using MODTRAN. Figure R6 below shows a comparison between the historical climate (1970-1999, solid curve) and the future climate under the A1B scenario (2070-2099, dashed curve). We estimated that compared with the end of last century, averaged over the entire global ocean, *LE* increases by 4.8 W m$^{-2}$ and sea surface temperature increases by 2.3 K by the end of this century. These are very close to the ensemble of climate model projections of 4.6 W m$^{-2}$ and 2.4 K, respectively. We intend to publish those results in future work as they are beyond the scope of the current manuscript.

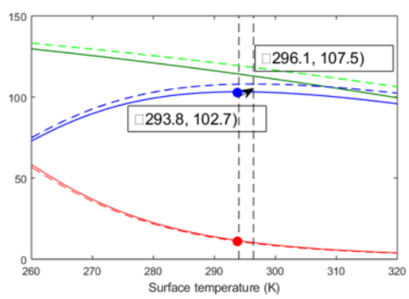



**Figure R6.** Variations of *LE* (blue), *H* (red) and $R_n$ (green) with surface temperature averaged over global ocean for historical period (1970-1999, solid curve) and future period under the A1B scenario (2070-2099, dashed curve).

**R2C5:** The authors write that a key issue of energy-balance based models of evaporation is that they consider that "Rn is an independent forcing of E". I don't understand precisely what they mean here, and the references that they cite are not clear. The notion of "independent forcing" is not clear to me. Is a forcing not always independent from the variable it forces, by definition? Additionally, in a climate model, where a coupling exists between the land and the atmosphere, this is not an issue, right? Even in an offline model, with observations, the impact of E on Rn is already included in observed Rn, so why it should be an issue to estimate E?

It is like saying that it is not possible to obtain a realistic simulation with an ocean model forced by observed atmospheric fields (including wind), because the wind field is in fact impacted by sea surface temperature. The implicit impact of sea surface temperature on wind is already included in wind forcing, so this is not an issue.

**Response:** We believe that this reviewer correctly understood our statement of "$R_n$ is not an independent forcing of $E$". Exactly as this reviewer understood, we meant that the impact of $E$ on $R_n$ is already included in observed $R_n$. The real underlying issue is $T_s$, because $T_s$ is neither independent of $R_n$ nor evaporation. This is not an issue of estimating evaporation, as demonstrated by long-standing validity of the Penman model and the Priestley-Taylor model in estimating evaporation. However, this is an issue of understanding evaporation (e.g., attributing evaporation changes by using the Penman model and/or the Priestley-Taylor model). For example, in the existing Penman model, $T_s$ is assumed unknown and that was why Penman developed an approximation. However, the Penman model also assumes $\underline{R}_n$ is known (which requires knowledge of $T_s$). However, as we note in our earlier work on oceans, as $T_s$ increases, $R_n$ actually declines which is the correct physics. However, here we clearly show that evaporation does not always increase with temperature; it depends on the competition with $R_n$-$T_s$ interactions. Moreover, we find that evaporation is not sensitive to changes in $T_s$ but instead, $T_s$ is very sensitive to changes in evaporation. This somewhat suggests that for a given solar radiation, temperature is more of a response rather than forcing of evaporation over wet surfaces.

Correctly understanding and parameterizing these processes/interactions are not only scientific significant but also of important practical uses. For example, here we highlight the implication for estimating potential evaporation, which is the actual evaporation **if the underlying surface were wet**. This implication is beyond the scope of the study but will be addressed in future work. For observations under wet conditions (e.g., the selected wet site-days in the current study), the observed actual evaporation conforms with the definition of potential evaporation, so using observed $R_n$ (or other meteorological forcing required by other models) to estimate potential evaporation is straightforward. However, when the surface is not wet, the observed $R_n$ can be different from the $R_n$ that would have been measured **if the underlying surface were wet** (here we show that $R_n$ decreases with $T_s$, so the observed $R_n$ over a dry surface will be smaller than that if the surface were wet because a wet surface usually has a lower $T_s$ than a dry surface when all else is equal). In the maximum evaporation approach, neither observed $R_n$ nor $T_s$ is required. Our testing results show that the maximum evaporation approach is able to recover the observed $R_n$, $T_s$ and evaporation over wet surfaces indeed suggest the possibility of using this approach to estimate potential evaporation in dry environments. That is why we need the forcing/s to be truly independent. This important implication is discussed in the manuscript.

**R2C6:**The authors criticize classical approaches to estimate potential evapotranspiration on a theoretical basis, and write that other studies indeed showed that these approaches are not perfect. OK, but their model is also not perfect, some strong assumptions and approximations have to be made, and its results are also not perfect, as shown in the paper. Therefore, they should compare their results with those obtained with a few common approaches to estimate potential evapotranspiration, using the FLUXNET dataset.

It is not too much work and this analysis clearly should be in this paper, as we want to know whether their model outperforms classical ones. It is possible as the paper is very short.

**Response:** Thanks for the suggestion. Please see our reply to R1C3.

**R2C7:** L75. See my major comments. OK, there is a maximum evaporation, but why this maximum evaporation should be equal to the actual evaporation?

**Response:** Please see our reply to R2C3.

**R2C8:** L83: The authors should discuss how land surface (with no water limitation) and ocean surface differ and how it may impact E.

**Response:** Thanks for the suggestion. We will add discussion on the difference between ocean and wet land surfaces in the introduction to better inform our motivation.

**R2C9:** L110: The selection of the days without water limitation seems very ad-hoc and subjective, with for example the step "50% of maximum soil moisture (taken to be the 98$^{th}$ percentile)".

How were the criteria chosen? Trial and error? How can we be sure that the criteria lead to a good separation of days with or without water limitation? Maybe the separation is not that good, which could be explain why the observed relationship between the Bowen ratio and Ts is not really the one expected by the authors?

More generally, are the results of the paper sensitive to the criteria used to select the days without water limitation? This should be tested.

**Response:** The selection of days without water limitation is largely based on the same selection criteria given by Maes et al. (2019). The 50% of maximum soil moisture is chosen because the field capacity of soil (evaporation is generally not limited by water if the soil moisture were higher than field capacity) usually lies in a range of 33% - 50% of the saturation point (assumed to be the maximum soil moisture at each site). The "98$^{th}$ percentile" is also directly taken from Maes et al. (2019). Although they did not explain why the "98$^{th}$ percentile" was used, we suspect that this is to ensure that the selection is not affected by a few unrealistic high soil moisture records commonly present in the FLUXNET dataset. More importantly, as also pointed out by this reviewer, the model performance is not sensitive to the selection criteria (see Figure R7, where the soil moisture criterion is set to 30% – 70% of the maximum soil moisture, the maximum soil moisture criterion is set to the 95$^{th}$ – 99$^{th}$ percentile, and the evaporative fraction criterion is set to 0.6 – 0.9. We will add this result in the supplementary material as a support.

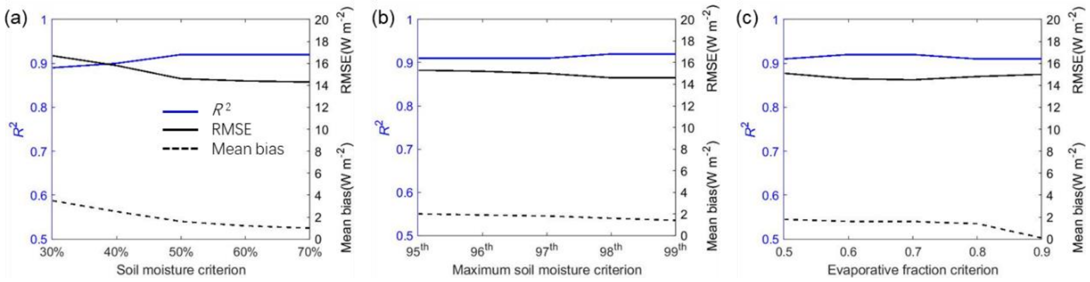



**Figure R7.** Model performance in estimating *LE* with varying selection criteria of unstressed evaporation observations. (a) The soil moisture criterion varies from 30% to 70%, (b) the Maximum soil moisture criterion varies from 95$^{th}$ to 99$^{th}$ percentile and (c) the evaporative fraction criterion varies from 0.5 to 0.9.

**R2C10:** L112. "To avoid dealing with strongly advective condition we additionally removed days having a negative H value." Are these conditions frequent? It is important to provide this information as if the approach proposed by the authors cannot deal with a large number of days, it limits its real-world applicability to estimate potential evaporation.

**Response:** The removal of strong advection condition is mainly to ensure the data quality as reliable observations by the EC tower is not guaranteed under strong advection conditions (Mizutani et al., 1997; Maes et al., 2019). Following your comments, we count the proportion of negative *H* values in the datasets. Out of all available data points, negative *H* values only account for about 5% of the total daily observations.

References:

*Aminzadeh, M., Roderick, M. L., and Or, D.: A generalized complementary relationship between actual and potential evaporation defined by a reference surface* *temperature, Water. Resour. Res., 52, 385–406, https://doi.org/10.1002/2015WR017969, 2016.*

*Andreas, E. L., Jordan, R. E., Mahrt, L., and Vickers, D.: Estimating the Bowen ratio over the open and ice-covered ocean, J. Geophys. Res. Oceans, 118, 4334–4345, https://doi.org/10.1002/jgrc.20295, 2013.*

*Brutsaert, W.: On a derivable formula for long-wave radiation from clear skies. Water. Resour. Res., 11(5), 742-744, https://doi.org/10.1029/WR011i005p00742, 1975.*

*Guo, X., Liu, H., and Yang, K.: On the application of the Priestley–Taylor relation on sub-daily time scales, Boundary. Layer. Meteorol., 156, 489–499, https://doi.org/10.1007/s10546-015-0031-y, 2015.*

*Hersbach, H., Bell, B., Berrisford, P., Hirahara, S., Horányi, A., Muñoz Sabater, J., et al.: The ERA5 global reanalysis. Q. J. R. Meteorol. Soc., 146(730), 1999-2049, https://doi.org/10.1002/qj.3803, 2020.*

*Kato, S., Rose, F.G., Rutan, D.A., Thorsen, T.J., Loeb, N.G., Doelling, D.R., et al.: Surface irradiances of edition 4.0 Clouds and the Earth' s Radiant Energy System (CERES) Energy Balanced and Filled (EBAF) data product. J. Clim., 31, 4501–4527, https://doi.org/10.1175/JCLI-D-17-0208.1, 2018.*

*Kleidon, A., and Renner, M.: Thermodynamic limits of hydrologic cycling within the Earth system: concepts, estimates and implications. Hydrol. Earth. Syst. Sci., 17, 2873–2892, https://doi.org/10.5194/hess-17-2873-2013, 2013.*

*Kleidon, A., Fraedrich, K., Kunz, T., and Lunkeit, F.: The atmospheric circulation and states of maximum entropy production. Geophys. Res. Lett., 30, 2223, https://doi.org/10.1029/2003GL018363, 23, 2003.*

*Kleidon, A., Fraedrich, K., Kirk, E., and Lunkeit, F.: Maximum entropy production and the strength of boundary layer exchange in an atmospheric general circulation model. Geophys. Res. Lett., 33, L06706, https://doi.org/10.1029/2005GL025373, 2006.*

*Kleidon, A., Kravitz, B., and Renner, M.: The hydrological sensitivity to global warming and solar geoengineering derived from thermodynamic constraints. Geophys. Res. Lett., https://doi.org/10.1002/2014GL062589, 42, 138–144,2015.*

*Lorenz, R.D., Lunine, J.I.,Withers, P.G., and McKay, C.P.:Titan, Mars and Earth: entropy production by latitudinal heat transport. Geophys. Res. Lett., 28, 415–418, https://doi.org/10.1029/2000GL012336, 2001.*

*Maes, W. H., Gentine, P., Verhoest, N. E., and Miralles, D. G.: Potential evaporation at eddy-covariance sites across the globe, Hydrol. Earth. Syst. Sci., 23, 925–948, https://doi.org/10.5194/hess-23-925-2019, 2019.*

*Milly, P. C. D., and Dunne, K. A.: Potential evapotranspiration and continental drying, Nat. Clim. Change, 6, 946–949, http://dx.doi.org/10.1038/nclimate3046, 2016.*

*Mizutani, K., Yamanoi, K., Ikeda, T., and Watanabe, T.: Applicability of the eddy correlation method to measure sensible heat transfer to forest under rainfall conditions, Agr. Forest Meteorol., 86, 193–203, https://doi.org/10.1016/S0168-1923(97)00012-9, 1997.*

*Paltridge, G.W.: The steady-state format of global climate. Q. J. Roy. Meteor. SocC., 104, 927–945, https://doi.org/10.1002/qj.49710444206, 1978.*

*Philip, J. R.: A physical bound on the Bowen ratio, J. Clim. Appl. Meteorol., 26, 1043–1045, https://doi.org/10.1175/1520-0450(1987)026<1043:APBOTB>2.0.CO;2, 1987.*

*Priestley, C. H. B., and Taylor, R. J.: On the assessment of surface heat flux and evaporation using large-scale parameters, Mon. Weather. Rev., 100, 81–92, https://doi.org/10.1175/1520-0493(1972)100<0081:OTAOSH>2.3.CO;2, 1972.*

*Rodell, M., Houser, P. R., Jambor, U. E. A., Gottschalck, J., Mitchell, K., Meng, C. J., et al.: The Global Land Data Assimilation System. Bull. Amer. Meteor. Soc., 85, 381–394, https://doi:10.1175/BAMS-85-3-381, 2004.*

*Shakespeare, C. J., and Roderick, M. L.: The clear sky downwelling longwave radiation at the surface in current and future climates. Q. J. R. Meteorol. Soc., https://doi.org/10.1002/qj.4176, 2021.*

*Sheffield, J., Goteti, G., and Wood, E.F.: Development of a 50-year high-resolution global dataset of meteorological forcings for land surface modeling. J. Clim., 19, 3088–3111, https://doi.org/10.1175/JCLI3790.1, 2006.*

*Slatyer, R.O., and McIlroy, I.C.: Practical microclimatology, Commonwealth Scientific and Industrial Research Organisation, Canberra, Australia, 1961.*

*Yang, Y., and Roderick, M. L.: Radiation, surface temperature and evaporation over wet surfaces. Q. J. R. Meteorol. Soc., 145(720), 1118–1129, https://doi.org/10.1002/qj.3481, 2019.*