Comment on hess-2021-436

Anonymous Referee #2

Referee comment on "Testing a maximum evaporation theory over saturated land: implications for potential evaporation estimation" by Zhuoyi Tu et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-436-RC2, 2021

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.

Major comments

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.

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 physcial arguments, why the actual evaporation should be equal to the maximum evaporation given by their model?

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.

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.

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.

Specific remarks

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

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

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 98th 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.

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.