Dear Reviewer,

We would like to thank you for taking the time to review our manuscript and for your valuable comments and suggestions. After reading your comments and those of the other reviewers, we decided to restructure the manuscript as described in the response to comment 4. This is to make it clearer and more concise. Please see below for responses to your comments point by point.

Sincerely,

Estefanía Muñoz
Andrés Ochoa
Germán Poveda

Major comments

1. As recognized in some lines of the manuscript (L173) but not elsewhere (e.g., L122), Laio's model does take into account the energy constraint, precisely via ET_{max}. With respect to other sensitivity analyses on ET_{max}, here a standard process-based model or an empirical relationship are used to explain variations in such parameter. But, in the end, in its current form, the manuscript appears a sensitivity analysis. Beyond the somewhat misleading framing of this work, all in all changes to the soil moisture balance due to different radiation levels are modest – something that was already concluded in Daly and Porporato (2006, Water Resources Research). Furthermore, neither Laio's model nor the proposed extension take into account the effect of extremely high soil moisture values.

   Authors' response: Thank you for your comment. Although Rodríguez-Iturbe et al. 1999, 2004 and Laio et al. 2002 mentioned that Emax can be obtained from equations like Penman-Monteith, they do not explicitly state that Emax is constrained by energy availability. According to the Penman-Monteith equation, transpiration, in addition to being a function of radiation, is a function of soil heat flux, air vapour pressure deficit, the specific heat of the air, and air temperature, among other variables. In this work, we analyze and separate the effect of the available energy when these other variables are present, and we obtained an empirical relationship to relate Emax with PAR. Besides, in a previous work (Muñoz 2019, https://repositorio.unal.edu.co/handle/unal/76885), results indicated that the stochastic behaviour of the radiation does not play an important role in the pdf of s if the Rodríguez-Iturbe model is used, but its mean values do. After this conclusion, and maintaining the other assumptions of Rodríguez-Iturbe et al. and Laio et al., this model is suitable in the energy-limited ecosystem for a fixed climate season as long as the value of the maximum transpiration is given by the radiation amount available at the site. Furthermore, based on the results shown in Figures 5 and 6, under some combination of parameter values, there are substantial differences in soil moisture dynamics when contrasting values of PAR are considered. We will better point out the highlights of the work in the manuscript.

2. More importantly, Laio's model is a stochastic soil moisture model, taking into account the randomness in precipitation timing and amount. Radiation fluctuates too, as also apparent from the data used for the empirical relationship. This work appears not to consider this aspect in any way, despite considering daily data (from the sub-hourly upscaling). I find this incorrect. Moreover, a solution to this problem is available in Daly and Porporato (2006, Water Resources Research). If, instead, the point is to consider the average seasonal radiation, then this should be made clear when applying Penman-Monteith and would mean dropping the empirical radiation-transpiration relationship.

   Authors' response: As we mentioned in the response to comment 2., the results of our previous work showed that the stochastic component of radiation (after removing seasonality) does not have a determining effect on the pdf of soil moisture when using the Rodríguez-Iturbe model and under its assumptions. As for seasonality, we did not consider it, but this could be the next step. Although we are assuming a deterministic value for energy availability, the Penman-Monteith equation assumes that transpiration increases linearly with radiation, which might be unrealistic given plant damage by energy excess, CO_2 limitations, and physiological constraints (e.g. photoinhibition), etc. Given this, we proposed an empirical equation to relate transpiration and PAR that shows a saturation value in which transpiration does not increase even though energy availability continues to do so. We will explain this point better in the manuscript to avoid confusion.

3. The relative role of Penman Monteith and the empirical radiation-transpiration relationship remains unclear. From my reading, it seems that one of them would suffice in reaching the goal of linking ET_{max} to radiation (but see point 2 regarding a potential crucial difference).

   Authors' response: Thank you for letting us note that this point is not clear in the manuscript. The Penman-Monteith equation was used to couple transpiration, stomatal conductivity and carbon assimilation using the data from the FLUXNET database. Thus, an empirical relationship was found between transpiration and available energy when multiple factors intervene (e.g. the physiological capacity of plants to transpire, the availability of CO_2, etc). Note that radiation not only influences the dynamics of transpiration (Penman-Monteith equation) but also that of assimilation (Farquhar model). We will clarify this in the manuscript.

4. I also found the manuscript difficult to follow. Aside from the role of Penman-Monteith vs the empirical relation (see point 3 above), the structure of the text (and subdivision in sections and subsections) is not intuitive and there are many details reported that appear of low relevance to the questions at hand, or so well established not to require anything beyond a reference (e.g., Table 1 and 2; Appendix A and B). I also note that a large number of references are reported in support of rather general points (e.g., L120), where one or two well-chosen references would suffice and serve the reader better.

   Authors' response: Thanks for pointing this out. We agree that the manuscript needs to be restructured to make it clearer and easier to follow. We will remove Appendices A and B and the citations that are not required. Besides, following the suggestion of another reviewer, we will modify the structure as follows: i) Introduction, ii) short recapitulation of the Laio model, iii) review of transpiration mechanism under water and energy-limited conditions, iv) sensitivity analysis, v) validation, and vi) conclusions. Chapter iii) will include the empirical function to relate transpiration and PAR and the replacement of Emax with Tmax(PAR)+Ew.

Minor comments

1. L88: the definition of s

Authors’ response: You are right. This definition will be replaced by “s is quantified as the ratio of the volumetric water content (ratio of water volume to soil volume) to soil porosity.”

2. L90: the fact that transpiration depends only on maximum stomatal conductance, where (as also apparent from Penman Monteith formula) transpiring biomass and leaf-atmosphere coupling play a role too

   Authors’ response: Thank you for letting us note that this sentence is incomplete. We will complete it.

- L173 (see point 1 above)

  Authors’ response: We will give the definition of E_{max} by Rodríguez-Iturbe et al. and Laio et al.

- L239: the pdf of s is obtained under *stochastic* steady state, not steady state. This is an important difference

  Authors’ response: You are right. We will fix this statement.

- There are also few typos, e.g., lines 92, 137, 730 (and elsewhere).

  Authors’ response: We will check the spelling, grammar, and punctuation of the entire manuscript.