The study analysed the impact of heuristic β-type water stress formulations, commonly adopted to many land-surface schemes in terrestrial biosphere models and identifies when such a formulation diverges for more detailed models that include explicit formulation of plant hydraulics. Additionally, it proposed a new dynamic β-type formulation that "emulates" with a very reduced complexity the limitations that originate from plant hydraulics. The study is focused and very well written, and clearly within the scope of HESS. I found particularly insightful the analysis with the simple plant hydraulic model that clearly shows when plant hydraulics are expected to play a major role, and the dynamic β model which can be easily adopted by exiting TBMs. I can suggest the manuscript for publication after the following comments have been addressed:

Specific comments:

- I believe that information from S4 should move to the main manuscript. While reading the manuscript I was confused whether soil moisture dynamics were simulated, or if soil moisture and soil water potential were set to the observed values at the site. I could also not tell what ψs corresponds to (i.e. root zone average potential? potential of root average soil moisture?). I appreciate that the authors like to present a focused manuscript, but bringing this information in the main article will improve its readability.

- Regarding the calibration of the dynamic δψψψψ model, to my understanding, the results from the full complexity PHM was used to derive the dependence of the stress factor to Tww and ψs. As this would not be the case with existing TBMs, can the authors suggest a general procedure on how a generic calibration could be achieved for
a “general-purpose” dynamic $\beta$ model?

- One aspect worth discussing is the use of capacitance within a plant hydraulic model. I would encourage the authors to expand their discussion regarding this point, as several TBMs now adopt a resistor/capacitor approximation when formulating their plant hydraulic modules.

- I agree with reviewer 1 regarding the interpretation of the results. The behaviour of $\beta$ models limiting particularly photosynthetic rates (or in some cases $V_{cmax}$), might have a different behaviour that the reported. That would be worth discussing further.

Minor comments:

- Line 101, 98: has instead of is?
- Line 133: Neutral atmosphere, instead of “negligible atmospheric stability”
- Line 137: “and codes will be made available online with acceptance of this manuscript”. Not a necessary statement in the manuscript. The code will appear upon acceptance.
- It would be nice to keep consistent units for transpiration and conductance terms throughout the manuscript.