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Comment on hess-2020-671

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Referee comment on "Plant hydraulic transport controls transpiration sensitivity to soil water stress" by Brandon P. Sloan et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-671-RC3, 2021

The manuscript by Sloan and coworkers presents a hierarchy of soil-plant-atmosphere models describing environmental and plant controls on transpiration. This hierarchy starts from a simple plant hydraulic model (minimalist PHM) that assumes a fixed soil-to-leaf hydraulic conductance and a leaf water potential dependent stomatal regulation. A more physiologically detailed model follows (complex PHM), which includes soil water potential dependent soil-to-root conductance and xylem water potential dependent plant conductance, in addition to leaf water potential regulation of stomatal conductance. The simpler 'beta model' (empirical piecewise relation between soil moisture and transpiration rate) is found to be a limit solution of the PHMs when the soil-to-leaf conductance tends to infinity. Finally, a land surface model (LSM) is proposed based on CLM version 5, and various transpiration regulation schemes are implemented to compare them in a realistic setting. The simple beta model is shown to perform well after it is modified to include atmospheric water demand.

This topic is suitable for HESS and is timely given the ongoing discussions on how to develop and implement plant hydraulic modules in land surface and ecohydrological models. The manuscript is also framed in a nice pedagogic way, from simple to complex models (a schematic roadmap of the various models used could also be useful). Overall the approach is sound, but I have some technical concerns and comments, and suggestions to improve clarity and provide an easier roadmap for the reader. Minor editorial comments are listed at the end.

Main comments

- Units and unit conversions: water flows are expressed in terms of W/m^2, which is fine, though not immediately intuitive for hydrologists. However, some choices of units are unusual (in most cases the choice will have no consequences on the model results). For example, expressing vapor pressure deficit in MPa (L80) is not consistent with usual units of kPa or mol/mol. The driving force of evaporation is typically expressed as a molar concentration difference (as also explained in L256 of the Supplement), and using a water potential difference would require some transformations because the water potential of water vapor is not a linear function of molar concentrations. Later, vapor pressure deficit is expressed in Pa (L113); I suggest making units consistent throughout. Stomatal conductance to water vapor is expressed in mol_H2O/m^2/s, but conductances are typically expressed in units referring to the carrier medium, while driving forces have the units of the scalar being transported. Here stomatal conductance should have units of mol_air/m^2/s and vapor pressure deficit mol_H2O/mol_air (=Pa/Pa). A dimensional analysis of the second term of Eq. (9) gives the same result. Similar issues arise in Eq. (13), where I could not recover the desired units for T_d.
- Model calibration (Table S3): the LSM is rather complex, with 15 free parameters. I wonder if some parameters could be prescribed to facilitate the calibration. For example, soil parameters and LAI might be constrained based on site-specific information. Some plant parameters could also be taken from the literature. The water potential at 50% loss of conductivity for Ponderosa pine ranges between -2 and -4 MPa (Domec and Pruyn, 2008; Maherali and DeLucia, 2000; Stout and Sala, 2003), very much in line with the calibrated value, and xylem conductivities are a bit lower than the calibrated parameters (see references above). Instead, the value of water potential at 50% stomatal closure is really low at almost -10 MPa. Pinus ponderosa is a rather conservative species when it comes to stomatal closure, with nearly full closure around -2 MPa (DeLucia and Heckathorn, 1989). This suggests that in the model stomatal closure essentially does not occur until the xylem is completely cavitated, which does not seem reasonable - perhaps a result of co-variation with other not well-constrained parameters?
- Definition of water transport regimes: I was a bit confused by the definitions of supply-limited, demand-limited, and transport-limited conditions. L208: supply is not really limited if the soil-to-leaf conductance is large - this condition would more demand-limited. L301: also in this case, I would think of riparian systems as not supply-limited. Perhaps the terminological issue arises because ‘supply’ in my mind refers to the whole soil-plant system, and ‘demand’ refers to the plant-atmosphere system.

Minor comments

MAIN TEXT

A figure schematically illustrating the model hierarchy, and how the different models are compared and interfaced would be a useful roadmap for the reader.

L38: beta functions per se only include soil moisture effects on transpiration, so they are insensitive to atmospheric dryness by construction - do you mean that the overall transpiration model is insensitive because of the multiplicative coupling of beta and atmospheric demand?

L52: through acclimation as well.

L53: also the intermediate step of upscale to plant level is not trivial, since plants are not uniform cylinders but have branching architecture with progressive and nonlinear variations in hydraulic properties along the water flow pathway.

L60: for clarity I would suggest to add “water” in front of “supply-demand framework”.

L74: in Manzoni et al. (2014) we did not assume a fixed soil-to-leaf conductance - our approach was more similar to the complex PHM, but with some simplifications aiming to obtain analytically the soil moisture thresholds in beta as a function of plant traits and soil properties.

L123: how are least squares used in this context?

L243: extra “the”.

L247 (and in the abstract L11): I am not sure I follow how variability of water demand comes into play - the results are about values of transpiration rates, not variability.

L268: if the model was calibrated, where does the bias originate? Does it originate because the calibration does not minimize square differences only, but uses a more complex objective function (Eq. (S4))?

L323: the point raised that an empirical correction of the beta function works well is consistent with the results presented, but generalizing this result is difficult - if every site requires a calibration of the corrections to the beta function, then it becomes simpler to use a full PHM. A comment on how results could be generalized would be useful.

SUPPLEMENT

Supplementary information: I would present first the LSM, and then results obtained using that model. Describing LSM input data and results before describing the model makes the supplement hard to read. This re-ordering of the sections would also allow referring to them in order in the main text.

Table S1: in the main text hydrological fluxes are expressed in W/m^2, here energy fluxes are expressed in water depth units. Reversing the units or using consistent units throughout would improve clarity.

Table S2: for leaf conductance and water potential at 50% reduction of conductance, I would specify that these parameters refer to stomatal closure, not leaf xylem cavitation.

Eq. S3: I am not sure why this metric was also normalized by the "relative soil saturation of soil water stress", or difference between theta_o and theta_c. It would also be useful to remind the reader of the meaning of these moisture thresholds.

Section S6.2.1: is LAI in this section the total LAI?

L203: by "diffuse leaves" is it meant "shaded leaves"?

L205: "value" singular.

L321: in Medlyn's model, transpiration is minimized for given photosynthesis; maximizing the ratio of photosynthesis and transpiration would not be a well-posed problem (for stomatal conductance going to zero, the photosynthesis-to-transpiration ratio is highest).

L322: another assumption is that leaves are optimized for light-limited conditions, not CO2 limited.

L358: I see the rationale for keeping the gas exchange model (relatively) simple, but could this assumption affect the results? Temperature effects on photosynthetic parameters will affect stomatal conductance via Medlyn's model and ultimately water demand as well.

L367: typo "differ from".

L372: typo "is given".

Section S6.4.3: it would be good to provide a clarification that the equations used to account for the leaf nitrogen profile do not assume a dynamic sub-daily allocation scheme (nitrogen cannot be re-allocated so quickly in the canopy), but are the result of integration of the vertical profile of a given photosynthetic parameter, so that the proportion of nitrogen in shaded or sunlit leaves changes during the day, but not the actual leaf nitrogen concentration at any given depth in the canopy (assuming I am interpreting correctly the equations in this section).

L385: typo "leaves".

Eq. S86: assuming vertically uniform hydraulic conductivity and sapwood area.

Eq. S89: assuming 1-dimensional transport in the soil, as in the xylem?

References

DeLucia, E.H., Heckathorn, S.A., 1989. The effect of soil drought on water-use efficiency in a contrasting Great-Basin desert and Sierran montane species. Plant Cell Environ. 12, 935–940.

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