

## Referee 2

This manuscript presents a new model of plant hydraulics in the framework FATES, named FATES-Hydro, and kind of explores its sensitivity on some parameters for the ponderosa pine forest US-CZ2.

The manuscript is a mixed bag of several elements, which leaves the reader alone on most of it. It might be that it presents the model development of FATES-Hydro (but this is not clear), it does a very informal sensitivity analysis, and it assesses model behaviour during a very long drought without the pretence of realism.

*To clarify, this is not a simple model development paper, nor a strict sensitivity paper. The main purpose is to use some sensitivity analysis to explore scientific questions. We made some manipulations on the original hydraulic module that has been written by Christoffersen et al, and that is described in more detail by Xu et al., in review, <https://doi.org/10.5194/egusphere-2023-278> and its supplementary material. We will add this statement in the introduction section to clarify this point and describe the specific changes that have been made relative to those also described in Xu et al.*

*We acknowledge that the biggest disadvantage of this study is the lack of sufficient field data to constrain the model. This is a result of using a natural drought as an experiment of opportunity, which because it was not anticipated, did not allow for as coordinated planning as would be the case in an experimentally-manipulated drought. The trees at that site had all died by the time we started this study.*

Was the newly model developed for the study? The text reads (somehow) as if it should have been presented in Fisher et al. (2015) and Koven et al. (2020), but they are rather for FATES itself. So I guess the model development is presented here for the first time. If not, the reference is missing.

As we note above, the more fundamental reference for the FATES-Hydro model is Xu et al., in review, <https://doi.org/10.5194/egusphere-2023-278>, which had not yet been submitted at the time of this manuscript's submission. We will rework some of the text to refer to more details referenced in the Xu et al. manuscript, and how the specific configuration here differs from that.

- If yes, it is presented very badly: It changes notation all the time, for example using LWP or  $\Psi_l$  for water leaf potential.  
*We will use more consistent notation in revisions.*
- It uses unusual notation such as  $S_e$  for saturation (called standardized relative water content in the manuscript).  
*We will use more standard notation in revisions*
- It uses strange definitions such as " $e_i$  is the saturation vapor pressure (Pa) inside the leaf at a given vegetation temperature when  $A_n=0$ ", which might be

true but it is not explained (sounds strange anyway, why saturation should depend on  $A_n$ ).

*We will use more standard notation in revisions*

- There are different parts of the model that are not connected in the manuscript. For example, how is the formulation of Vesala et al. (2017) connected to the rest such as Ball-Berry?  
*We will explain this better in revisions.*
- Also in Vesala et al. (2017), what is  $k_{LWP}$  in the Kelvin equation? It is not given in Vesala et al.  
*We will explain this better in revisions.*
- It is not explained how  $\Psi_l$  is calculated.  
*We will explain this better in revisions.*
- Are there several stem sections?  
*There is one stem section per cohort.*
- It is not mentioned how root water uptake is calculated. I seem to have guessed at one point in the result section that it might be proportional to root length density.  
*That is correct. We will explain this better in revisions.*
- What is "we have sequentially solved the Richards' equation for each individual soil layer"? How is this working?

*This means we solve the equation for soil layers one by one, not simultaneous solve the equations for all the layers at the same time. There is more detail on this in Xu et al., in review.*

*We have made corrections accordingly for all of the above, and added the detailed description of the hydraulic module in both the FATES tech notes, and in Xu et al. manuscript.*

There are also weird choices like using Ball-Berry while calculating leaf water potential. There are a number of good papers that discuss this such as Anderegg et al. (PLoS One 2017, 10.1371/journal.pone.0185481) and references therein.

*To clarify, we didn't employ the Ball-Berry model in calculating leaf water potential, instead we incorporate the constraint of leaf water potential on photosynthetic rate and stomatal conductance through leaf water potential. Leaf water potential is calculated purely based on the physical process of transpiration. There are many different stomatal models used in the literature, which reflects real epistemic uncertainty in how to handle the process of stomatal conductance.. There is no right or wrong stomatal conductance model. Moreover, several studies (e.g. Franks et al. 2018, <https://onlinelibrary.wiley.com/doi/full/10.1111/gcb.14445>; or Knauer et al., 2015, <https://doi.org/10.1002/2015JG003114>) have shown a high degree of overlap between stomatal models such that the parametric uncertainty within any given stomatal model is greater than the structural*

*uncertainty associated with which model to choose. In any case, this uncertainty was not the focus of the paper, so we chose a single stomatal model and used it consistently across scenarios.*

It would also have been interesting to know why the factor  $\beta_t$  (why  $t$ ?) is applied on  $V_{cmax}$  when using actual leaf water potential. What is the physiological mechanism that reduces  $V_{cmax}$  during the day following leaf water potential?

*In the model,  $\beta_t$  is an overall measure of leaf water content. Low water content can reduce the biochemical reaction of the photosynthesis processes as those reaction requires water. In addition, reduced water content may also increase leaf temperature which may further inhibits the photosynthesis. We agree that there is further structural uncertainty in terms of how the hydraulic limitations affect both photosynthetic and conductance processes, and plan to explore this in future work.*

However, I was wondering why the model was developed in the first place given that the current manuscript uses CLM5 in FATES, which already includes plant hydraulics.

*The difference is that CLM5 is not a demographic model but a big-leaf model, while FATES is a demographic model that allow us to simulate the vegetation dynamics through demographic processes. Because the fundamental unit shifts in this transition from a PFT to a cohort of a given size, canopy layer and PFT on a given patch of shared disturbance history, this requires a different approach to representing hydraulic dynamics than in the unstructured big-leaf model.*

There are also different developments of plant hydraulics in the literature such as all the work about the models SurEau and FETCH, Janott et al. (Plant and Soil 2011, 10.1007/s11104-010-0639-0), Huang et al. (New Phytolo 2017, 10.1111/nph.14273), to name just a few that could have been considered or discussed.

*We have discussed these different hydraulic models in the MS, L 209 – 221, where we indeed cited Janott et al paper, though not Huang et al. We will cite the Huang et al paper in the revised manuscript as well.*

For me, the really interesting part would have been the interactions between soil water and cohorts. It sounds like that the model has one tile per cohort so there is no interaction between cohorts. This is not explained in the manuscript let alone explored or discussed.

*In the model, multiple cohorts grow on the same land unit, and share the soil water. So there are interactions between cohorts. We will make this point more clear in the revised manuscript. In this case, we wanted to first understand the direct trait control in the absence of structural differences, which is why we held ecosystem structure constant and turned off growth and mortality processes. However, the model is capable to investigate dynamic competitive effects when growth and mortality processes are turned on. This will be the next step that we plan to explore in subsequent work. And this is also the reason why we choose to use FATES-Hydro.*

The manuscript presents further an ad hoc sensitivity analysis. It is also not really explained so I have to guess. Presenting sensitivity as change of model output due to change of a parameter within limits depends, of course, on the chosen limits. "the effective rooting depth, above which 95% of root biomass stays, varies from 1m to 8m", which is such a large range that, of course, everything will depend on it. I need no hydraulic model to know that.

*We choose the range of variation of these parameters based on the allowable biological range. If the range of root distribution can be that large in reality, which has been shown in many studies, then root distribution is the most important plant traits that can affect the response of the trees at that site. We are not trying to find which is the most sensitive parameters for a fixed fractional change in parameter value, but rather to identify what are the most important plant traits in that region. We will emphasize this point in the introduction section. In addition, which traits/parameters have strongest impact on model outcome largely depends on the physical environment and the temporal pattern of their variation.*

The explanation why the authors chose to change only P50\_gs and not ags is wrong (p11, l297ff; p13, l354ff). P50\_gs tells "only" at what potential close the stomata, eventually. It is actually ags that determines the strategy, i.e. isohydric vs anisohydric behaviour (I think to remember that this is explained in one of the Sperry papers).

*We choose the parameters that are relatively well understood and allow us to catch the safe vs. risky strategies as described by Skelton et al., rather than exhausting all possible parameters in the model, which would come with high computational costs but yields limited further scientific value. In essence, the different combination of p50gs and the shape parameter(ags) can generate similar stomatal response curves. For example, small negative p50gs with small ags would result in flat stoma response curve which is similar to a large negative p50gs combined with large ags. P50gs is well understood and has more observed data, while ags is less studied and has less availability of observed data. Given the formula of FATES-Hydro, with the same shape parameter (ags) value, the slope of the stomatal response curve at P50gs is determined by P50gs in the way that larger negative P50gs results in flatter response curve, and vice versa, which resemble the risky stoma (red curve) and safe stoma (blue curve) in fig 1A in Skelton et al. (2015) respectively. Given that we don't have any measurements of ags at our site, we used the default value of conifers given by Christoffersen et al., (2016).*

I was wondering at the results section if it would not have been better to adapt the model to the site first. Now it looks like that the model cannot reproduce GPP with any of the hydraulic strategies.

*Yes, we agree that there is a bias in the GPP across these cases. Hydraulic strategy alone certainly will not make the model to match both transpiration and GPP, whereas the latter is more controlled by traits*

*that affect biochemical reaction rate, e.g.  $V_{cmax}$  at leaf level and light distribution (or radiation penetration process) at canopy level. We didn't focus the model on the joint calibration of both ET and GPP for several reasons. The first is that we were more interested in exploring the parameter uncertainty and what is said about ecological dynamics of plants with different traits than in calibrating the model per se. Second is that here is some degree of uncertainty in the eddy covariance observations, and that the simulations here are still useful even in the presence of some bias in the GPP predictions.*

Also, the  $\Psi_l$  values are so strange that one should ask oneself if there is not something wrong with the model.

*We assume by "strange", the reviewer means the leaf water potential is too low which has also been pointed out by the first reviewer. The main reason is the model is run under a static stand mode, the stand density and LAI do not change. This counterfactual experiment is meant to allow comparison of the direct trait effects on ecosystem function, rather than the indirect effects of plant traits on ecosystem composition and function, which is a more complex process occurring over a longer timescale. In reality, the leaves would die when dehydrated. We have discussed the issue in the discussion section.*

Are the curves of Fig. 1 realistic? How do they compare to measurements? Are there PLC (percent loss of conductance) curves for ponderosa pine?

*Unfortunately, we don't have measurement of the vulnerability curve of the pines at that site since they had all died by the time we started this project.*

Why are the  $K_{max}$  different for the safe and efficient strategies?

*We covaried  $K_{max}$  with the  $P50$  of xylem because we want to represent the safety-efficiency tradeoff of the xylem system.*

How comes that  $\theta/\theta_{sat}$  increase at the bottom of the soil column in Fig. 5? Is there no drainage (Fig. 6 suggests otherwise) or is  $\theta_{sat}$  very different at the bottom of the soil column?

*This is mainly due to reduced water uptake at the bottom by roots.*

I would have lots of further comments but I think the manuscript should be brought into shape first.

Just as a last comment on the conclusions: it is very obvious that deeper rooting plants are less influenced by droughts, and that risky and safer xylem is more and less vulnerable to drought but it also has more and less GPP, by definition. These obvious results should not be the main conclusions. And the statement "that deep-rooted pines with risky stomata have the highest GPP but also the highest drought mortality risk" is misleading because shallow-rooted pines with risky stomata actually have the highest drought mortality risk.

*This statement refers to the result that deep rooted trees not refer to all the strategies, only the ones that might survive during the pre-drought period. We will reword this sentence to "that deep-rooted pines with risky stomata have the highest GPP but also the highest drought mortality risk of the plant trait combinations that may survive under non-drought conditions" This point is made in the prior paragraph but for consistency we will make it again here.*