Reviewer 2

The study presents a new trait-based plant hydraulics model that can scale tissue-level hydrodynamics to stand-level water use and hydraulic risks (SurEau-Ecos). The new model represents four plant water pools (leaf+stem X apoplasmic+symplasmic) and three soil water pools. The manuscript reports explorations of different numerical resolutions (explicit, implicit, semi-explicit) and recommends a time-step around 1min using implicit/semi-implicit methods. The difference between the SurEau-Ecos and SurEau, a more detailed individual-level version, is shown to be small. Sensitivity analysis suggests stand-level parameters determine the time to hydraulic failure while hydraulic traits such as psi_50 for leaves contribute more to the drought-driven mortality risk. Finally, predictions from SurEau-Ecos at the regional scale are cross-validated with species distributions for two temperate species in France.

Overall, I really enjoy reading the manuscript partly because the equations and model structures are presented in a clear way, starting from the governing equations and then diving into different components. I appreciate the analysis of numerical schemes, which we also struggled with when developing the plant hydraulics in ED2 (and thanks for showing the biases of our semi-implicit method in a more robust way).

Meanwhile, I feel the manuscript can become more useful to the community if expanding discussions on the "necessary/optimal" complexity in plant hydraulics at ecosystem scales. In addition, lack of competition and succession can really limit the utility of the model at longer timescales in my opinion. Here are my comments following the order of the manuscript

We thank the reviewer for his positive appreciation of our work and his thorough revision of the manuscript.

We totally agree with the reviewer comment regarding the potential benefit of a discussion around the complexity of mechanisms and processes in plant hydraulic models. One of the main reasons why we developed *SurEau-Ecos* is that we aimed for a different balancing between plant representation and the possibility to apply the model for operative large-scale purpose compared to *SurEau* (Cochard *et al.*, 2021). Following the reviewer's comments and the other reviewer's general remarks, we added a new section in the manuscript that addresses to address several important points. Frist, we tried to reinforce our manuscript by providing more elements regarding the parametrization of *SurEau-Ecos*. There is now an entire section dedicated to parametrization in the revised version of our manuscript, including a table that summarize, for each parameter, how to determine their value and their importance in the model. Second, we took also special care to provide a more thorough evaluation of the impact of apoplasmic and symplasmic hydraulic capacitances on the model outputs. Our results showed that both the apoplasmic and symplasmic compartments had an important impact on the time to hydraulic failure and the dynamics of leaf water potentials. We added a section in the manuscript to present these new analyses and discuss the results.

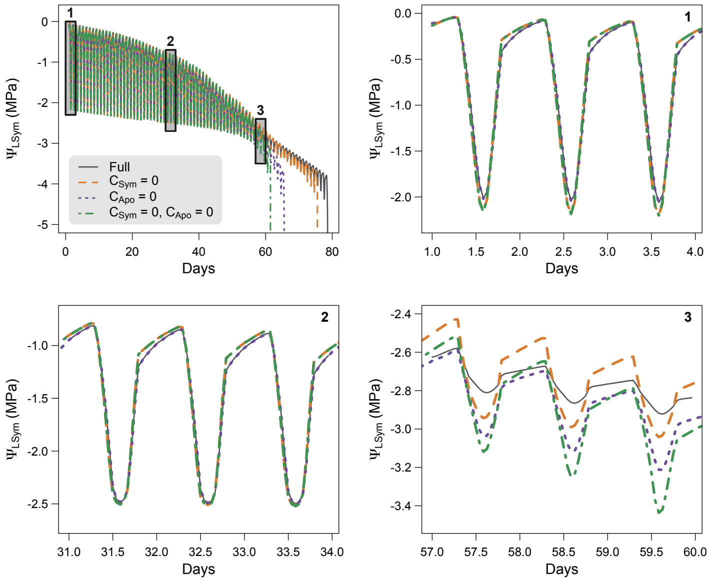
We also agree with the reviewer that the lack of competition and succession can really limit the utility of the model, especially for applications that require longer time scales. The processes related to photosynthesis, respiration, growth and carbon allocation that are necessary to account for legacy effects of drought or acclimation have been overlooked there. However, we are confident that *SurEau-Ecos* could provide a comprehensive hydraulic basis for larger scale land surface, ecosystem or community models Current projects of the group aim at integrating SurEau-Ecos with the forest growth models CASTANEA (Dufrêne et al., 2005) and GO+ (Moreaux et al. 2020) and the gap model ForCEEPS (Morin et al., 2021) under the Capsis platform (Dufour-Kowalski et al., 2012). Thus, future researches and development should focus on how to link carbon and growth metabolism to hydraulic properties and how to model feedbacks between growth and hydraulic properties. We provide more details about the limitations and future developments in the new section in the manuscript.

Line 95 - Fig. 1 It is great to see energy balance of plant tissues is considered since leaf temperature can be quite a few degrees different from air temperature during drought. I was wondering whether leaf temperature dynamics have been evaluated? My experience with ED2-hydro is that it tends to overestimate leaf temperature during middays (compared with thermal camera data), which exacerbated water stress, led to more stomatal closure, less transpiration, then even higher leaf temperature.

Thank you for this remark and this suggestion. We took indeed special care to consider the energy balance of leaf tissues as it is our belief that leaf temperature is an important driver of plant stomatal and residual transpiration that should be taken in to account. Unfortunately, we did not have the opportunity so far to evaluate whether our estimations of leaf temperature were in accordance with leaf temperature measurements.

Additional comment on Fig.1. I like the idea to separate apoplasmic and symplasmic water pools, which is more realistic in terms of physiology. However, is it necessary (or in what scenarios is it necessary), and what is the additional computational cost associated with the separation? From Line 388-395, it seems the model itself is not sensitive of apoplasmic water storage. I guess the advantage is to better assimilate plant hydraulic trait measurements while I wonder what would the biases be if ignoring these water pools.

The question as to whether the separation between apoplasmic and symplasmic plant compartments affects our simulations of plant response to drought was indeed not specifically addressed in the manuscript and we thank the reviewer for raising this point. We do not agree with the reviewer on the fact that the model is not sensitive to the apoplasmic water storage as our sensitivity analyses showed the importance of *Vs* (stem water quantity) for survival time (the time between stomatal closure and plant mortality). To further evaluate how apoplasmic and symplasmic capacitances affect the general behavior of the model, we run some simulations where either apoplasmic and/or symplasmic compartments were removed (*i.e.*, set to 0) and evaluated how it affected the dynamics of plant water potentials and the time to hydraulic failure in our reference simulations. Our results (see figure below) showed that removing the apoplasmic compartments had an important impact on the time to hydraulic failure and both the dynamics of leaf water potentials. By contrast, removing the effect of the symplasmic compartments affected the infra daily temporal dynamics of leaf water potentials compared to the reference simulations but did not affect the time to hydraulic failure. This figure and the description of the results were in added the manuscript.



Line 138, I am curious about the hydraulic redistribution part. I guess it happens when psi_soil is lower than psi_sapo? We found that enabling water out-flow from root to soil and using the same soil-root hydraulic conductance formulation can lead to too much hydraulic redistribution that is homogenizing soil water across vertical layers. Some studies suggest that soil-root conductance can be higher than root-soil conductance (Prieto et al. 2012).

Prieto, I., C. Armas, and F. I. Pugnaire. 2012. Water release through plant roots: new insights into its consequences at the plant and ecosystem level. New Phytologist 193:830–841.

We thank the reviewer for this comment. Differences between soil-root and root-soil conductances have not been implemented in SurEau-Ecos but this is clearly some way to future improvement, in accordance with the mechanisms proposed by Prieto *et al.*, (2012).

Line 188. Why three layers? Why not making it adaptive based on total soil depth?

We chose to implement ‘only’ three soil layers as we considered it to be the minimum number of soil layers required to simulate plant water dynamics in complex environments, based on the results of previous water balance models (De Cáceres *al.*, 2015, Ruffault *et al.*, 2013). The first, usually rather thin, soil layer is used to compute soil evaporation. the second soil layer usually includes the soil until bedrock is reached. the third soil layer has usually an elevated rock fragment content. Adding this third soi layer may be important in water-limited environments where evidence shows that plants can expand their roots into cracks of the bedrock to get access to more water during the summer.

Line 320, Section 2.6 For numerical schemes, have you tried Runge-Kutta? In ED2, we used the fourth order RK method for integrating various PDEs, which seems to give a good balance of accuracy and computational cost.

We thank the reviewer for this suggestion. We did not try higher order schemes, as we found that the accuracy was limited by the temporal resolution of sources and sinks (in particular the fast changing of stomatal conductance with light imposes relatively small-time steps) rather than the numerical accuracy. For those time steps, we already reached the convergence with the low order scheme. However, we agree that with slower variations of these terms, a higher order scheme (as the Runge-Kutta) would surely have allowed to reach accuracy with larger time steps.

Line 520 Fig. 4. I am curious why osmotic potential plays such a minor role in all these metrics. Is it only used to convert RWC and Q? osmotic potential can have large inter- and intra- species variations (even large diurnal changes) that can change leaf turgor loss point, which is tightly associated with psi_gs_50. From this figure, it seems psi_gs50 and pi_0 are decoupled?

That is true. In *SurEau-Ecos*, stomatal closure is determined by a regulation factor (γ). Several options are implemented in *SurEau-Ecos* to determine γ. In the version of the model presented in this manuscript, γ is determined according to and sigmoid function depending on the potential at 50 % of stomatal closure ($\psi_{gs,50}$) and a shape parameter ($slope_{gs}$) such as (see equation 34 in main text):

$$\gamma = 1 - \frac{1}{1 + e^{\frac{slope}{25}(\psi_{LSym} - \psi_{gs50})}}$$

This means that, with these settings, π_0 affects the model's response to drought only through its effect on the symplasmic leaf capacitances, which play a less important role on plant water dynamics than stomatal regulation.

However, several other alternative options to determine γ are currently under development *in SurEau-Ecos*, including one where γ is a direct function of π_{TLP} (turgor regulation) as in Martin-StPaul *et al.*, (2017).

In addition, it is interesting to see that cuticular conductance is very important to determine survival as well. I also found the strong influence of cuticular gs on plant hydrodynamics in ED2-hydro. Are there good data sets to constrain the variations in the parameter? In general, it can be rather useful to point out which parameters can be readily acquired/measured.

We included a new section and a table in the manuscript to thoroughly describe how to parametrize the model

Line 540, typo, "the leaf and leaf ", should be "the leaf and stem"

Corrected

Line 595. Fig.5, the Quercus ilex result is very hard to interpret with little explanations in the text. Could it because the lack of competition in the model?

Yes, we thank the reviewer for this comment which was also raised by the other reviewer. We added a sentence in the text to explain that while the risk of hydraulic failure was close to 0 in the temperate part of the country, where summer drought is less intense, *Quercus ilex* was not observed surely because of other mechanisms that are not simulated by *Sureau-Ecos*, such as competition by more productive species, cold resistance or forest management.

Line 615-620, treating LAImax as a model parameter indicates the model only considers mature forest that has reached LAImax. This might be fine for qualitative assessment of mortality risk. However, shouldn't forests reach a new equilibrium with lower LAImax under drier conditions? (i.e. LAImax should change over time) For example, in Fig. 5, how would the mortality risk change if the forests are thinner with lower LAImax?

That is true. As discussed above, *SurEau-Ecos* do not simulate vegetation carbon fluxes and dynamics so we did not investigate such hypothesis. Couplings with forest growth models that are currently under developments will allow to explore the impact of forest dynamics on the risk of mortality, including LAI adjustments.

Tab. B1, symplasm pi_0 for leaf should be -2.1

Corrected

Fig. B2-B3. Given the computational cost vary so much with longer time step, I wonder how much the difference matters at the regional scale between 1min and 10min... How worrisome we should be if models take a semi-implicit scheme with a somewhat long time-step

The reviewer is right. Depending on the applications, different time steps were implemented in the code with the assumption that, depending on the application, the balance between model accuracy and computing time may not be similar.

References

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