Review of "*FORHYCS v1.0: A spatially distributed model combining hydrology and forest dynamics*" by Speich et al.

The study of Speich et al. couples the hydrological model PREVAH with the forest model TreeMig, which is together presented as the new model FORHYCS. The study area is the Navizence catchment in Switzerland and several modelling scenarios are applied here. The modelling scenarios consist of one group of scenarios that models the full succession, and one that models under climate change. Additionally, the model is run in uncoupled and coupled mode with different settings switched on and off. The authors conclude that including drought-induced height limitations for vegetation improves model performance, but the effects on streamflow by coupling the models was limited. Coupling the model also led to reduced sensitivity of streamflow to increased temperatures and precipitation in the climate change scenarios.

The authors present a thorough analysis in a generally well-written manuscript. The work seems solid, and all the different model scenarios seemed a large effort to me. However, I do have several comments that the authors may need to improve on.

Major Comments

My first comment is more a question out of curiosity, as the overestimation of LAI by the model intrigues me. It will strongly depend on the formulation of leaf area dynamics in Eq. 1, so how confident are you that this equation (or more the parameterization of this equation) is correct? How many trees were for example used to derive the allometric relations? Besides, the species specific parameters (like SLA, and a_1,a_2) are not reported, also not in the Supplement, so can you add these? So in general, could your leaf area formulation be the reason for the observed over-estimation?

I am also a bit confused by equation 3. The fractional cover in LSM's or remote sensing products is often related related to LAI by the Lambert-Beer relation: FC = 1 – exp(-K * LAI), where LAI is the total leaf area index (or crown index), FC is fractional cover, K is an extinction coefficient, (e.g. Bréda, 2003; Choudhury, 1987; Monsi, 2004). The extinction coefficient is a function of leaf inclination and often set to 0.5. Here, this seems to be set to 1 for all species, which seems a bit high, is that correct? In addition, why are the exponents summed? Shouldn't you just add up the different final fractional covers of the species when the area stays the same? This is also what you describe on page 17 (if I am not mistaken), where you take the cumulative sums of the classes.

In addition, LAI and fractional cover are compared by two newly developed error measures, which only compare on one specific moment in time. However, getting the seasonality right in these models is quite important, and one of the minimum things the model should be able to represent is the seasonal signal. Did you compare the timeseries of simulated and observed LAI? It's rather simple to do, and, in my view, provides much more information then the error measures as presented by the authors. So how well is the seasonality captured by the model?

I am also a bit confused on how the effect of elevated CO_2 is studied. How can you evaluate the effect of elevated CO_2 if you switch off the stomatal response to high CO_2 (P22.L3)? Do you mean you keep the conductance the same? It would be much more interesting to keep the feedbacks in place, so why you do this? However, later on in the manuscript, the stomatal conductance is discussed, so can you clarify what you do exactly?

I am not too familiar with PREVAH, unfortunately, but the authors state that the model structure is similar to HBV. That would mean there are also several parameters that do not relate to vegetation (such as recession parameters, routing, snow parameters), so how are these determined? It can also be seen in Figure 5 that snow melt and recessions are quite off compared to the observations, which is probably just due to the remaining parameters. It may also affect the conclusions based on the climate change scenarios, as the snow melt is highly affected by the temperature changes.

My last, but most important comment is on several key-findings which do not seem to be (entirely) supported by data. For example, one of the key findings presented in the manuscript concerns the effect of the climate change scenarios on streamflow. However, the result of only one specific catchment is shown in Figure 10, how do the results for the other catchments look like? Similarly, an analysis on elevated CO_2-levels is described, but no results are shown in any of the graphs. Please add some graphs and evidence to support the statements you make here.

Generally, I enjoyed reading the manuscript, but some revisions may be necessary. I hope the authors find my comments useful, and I look forward to an improved version of the manuscript.

Minor comments

I would like to suggest to change names of the modelling scenarios into more meaningful names, or add clarifications in the text when discussing a certain scenario. Names like Succ_TM_BEK, or T6_P-10, are not very informative and make it hard to understand what happens without looking at the table all the time.

P8.L8-9. In this way, the equation does not seem consistent in units. What is the unit of $P_{d,sp}$?
P8.L10. This seems a rather arbitrary number to me, why 833 m^2?
P8.L18-20. How does crown area relate to leaf area?
P9.L25-30. So the used transpiration values are model outputs, correct?
P10.L9. Is f_{DS} not a single yearly value, as DI is a single year value too? What do you use to calculate the geometric mean in that case?
P10.L18. “i.e. with a decrease...is at kDT”, I am not sure I follow, can you please clarify?
P11.L1. he –> the
P11.L26. Why did you use these numbers? Seems a bit arbitrary.
P13. So PP_o includes the carbon costs? What are these values based on?
P16.L1-5. If a large amount is diverted by pipelines, can you compare modelled and observed discharge? Which sub-catchments are affected by this?
P16.L27. “As the sampling plots… a larger area.” This sentence is a bit unclear to me, what do you mean?
P17.L19. Please correct reference.
P22.L31 The plot only shows Acer spp., so how can I see this?
P24.L21. This sounds a bit counter-intuitive, wouldn’t you expect a higher biomass when there is no LAI-reduction? What is the reason for this?
P29.L10-11. Is this not counter-intuitive too? You would expect (also because of equations 7 and 8, where additional carbon is allocated for roots under stress) that the roots will go deeper in case of drier scenarios, and that LAI would go down, correct?
P31.L4-5. You described earlier that Eq. 12 was set to 1, correct?
P32.L4-14. How different are the landuses eventually at the end of the runs? Are the differences mainly due to different forest covers under the different scenarios?

P33.L14. Based on the data as shown, you cannot claim that the sensitivity of streamflow to vegetation properties varies spatially. This would also mean you need to have the same vegetation in different places and observed different changes in streamflow, but I believe that is not the case.
P37.L7-8. “the greatest effects occurred at low elevations, and in regions currently above the treeline”, where do you show this? Please back this up with some evidence in the main manuscript, especially when it is a key-finding.

Eq3. Please define all variables and subscripts
Table1. There two Succ_noHmax-scenarios, please correct.
Table 2. Please describe the abbreviations in the caption or replace them with a description.
Fig1a. Please define SFC also in the figure.
Fig5. There are a couple of things that seem a bit odd to me in this plot. In Fig 5a, Prevah seems to be much closer to the observations then the other two model set-ups, but in Table 2 the KGE-values are lower. Is that correct? In addition, Forhycs00 and Forhycs11 are on top of each other in Figure 5a, whereas Figure 5b suggests a difference of up to 0.3 m3/s.
Code availability: I would suggest to share your code on github or gitlab, instead of the supplement. Please also add links to the actual datasets used in the study.
Appendix A: Why is there an appendix in the main manuscript and also a supplement? Should Table A1 not just be part of the Supplement then?

References

Bréda, N.J.J., 2003. Ground based measurements of leaf area index: a review of methods, instruments and current controversies. J Exp Bot 54, 2403–2417. https://doi.org/10.1093/jxb/erg263

Choudhury, B.J., 1987. Relationships between vegetation indices, radiation absorption, and net photosynthesis evaluated by a sensitivity analysis. Remote Sensing of Environment 22, 209–233. https://doi.org/10.1016/0034-4257(87)90059-9

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