Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
https://doi.org/10.5194/hess-2021-265-RC1, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on hess-2021-265

Anonymous Referee #1

Referee comment on "Does maximization of net carbon profit enable the prediction of vegetation behaviour in savanna sites along a precipitation gradient?" by Remko C. Nijzink et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-265-RC1, 2021

The study by Nijzink et al examined a vegetation optimality model is predicting water and carbon fluxes as well as vegetation properties across five Australian savanna sites along a precipitation gradient. In general, I like the motivation of the study. I am a big fan of optimality theories and I also agree with the authors that the optimality models are promising tools (and may be better tools than diagnostic models) to predicting and understanding the interactions between biosphere and climate changes. The current study did a comprehensive evaluation of one optimality model that is based on the maximization of net carbon profit across environmental gradient, which is an important step towards better understanding of the optimality theory itself and developing related optimality models. In this light, I am supportive of publication of this study.

However, into details, I am not satisfied with the manuscript organization and the writing itself. Overall, I found there are many information currently included in the manuscript is unnecessary. The introduction is too long and contains many individual studies, which should be largely shorten with more highly-summarized findings/conclusion from existing individual studies. The detailed site description is also not needed, simply summarize the five sites with their specific properties listed in Table1. Table 2 is suggested to move to supplementary.

On to content, I think the part of dealing with water transport cost parameter is more or less deviates from the main line. I would suggest remove the second hypothesis but describe how this parameter was chosen (either prescribed following previous studies or locally parameterized) in the manuscript. Then, the overall structure of the manuscript become: 1) test the VOM using site observations and compare it with TBMs; 2) what happened if remotely sensed vegetation cover was used? 3) what happened if prescribed rooting depth was used. Followed by discussion. I understand that the water transport cost parameter is also related to the overall performance of the model, but if that is included, why not other model parameters? And also you will need to describe you model in detail to allow readers who do not familiar with the model understand the role of this parameter in the model.

Another question is why not include a scenario that consider both prescribed vegetation cover and rooting depth, in comparison to the scenario with both vegetation properties optimized.

Other comments:

Line 215ï¼□infiltrate -> infiltration

Line 216: Why 30m? Is this the defined soil depth in the model? Not sure if the choice of this depth impacts the modelling results.

Line 225-230. This may present a source of uncertainty, as the observed fluxes are directly linked to the observed meteorological forcing at the sites, whereas the SILO data was used here to inform the model. Suggest to at least evaluate the used SILO data at each site against site-observed meteorological variables during their overlapping periods.

Line 223 Is there any published paper supporting this? Otherwise, simply states this information is measured at each site.

Line 260-263 and the following sections. If I understood correctly, the last two hypotheses are related to replacing vegetation properties with prescribed values and the second hypothesis is about water transport cost parameter. Please check.

Line 246 and throughout: evapotranspiration is often written without a hyphen.

Line 316-317: Where is the evidence for this? Figure 3. Please indicate where needed. In addition, in figure 2 at Howard Springs (but not for all other sites), there is a light green curve indicating the results of Schymanski 2015. What is the difference is model configuration between Schymanski 2015 and this study? And what is this for? It is not introduced.

Line 400. This is an overstatement. Looking at Figure 2, the VOM considerably overestimates GPP from observation and even compared with other TBMs.

Line 500. I do not agree with this hypothesis/statement. This may simply caused by the fact that the adopted VOM was not able to reproduce the actual rooting depth using the embedded optimality principles. Many previous studies have already demonstrated the importance of accurately representing rooting depth in the hydrological model to improve the modelled fluxes, for example, those by Kleidon and Heimann (1998) and more recently by Wang et al. (2016) and Yang et al. (2016).

Kleidon, A., and M. Heimann (1998), A method of determining rooting depth from a terrestrial biosphere model and its impacts on the global water and carbon cycle, Global Change Biol., 4(3), 275–286.

Wang-Erlandsson, L., W. G. M. Bastiaanssen, H. Gao, J. J€agermeyr, G. B. Senay, A. I. J. M. van Dijk, J. P. Guerschman, P. W. Keys, L. J. Gordon, and H. H. G. Savenije (2016), Global root zone storage capacity from satellite-based evaporation, Hydrol. Earth Syst. Sci., 20(4), 1459–1481.

Yang, Y., R. J. Donohue, T. R McVicar (2016), Global estimation of effective plant rooting depth: Implications for hydrological modeling. Water Resources Research, 52, 8260-8276.