Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-723-RC2, 2018

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Testing an optimality-based model of rooting zone water storage capacity in temperate forests" *by* Matthias J. R. Speich et al.

Anonymous Referee #2

Received and published: 18 January 2018

GENERAL COMMENTS

The reviewed manuscript makes an effort to evaluate an optimality model for zone storage capacity in temperate forests by the use of a hydrological model and eddy covariance data across Europe. Research in root zone storage capacity is important for both improving modelling, and for understanding basic ecological processes. The manuscript is therefore subject-wise appropriate for publication in HESS. However, the research question lacks clear focus and conclusions are made about topics not listed among the research goals, which makes it difficult to comprehend the rationale for the research design as well as the scope of the study. The precise contribution of this

study is not clear, lost somewhere between validity of the FORHYTM itself for application in different regions and the comparison of different parameterisation methods of S_r. There is a lack of overview and cross referencing, but an overflow of details in the main manuscript body. I found the document difficult to navigate. Perhaps most worrying, it seems that the authors have not properly isolated the effect of S_r on modelling results and are making speculative claims unsupported by the analyses performed as detailed below. I agree with Prof. Savenije's comment that it would be insightful to provide analyses and explanation of the presumably contrasting mechanisms of root zone storage evolution in boreal and Mediterranean ecosystems. Other general issues include:

- Given the research question stated and some of the conclusions made, I would have expected some more straightforward comparison of evaporation simulation results for the three S_r values: calibrated, Guswa 2008, and Guswa 2010. Please consider providing such figures from such analyses.
- From the Supplementary figures (and the in general relatively large standard deviations shown in Table 5), it appears that a wide range of S_r values can generate high KGE values for most sites, and the same S_r values can also generate vastly different KGE values. This makes me wonder (1) at which sites is S_r of importance for modelling results, and (2) how sensitive the KGE values is to the other calibration parameters listed in Table 4, which the authors also point out on e.g., P24L22. Please consider presenting individual model parameter sensitivity results in a revised manuscript.
- In terms of presentation of methods and results, the authors could do much more to facilitate for the reader. E.g., sites are listed by names or abbreviations, but given that readers will not be taking the effort to memorise the location, vegetation type, and climate of each site, it is very cumbersome for the readers to interpret the results in e.g., Table 5. Please consider adding colour coding or

other visualisation techniques to provide such relevant information.

- Please make relevant cross references when interesting results are discussed. E.g., P25L16, it would have been useful for the reader to be able to cross-check with a figure for exactly how much FORHYTM failed to represent the local water balance under Mediterranean climate.
- Difficulties navigating the different model runs and datasets. Please consider providing an overview table listing the different experiments, and add cross references where appropriate.
- Please discuss the uncertainties in eddy covariance data.

SPECIFIC COMMENTS

Abstract: "the concept of a single rooting zone storage capacity was appropriate at most temperate and cold sites" This conclusion seems too strong/general. Can e.g., parametrisation, data uncertainty, or model structures not be the reason given the research design and the scope of the performed analyses?

Abstract: "mismatched were attributed to...**[]**...**oxygen stress and low soil temperature".** It is not clear to me how the attribution was made. Please consider providing searchable key words that make it easier to locate the related analyses. (I searched for "oxygen" and "attrib" without finding any related analyses).

Abstract: "Nevertheless, the overall good agreement suggests that this model may be useful for generating estimates of rooting zone storage capacity for both hydrological and ecological applications. Another potential use is the dynamic parameterization of the rooting zone in process-based models, which greatly increases the reliability of transient climate-impact assessment studies." These are not key conclusions from the study, and rather speculative. I would suggest removing these statements. Introduction: Please clearly state the research questions and

scope. I find the research goal statement too vague at the moment.

Methods: Please consider presenting part of the Methods description in Appendix and focus on explaining key features and rationale of the model, with cross-references to Appendix. Notations: Due to the large number of symbols, please consider providing a table in an Appendix section that list all notations.

P3L4-5: "Yang et al. (2016) identified the approach proposed by Guswa (2008) as the most meaningful from a hydrological and ecological point of view." This sentence suggests that Yang et al (2016) made a comparison between all aforementioned approaches, which was not the case. The word meaningful is also vague – do you for example mean that this approach yields best performance in both hydrological and ecological modelling or that their approach captures the most major hydrological and ecological drivers of Z_e?

P12-Table3: "LAI". Do you mean "maximum LAI"? Where is it described how LAI is varied?

P15-Eq20: "otherwise". Please consider replacing with formal mathematical expression (VPD=>lvpd?).

P18-Fig5 caption: "There is a relatively narrow range of S_r leading to Pareto-optimal scores". The black S_r dots appear to range between approx. 50 and 280 mm. I would be hesitant to refer to this as a narrow range.

P18-Fig5 caption: "conducted using the optimal parameter sets" Please be specific and add cross reference. It is not entirely clear which optimal parameter set is considered. The suggested overview table (see General comments) of simulation settings/parameter combinations would be helpful to cross refer to.

P18 Fig 5 (and SI figures): Please consider changing the line color and style. At first sight, one might think that the black colors share some common point, which is not the case.

P21-Fig 7: Possibly consider collapsing the two subplot columns G08 and G10 into one column, and use colour coding or other visual cues for identifying the model approach used.

P23: The cross reference to Fig 4 seems to be wrong.

P25: "The results of this study suggest that G10 better captures the behavior of forests under energy-limited conditions". Please consider to add a cross reference. I have difficulties understanding how the analyses and results support this statement.

P25L17: "suggesting that the use of a bulk S_r is inappropriate at these locations". I struggle to understand how this claim is supported by the performed analyses. In my view, to be able to make such as claim would require a comparison between a model structure with bulk S_r and a model structure without bulk S_r (e.g., some other structure hypothesised for Mediterranean conditions), and this comparison would need to show that the model structure without bulk S_r performs better than the other one. It seems to me that current analyses only suggest that FORHYTM as a whole does not appear appropriate for modelling evaporation in Mediterranean conditions.

SI: Please provide figure numbers and figure captions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-723, 2017.