

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/hess-2021-204-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on hess-2021-204

Anonymous Referee #2

Referee comment on "Ecosystem adaptation to climate change: the sensitivity of hydrological predictions to time-dynamic model parameters" by Laurène J. E. Bouaziz et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-204-RC2>, 2021

This manuscript evaluates how predicted changes in climate, e.g. aridity and seasonality, reflect on catchment hydrology in an exemplary basin using a hydrological model. The novelty lies in the accounting for the necessary adaptation in the vegetation root zone storage (essentially rooting depth) to actually satisfy predicted changes in actual evapotranspiration. For this, the authors first establish the expected rooting depth required to satisfy evapotranspiration due to climatic shifts of precipitation, evapotranspiration and their timing. Next they use those in a hydrological model to show that vegetation root adaptation and to a lesser extent als land use changes have a discernible effect on predicted catchment water balance. The authors conclude that this study serves as a proof of concept that adaptive vegetation has to be considered when evaluating climate change effects on hydrology. I agree with this conclusion and believe (although I have some questions) that the methodology is suitable to make this statement. I think this is a valuable contribution and of interest to the readership of HESS. The manuscript is formulated grammatically well. Having said this, it does not read well, for reasons stated below and requires revision. I fact, I really had to fight my way through the methods section. I alos have some serious concerns on lack of information and general organization of the manuscript. I recommend major revisions.

Major comments

I have some concerns about missing information or implications of some assumptions that prevents me from fully evaluating the results.

- I find it difficult to understand how the evapotranspiration was estimated for the model, and this needs to be laid out more clearly. For the rooting depth estimation ET from the root zone was derived from applying the observed ω to the predicted potential

ET. But what was used for forcing the hydrological model? Potential ET from the climate prediction? What happens in the hydrological model, when the root zone storage runs dry? I read in the discussion that water limitation reflects on ET, but there is no mention how?

- Fig 3 is very repetitive, while the essential difference between the hillslope, plateau and wetlands is difficult to spot: It is whether or not the model allows for ground water exchange. Now, since the vegetation types are attributed to either hillslope (broadleaved forest) and plateau (conifers, agriculture) this small detail becomes important (and should be spelled out). How is this accounted for when the vegetation is swapped? Are also the HRUs swapped, e.g. does the area capable of ground water recharge increase / decrease as a result of the swap? In other words, does the model structure change as a result of the swap?
- As a follow up on that, I was left unclear as to whether all 2K scenarios see the same climate forcing? Does the change in ω only apply to the rooting depth parameter or also to the evapotranspiration forcing? Please spell this out.

I would appreciate an extension of the discussion to critically review the results.

- The discussion already has a section called „limitations“, which is good. But it should include some more discussion on the assumptions above.
- Correlations between parameters / vegetation and the environment are neglected in this study. For example, could the differences in ω between catchments in France and the Belgium partly be related to differences in geology, topography etc. besides forest cover? Can you safely assume that the calibrated catchment parameters obtained for a specific vegetation distribution are still valid when changing the vegetation? I agree with the general statement that this a modeling study to provide a proof of concept, but would be good to include this in the discussion.
- The manuscript starts with hypotheses which is nice and suitable for this study. It would be good to come back to them specifically in an interpretation section of the discussion.

General editorial

The manuscript reads technical at many levels, and this seriously prevents communication to the point where important information seems to be missing. For example,

- The introduction of the simulated climate in section 3.2. gives information about the origin of the time series, but leaves out which variables were actually used in the study. Specifically, the reader is left to guess whether it is potential ET or actual ET ?
- Similarly, the structure of the hydrological model is shown in Fig 3, and given in a very short section 4.2. The model description does not include a reference to how root zone storage affects actual evapotranspiration. In this study on rooting depth and effects on the water cycle this is a central point and should not be left out. It is only mentioned (I believe once) in the discussion.
- I am assuming that two parameters for the root zone storage capacity are used in each model run, one for shallow (agriculture and coniferous forest) and one for deeper rooted (broadleaf forests) vegetation. I am not sure whether I overlooked this, but it would be good to spell this out in the section where the model or the calibration are introduced.

The manuscript requires revision for accessibility.

- There are plenty of abbreviations that are barely introduced, sometimes the introduction appears even in a subheading.
- The order within the methods section prevents understanding the methods. For example, there are many references to the model runs, before the model structure is even introduced. Therefore it is really difficult to digest the information or interpret what the assumptions mean for the model. etc.
- Currently the headings and subheadings are not suitable for a reader navigating the text. Consider that they should help finding information when the reader does not dive into the main text completely. For example take section „4.1.2 Seasonal water balance for estimating the change in root zone storage capacity $S_{R,max}$ “, would be more easily called „4.1.2 Estimation of root zone storage capacity“. I could make such propositions for almost every heading. Please revise.
- It is difficult to interpret the results without a table showing an overview of the climate of the different scenarios, e.g. precipitation, E_{pot} , aridity, seasonality, if applicable actual evapotranspiration used as forcing, actual evapotranspiration as model output.
- I believe the manuscript can be shortened and the important information be fleshed out to improve it being understood.

Detailed comments

Abstract

L 8-10: Needs to become obvious that these are modeling hypotheses. Please reformulate

L 14-15: At this point in the manuscript it is difficult to understand why those particular changes are considered. Maybe formulate more general

L 17-18 Are these numbers consistent with the water balance? They do not look like they do ..

Introduction

L 25-27: There should be more appropriate references for this very general comment.

L 42: „stationarity is dead“ - use citation marks, otherwise it seems a bit awkward language, as strictly speaking stationarity never lived.

L 55: „require ...“ I do not agree. In a distributed model it could also s just be represented by distribution of land cover. This does not require a priori knowledge of the relation to catchment outflow.

L 56 „uncertainty in ..“ this statement is very vague. Can you be more explicit?

L 72-25: As it stands, this appears quite unrelated. Either erase or put into context.

L 76: „match expectations of the Budyko curve“ - Unclear, please be more specific: Which expectations?

L 77-78: „Vegetation tends to efficiently adapt its root-zone storage capacity to satisfy canopy water demand.“ - reference needed, ideally with an observation component.

L 78-79: I believe Yang et al., 2016 wold be good to cite here

L 95-98: Very difficult to grasp. I am not sure whether this paragraph really helps to understand what is coming.

L 99-100: Any reasons for this hypothesis? Also, would be good to come back to it specifically in the discussion.

102-105: Again, not sure this really helps. It is too detailed to soon.

Study area

L 119-120: Reference missing

L 131: Reference on the biodiversity statement required.

Data

L 138: „E-Obs“ Add definition also in the text, not only in subtitle

L 147: Same as above, please introduce abbreviations in the text before using them. Also, with 2 K you probably refer to 2 Kelvin. Please spell this out as well.

L 150: Spell out RACMO2 and HTESSEL ?

L 168-180: Generally, it is a good idea to explain what is coming, but I really did not get it. Maybe try rewording in plainer language and less specific?

L 193-195: Here I was entirely confused. Is ω_{change} derived from the climate data using E_A from there? This part is very opaque, but really critical to understanding the methods.

L203-206: Can you be sure that the runoff coefficients only depend on the forest cover and not on the geology? It seems that the regions with high / low cover are geographically distinct. How to avoid misinterpretation?

L 208 „we expect ...“ First off, I appreciate the formulation of hypotheses. I would only state them at the end of the introduction however. Also, where hypotheses are formulated, it can be highly confusing to leave ambiguity between transpiration, bare soil evaporation, or evapotranspiration for the two combined. Please specify. Finally, where does the hypothesis come from? Please add references.

L 222 „match expectations .. “: Unclear formulation, please be more specific on what type of expectation.

L 279 What is meant with „imposing“? I do not understand what is done here.

Table 1

I am confused about the last Last column: Over what sample is the max and min taken? Why do those max and min not appear on the first two lines?

L 298: See my previous notes on abbreviations. Better would be „Hydrological model: xxxx“

L 303-304: The values appear arbitrary, and maybe are explained in the references. I suggest adding an explanation of their origin, so that the reader can understand the general idea without need to refer elsewhere.

L 306: Am I understanding correctly that hillslope vs plateau was derived from a vegetation map? If yes, please spell this out more clearly, it is very opaque from the current description. Also, in other words, the main difference between hillslope and plateau, which is the consideration of deep drainage, depends on the vegetation as well, with agricultural areas allowing for deep drainage and forested areas (per definition of the locations) does not? How does this affect the model results? This needs to enter the discussion.

L 307 - 311: Important information is missing. Important information would be, what happens, when the root zone storage runs dry? Does this affect ET at all? What happens when ET cannot be satisfied?

Given the general topic of the paper, it needs to be clearly explained how vegetation affects hydrology in this model, especially E_A. At this point I am assuming that E_A is imposed either from observations or regional climate model and further modified to accommodate the different runoff coefficients that are taken to represent the vegetation cover? Later (in the discussion) I am learning that water availability actually affects E_A and I am back at point zero. This section really needs attention.

L 324-326: We learn elsewhere that this corresponds to plateau corresponds to agriculture and hill slope to forest. Please repeat this here. This is an important part of the study.

L 339-340: In other words, potential interactions between the model parameters are neglected? Was this tested?

L 358: Here and in D: Does the forcing for ET change as a result of the land use change?

Results

L 378-379: Sounds like interpretation and this should go to discussion.

L 389-399: Maybe merge the sections on root zone storage across scenarios A-D?

L 395-396: Would be good to have an overview table with the climate conditions (aridity,

seasonality, P, E_pot) for all scenarios, including separate listing of E_pot and E_A for the scenarios.

L 445-455: Was very confused about how the E_A was obtained. It is a model output or forcing?

L 467-468: „result of soil moisture stress in the root-zone“ This is not mentioned in the model description and is absolutely a must. Also, please report on times of soil water stress in the model scenarios.

Discussion

I find it more logical that the limitations are stated first, followed by interoperation and implications last.

L 501-503: This sentence can be erased without losing information.

L 547-548: Also rooting depth is species specific, and mono-cultures would have limited capacity to adapt.

Figure 2

The arrow with script $\Delta \omega$ is misleading. What is shown is $\Delta (E_A / P)$, which is really not the same.

Figure 3

See above major comments. The important difference is in how the interaction with groundwater is accounted for in the different slope positions, which are at the same time directly linked to vegetation cover. This is an important detail and should be made obvious. In contrast, the remainder of the Figure is not very important and could in my opinion go to the appendix.

Reference

Yang, Y., Donohue, R. J., and McVicar, T. R. (2016). Global estimation of effective plant rooting depth: Implications for hydrological modeling. *Water Resour. Res.* 52, 8260–8276. doi:10.1002/2016WR019392.