Review of the manuscript “The importance of ecosystem adaptation on hydrological model predictions in response to climate change”, ID hess-2021-204

General comments

As my background lies in ecological and ecohydrological modelling, my ability to judge the novelty of this study in the field of hydrological modelling is limited. I rather take the perspective of the interested ecologist, who appreciates this important step to integrate vegetation in a more realistic way into a hydrological model.

Short summary

In this proof-of-concept study, the authors propose a top-down approach to include potential land-use and vegetation changes under climate change in process-based hydrological models to make predictions of future hydrological conditions more accurate.
In a multistep process, the root-zone storage capacity is calculated for different climate and land-use scenarios using observed climate, streamflow and land cover data and simulated historical and 2K climate change data from the Meuse Basin.
The model is calibrated and validated for historical climate and land-use data and run with 4 different climate change scenarios (all 2K warming): a stationary system (no change in root-zone storage capacity and land-use), a system with no land-use change but an adaptation of the root-zone storage capacity to changed climate and, a system with adaptation of the root-zone storage capacity to changed climate combined with land-use change from broadleaved forest to agriculture/coniferous forest and vice versa.
The results show that root-zone storage capacity parameter increased with climate change whereas it differs only slightly between the different land-use scenarios.
The catchment hydrological response of the stationary root-zone storage scenario differed from the scenarios with adapted root-zone storage capacity. The non-stationary systems showed higher evaporation, lower runoff coefficients and streamflow. Land-use change did not influence the results to the same extent as adaption of the root-zone storage capacity.
In the discussion, the authors emphasize the impact of vegetation (root-zone storage capacity) on the water cycle and propose their approach as a way to move past stationary hydrological systems when modelling climate change impact on hydrology with process-based hydrological models.

Scope and conclusions

Overall, the manuscript is well written and structured and the method provides an interesting solution to adapt the hydrological system to climate change by using available data on a catchment. The stepwise approach allows to systematically assess the effects of land-use and climate change separately and combined.

The manuscript addresses relevant scientific questions of the impact of land-use and climate change on ecosystem properties with relevance for hydrological models. This is an important question and will contribute to an improved prediction of climate change impacts on hydrological processes in the future. The questions addressed in the manuscript lie within the scope of the HESS journal that publishes research on the role of biological processes on continental water cycles, temporal characteristics of water resources and the impact of human activity on the water cycle.

Main concerns

Study objectives are not clearly and consistently stated

According to the abstract, introduction and conclusion, the study has two main objectives. The first is to propose a top-down approach to include vegetation change into hydrological models via the root-zone storage capacity (l. 4-5, 575, 581-583). The second is the quantification of the sensitivity of modelled hydrology to changes in root-zone storage capacity under climate change and related to that, the testing of the hypothesis that changes will be more pronounced when considering an adapted root-zone storage (l. 93 ff).

Although these two objectives are clearly connected, they are never stated together. The first objective (proof-of-concept and methodological aspect) of the study is stressed in the discussion and conclusion, whereas the introduction highlights only the second objective (application and sensitivity analysis). The objectives of the study should be more clearly stated in the introduction and the discussion and conclusion should build on these objectives.

Discussion and conclusions leave open questions

The discussion could be more thorough and consistent regarding both, the modelling results and the methodological approach.

The discussion is structured into two separate parts: *Implications* (l. 500- 539) and *Limitations and knowledge gaps* (541-577).

However, two paragraphs from the first section (Implications) are better suited for the second section (limitations): l. 512-519 on possible further exploration of the space-for-time concept and l. 535-539 on the limitations of the simulated climate time series used in the study.

Also, given that one major objective of the study is to propose an approach to include vegetation change into hydrological models, I feel that the model results are not thoroughly discussed as to whether the proof-of concept of the method was successful.

The following questions/issues remain unaddressed:

- The approach showed that the root-zone storage capacity parameter has a potentially large effect on future water flows. How realistic are the values for root-zone storage capacity that were calculated for the different scenarios? Is there any evidence from literature regarding the extent to which plants adapt their root system to changing climate? Does this adaptation depend on vegetation type (e.g. crop/grass vs. tree) or species?
- Are the results regarding the water flow under future conditions realistic? Is this what could be expected under climate change?
- In which situations can this method be applied? Which hydrological models? Which ecosystems?
- What are the limitations and chances of this approach?

Methods: no limitation of the root-zone storage capacity

The methodological approach assumes a limitless adaptation of the root-zone storage capacity to changing aridity index (compare l. 243). I was wondering whether this is realistic. The adaptability of the root-zone depends on the vegetation's capability to change the root system following a change in climate/water demand. This capability probably depends on the vegetation type (crop, grass, or tree) but also on the species. Also, adapting the root-zone storage capacity is not the only way that plants/vegetation might adapt to a change in aridity index. Plants can adapt to drier conditions by closing their stomata and reducing gas exchange with the atmosphere and hence transpiration. Also, overall vegetation cover could decrease if the water supply is not sufficient to support the same cover. Although I think it is not necessary to consider this limitation in this proof-of concept study, it is nevertheless an important point to discuss in the discussion section.

Links to ecohydrological modelling or dynamic global vegetation models (DGVMs) missing
Although this study is about hydrological modelling, I think that the advances and contributions of ecohydrological models and DGVMs to studying the feedbacks between vegetation and the water cycle should be mentioned and discussed in the introduction and, if applicable, also in the discussion section of the manuscript. Please find some hints on where to start in the following:

One prominent model is e.g. the DGVM LPJmL which dynamically models carbon, nitrogen and water flows. The model has been applied to various question among them also questions related to water flows under climate and land-use change.

You can e.g. have a look at the following publication:
Rost et al. (2008), *Water Resources Research*. https://doi.org/10.1029/2007WR006331

Here you can find a list of some key publications of the model:

https://www.pik-potsdam.de/en/institute/departments/activities/biosphere-water-modelling/lpjml/key-publications
In the field of ecohydrological modelling, you could have a look at the works of Ignacio Rodriguez-Iturbe and Amilcare Porporato. An ecohydrological study to look at might be Tietjen et al. (2017), *Global Change Biology* https://doi.org/10.1111/gcb.13598. The study looks at feedbacks between soil water availability, vegetation change and climate change and they disentangle the effects of climate change alone and climate change in combination with vegetation change.

Specific comments

Introduction

l. 58: optimality principles: is this an established term? If not specify what is optimized in this approach (probably it's vegetation growth or something similar)

l. 95: "land-use change under future conditions": The manuscript does not tackle land-use under future conditions. The authors test what happens if land-use is the same in the whole catchment based on what is already there. But it is never discussed which land-use types are realistic for the future or whether there is a trend in land-use towards any of the present land-use types. Rephrase to make clear that this is just a theoretical assessment of the sensitivity towards different types of land-use instead of a projection into the future. Also, the statement "we exchange space-for-time" (l. 96) suggests, that there is a known land-use trend for the future.

Methods

General comment
The method description is generally a bit confusing. I feel that generally it could be a bit shorter (e.g. the scenario description and the description of the 4 different root-zone storage capacities) are repetitive at some points. It might also help to provide a supportive figure of the study's workflow that clearly separates between different sources of input data, generation of scenarios and model application (instead of Fig. 3 which would fit better in the Supplemental material). Please revise the method section for more clarity and structure. The specific comments below hopefully help to do that.

Study area

l. 109: "divided into three main zones": It would be nice to see these three main zones in the Figure as well. In the figure, it is unclear which part of the catchment represents which of these three zones.

l. 120: reference is missing for the meteorological variables

l. 122: always refer to the specific label of the figure if possible (here it's Fig. 1c and not Fig. 1)

Data

l. 147-161: A figure or some numbers comparing the simulated historical and 2K climate scenarios could be a nice addition. From the description, it remains unclear what a “globally 2K warmer world” (l. 158) will translate to in this regional data set. Does this 2K warmer world lead to a mean 2K warmer regional climate? What’s the difference in mean annual temperature and mean annual precipitation in 2K vs. historical climate?

l. 164: It would be helpful to add Borgharen to the catchment map in Fig. 1

Methods

General: The decision to divide the land-use types into broadleaved forest on the one hand, and coniferous forest/agriculture on the other hand needs better explaining. Why is a tree-dominated (coniferous) vegetation grouped with crops? I would expect that crops and trees are very different with regard to their effect on the water cycle and concerning their root-storage capacity.

l. 233: Why is Imax taken as 2mm?

l. 262: Why are E-OBS data taken from 1980-2018 while streamflow data is only from 2005-2017? Would the results have been different if E-OBS data from 2005-2017 were used instead?

l. 289 ff: How were the ω values sampled?

l. 306: hillslopes are associated with forest and plateau with agriculture. But which type of forest do you mean here? Broadleaved or coniferous?

l. 331ff: “the performance … for the ensemble of retained parameter sets”: From the 10000 calibration runs: how many parameter sets were obtained for the model runs? From the supplemental material it looks like the prior is almost the same as the posterior parameter distribution.

l. 334-337: This section can be removed as it is a repetition of what was already mentioned above in lines 272-274.

Scenario description in 4.4:

- It is unclear which values of S_R with regard to the return period are used (2 years or 20 years?)
- How did you decide for the return period in the mixed agricultural/coniferous land-use? Agriculture should be 2 years and forest 20 years (l. 251-253)

l. 357, 362, 369: no need to repeat that $S_{Rmax,a}$ is used as a parameter in the historical run for every scenario. Better to mention it once, when the historical run is explained.

Results

General comment: It is not always clear what the reported numbers represent. Median and standard deviation? Mean and standard error of the mean? E.g. l. 374 & 377, l. 382, l. 390 & 391, l. 402, l. 408. If the reported values are always the same, you could also mention it once and state that all subsequent values represent the same measures.

l. 376: should this be ω_{obs} instead of ω?

l. 377: should this be transpiration instead of evaporation? This is a general issue: there is no clear distinction between evaporation, transpiration and evapotranspiration in the text.

l. 377-379: The differences of ω between the catchments is mainly attributed to the differences in the main vegetation type (broadleaved vs. coniferous/agriculture, l. 377-379). However, the catchments also differ substantially in other characteristics (French part: thick soils and gentle slopes, thin soils and steep terrain in the Ardennes, porous chalk in Wallonia (l. 109-113)). It should be discussed to what extent the differences in ω might not be dependent on the vegetation cover alone but also on the topography and soil type/thickness. Also, what are the implications of this regarding the method? How sure are you that the differences in hydrology between land-use types are really caused by the vegetation cover and not by the underlying topographical and soil characteristics?

l. 394: Fig. 2b should either be referenced earlier in the text, e.g. when talking about the difference between the historical and the 2K climate time series in the method section or it should be a separate result figure that comes later in the text.

l. 424: "median values of approximately 0.93": why approximately?

l. 431: "streamflow during the wettest months": include which months you mean by "wettest months"

Results for ω depending on the

Discussion

Implications

l. 500: "shows distinct patterns of change": more precise language could be used: Which response variables differ and are they larger or smaller compared to the stationary scenario?

l. 512-519: This section does not fit in the "Implications" section of the discussion. It is more of a limitation of the current study or an outlook of what could be done next. It could e.g. be moved to the "Limitations and Knowledge gaps" section of the discussion.

l. 524-256: It is not clear to me, why the results on actual evaporation differences between the scenarios indicate disagreements among model process representations. Please elaborate more on this point. Also, what are the specific "processes that become relevant in the future"?

l. 333-334: The conclusion, that vegetation is important for regulating the water cycle is correct but it is also quite established and not really a specific discussion of your results.

l. 535-539: This discussion is also a limitation of your study or an outlook to further work. It should not be under the "Implications" subheading of your discussion.

Limitations and knowledge gaps

l. 542: "it is unclear how ecosystems will cope with climate change": A discussion of how useful your approach to include vegetation into hydrological models under climate change in the light of this uncertainty would be interesting. To what extent can we be sure that the root-zone storage capacity can adapt to changing climate? What evidence is there from other studies regarding this issue? How would you proceed with your approach if vegetation changes to a vegetation type for which there is no data from the same region?

At the end of the discussion, you mention several times that this study should be read as a sensitivity analysis (l. 571) and a proof-of-concept (l. 575). This should also be made clear in the abstract. Also, a thorough discussion of the advantages and disadvantages of the presented method is missing. What are possible applications of it, to what types of regions/questions can it be applied? What are the limitation and what could be improved?

Figures

- General: figure labels should be in the same position for all figures (e.g. top left)
- Figure labels could be bold for better visibility?
- Why are the scenario names (2Ka-d) that are defined in Fig. 4 never used? Instead $S_{rmaxa\text{-}d}$ is used in Figs. 5,8,9? If scenario names are given, they should be used consistently.

Fig. 1:

- colours of figure b): better use some continuous colour scheme

- Figure labels are inconsistent, b and c not on the same height
- Fig. 1b: what are the black points? Are they the streamflow measurement locations? Mention in the caption
- Fig. 1 does not reflect well many aspects mentioned in the text (2.1 landscape and 2.2 land use)
    - Which are the three zones mentioned in l. 109? Are they represented in Fig. 1b? If yes you could add this to the caption. It is not clear what is the French, the Ardennes and the Wallonia part mentioned several times in the text
    - Fig. 1b: The numbers don't really match with the text. In Walloon 44% of the broadleaved forest should be there (l. 126), but in the figure the max. percentage is 38%.

Fig. 3:

- Maybe this figure fits better in Supplement S3 because it is part of the model description? I don't find it very helpful in the manuscript without the context of the model formulas

Fig. 5:

- Labels are missing
- Figures are a bit small: Could be a made bigger if empty space between panels is reduced
- 5b:
    - ω_{obs} should be on the y-axis not just ω
    - Axis text: No % because it's already in x-axis title
- 5c:
    - Caption last sentence: "A similar but reversed approach is applied …" It is the *same* and not a *similar* approach that was used.

Fig. 6:

- What is the ribbon for the modelled values: range from all realistic parameter sets of the calibration?

Fig. 7:

- Could be larger: box is not visible
- Don't use transparent colours to distinguish the panels. In my opinion they are already distinguished enough by the panel titles and labels in the caption (same for figures in Supplement S3)
- Labelling is not consistent (compare to labelling of Fig. 6)
- Why is there such a big difference between Borgharen and the 34 catchments? Isn't Borgharen just a summary of all the catchments?

Fig. 8:

- Caption 8e) maybe mention that y-axis is different scale (compare to caption of Fig. 7)

Fig. 9:

- What are the ribbons and lines? Median + conf. interval?

Supplemental Material

S1: Monthly correction factors for E-OBS precipitation data

- First sentence: Citation missing

S4: Prior and posterior parameter distributions

- State in table heading, that the last 3 columns are the posterior parameter distributions

Technical corrections

l. 54: rephrase to: sensitivity of the hydrological response to change in …

l. 62: remove "as often referred to"

l. 79: remove the full stop before the list of references

l. 191 & 1197: same style for (p1), (p2) and p3 (either with or without brackets)

l. 392: replace “return periods of 2 year” with either “2 year return period” or “return period of 2 years”. Also check the subsequent text as this mistake happens several times.

l. 410: Vertical space is missing as a new paragraph begins in line 411

l. 500: “compared to” instead of “with respect to”?

l. 592: “distinct change of sign”: remove distinct

Avoid unspecific adverbs. Either remove them, or state specifically what you mean by them. E.g.

- l.114: “relatively short response time” (how short is relatively short?)
- l. 422: “relatively well reproduced”
- l.423: “slight underestimation” and “relatively similar performance”