Reply to Anonymous Referee #2

Comment:

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 throughthe methods section. I alos have some serious concerns on lack of information and general organization of the manuscript. I recommend major revisions.

Reply:

We appreciate the reviewer's overall positive assessment of the manuscript and we are thankful for his/her thoughtful comments. We provide detailed clarifications below on how we will revise the manuscript.

Comment:

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 theroot zone was derived from applying the observed \omega to the predicted potential ET. But what was used for forcing the hydrological model? Potential ET from the climateprediction? 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?

Reply

The three inputs used to force the hydrological model are indeed potential evaporation, temperature and precipitation from the observed and simulated historical and future climate data. We will make sure to clearly state this in the model description of the revised version of the manuscript. For the actual evaporation from the root-zone storage in the hydrological model, we apply a simple formulation to express water stress. The equation is provided in Table S4 of the Supplementary *material and describes how actual evaporation is linearly reduced when the root-zone storage is below a certain threshold (parameter). This standard formulation is used in many conceptual models, including HBV, NAM and VHM (Bouaziz et al., 2021). We will clarify this in the revised version of the manuscript.*

Comment:

Fig 3 is very repetitive, while the essential difference between the hillslope, plateau andwetlands 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 isswapped? 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?

Reply

This is an interesting suggestion. However, in the land-use change scenarios, we did not change the percentages of each HRU in our model structure. The approach we propose to estimate the effect of land-use change really is a top-down approach based on assumed trajectories within the Budyko framework, but without the level of detailed required to specifically change land-use type at the pixel level. Therefore, we did not have the data available to change the percentage of each Hydrological Response Unit in our theoretical land-use change experiments. However, we think it is a good suggestion to discuss this limitation in the discussion section of the revised manuscript. We will also make sure to add a more detailed model description in the Supplement of the revised version of the manuscript, and to clarify the main differences in the caption of the Figure. In relation with the comments from Referee #1, we propose to move Figure 3 to the Supplement.

Comment:

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 \omega only apply to the rooting depth parameteror also to the evapotranspiration forcing? Please spell this out.

Reply

In the revised version of the manuscript, we will clarify that the same climate forcing is used for each of the 2K scenarios. The change in omega in combination with the change in climate data are indeed translated to a change in root-zone storage capacity parameter. However, we did not change the potential evaporation. We will clarify this in the revised version of the manuscript.

Comment:

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

a) The discussion already has a section called „limitations“, which is good. But it should include some more discussion on the assumptions above.

b) Correlations between parameters / vegetation and the environment are neglected in this study. For example, could the differences in \omega between catchments in Franceand 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.

c) 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.

Reply

a) We agree with the suggestions of the Referee #1 and Referee #2 to revise the Discussion Section in the revised version of the manuscript.

b) This is in an interesting question, which we will discuss in the revised version of the manuscript. We agree that the differences in omega-values are most likely related to a combination of biophysical characteristics. However, the omega parameter describes the hydrological partitioning and because transpiration is the largest continental flux, we think it is reasonable to assume that land use plays a major role to explain the differences in omega values (Teuling et al., 2019). Therefore, the variability in omega values is largely controlled by the water volume accessible to the roots of vegetation for transpiration (i.e. $S_{R,max}$). Topography, geology and soil type are likely implicitly integrated in other model parameters, e.g. the various recession time-scales of the linear reservoirs, which represent subsurface flow resistance throughout the system. We will include this in the revised discussion.

c) Thank you, this is a good point and we will make sure to clearly come back to the hypothesis in the Discussion of the revised manuscript.

Comment:

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

a) 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 ?

b) 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 onthe water cycle this is a central point and should not be left out. It is only mentioned (Ibelieve once) in the discussion.

c) 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.

Reply

a) *In the revised manuscript, we will clarify that potential evaporation is used to force the model and that actual evaporation is estimated in the model, following the equations provided in the Supplement.*

b) *The equations that describe how root-zone storage affects transpiration are included in the supplement. Following the suggestion of Referee #1, we will move Fig 3 to the Supplement of the revised version and include a more detailed description of the modelled processes.*

c) *For the root-zone storage capacity parameter, we use a return period of 2 years for the wetland and plateau classes and a return period of 20 years for the hillslope HRU. This is indeed already mentioned in the Model calibration section (4.3.1).*

Comment:

There are plenty of abbreviations that are barely introduced, sometimes the introduction appears even in a subheading.

Reply

Thank you for pointing this out, we will make sure to clarify the abbreviations in the revised version of the manuscript.

Comment:

The order within the methods section prevents understanding the methods. For example, there are many references to the model runs, before the model structure iseven introduced. Therefore it is really difficult to digest the information or interpret what the assumptions mean for the model. etc.

Reply

In the revised version of the manuscript, we will try to restructure the Method section to clarify our approach and experiments. We think that improving Figure 4 and introducing it earlier in the manuscript (perhaps already at the start of the Method section) can potentially also improve the clarity of the reading.

Comment:

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 diveinto the main text completely. For example take section „4.1.2 Seasonal water balancefor 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.

Reply

We thank the reviewer for this good suggestion, we will go through the section titles and simplify them in the revised version of the manuscript.

Comment:

It is difficult to interpret the results without a table showing an overview of the climateof the different scenarios, e.g. precipitation, E_pot, aridity, seasonality, if applicable actual evapotranspiration used as forcing, actual evapotranspiration as model output.

Reply

We agree with this suggestion of the referee and will include such a table in the revised version of the manuscript.

Comment:

I believe the manuscript can be shortened and the important information be fleshed outto improve it being understood.

Reply

This is a good point and we will critically go through our manuscript to see which part of the analyses can possibly be moved to the Supplement to cut down on some technical details. We will also revise the Discussion to include additional implications and limitations as suggested by the referees.

Comment:

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

Reply

We will clarify that our hypothesis relates to a modeling study.

Comment:

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

Reply

We agree and will remove "from coniferous plantations/agriculture towards broadleaved forest and vice versa" and only keep the "two hypothetical changes in land use.".

Comment:

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

Reply

The numbers mentioned here reflect the mean differences between, on the one hand, the change in mean annual streamflow and evaporation between the scenarios $2K_B$, $2K_C$, $2K_D$ with adaptive ecosystems and, on the other hand, the stationary scenario $2K_A$ (see section 5.3.5). Therefore, these fractions do not relate to the total water balance. We will rephrase these sentences: "We found that the larger root-zone storage capacities (+34%) in response to a more pronounced seasonality with drier summers under 2K global warming strongly alter seasonal patterns of the hydrological response. The differences in the change of mean annual evaporation, recharge and streamflow between, on the one hand, the three scenarios with adaptive root-zone storage capacity and, on the other hand, the stationary system are +4%, -6% and -7%, respectively."

Comment:

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

Reply

We will add additional references on the increasing evidence that ecosystems have the capacity to adapt to local (and changing) climate conditions, including Guswa, 2008; Schymanski et al., 2008; Gentine et al., 2012; Harman and Troch, 2014; Hrachowitz et al., 2020.

Comment:

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

Reply

We agree and will add the citation marks.

Comment:

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

Reply

We are not sure to understand the comment made by the reviewer, but what we mean is that in a *distributed model, the land cover map somehow needs to be translated to parameter values. Often, look-up tables retrieved from literature are used to relate a specific land use to a model parameter value. An alternative approach is to transfer parameters values from one location to another location through regionalization approaches. However, there is considerable uncertainty in both of these a priori parameter estimations.*

Comment:

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

Reply

See our reply to the previous comment. We will try to be more explicit in the revised version of the manuscript.

Comment:

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

Reply

We agree and we will rephrase to clarify the context in the revised manuscript to better introduce this paragraph. Here, we want to emphasize that there are multiple factors, besides the aridity index, affecting the position of a catchment in the Budyko space. One of these factors relates to the responses of ecosystems to elevated CO_2 levels, which are complex and can counteract one another (Jasechko 2018). On the one hand, vegetation density may increase from CO_2 fertilization, leading to increased transpiration. On the other hand, higher water use efficiencies may lead to declining transpiration rates as plants may transpire less water per unit of CO_2 taken up.

Comment:

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

Reply

We agree and we will rephrase. What we mean here is that the fact that most catchments worldwide scatter closely around the analytical Budyko curve is evidence for the co-evolution of catchment vegetation and soils with climate.

Comment:

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

Reply

Good point, actually the references are mentioned after the next sentence, but we will move them earlier in the revised version of the manuscript.

Comment:

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

Reply

We thank the referee for this very interesting reference, which we will include.

Comment:

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

Reply

We will rephrase the last two paragraphs of the introduction to emphasize that current studies assume that model parameter remain constant in a changing system. The objective of our study is to test how sensitive hydrological predictions are when changing vegetation related parameters, thereby accounting for the adaption of vegetation to future climate conditions.

Comment:

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

Reply

This is a good suggestion, we will clarify in the discussion that we expect the changes in the predicted hydrological response as a result of 2K global warming to be more pronounced in comparison to current-day conditions due to the potentially drier and warmer summers.

Comment:

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

Reply

Agree, we will remove these lines in the revised version of the manuscript.

Comment:

L 119-120: Reference missing

Reply

You are right, in the revised version, we will clarify that these numbers are calculated from the observed historical E-OBS data (section 3.1) and the streamflow data at Borgharen (section 3.3).

Comment:

L 131: Reference on the biodiversity statement required.

Reply

Agreed, we will add the reference of Kervyn et al. (2018) here.

Comment:

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

Reply

Thanks, we will clarify this.

Comment:

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.

Reply

Agreed.

Comment:

L 150: Spell out RACMO2 and HTESSEL ?

Reply

Agreed.

Comment:

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

Reply

We will try to rephrase this paragraph to increase readability. In the first sentence, we will explicitly refer to the change in vegetation related parameters in hydrological models (more specifically the root-zone storage capacity) in response to climate change. We will try to be less specific to clarify the broader picture.

Comment:

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

Reply

We will rephrase to clarify that the long-term E_A is derived from trajectories in the Budyko space considering a change in aridity index (from the climate data) and a potential change in omega-values. The change in omega-values are derived from historical omega-values in catchments with relatively high and relatively low percentages of broadleaved forests.

Comment:

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 geographicallydistinct. How to avoid misinterpretation?

Reply

In the revised version of the manuscript, we will acknowledge that runoff coefficients are of course also related to other physical catchment characteristics besides land cover. However, as transpiration is the largest continental flux, we assume that vegetation plays a major role in the hydrological partitioning. There is increasing evidence that vegetation develops root systems in an optimal way to fulfill their needs. It is important to make the distinction between rooting depth and root-zone storage capacity. The root-zone storage capacity is independent from the soil type, as in clayey soils the rooting depth may be shallower than in sandy soils for an identical root-zone storage capacity (= root-accessible water volume). Geology also plays an important role in the hydrological response but *is likely implicit in other model parameters (e.g. the recession time scales of the different reservoirs, which represent subsurface flow resistances through the system). We will include this in the discussion of the revised manuscript.*

Comment:

L 208 „we expect …" First off, I appreciate the formulation of hypotheses. I wold only statethem 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.

Reply

We will clarify in the revised version of the manuscript that we use the term evaporation to represent all different evaporative fluxes (interception, soil evaporation, transpiration). We follow the terminology proposed by Savenije (2004) and Miralles et al. (2020), where evaporation instead of evapotranspiration is used to refer to all evaporative fluxes.

In the revised version of the manuscript, we will also include the references of Fenicia et al. (2009), Teuling et al. (2019) and Stephens et al. (2021).

Comment:

L 222 „match expectations .. ": Unclear formulation, please be more specific on what typeof expectation.

Reply

Also here, we will rephrase that it is about the expectation that catchments scatter closely around the analytical Budyko curve, suggesting a co-evolution of vegetation and soils with climate.

Comment:

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

Reply

Agree, we will rephrase to explain that we added the difference between the simulated 2K and historical water deficits to the observed historical climate deficits (E-OBS data). This was done to account for the bias between the simulated and observed historical climate data. Perhaps this technical detail can be moved to the supplement of the revised version of the manuscript not to confuse the reader.

Comment:

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?

Reply

See also our reply to the previous comment, for the 2K scenario, we add the difference between the simulated 2K and historical water deficits to the observed historical climate deficits to account for the bias between the simulated and observed historical climate data. In the revised version of the manuscript, we will move these technical details on this "bias-correction" to the supplement in order not to confuse the reader in the main storyline.

Comment:

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

Reply

Agreed, we will change this.

Comment:

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

Reply

Agreed, these values were retrieved from the study of Gharari et al. (2011), we will clarify this in the revised version of the manuscript.

Comment:

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 thelocations) does not? How does this affect the model results? This needs to enter the discussion.

Reply

In the revised version of the manuscript, we will make sure to clarify that the Hydrological Response

Units hillslope, plateau and wetland are first derived from topographical information based on thresholds for the Height Above the Nearest Drain and slope. As additional step, we associate forest with hillslope and agricultural area with plateau using the land use map. Groundwater recharge occurs both in the plateau and the hillslope HRU through preferential recharge from the root-zone storage. In the plateau class, there is also recharge through percolation from the root-zone to the groundwater. In the land-use scenarios, we did not change the percentages HRU as we are using a top-down approach to estimate the changes in runoff coefficient through trajectories in the Budyko space, which does not include detailed information on the exact spatial extent of change. We do not expect a large effect of this limitation on our results, but we will mention it in the discussion.

Comment:

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 happenswhen 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 vegetationcover? Later (in the discussion) I am learning that water availability actually affects E_Aand I am back at point zero. This section really needs attention.

Reply

In the revised version of the manuscript, we will clearly explain that we use a standard formulation to express water stress. Evaporation from the root-zone (E_R) is reduced when the storage is below a certain threshold. The detailed equations are provided in the Supplement (Table S4).
The model is forced with potential evaporation and actual evaporation is an output of the model.

Comment:

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

Reply

This is a good point, the HRU are actually derived both from topographical and land use data, we will repeat it here.

Comment:

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

Reply

We agree that it is not unlikely that future changes may also influence other system characteristics. However, the mutual interactions between parameters are so far unknown and were therefore not explicitly considered. We did use an ensemble of parameter sets to somehow account for the uncertainty in model parameters and the possibility that parameters compensate for each other due to simplistic process representation. In many studies, the hydrological model used for future simulations remains completely identical to the model structure derived for historical conditions. In our study, we perform a controlled experiment to test the sensitivity of changing the root-zone storage capacity, which we can estimate from the future climate data. We will discuss this in the limitation section of the discussion of the revised manuscript.

Comment:

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

Reply

The potential evaporation used as forcing was calculated with the Makkink formula and we did not change it as a result of the land-use change. We will mention this in the limitation section of the discussion.

Comment:

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

Reply

We agree that this sentence could be perceived as discussion. However, it is always difficult to clearly separate results from discussion. Here, we think it provides guidance to the reader to place the results in a broader context

Comment:

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

Reply

We agree that splitting the results with separate sections for each scenario leads to some repetitions. However, we also think it increases the clarity to treat each scenario separately. In the revised version of the manuscript, we will critically reflect on how we can further clarify the structure to present the different scenarios.

Comment:

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.

Reply

We thank the reviewer for this suggestion, and we will include such a table in the revised version of the manuscript.

Comment:

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

Reply

In the revised version of the manuscript, we will clarify that actual evaporation is model output. The forcing variables of the model are potential evaporation, precipitation and temperature.

Comment:

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

Reply

As also mentioned in earlier replies, we will explain that we use a standard formulation to represent water stress, as described in the model equations of Table S4.

Comment:

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

Reply

This is perhaps a matter of taste, but we think limitations can also be read as an outlook for future work and we therefore think it might better fit after the implication section.

Comment:

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

Reply

Agree, we will remove this sentence.

Comment:

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

Reply

We agree. As also mentioned in one of the earlier replies, there is a distinction between rooting depth and root-zone storage capacity (i.e. the water volume accessible to the roots of vegetation for transpiration).

Comment:

Figure 2: The arrow with script \Delta \omega is misleading. What is shown is \Delta (E_A / P),which is really not the same.

Reply

Thank you for this good point. We will remove the arrow from the schematization and revise the caption.

Comment:

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 timedirectly 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.

Reply

Thank you for this suggestion. We will emphasize the differences in the caption and we agree to move the Figure to the Supplement. This way, it will be clearly connected to the model equations.

References

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