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Hydrology and Earth System Sciences Discussions

Interactive comment on "A simple water-energy balance framework to predict the sensitivity of streamflow to climate change" *by* M. Renner et al.

M. Renner et al.

maik.renner@mailbox.tu-dresden.de

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First, we would like to thank the Editor Dr. Stan Schymanski for his careful reflections on the versions of our manuscript. His thoughts and also the comments of the three Referees made the revision process to a (hopefully fruitful) learning and understanding process.

Please note, that the manuscript has already been revised two times and the revision notes have only been attached in the internal review process. To catch up on the interactive discussion we provide a shorter copy of both revision notes within this document. For compactness we did not include the extensive list of changes.

Notes on the first revision of the manuscript

The most important point, which was not stated in the first version of our manuscript is that the CCUW hypothesis also includes the assumption that the concept of Tomer and Schilling (2009) is also valid when the aridity index is different from one. This view led us to revise our conclusions, particularly the usability of the CCUW. Further, we changed the title of the manuscript, because the former title may not be accurate as it gives the impression of introducing a generally applicable method.

Another point raised by referee Ryan Teuling, is the weak definition of the term "catchment efficiency". It can not be used to provide a theoretical justification of the concept of Tomer and Schilling (2009). Instead it is a consequence of the CCUW hypothesis. This led us to revise section 2.2. Doing so, we would like to revise our earlier comment when replying to Ryan Teuling.

The points above resulted to change the perspective of the revised manuscript towards stating a hypothesis relevant for the problem of streamflow sensitivity to climate variations, and evaluating its consequences -a point which was raised by the first referee. However, this does not change the mathematical derivation of the sensitivity terms in sections 2.3 to 2.5. Also the evaluation of the CCUW under different climatic conditions, section 3.1 to 3.3 is still relevant.

A contrasting juxtaposition of the CCUW with the Budyko framework of Roderick and Farquhar (2011) revealed some interesting insights on the role of the catchment properties, represented by the catchment parameter. It may also help to understand the meaning of the CCUW hypothesis. Therefore we completely revised section 3.4 including the substitution of Fig. 7.

Reply to the comments of the editor

We followed the suggestion of the editor and rethought the critical points raised by the referees and the editor while revising the manuscript.

The first point raised by the editor was that there is some ambiguity in definitions in our manuscript. In particular we refer to climate changes / variations, when we mean a change either in the long-term average precipitation and or a change in long-term average potential evapotranspiration only, i.e. a change in long-term average aridity. Any other type of climatic changes are treated differently in the context of the framework of Tomer and Schilling (2009) as well as in the Budyko frameworks. Examples are a change in spatial distribution of precipitation while not changing the long-term average, or a change in seasonality, intensity as well as effects of increasing CO2 levels as mentioned by the Editor. Such changes which are "climatic" are essentially attributed to some kind of basin characteristic changes. Thus, to avoid any confusion, we now state this clearly in the manuscript.

Another point raised by the editor was the discussion of the inconsistency of the CCUW framework which does not respect the water limit under certain conditions. We addressed this important point in the discussion of section 3.4 and within the conclusions. The editor rightly complained about our argument that the CCUW does not need calibration. Clearly, we must agree that CE needs to be estimated from data (equation (5)). Also n needs to be estimated from data using some kind of calibration technique. The difference is that n may depend on the calibration technique, while is CE clearly defined. Still, both require the same data (P,E_p,E_T) to be estimated. We removed the argument from the manuscript.

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Notes to the 2nd revision of the manuscript

Specific replies

Justification of CE

Editor: However, I am still missing an adequate consideration of the concerns I listed under Points 2 and 3 of my own comment: "I would like to urge the authors to either give a more indepth justification why CE should be considered an objective function to be maximised (or how it refers to "ecosystem status" or why it should be constant for constant land use and vegetation), or supply evidence that it is indeed useful to separate climate change from land use change effects. If this cannot be done in the present manuscript but relies on the other manuscript submitted to HESS, then please let me know and I will inquire whether the two articles can be reviewed together."

Author reply: The link between CE and catchment properties was emphasized in the first version of the manuscript and also in the author comments published on the discussion page. Rethinking the Editor comment published on the discussion page actually led us to revise these statements in the revised manuscript.

We have to agree that the definition of CE is somewhat arbitrary and we cannot provide additional justifications for or against this measure. It has been useful in mapping the CCUW into Budyko space and as the Editor has illustrated with the bucket example, there are certain physical limits of CE. We hope that we have now clearly emphasized that constant CE is a consequence of the CCUW hypothesis (eq. 8) and nothing more.

"The authors do point out that CE can only be constant as long as the curve stays within the Budyko envelope and they conclude that a catchment approaching the Budyko

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envelope due to climate change would "expect a decline of the ecosystem status" (P. 8808, Ll. 20-23). This should be supported by theory or data, before claiming that "the CCUW hypothesis provides some evidence" how big climate change impacts are in a given basin (P. 8814, Ll. 13-15)."

Author reply: The simple bucket example of the Editor illustrating that $E_T = min(P, E_P)$ already shows that changes in CE can be driven only by changes in water or energy supply. Thus, our arguments with regard to a decline in catchment ecosystem when climate is changing is rather speculative. Therefore, we removed these arguments and related claims in section 4.

Description and comparison of the Tomer and Schilling (2009) approach

Editor: "In the revised manuscript, I found a helpful description of the TS2009 approach (Tomer and Schilling, 2009) and the information that their approach was based on observations in four experimental watersheds with different soil conservation treatments. I would qualify this as "encouraging anecdotal evidence", but the generality of the observed trends needs to be tested before the approach can be applied to separation of land use and climatic effects on runoff or used for predictions."

Author reply: We do agree with the Editor that there is a need for testing this framework. A verification with observational data is not aimed in this manuscript. Instead, we formalise the TS2009 concept using the CCUW hypothesis and its derivatives. This allows a theoretical examination of the properties of this concept under any kind of hydro-climatic conditions and we can compare it with the existing and widely recognised Budyko framework. We believe that the comparison as well as the examination under different hydro-climatic states showed that the CCUW and thus the concept of TS2009 is a special case of the RF2011 framework, if $P = E_p$. As evident from Figure 2, the slope of $\Delta U/\Delta W$ changes with aridity and n. Thus, also the climate change direction (CCD) of the RF2011 framework can be computed and the separation framework of TS2009 can be corrected according to the CCD of the RF2011 framework.

We think that this is a relevant point which may clarify the concept of TS2009 and the CCUW hypothesis. Therefore, we have included a short discussion in section 3.1. Additionally, we illustrate how to compute the climatic change direction for the Mezentsev function in an appendix. Further, we compared the observed CCD with the one from the Mezentsev function in the case studies of section 4. Last, we added this general finding in the conclusions in section 5.1.

We hope that these steps improved to manuscript.

Example of Tomer and Schilling (2009)

Editor: Clearly, the TS2009 approach is not applicable near the Budyko envelope, which is also where it deviates most significantly from the RF2011 approach. From this, in the absence of other evidence, I would immediately draw the conclusion that the TS2009 approach is not very useful, as the RF2011 approach leads to similar results and in addition satisfies conservation of mass and energy where the TS2009 approach does not. This result by itself would make the paper publishable in HESS. My conclusion could actually be examined by taking the data from TS2009 and testing whether the same separation between land use and climatic changes would result from using the RF2011 approach."

Author reply: We have stated in the revised manuscript that TS2009 can be regarded as a special case, when the respective basin is close to a aridity index of one. This is illustrated for the TS2009 experimental watersheds as reported in Fig. 1 of this author reply. The aridity index of these watersheds is about 1.26, but changed over time (1.33, 1.27, 1.14). The basins have slightly different evaporation ratios, which was attributed to conservation treatments by Tomer and Schilling (2009). Assuming, that

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over time only the long term average in P and E_p changed, gives an idea of how climate variability affects E_T. This is illustrated for the UW space in the left panel of Fig. 1. We then computed the climate change directions of the RF2011 approach an plotted these for the second period. This can be compared with the CCUW having an invariant CCD of -1. From Fig. 1 we can see that the observed temporal changes match the theoretical climatic change directions. Further, the differences between both approaches are smaller than the observed changes.
The basin change effect may also be assessed in UW space. There, the distance between the tangents of each watershed (or the differences in n / CE) gives an idea of the effect of different basin properties, while all watersheds are assumed to have the same water and energy inputs.

This example shows that both approaches yield similar results under non-limited conditions.

Disclaimer on the validity of the CCUW on P23L20ff

P23L20ff: "However, the result obtained with the CCUW hypothesis should be taken with care, because it is derived by putting the strong assumption that the concept of Tomer and Schilling (2009) and thus the CCUW hypothesis is valid for any given aridity index."

Editor: "This is a well hidden disclaimer. The whole paper should be written in a way that the reader is aware of this disclaimer throughout the document."

Author reply: We agree with the editor, that this disclaimer may be easily missed by the reader. The limits of the CCUW get most apparent in section 3.2 when mapping the CCUW to Budyko space. We have rephrased this discussion and clearly state that the CCUW can be non-valid when approaching the Budyko limits. In the abstract and in the revised conclusions we have added a statement on the invalidity of the CCUW.

Please also note, that we discuss potential limitation of the ecohydrological approach in section 2.2, P7L22f.

Notation of CE

Editor: Another small issue I would like you to change is to replace the symbol CE by C_E (with a subscript E) for consistence with the other symbols and to avoid confusion with $C * E$.

Author reply: We have changed this in the revision.

Replies to Referee I (Michael Roderick)

- Referee: "P12, L12. Not Arora 2002. That work did not consider the term for dn." *Author reply:* We removed the citation. The citation of Arora (2002) originally referred to employing the first total derivative of a Budyko function.
- Referee: "5. P19, L2. That is true in this example (n = 1.8) but I doubt whether that is general for all reasonable values (0.6 up to say 3) of n" *Author reply:* Thank you for this comment, the statement ("n/E_T is rising faster than P/E_T.") is valid only for $n > 1$.
- Referee: "6. P24, L26. Instead of 2/3 why not 0.6." *Author reply:* we have chosen $2/3$ because it is the reciprocal of 1.5, emphasising the symmetry of the water and energy limitations

Replies to Referee III (anonymous)

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- P.9: "Under extreme arid or humid conditions we would find a value of CE of about 1" I think that the catchment conditions (e.g. vegetation) are different under extreme arid and humid conditions, but CEs equal 1 under the two fully different conditions. More explanations are required. *Author reply:* This point has also been raised by the Editor and we rephrased the text.
- P.15 and Fig.3: Whether the CCUW hypothesis also has two asymptotes? In the other words, what is the extent of the space (Et/P, Ep/P)? *Author reply:* Mathematically, the asymptotes of the CCUW are set by the value of CE. However, as discussed in the revised manuscript, $E_T > P$ is non-physical.
- Fig.5: I suggest more lines according to the CCUW in this figure. Or this figure should be removed, because it is not necessary to plot the elasticity for the Mezentsev function here. *Author reply:* Well, the CCUW has just one curve for the sensitivity of E_T to climatic changes. From Eq. 15 one can see that $\varepsilon_{E_T;ccuw}$ only depends on P and E_p. This should not be confused with the sensitivity graphs in Figure 6. There, one needs some value of E_T, because $\varepsilon_{Q;ccuw}$ is also dependent on E_T. There we illustrate the behaviour for some fixed values of CE.
- Fig.7: The y-axis of sub-figure (e) should be Q (not Et). *Author reply:* The labels are correct. Panels (b) and (e) are same, but only the x-axis is different (P/E_p or E_p/P).
- Fig.8: the CCUW shows that 20% P increase leads to 100% Q increase, while the Mezentsev predicts that 20% P increase leads to about 50% Q increase. There is a large different. Please check against actual conditions! *Author reply:* The Figure is drawn by extrapolating the two approaches for different change scenarios. For now we only have one point in this diagram, when taking the data from Roderick and Farquhar (2011). As we state in section 4.3, there is a 3.7% decrease in P, a 1.3% increase in E_p, leading to a 20% decrease in Q. This point is actually more close to the CCUW than to the Budyko framework of

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Roderick and Farquhar (2011). However, note that this is just one point of data and as Roderick and Farquhar (2011) point out, an interannual storage change, some change in the variability of P or some kind of a basin change could have influenced this.

References

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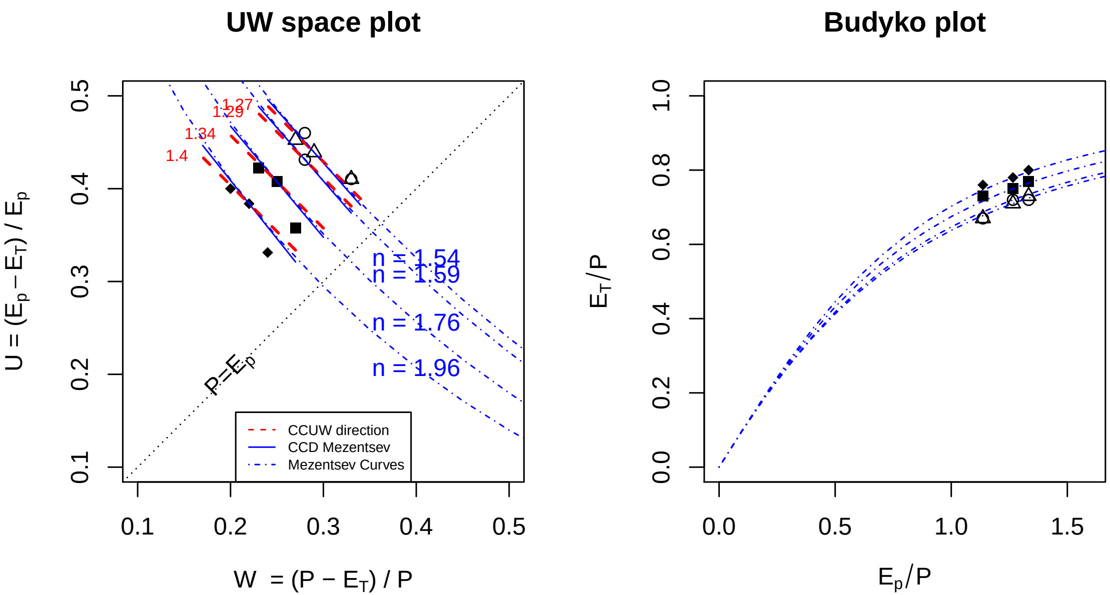

Fig. 1. Application of the climate change directions frameworks of the CCUW and the approach of RF2011 using the Mezentsev function. Data is taken by eye from Fig. 1 of TS2009 using similar plotting symbols.

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