Hydrology and Earth System Sciences Discussions

Interactive comment on "Applying a simple water-energy balance framework to predict the climate sensitivity of streamflow over the continental United States" *by* M. Renner and C. Bernhofer

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Author reply to referee II

The referee has raised several important issues which were quite helpful for revising the manuscript. Especially, pointing us to the problem of significant and insignificant

changes helped us to restructure our analysis and validation procedure.

Below we discuss the main points. Please note, that this reply is copied to a large part from our author revision notes, which have been attached to the revision of the manuscript.

More discussion on the definition of CE

We agree with the referee, that the definition of catchment efficiency CE is a somewhat arbitrary chosen functional form. That point was raised in the discussion of our companion paper in HESSD introducing the whole concept (Renner et al., 2011). During revision we understood, that $CE = E_T/P + E_T/E_p$ is rather a consequence of the CCUW hypothesis, i.e. $\Delta U/\Delta W = -1$, than a justification of this hypothesis. It does not justify the CCUW as it cannot be derived by first-order principles, a point noted by Ryan Teuling and Stan Schymanski (referee and the editor of the companion paper). This is now discussed in the revised companion paper (which should shortly be published in HESS) as well as in the revision of this manuscript (Section 2.2).

A very interesting point raised by the referee is indeed that CE, if it exists, is some non-linear combination of U and W. The non-linearity is probably necessary to account for water-, or energy limitation. However, we leave this point for further research.

Also the question of if/how climate-vegetation co-evolution is reflected in CE can not be answered in this paper, because we are unable to separate such natural effects from human alterations of the hydrological response. The results in the revised version of this manuscript may indeed show that especially in the western part of the US such co-evolution effects may have happened. We find that basin changes occurred which compensate for the observed climatic changes. But such compensation may also be caused by anthropogenic controls, e.g. by means of agricultural management and irrigation.

Validation and significant changes

The referee correctly states that the ... "methods follow straight from the assumptions, its validation is weak". Additionally he suggested to revise the results by looking at significant changes only.

We recognised this weakness and set up a hopefully better strategy for dealing with these points in the revised manuscript. The idea is to simply combine the testing results for the significance of the changes for each of the observed variables (i.e. P, E_p and Q). The combination provides a simple data-based classification of potential impacts of climate and basin changes on streamflow. Similar groupings have e.g. been used in Milliman et al. (2008). The classification strategy is described in a additional subsection of the Methods section (2.4).

This new strategy allows to (i) structure our results according to the significance of observed changes and moreover (ii) we can check if the predictions of the two climate sensitivity frameworks actually match with the potential impacts derived from the classification method.

This new strategy has led us to completely revise section 4.3, which improves the presentation of the results and its discussion.

Uncertainties of input data

The referee is concerned about uncertainties in input data and how these influence our results and conclusions. Both frameworks rely on the assumption that input data are correct and do not allow for uncertainties, e.g. by an error term. This means that any uncertainty in the input is easily mistaken as an impact of basin changes, which is discussed in section 4.4.

Generally, there is uncertainty in the long-term averages of the observed variables used to drive both frameworks. If one is interested in the effect on E_T or Q of such errors, one can use the sensitivity coefficients provided in this and the companion paper. As we emphasise, e.g. in section 4.2. the magnitude of such effects is primarily controlled by the inverse of the runoff ratio and the aridity index of a given basin. So, to conclude, such types of error could be quantified by a Monte-Carlo study, which requires some assumptions on the error structure of the input data. However, this is not the main aim of this paper.

Another type of error which is rather systematic comes from the definitions of potential evapotranspiration. Especially the use of the temperature range based Hargreaves equation was criticised by the referee. We tried to get hold of this type of error by using the temperature based Hamon equation and an external product which includes cloudiness information (CRU TS 3.1). The comparison to the climatology estimate delivered with the MOPEX dataset as well as the correlation to the observed changes actually reinstates the problems with the Hargreaves estimates (more significant changes in E_p than with the other tow methods).

The effect of the different E_p methods on the observed change direction ω_{obs} can be seen in Figure 1 of this reply. For basins with ω_{obs} close to $270°$ we find very small differences, which is due to large decreases in relative excess energy U. The differences increase towards larger ω_{obs}. This is because also the aridity index decreases (cf. Fig. 8 of the revision) and the limiting role of E_p gets more important and thus also the uncertainties in E_p. Further, under the dominant impact of potential basin changes $\omega_{obs} \approx 225, 45°$ the effect of uncertainties in E_p on ω_{obs} can be substantial. This is because the errors in the input are directly translated as basin change impacts.

We removed our previous claim, that CCUW is on average slightly better than the Budyko framework. Although CCUW has a slightly lower RMSE than the Budyko framework (cf. Table 3), we note in the manuscript that the differences between both frameworks are on average much smaller than the effects of potential basin changes.

Additionally, we found some evidence that the Budyko approaches are more realistic under water limited conditions than the CCUW framework (Fig. 3 and Fig. 8) of the revised manuscript.

Use of other Budyko curves

The referee wished that results for the Schreiber equation should be shown in the results. Instead, Referee I suggested to remove all the non-parametric Budyko curves from the analysis. For reasons of clarity, we followed the suggestion of Referee I and removed the other Budyko functions from the manuscript.

Here we attach two figures which show that there are no strong deviations in the results when using the Schreiber equation for streamflow sensitivity. Figure 2 shows the difference in streamflow change predictions of the Schreiber method as function of the CCUW. For basins with low E_T/P the predictions are quite similar, while for higher E_T/P and moderate streamflow increases the Schreiber equation predicts smaller changes than CCUW.

The following table shows correlation between the observed and predicted changes in streamflow. When correlating the predicted with the observed change for all basins, we find that the Mezentsev curve yields lowest correlation (0.54), CCUW is slightly higher (0.55) while Schreiber yields 0.60.

	ΔQ	ΔQ_{CCUW}	ΔQ_{Mez}
ΔQ			
ΔQ_{CCUW}	0.55		
ΔQ_{Mez}	0.54	0.99	
$\Delta Q_{Schreiber}$	0.60	0.99	0.98

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Figure 3 (this reply) is the same as Fig.6 in the revision, but includes values for Schreiber as well. Generally, the differences are not very large. For the climate and runoff change group Schreiber is on average more close to 0 than the other approaches. This shows, that using the Schreiber equation for estimating streamflow sensitivity seems to be equivalent to the other two approaches. However, from Figure 4 (this reply, similar to Fig. 3) it gets apparent that the sensitivity estimates show larger deviations to the CCUW and the Mezentsev approach. If $E_T/P < 0.7$ then Schreiber and Oldekop overestimate the sensitivity, but if $E_T/P > 0.7$ there is some underestimation. When averaging these effects possibly cancel out and look similar to the other two approaches. The main reason for this behaviour is that the non-parametric sensitivity estimates are independent of the hydrological response of a given basin. This allows prediction in ungauges basins, however, on the cost of possibly biased estimates.

References

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Renner, M., Seppelt, R., and Bernhofer, C.: A simple water-energy balance framework to predict the sensitivity of streamflow to climate change, Hydrology and Earth System Sciences Discussions, 8, 8793–8830, doi:10.5194/hessd-8-8793-2011, http://www.hydrol-earth-syst-sci-discuss.net/8/8793/2011/, 2011.

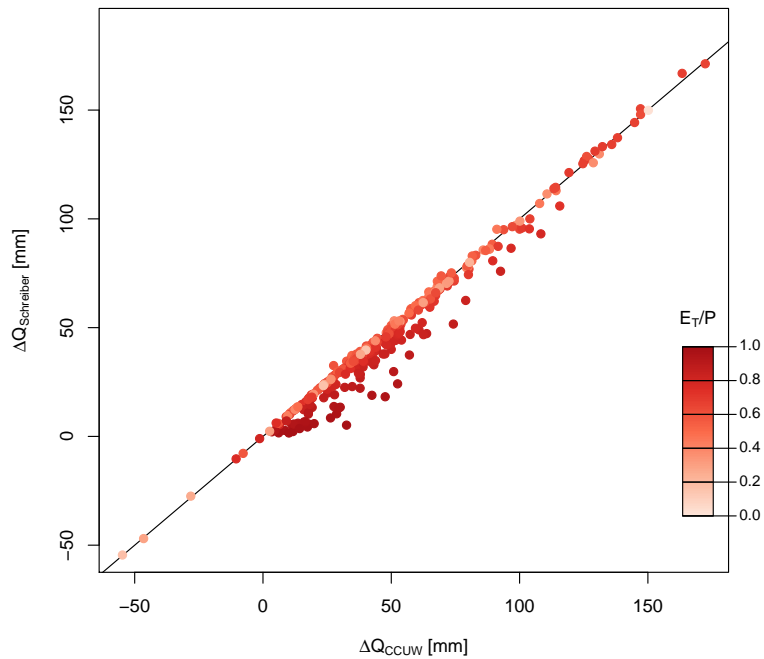Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 10825, 2011.

Change direction ω | E_p formulation
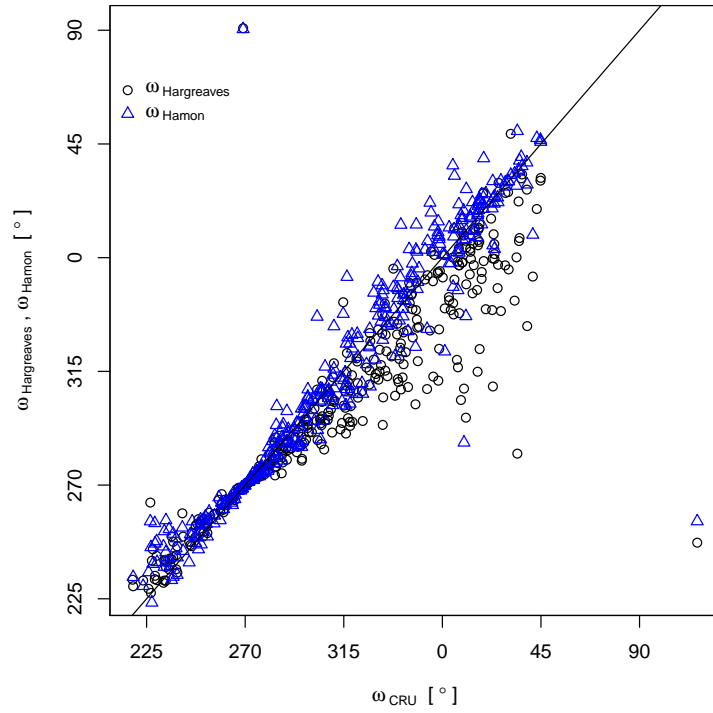
Fig. 1. Observed change direction in UW space compared for different potential evapotranspiration inputs.
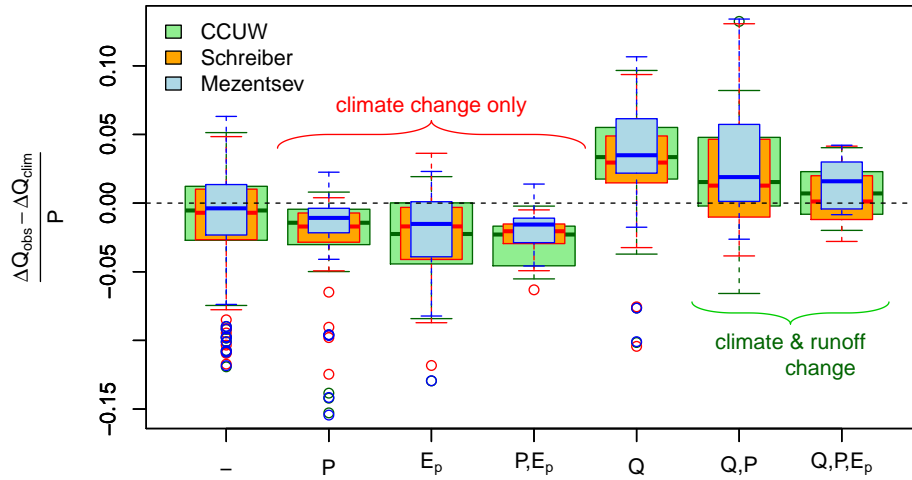
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Predicted change in streamflow due to climatic changes

Fig. 2. Scatterplot of predicted streamflow changes of CCUW vs. Schreiber.
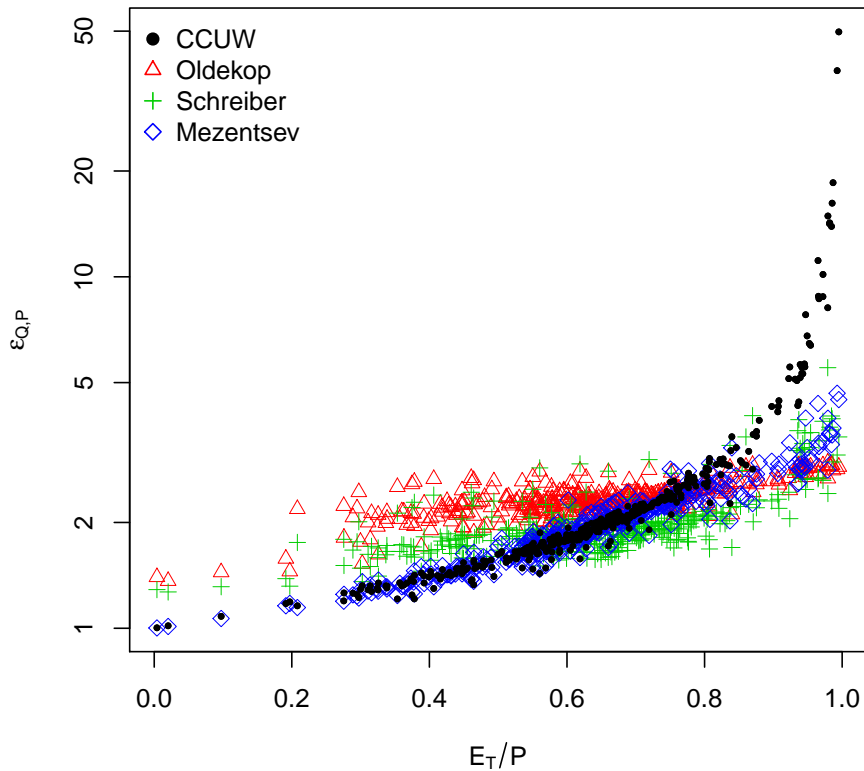
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Normalised basin change impact subset by change classification (α= 0.05)

Fig. 3. Boxplot of normalised difference of observed minus predicted streamflow change subset by the t-test classification.

Fig. 4. Comparision of streamflow sensitivity estimates as function of the evaporation ratio.