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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.

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Dear Reviewer,

thank you for your constructive critique and rich comments on our paper, which will help to improve the manuscript. We would first discuss the main points of concern and then give detailed replies on the other comments.

Remarks about the differences of streamflow sensitivity methods following Budyko curves and the CCUW hypothesis

Referee I is concerned about the theoretical basis of the CCUW approach, which is

claimed in section 2.3 (P8801L18-19).

In section 2.3 of the manuscript we show how to use the CCUW hypothesis to predict changes in evapotranspiration/streamflow given a change in climate. Further, we state that this approach is different to previous methods such as sensitivity methods based on the Budyko hypothesis e.g. Dooge (1992); Arora (2002); Yang et al. (2008); Roderick and Farquhar (2011); Yang and Yang (2011).

The CCUW hypothesis assumes that changes in relative excess water compensate for changes in relative excess energy, which is equivalent to assume constant catchment efficiency CE. Thus, we regard this as a theoretical assumption on how changes in climate impact hydrological response.

In contrast, the Budyko functions can be regarded as empirical relations (Sankarasubramanian et al., 2001) derived by looking at a multitude of basins.

Further, referee I states that the theoretical approach of our manuscript does not differ from the approach from Roderick and Farquhar (2011) and may be just a variation of it. For example, referee I states that the CCUW hypothesis is mathematically equivalent to prescribe how the catchment moves through the Budyko space.

To better understand the implications of the CCUW we map it into Budyko space (Fig. 3) using equation (28). The graphs show that the CCUW hypothesis results in similar curves than the (parametric) Budyko functions. First, this is a sign that our approach makes sense. But there are important similarities and differences: equation (28) has some similarity to the Mezentsev equation and both, the Mezentsev function with $n = 1$ and the CCUW curve for $CE = 1$ show to be identical. Apart from this case the curves are different. A striking difference is the asymptotic behaviour towards large aridity index values. As we discuss in section 3, the CCUW hypothesis does not obey the water limit of the Budyko framework. This feature results in different sensitivities for evapotranspiration and streamflow compared to the Budyko approaches.

Number of Budyko curves

Referee II asks for only one parameterisation of the Budyko curve, while referee I suggests to remove the non-parametric Budyko functions.

We agree that the number of Budyko functions are not necessary for communicating our main message. Moreover, the discussion of different non-parametric Budyko curves (Schreiber, Ol'Dekop) has been already published in the literature, e.g. Arora (2002). Also, the parametric Budyko functions have been discussed sufficiently (Yang et al., 2008; Roderick and Farquhar, 2011).

The CCUW hypothesis provides different functions for different hydro-climatic states expressed by CE. We think that a comparison with a parametric Budyko curve and different values of a catchment parameter is valuable. Therefore, we would restrict the manuscript to a comparison with the Mezentsev function and different parameters n.

Separability of climate and landuse impacts on streamflow

Referee I states that changes in the catchment properties (e.g. changes in catchment parameter n), are not strictly separable from changes in climate. Therefore he further states, that a mapping of the concept of Tomer and Schilling (2009) is "tentative at best".

We do agree in that in general, because for any method which is solely based on long term average data, such as the Budyko or the CCUW approaches, will be affected by changes not only in spatial distribution, but also the seasonal distribution of precipitation. So in effect, any climatic changes at the sub-scale will be attributed to changes in

basin characteristics. Thus, this argument applies not only to the conceptual model of Tomer and Schilling (2009) to separate climate from basin characteristics changes.

Besides that, we believe that the mapping of change using the Tomer and Schilling (2009) framework provides a valuable first-guess from long-term average data, without the need of complex modelling. See also the results of Renner and Bernhofer (2011), where we applied this framework to data of more than 400 basins in the US.

Comments

1. P. 8798, lines 17-18. Whether ET can be larger than EP depends on the way EP is defined. Here EP is defined as the water equivalent of net radiation. So can ET be larger than net radiation? The answer is yes – when the net sensible heat flux is into the surface (i.e., a negative Bowen ratio). This happens all the time, especially in cold regions when the air is warmer then the surface. Please modify accordingly.
We agree and change this accordingly:

"Naturally both terms are within the interval [0, 1], because ET is generally positive and it cannot be larger than P or Ep , which is also known as the water and energy limits proposed by Budyko (1974). "

is modified to: "Usually both terms are within the interval [0, 1], because E_T is generally positive, it cannot be larger than P and it is mostly smaller than E_p excluding cases where we find a negative Bowen ratio. These limits are also known as the water and energy limits proposed by Budyko (1974). "

2. P. 8805, Eqn 23 & lines 10-14. Eqn 23 includes terms for changes in climate (dP, dEp) and for changes in catchment properties (dn). That needs to be stated.

We add that information.

3. p. 8803, Eqn 15. Mistake. The partials should be of w not u.
Thank you, that has been overseen.

4. p. 8808, lines 3-5. What is the difference between predicting the climate and predicting the change? Eqn 28 is for the climatology?

The referee is concerned about this paragraph: "The actual value of the catchment efficiency CE determines the asymptote for $E_p/P \to \infty$. This makes a distinction from the Budyko hypothesis, which employs the water limit $E_T/P = 1$ as asymptote for $E_p/P \to \infty$. However, note, that (28) is not intended to be used for prediction of E_T from climate data, but for estimating the effect of climate changes on E_T."

To prevent any confusion we will remove the last sentence in the revised manuscript.

Actually, eq.(28) has been intended to provide a comparison with the Budyko curves. From Figure 3 in the manuscript it is visible, that there are certain combinations of CE and aridity indexes (e.g. $CE = 1.3,\ E_p/P > 3\frac{1}{3}$), which will result in $E_T/P > 1$, i.e. the water limit of Budyko will be crossed. Thus, not all combinations of CE and aridity indexes make hydrological sense.

Therefore, using eq. (28) to predict E_T with a fixed value of CE may give non-reasonable results. In addition, for estimation of CE, E_T is actually required.

However, for the case of predicting the change in E_T of a basin with a known CE, the basin would move along the curve given by eq. (28) (when applying the CCUW hypothesis). There may be the case, that the water limit is reached, when aridity increases. This could then imply a dry up of the river and also a decline in CE.

5. P. 8808, line 21. I did not understand the point. i.e., "....not enough water to sustain ET."
This point was also raised by the second referee, Dr. Teuling.

He suggests to rephrase: "Then at some point the water limit will be reached, which implies no streamflow as well as not enough water to sustain E_T. "

$\rightarrow$

"Then at some point the water limit will be reached, which implies no precipitation, no streamflow, as well as not enough water to sustain any evaporation."

6. P. 8810, lines 11-16. Why compare with an Oldekop function? (See main comments).

In this point we used an Oldekop curve, because it is non-parametric and no further parameter is needed. We intend to use only the Mezentsev curve with a fixed value of n.

7. P. 8810, line 1. Typo. show should be shown.
If P8811L2 is referred to, "show" is correct.

8. P. 8813, lines 20-24 & p. 8814, lines 12-15. The thrust of the argument here is a little confusing. Firstly, it is argued from the data that dn may not equal 0, and then it is argued that the climate change impact on runoff may be larger than predicted by the Budyko curve. First, Roderick & Farquhar 2011 (hereafter RF11) assumed dn = 0 but did point out that dn might in fact change. The issue here is that the Budyko prediction missed the observed change by $\tilde{2}$ mm per annum. Alternatively, RF11 also pointed out that change in storage of that magnitude could also easily account for the difference. Given the uncertainty in storage

change discussed by RF11, it is perhaps premature to claim a larger impact of climate change.

The confusion noted by the referee is due to the differences in predicting the climate change impact on streamflow for the MDB basin using the approach of Roderick and Farquhar (2011) or the CCUW approach. From table 2 of our manuscript one can see, that we observed a change in Q by -5.6 mm. RF11 predicted -3.2 mm and argue that this underprediction may be due to several reasons such as a change in long term storage. However, there is no detailed analysis on that topic. They also argue that a change in dn is possible, and as we show, there is actually a change in n when calibrating n for both periods. That means, following the Budyko approach of Roderick and Farquhar (2011), only (-3.2 mm) can be attributed to climate change, while the remainder (-5.6 - -3.2 = -2.4 mm) could be attributed to uncertainties or to changes in catchment properties.

Using the CCUW method, we predict a change of -5.7 mm, which is very close to the observed value. That means by only considering climate impacts, the CCUW hypothesis is seemingly able to predict the observed change. Therefore, we argue that climate change impacts as predicted by the CCUW method are larger than previously estimated. This result also provides us with confidence in our sensitivity estimates (see Fig 8 and Table 2), which are distinctly larger than the estimates reported by Roderick and Farquhar (2011) and references therein (see Fig 8 and Table 2).

9. Table 2. The units for all hydrologic fluxes should be mm/yr instead of mm.
OK

10. P. 8820 Eqn 27. The Mezentsev Eqn is wrong. On the denominator it should be $(P^n + Ep^n)^(1/n)$.
Thank you.

11. P. 8830, Fig. 8. The caption is incorrect with respect to left and right panels.
Thank you, I was mixing left and right.

12. P. 8830, Fig. 8 and associated text. This figure summarises the main differences and shows that for the MDB, the CCUW approach predicts an overall sensitivity that far exceeds the RF11 scheme. For example, previous research, and CSIRO modelling gives a sensitivity to change in P of around 2-3 for the entire MDB (see citations in RF11), i.e. a 10% change in P produces a 20-30% change in runoff. The RF11 (left panel) scheme is consistent with that range. Note that the RF11 scheme is a first order perturbation (and is therefore symmetrical) and limited to small changes. In contrast, the CCUW scheme suggests that a decrease in P of 10% decreases runoff by around 40% and an increase in P of 10% increases runoff by around 50%. Hence while I agree that the CCUW hypothesis leads to greater sensitivity, I think it would be useful to point out that these estimates are larger than the effects actually observed.

In our understanding all sensitivity coefficients are estimates rather than observed values. This includes, the CCUW approach, the Budyko approaches as well as the CSIRO modelling results. Thus, we will add that the estimates of the CCUW approach are larger than previous estimates of the sensitivity of streamflow to precipitation at the MDB, e.g. reported by Roderick and Farquhar (2011).

One way to actually test these coefficients is to subset the data in two periods and compare observed and predicted values, such as Roderick and Farquhar (2011) did. However, then we are left with the problem of attributing observed changes in runoff to climatic or other changes (catchment properties, storage changes, ...).

Still, we are reasonably confident in the estimates of the CCUW method, because (i) good matching of observed and predicted streamflow changes and (ii) change in

$\Delta U/\Delta W = -1$, i.e. in climate change direction following the framework of Tomer and Schilling (2009).

Best Regards,

Maik Renner (in the name of my co-authors)

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