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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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Before the authors formulate their final response, I would like them to consider some of my own thoughts.

1. It appears to me that the conceptual model by Tomer and Schilling (2009) is based on the assumption that changes in E_T at invariant P and E_p are due to changes in vegetation or management only. It further appears to assume that climate change results in a change in P/E_p. The prior assumption would be violated if E_T changed in response to e.g. a change in atmospheric CO2 concentrations or the fraction of diffuse light, which could happen without a change in P, E_p, vegetation or management. The latter assumption would be violated, if climate change led for example to a simultane-

ous increase or decrease in both P and E_p, which could evidently affect E_T. I would like to ask the authors to describe the model by Tomer and Schilling (2009) in a few more words, so that the present manuscript becomes self-contained without reading Tomer and Schilling (2009). In this context, it would also be good to discuss the limits of the approach and the potential shortcomings, e.g. as mentioned above.

2. In their response to Dr. Teuling's comments, the authors argue that maximisation of E_T is achieved by maximising the term E_T/P + E_T/E_p. I cannot follow this line of argument. Maximisation of E_T subject to energy and mass balance constraints would simply mean that E_T = min(E_p, P), which is equivalent to tracking the envelope in the Budyko diagram. At the present stage, I have to agree with the reviewers that the catchment efficiency index (CE = E_T/P + E_T/E_p) is not related to any first principles so its usefulness is a hypothesis that needs testing. Therefore, 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.

3. In Section 3.4, the authors point out that the Budyko curve is a regression through the long-term means of different catchments and does not account for the specific characteristics of the basin under investigation. This is an interesting point and I believe that the reader would benefit from a more in-depth discussion. In my view, the question it boils down to is: What is the trajectory of a particular catchment over time under climate change in the absence of other changes? The authors propose that the trajectory is not constant n (Roderick and Farquhar, 2011) but constant CE. 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

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).

4. On several occasions (e.g. P. 8816, L. 4), the authors claim that the proposed method does not require calibration, as it does not involve e.g. fitting a parameter (e.g. n) to data. This appears misleading to me, as both n in the Budyko-based approach or CE in the approach proposed here has to be obtained from data. Please explain. The claims made in Section 5.3 seem to largely rely on the assumption that CE does indeed represent a catchment-inherent property that does not co-vary with climate. Beside the authors' own disclaimer that it would change with climate under certain conditions (see above), I have not found any evidence to support or falsify this assumption. If this assumption is as central to the argument as it seems, it does require a lot more support than I have found in the manuscript so far (see Point 2).

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