**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.

Dr. Teuling (Referee)

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First of all, I would like to apologize to the authors for submitting my review rather late. I realise this limits the time for the authors to respond to my comments, nonetheless I hope they manage to do so in the remaining time.

The manuscript by Renner et al. deals with quantifying changes in streamflow in response to changes in climate conditions on the one hand and catchment properties (land use) on the other hand. This is a highly relevant issue for hydrologists and water managers given the on-going debate on which of the two changes dominates changes in streamflow. It provides an interesting new perspective on the framework presented by Tomer and Schilling (2009), by showing how this framework can be understood

from more theoretical considerations. The manuscript is well written and generally well-illustrated with clear graphs and example applications taken from literature, and overall I believe the manuscript has the potential to make a significant contribution to HESS. My only concern deals with the terminology and/or phrasing used mainly in the theory part of the manuscript where the catchment efficiency is introduced (see below). This prevents me from advising to publish the manuscript in its current form. While the required changes would classify as minor rather than major, I list them as major in order to be able to see the revised manuscript.

General comments

One of the authors' claims in the manuscript is that they have found a theoretical "foundation" of the concept presented previously by Tomer and Schilling (2009). However, this "foundation" is fully based on the definition of catchment efficiency (Equation 5), which seems somewhat arbitrary, or at least the theoretical foundation of this equation is not well explained (and also no references are provided). Upon reading and re-reading, I was left with the impression that this definition was found to be the necessary condition for a more "theoretical" basis of the work by Tomer and Schilling (2009); however in the manuscript it is presented differently. While there is nothing wrong with the use of CE as a necessary condition for the derived framework, it should be made clear across the manuscript that the result are conditional to the *assumption* that CE is the fundamental catchment property determining catchment response. Mathematically, it is just the sum of two arbitrary ratio's, and I can't think of any reason why the "real-world" efficiency would be the sum and not, for instance, the product of both ratio's. In summary, please provide arguments and/or references justifying CE as a fundamental catchment property that can be derived from theory, or make clear throughout the manuscript that it is a necessary assumption in order to come to a more theoretical understanding of the Tomer and Schilling (2009) framework.

Furthermore, I agree with the first reviewer that by considering only one parameterization of the Budyko curve rather than three can help to highlight the main message of

the manuscript.

Detailed comments

Page 8794, Line 9: Theoretical foundation or explanation?

Page 8798, Line 6: whether or either?

Page 8803, Line 11: numerical or analytical?

Page 8808, Line 21: "to sustain E_T" -> not clear. Do you mean to sustain E_T at the potential rate? Or, to sustain any evaporation. In this case please rephrase into: "which implies no precipitation, no streamflow, as well as not enough water to sustain any evaporation."

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 8793, 2011.