Author responses to Anonymous Referee Comments for

“Hydroclimatic regimes: A distributed water-balance framework for hydrologic assessment and classification”
Author(s): P.K. Weiskel et al.
HESS MS No.: hess-2014-42

Review #1

General Comments

Response:
Thank you for your comments.

Specific Comments

Comment 1. Page 2937 Line 15 From the viewpoint of provisional service, the original definitions of green (ET) and blue water (Lout) by Falkenmark and Rockström (2004, 2006, 2010) look still valuable. Just a comment.

Response:
We agree with the Reviewer—the original definition of green water by Falkenmark and Rockström (2004, 2006, 2010) remains valuable. Specifically, defining green water flux strictly as ET is valuable in the catchment spatial context in which they developed their framework. In a catchment context, precipitation (P) is considered by them to be an “undifferentiated” inflow term, which is subsequently partitioned into green (ET) and blue water (Lout) outflows.

However, as we attempted to explain in the text, in the open-system, hydrologic-unit spatial framework which we adopt for this paper, there are two potential inflows terms (P and Lin). For the sake of symmetry in our statement of the open-system water balance equation, we believe it is useful to define precipitation as a green inflow, rather than as an undifferentiated inflow.

In all other respects, we agree completely with Falkenmark and Rockström’s green-blue water terminology. That is, we agree (1) that green-water storage consists of soil moisture in the unsaturated zone, and (2) blue water fluxes and storage are exactly as described in their papers.

However, in response to your comment, we will add the following sentence to the end of Section 2.2 (line 22): *“In summary, our re-interpretation of green-blue water terminology attempts to place the original definitions of Falkenmark and Rockström (2004, 2006, 2010) into a more general, open-system spatial context, whereby both types of inflow to a hydrologic unit (landscape inflows and precipitation) are available for partition into blue and green outflows.”*

Comment 2. Page 2941 Line 9 Please provide your reason or a reference for the assumption “the river corridor is 30% of the total hydrologic unit area”.

Response:
The reason for choosing the 30% value is provided on Line 11 (2 lines further down):

“The percentages used in the calculations were determined by subjective [*trial-and-error*] calibration of the model to measured streamflow in arid-region rivers that are known to lose water due to ground- and surface-water evapotranspiration in the downstream direction.”

To improve clarity, we will insert the phrase “trial-and-error” to the Line 11 sentence, as shown above.

Comment 3. Page 2962 Figure 1e Please use different symbols for catchments (a-d). They are confusing with figure identifiers (a-f) especially in the caption.

Response:
Thank you for noting this problem with the figure. We have re-labeled catchments (a-d) as “(1-4)”, to avoid any confusion with Figures 1a, b, c, d, and e. We have made this change throughout the text, the captions, and on Figures 1c and 1e, where these example catchments are depicted graphically.

Thank you very much for your thorough and useful review of our paper.

Review #2.

This study follows up on previous work by the same authors. Dry regions of the world are important and the authors expand their work to include these as well. The new indices are tested in the US, which is quite appropriate given the large climatic variability present. The authors present interesting results with respect of where runoff is generated or consumed, whether vertical fluxes dominate and what spatial variability can be identified. This manuscript is a nice contribution and should be published after some adjustments. I have a few suggestions to strengthen the storyline.

Comment [1]. This work follows the extensive calls by a wide range of people for new strategies to hydrological science in line with the needs or a changing and increasingly impacted world (Wagener et al., 2010, WRR). One important issue is the presence or absence of data. It might be nice for the authors to discuss the data needs for their approach in the conclusions section since it will help people understand how transferrable these ideas are to other regions of the world. Most dry regions of the world are in developing countries where data is much more sparse than in the US.

Response to Comment 1:

Many thanks for this comment. Our paper is intended to be primarily conceptual and mathematical—proposing a rigorously re-interpreted version of the Falkenmark/ Rockström green-blue water paradigm, to improve understanding of water availability in all environmental settings. However, we agree that data is a critical issue. So in response to your comment, we will add material to the Conclusions on the data requirements of our approach (i.e., gridded hydrologic-unit boundaries, elevation, and climate data), and point the reader to our Supplement, where links to several important datasets are already provided. (We will also add more links to relevant datasets.) In the conclusions, we will also briefly mention the usefulness of continental and global water-balance models, driven by—and calibrated to—data. The good news is that most of the required data, at least for temporally-averaged analyses, is readily available. Even a global network of streamgage data is now available for use in model calibration. Also, existing gridded global water-balance models (by groups led by Vörösmarty, Alcamo, Döll, Oki, and others) could be configured to map our new indicators—with the exception of runoff-consuming (sink) areas, which seem to have received little or no attention, to date, in global models.

Comment [2]. The study presented only includes 4 figures and is rather brief on the insights provided. Recent studies that aimed at understanding variability in time had problems to decipher differences in controls on variability (e.g. Sawicz et al., 2014, HESS). I would very much like to know how the classification presented has changed over the decades analysed? I think that the long-term average is less interesting. Given that places are weak sources or sinks, have they changed in character with time? Where are the limits of current data in understanding this variability?

Response to Comment 2

We accept your point—that the study is rather brief in describing insights provided by the new approach with regard to continental-scale temporal variation (seasonal, inter-annual, or decadal) in the new indicators across the U.S. This is for two reasons.

First, as previously noted, the study is primarily intended as a theoretical contribution, providing a re-interpretation of the widely used green-blue water paradigm in a fully general (as opposed to strictly catchment) version of the landscape water balance.

Second, there are serious data limitations associated with any attempt to do spatially *and* temporally distributed analysis, at a continental scale, simultaneously. This is because temporal variation in the blue boundary fluxes of each hydrologic unit (Lout, Lin) depend not only on climate forcing (P and ET, which would be obtainable on a time-varying basis), but on the storage properties of each hydro-unit—about which we claim no knowledge in this initial, proof-of-concept demonstration. That is why the USA-wide analysis is steady state (ie, dS/dt is assumed to = zero). We acknowledge that this is a simplification, even at the 100-yr time scale. However, the steady-state approach allows us to focus on the very large and interesting *spatial* variations in hydroclimate across the USA—which are not well represented by traditional indicators (such as local runoff). We extensively describe this variation in Figs. 2a, b, d, e and Fig. 3a, b, c, d, e, and f, and Supplement Fig S1a, b, c, d, e. In Figure 3 (and supporting Fig. S1), we systematically compare our new indicators to their traditional counterparts (e.g., see comparison between Local Runoff and Total Water Availability in Fig. 3a, b, and Fig. S1a). In short—data limitations presently restrict us to a *spatial* analysis at the continental scale, but we believe that these variations are interesting in themselves, and are extensively described.

Finally, note that we actually do provide an analysis of time-varying hydroclimatic regimes in Section 4.2, and Figs. 2c and f—where we analyze the model-simulated response of a single New England hydro-unit to time-varying P and ET forcing over a 40-yr period. In this case we know the storage properties of the hydrologic unit from previously published work—and show 480 monthly regimes on a regime plot, in addition to median monthly regimes (including dS/dt variations). Fresh insights are gained from this analysis—about the very large variation in hydroclimatic regime resulting from the strongly seasonal ET cycle in this northern temperate location.

We thank you for your observations on this issue. In response to your comment, we will add the following sentence to Section 6.

“Data limitations concerning the storage properties of hydrologic units presently restrict us to a spatial analysis at the continental scale, but assessing temporal variation in hydroclimatic regimes at this scale is a subject for future research.”

This statement will be part of a renamed Section 6: “Summary, Conclusions, and Future Research Directions.”

Comment [3]. What is the opportunity for using these indices for projecting into the future? Are there projections of both the climate and the water demand (or the economic and population growth) that could be used to estimate these indicators into the next decades?

Response to Comment 3

Thanks for this comment. In response, the key point is that all of the new indicators are derived from a complete statement of the landscape water balance equation (see Section 2.1, Equations 1, 2a, 2b). Therefore, effects of future changes in climate (P, ET) and water withdrawals and return flows (Hout, Hin) are captured by our indicators— if the needed climate and water-use projections are available. Fortunately, gridded climate projections are becoming widely available from downscaled GCM results, and georeferenced human water use data are either directly available (e.g., in USA and Europe), or can be estimated from socioeconomic data in the remainder of the world (using methods pioneered by Vörösmarty, Alcamo, Döll, and others). A statement about the usefulness of the indicators for showing hydrologic effects of projected changes in climate and human water use will be added to Section 6.

Comment [4]. How would you bring in other datasets, e.g. about groundwater?

Response to Comment 4.

It is definitely possible to bring available groundwater datasets into the approach. In fact we have already done it, in an earlier paper focused on wetland classification (Lent et al., 1997, see our References). That paper had data from previously published water-budget studies of 10 wetland and lake systems, allowing differentiation of groundwater and surface-water inflows and outflows. We could do the same for all landscape types, using the current indicator framework, assuming the data were available. We could differentiate (Lin, out) flows into (SWin, out) and (GWin, out), giving a total of 3 inflow terms (SWin, GWin, and P), and 3 outflow terms (SWout, GWout, and ET). As result, instead of the four (2^2) end-member regimes described in the present paper, we would have nine (3^2) end-member regimes. Since we are working with 53,400 landscape units in the present study (rather than 10 wetlands), lack of appropriate data on groundwater fluxes, forced us to take a lumped approach to GW and SW flows, by introducing the landscape flow terminology (Lin and Lout).

Finally, note that Section 1.3 of the Supplement already addresses groundwater in its explanation of the newly introduced landscape flow terminology. However, in response to your comment, we will add a sentence (in Section 6 of the main-body) on the importance of differentiating GW and SW flows in future research on hydroclimatic regimes.

Comment [5]. What is missing from the manuscript right now is a discussion about how these new indicators and the results are more informative for water management. The discussion of the results is a bit brief and it does not sufficiently link into water management.The authors would make a stronger case if they would really state what this new classification means for understanding and managing water resources. The results are described, but could be interpreted more.

Response to Comment 5.

Thanks very much for this comment. We agree. In fact, we have already written a section “Implications for Sustainable Water Management”, that appeared in an earlier version of the Supplement. It was then dropped from the Supplement in the HESS-Discussions version of the paper, for the sake of brevity, and because it was not appropriate for inclusion in the Supplement.

We will restore the water management section, as a new Section (Section 5.3) of the main-body paper. This new section will constitute the final subsection of Section 5 (Discussion).

In accord with this change, we will update the title of the paper to:

Hydroclimatic regimes: A distributed water-balance framework for hydrologic assessment, classification, and management.

Thanks very much for your helpful and insightful comments on our paper.