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Hydrology and Earth System Sciences Discussions

Interactive comment on "Stream recession curves and storage variability in small watersheds" *by* N. Y. Krakauer and M. Temimi

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We thank the reviewers for their careful reading, and submit a revised and improved version of our paper. Our responses to the comments made are as follows:

1 Comment (Marani):

I would like to point out to the Authors a recent contribution to the subject of recession curves, which, I believe, is relevant to the Authorsâ€$™ reasoning

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Interactive Discussion

Discussion Paper

We thank the commentor for pointing this paper out and now refer to it.

2 Review 1:

1. The authors should spend more time justifying their assumption of a 1-1 storage-discharge relationship in these watersheds. Even a simple groundwater model with a basis in physics will generate aquifer discharge that is not a 1-1 function of storage (Sloan, 2000; Rupp et al., 2009) and this lack of 1-1 relationship has been observed in basins (e.g., Rupp et. al., 2008). The consequences of lumping together individual recession curves that do not overlap should be discussed. Not only will there be an offset in the dQ/dt vs Q relationship, the slope of this relationship will be affected, and thus the entire tau(Q) function that the authors are generating. The authors should comment on the direction and magnitude of this effect/bias. While the authors discuss the how the inferred storage-discharge relationship based on periods of low precip, snowmelt and evapotranspiration may not hold when these forcings are greater (p. 1848), this does not address that a 1-1 relationships may not be valid even when these forcings are negligible.

We agree that this is a legitimate concern and now discuss it: "assuming that a particular stream follows a single recession curve can be taken to imply that discharge for that stream is a single-valued function of basin water storage. While this assumption holds in analytical solution of some very simple aquifer models and for flow systems dominated by deep, homogenous aquifers (Brutsaert and Nieber 1977; Dewandel et al. 2003; Rupp et al., 2006a), more complex flow models show a clear dependence of flow rate on the time history of water input (rainfall or snowmelt), so that flow is not a single-valued function of basin storage and a recession curve plot will show substantial scatter (Sloan, 2000; Rupp et al., 2009). In such cases our approach to fitting a recession curve will produce averages of the rate of change in flow $\dot{Q}$ at given flow

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rates Q. However, using these averages may lead to biased estimates of the recession timescale $\tau(Q) = -Q/\dot{Q}$ (since in general $-Q/\overline{\dot{Q}} \neq -Q\overline{(1/\dot{Q})}$). This bias propagates to the dynamic storage, estimated by integrating $\tau(Q)$ (Eq. 8). Further study is needed to determine the likely magnitude of this bias and how best to correct for it in streams like the ones in our sample (perhaps by determining $\tau(Q)$ separately for individual recession events and then averaging (Biswal and Marani 2010))."

2. Figure 4a may be the most important figure in the paper. Unfortunately, I believe that using a locally-weighted least squares linear regression to estimate tau(Q) is not the preferred option here. This LOWESS-type method produces curves that are too locally "jagged" (e.g. Fig 3b) resulting in the spaghetti of curves in Fig. 4a from which it is very difficult to discern patterns among individual curves (though the overall pattern of decreasing tau with increasing Q is apparent). Kirchner (2009) used a quadratic equation, which is nicely smoother, but as the authors point out, may not be as flexible as one would like when analyzing many watersheds. There are alternatives, and a cubic spline might be a good choice. The maximum number of knots in the spline can be set such that curves aren't too wiggly and the general overall pattern is not lost in over-fitting at the local level. For example, packages for fitting generalized additive models (GAMS) that use cubic splines could be applied to fit locally smoother curves to the data.

We added discussion and a figure illustrating the smoothing cubic spline as an alternative smoothing method: "Alternative flexible functions to fit the recession curve are available, and should perform similarly to the piecewise linear (LOWESS-like) method we used. As an example, Figure 4 shows a smoothing cubic spline fit to binned discharge data along with the piecewise linear fit, with the smoothing parameter again determined by generalized cross validation . . . Both functions fit the data acceptably, though in this particular case generalized cross validation yields an undersmoothed (jagged) cubic spline. In this study, we chose to use the piecewise linear fit rather than the cubic spline because the former gives straightforward estimates of the uncertainty of the fit at each point and because it has more predictable behavior at the extremes of the range of available streamflows. By contrast, linear regression does not represent the trend of the data well, while quadratic regression gives reasonable results for most of the observed range but does not capture the observed flattening of the recession time at both extremes (Figure 4)."

*3. Abstract, lines 4-5: "However, it..." The pronoun "it" should be replaced by the thing it is representing, because as it stands, "it" could refer to "the pattern of streamflow recessionâ€İ or the â€{œ*relationship between watershed runoff and watershed storage".

4. Abstract, lines 5-6: The authors claim that "...it has not been ...related to independent assessments of terrestrial water storage", yet later in the introduction the authors cite Brutsaert (2008) as comparing recession-inferred storage changes to storage changes inferred from well data. The authors should soften their statement in the abstract that independent assessments have not been made prior to their study.

We rephrased this sentence to remove the ambiguity and take into account the contribution of Brutsaert (2008).

5. p. 1831, lines 13-19: The authors discard the "lower envelope" approach to fitting because, in part, it involves arbitrary thresholds. While this is true that there are practical issues with its application (namely because of noise and error in the data), there is a physical argument to the lower envelope method (Brutsaert and Nieber, 1977). The authors should discuss this argument and explain why they believe they are justified in not using a lower envelope method in terms of their conceptual model of the system. The difference between the lower envelope methods and fitting a curve through the entire data cloud using a least squares error procedure is not simply a matter of technique, but of interpretation of what the data represent.

We now elaborate on this point: "Brutsaert and Nieber (1977) chose to fit the lower envelope of $\log(-\dot{Q})$ also on physical grounds – to select conditions under which groundwater flow is dominant, as opposed to other modes of flow with shorter recession timescales. In this study, our interest is in the behavior of total streamflow, not in the groundwater component as such. For this objective, averaging all streamflow data that meet the selection criteria is more appropriate."

6. p. 1832, lines 19-22: The authors cite Rupp and Selker (2006a) when referring to bias in estimating the dQ/dt = f(Q) relationship when the recession timescale is of the order of the time interval of the discharge data. While this is a valid citation, a more detailed discussion of this bias and exact analytical expressions for this bias are given in Rupp and Woods (2008).

We now include the additional reference.

7. p. 1839, lines 1-4: How was the reference discharge (Q0) chosen to calculate S - S0 from eq. (7)? While it can be arbitrary if only anomalies are of interest, it could be informative to estimate the magnitude of the dynamic storage (S-S0) using the generated tau(Q) functions to check if the estimated dynamic storages are consistent with expectations.

Changing Q_0 only shifts the computed storage S by a constant. The calculated dynamic storage – e.g. the change in storage between any two given times, or between any two flow rates – is not affected by the choice of Q_0, which is why we say this choice is arbitrary.

8. p. 1839, lines 13-16: Precipitation, snowfall, evaporation: Were these annual means? Please specify.

We now specify that annual means were used.

9. p. 1839, lines 13-16: Why was stream length divided by basin area (L/A) or (L/A)^2 not tested as predictor variables? This ratio appears in the expression for the recession time constant in many analytical equations for recession discharge (see review Rupp and Selker, 2006b).

We now report that “Ratios of channel length to drainage area, which figure in many analytical expressions for recession time constants of idealized aquifers (Rupp and Selker 2006b, Table 3), also were not significant predictors.”

Furthermore, how about the ratio of evaporation to precipitation, and a dryness index (potential evapotranspiration over precipitation? Plotting these two ratios against each would give the reader a better idea of the range of different watershed climate types in the data set (beyond the map of the US). See for example, Milly and Dunne (2002).

Figure 1 now shows the mean discharge per unit area of each watershed, as an indication of the range of climate moisture types represented.

10. p. 1842, lines 11-12: Channel length is listed twice. Remove one of them. Further study is needed to determine the likely magnitude of this bias (perhaps by determining $\tau(Q)$ separately for individual recession events and then averaging) and how best to correct for it in streams like the ones in our sample.

We did so.

*11. p. 1844, line 2: As the authors begin the Discussion section, it would be helpful here to remind the reader what is meant by â€{œ*inter-stream variability in the recession curveâ€İ, by stating that it is the variability in tau(Q). E.g., â€{œIn our sample, inter-stream variability in the recession curve time constant tau(Q) was . . .â€İ

We have changed this sentence accordingly.

*12. Fig 7. The axis labels say â€{œ*seasonal storageâ€İ and â€{œinterannual storageâ€İ, but the figure captions say the standard deviation of the seasonal cycle and the standard deviation of the interannual variability. Please be more clear as to what is being plotted. If it is standard deviation, then the the axes labels should say standard deviation. It would be clearer if the caption said something such as â€{œstandard deviation of (a) monthly storage anomalies and standard deviation of (b) annual storage anomaliesâ€İ, if this is, in effect, what is being plotted.

We have modified the caption to improve clarity.

3 Review 2:

Unnecessary and unclear words like "coterminous USA" (bordering to the USA?) are superfluous and not found in dictionaries.

We have added a definition for those unfamiliar with the term.

Data are "binned" or sorted in "bins", which makes the reader think of dust bins, instead of "sorted in classes".

We followed the terminology of Kirchner (2009).

Also equations are written in another manner than commonly used, particularly in most of the cited references (example Ã" (Q with a point on it instead of dQ/dt).

We adopted $\dot{Q}$, etc. for time derivatives because in this paper they often appear in ratios with other quantities, where the longer form dQ/dt would be awkward. We added a note at the beginning of our presentation to alert readers to our notation.

The authors used hourly streamflow values instead of daily values for more accuracy but they donâ€^{TM}t seem to realize that these data are not "discharge measurements" or "measured discharges" or "streamflow measurements" and are not "observable as river discharge" but are all determined from measured water levels at gauging stations using more or less unsecure rating curves. So the wanted accuracy may be only a computational one.

We do not claim that hourly streamflow values are more accurate than daily ones, only that "Using hourly, as compared to daily, streamflow data enables the selection of low-evaporation periods and avoids bias in recession time estimates at higher flow rates when the recession timescale $\tau = -Q/\dot{Q}$ is of order 1 day."

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Is "the conceptual pool that supplies streamflow during dry periods" (Abstract) baseflow as meant in most of the cited references, thus groundwater outflow, which is indeed the main contributor to streamflow? Baseflow supplies flow not only during dry periods but practically always!

The fractional importance to streamflow of storages that contribute baseflow at a given watershed is largest during dry periods; we do not say that they make no contribution during rain events.

What makes the authors assume in lines 22-25 of page 1831, that "time scale for streamflow generated within the watershed to reach the gauge is not much more than one hour ..."? At least subsurface flow will take much longer and baseflow response to rainfall will not consist of rainwater of this event but of groundwater pressed out because the hydraulic head of the aquifer is increased by infiltration and percolation.

We are referring to the timescale of streamflow response to water input during a rain event, not to the age of the streamflow water, which may indeed be much greater. We have made our statements more specific: "we consider only small watersheds ($<$100 km^2), so that the lag between runoff generation within the watershed and streamflow at the gauge is not much more than an hour and the measured discharge gives a reasonable estimate of hourly runoff ... To account for delay between runoff generation and streamflow at the gauge location, we followed Kirchner (2009) in estimating this lag for each basin from the position of the maximum lagged cross-correlation between precipitation and $\dot{Q}$. ..."

Also the used predictors (point 2.5) are lacking physical significance. As recession flow is mainly outflow from the saturated subsurface zone (groundwater) it is essentially influenced by geology and aquifer properties (evidently not considered) and much less a function of the chosen surface parameters. Surface water storage has not much to do with flow recession as soil moisture is adhesive and hardly released as runoff.

The set of predictors used was limited by the available data for our sample of watersheds, which did not include many geology and aquifer properties that may indeed directly influence the shape of the runoff curve, while whenever possible including variables shown to be important in previous studies. We agree that this is not ideal, and say in the Discussion section that "Intensive studies of flow pathways in research watersheds as well as studies of large samples of small gauged watersheds with watershed properties estimated from remote sensing and other distributed data sets can help characterize the link between watershed geology and morphology, on the one hand, and stream hydrology as reflected in the recession timescale, on the other, on a regional to global scale."

Likewise it is erroneous that high forest cover leads to longer recession time (page 1844, line 15ff). The presence of vegetation may foster the retention of water (line 19) but the retention in the canopy (interception!) is lost by evaporation and water retained in the root zone (line 20) will not be released to the river but consumed by the plants. Particularly, deep rooted trees will consume groundwater and lower its levels. The reduction of surface evaporation by vegetation shading is normally highly surpassed by evapotranspiration from the plant surfaces. Thus, contrary to the assumption in the paper, vegetation and forests speed flow recession (s. articles: Federer, C.A., Forest Transpiration Greatly Speeds Streamflow Recession. Water R.R., Vol 9 No 6, 1599-1604, 1973; R. Johnson, The forest cycle and low river flows: a review of UK and international studies. Forest Ecology and Management, 109,1-7,1998 or R.N. Weisman, op.cit. and many others).

We now address these points in our treatment of the impact of forest cover in the Discussion: "High forest cover, typically associated with moist conditions, was associated with longer recession time . . . The positive correlation between forest cover and recession timescale was found only at relatively high flow rates. Vegetation cover and density has a major impact on the spatial organization of soil moisture. The presence of vegetation fosters the retention of water in the canopy, litter layer, and root zone, which leads to slower drainage and therefore longer recession timescale soon after storms. . . . the high transpiration levels of forests tend to drive down deep soil moisture during dry spells and reduce summer low flows, presumably corresponding to shorter recession timescales (Federer 1973; Johnson 1998). These impacts should be less pronounced in our analysis because we excluded periods with high evaporation when computing recession curves."

Ignoring basic processes of flow generation, this article is not really a hydrological work but rather a computational one.

The questions treated in this article are those of watershed-scale hydrology, namely (from our Introduction)

"(1) What is the variability across streams of the recession timescale at different flow rates? How much of this variability is correlated with factors such as climate regime and topography?

(2) How does the variability in basin water storage inferred from streamflow recession curve analysis compare to basin water storage variability inferred from other, independent methods?"

Due to the continental scale of the data we use, our analysis, taken on its own, cannot easily discriminate specific runoff generation processes. However, we hope and expect that our analysis and tools, along with observations and modeling at smaller scales, can be integrated to provide a physically based picture of hydrologic variability.

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