Landscape structure and rainstorms swing the response of recession nonlinearity

SUMMARY

This paper examines variability in streamflow recessions from and across 19 catchments in Taiwan. Recessions are characterized by the power-law recession parameters *a* and *b* in $-dQ/dt = aQ^b$. Differences in these parameters among recessions from a single catchment are compared to what are effectively antecedent moisture conditions using, as proxies, variables such precipitation amount and duration, discharge at the beginning of the recession, among others. Differences in parameters across catchments are compared to landscape properties, such as catchment area, shape, drainage density, stream length, among others. Parameter *b*, a measure of nonlinearity (where $b = 1$ indicates linearity) is found to increase with antecedent moisture in some basins but decrease in others. Large basins tend to show the former response, while smaller basins show the latter. In general, the smaller basins show the strongest relationship of *b* with landscape properties: e.g., *b* decreases with increasing drainage density in these basins. A hypothesis related to the degree of landscape heterogeneity in a basin is given for the contrasting responses between smaller and larger catchments, and two types of smaller catchments.

GENERAL COMMENTS

The main contribution of this paper is the identification of different responses of recession shape (as characterized by *b*) to antecedent moisture and the apparent connection of these different responses to landscape properties. This is an important finding and may help explain contrasting results from other studies. Ultimately, I think this work could and should be published.

However, the paper would require major revisions as there is one serious issue and a couple of weaknesses, which I describe below.

1. The most serious issue in the paper is the faulty analysis of the parameter *a*. The fact is that no physical significance can be ascribed to changes in *a* when *b* also changes concurrently, therefore the paper's interpretation of the variability of *a* in this paper is flawed.

A problem arises from the units *a*, which change as *b* changes, as such the paper makes a nonsensical comparison of values of *a* with different units. One consequence of the scale dependence of *a* on *b* is that the reported differences in *a* among recession events is dependent, even in a relative sense, on the units the authors use for discharge *Q*. If this study were to use units other than mm hr^{-1}, not only would the relative magnitudes of the differences change, so could the sign of the difference (while zero change is also possible given the correct units). If the reported differences in *a* had a physical significance, simply changing the units shouldn't change the physical interpretation. If in doubt, I suggest the authors redo some of their analysis after changing the units of streamflow from mm hr^{-1} to km hr^{-1} and nm hr^{-1} to see the effect.

This unit dependence of *a* on *b* is why, for example, Tashie et al. (2020b) and others fix *b* within a catchment and estimate *a* for each of the catchment's recessions. However, this does not solve the dilemma of comparing *a across* catchments where the catchments have different values of *b*.

I recommend the authors look to Dralle et al. (2015) and Biswal (2021) for further discussion on the relationship between the power law coefficients.

2. Although the authors reference various papers that have empirically examined the relationships between the power law recession parameters and environmental factors, very much has been published on these topics that is not referenced including relatively recent work (e.g., Tashie et al. 2020b). The paper should have a more comprehensive summary of prior work, followed by a clearer statement of what is still poorly understood, and finishing with what this study proposed to do to address one or more outstanding questions. While the intro does this to some extent, it is not sufficient.

A very valuable contribution would be, possibly in tabular form, a list of those environmental factors considered along with the studies that have found positive/negative/no relationship between these factors with recession parameters. This would clearly illustrate how much has been done and, hopefully, demonstrate why yet another study of this type is still necessary.

Note that while I have referenced numerous papers in this review, they do not include many empirical studies of recession parameters.

3. The paper would greatly benefit from a discussion of what theory would predict for the influence of environmental factors on recession parameters. For examples, Figures 2 and 3 in Rupp and Selker (2006b) show how initial water table height (i.e., antecedent moisture), drainage density, hillslope slope and hillslope length/height ratio determine *a* and how vertical heterogeneity and initial conditions influence *b*. Are the results of this paper consistent with theory? If not, why not? Perhaps theory breaks down outside of the idealized conditions upon which the theory is based? Theoretical work has also shown how planform shape and downstream boundary conditions (e.g., Troch et al. 2013) as well as a draining vadose zone (Luo et al., 2018) and drainage network geomorphology (Biswal and Marani 2010) can affect recession parameters.

LINE-BY-LINE COMMENTS

L1: The title could be improved. First, the meaning of the word "swing" in this context is unclear. A pendulum swings. I don't think that is what the authors mean to say. Suggested replacements for "swing" are "modify" or "alter". Second, "the response of recession nonlinearity" is also unclear.

L12. Is it 260 sets of recession parameters per catchment, or in total over all 19 catchments?

L29-31: This sentence is out of place and not particularly relevant. The previous sentences are about analyzing individual recession events, while this is about projections of future rare rainfall events. Unless a stronger link is made, I would delete this sentence.

L32: Define *Q* and *t* in $-dQ/dt = aQ^b$.

L34. I have an issue with calling *a* the recession rate. The units of *a* vary with *b*, so are not universally consistent with a "rate".

L38-41: It should be stated where discharge has been normalized by catchment area. From Figure 2, I take it that is has for the authors' analysis. The authors should be careful when discussing results from other studies that may not have normalized discharge. Brutsaert and Nieber (1977), for example, do not normalize discharge prior to comparing *a* from different basins with drainage density and network length but clearly dividing by area first would affect their values of *a*. Brutsaert and Nieber (1977) show an inverse relationship of *a* with total stream length (their Fig. 9) with seems to contradict the attribution to Bogaart et al. (2016) that *a* has a positive correlation with total stream length. Also, flow-path length and height need to be clearly defined.

L42-43: Please cite the "few studies".

L46-47: I believe all the references except this one concern empirical studies, which makes this reference to theoretical work out of place. The statement is also unclear without more context. How does spatial heterogeneity affect whether increasing the steady-state recharge rate (I assume the authors mean a steady-state recharge rate immediately prior to the beginning of the recession) increases or reduces *a*? Maybe 1-2 paragraphs devoted to theoretical work could be included (see COMMENTS above).

L50: Start new paragraph at "Due to…".

L76-77: Clearer definitions of L, H, and G are needed. Table S1 gives definitions in the footnotes, but they do not appear to be consistent with what is in the main text. The text says "flow-path length [L]" but Tables S1 says L is total length of the drainage networks. Are they the same thing? If so, the text on line 40 is confusing because total stream length and flow-path length are treated there as if they are distinct measures. The text also says "gradient (G) above the nearest channel" but the Table S1 footnote says "G is the average gradient of the drainage networks". These do not sound like the same thing.

L86: Theissen polygons can be quite poor for interpolation of rainfall, particular in regions with sharp rainfall gradients such as in Taiwan. Is there additional information that can aid the interpolation, even a rainfall vs. elevation relationship? Are there any gridded climatologies (such as PRISM maps) that can be used to improve interpolation? What is the rain gauge density? Can the gauge locations be shown on a map?

L104-106: Dralle et al. (2017) could also be cited as an example of an examination of the effects of methodological choices.

L127-L131: How many recession events were ultimately included per station? Were they the same events in time per station?

L134: How exactly were d*Q*/d*t* and *Q* estimated from the data?

L152: Define elongation (ELO). How is it calculated? This should be explained in the methods section.

L145-146: This sentence is confusing. First, please explain what it means that “the two parameters are interactively dependent”. Second, why are they “particularly” dependent “when the number of points is huge”. Lastly, what does this dependence have to do the ordinary least squares method?

L152-153: If the properties of W8 and W18 are described here, then so should they be for W5. How is W5 distinct from W8 and W18?

L153-154: It is meaningless to rank in descending order of *a* if all the *a* do not have the same units. I suggest ranking them in order of *b*.

L154-155: Why are the mean and median of *b* stated for W8 but not for the others?

L155-156: This sentence is confusing and I wonder if there is an error. Why would the median > mean of the recession rate (*a*) imply that the distribution of nonlinearity (*b*) is right-skewed? Fig 2c doesn’t actually give the mean of *a*.

L156-157: A plot of *b* vs storm magnitude and/or Q_{ini} for each of these three watersheds would be very helpful to illustrate the point being made, and show how strong these relationships actually are.

L157-158: I think the opposite response of W8 and W5 to storm magnitude being associated with differences in landscape properties should be left for the Discussion, but it is OK to foreshadow the discussion here. If this sentence is kept, I would follow it by saying that this apparent association will be explored further in the Discussion section.

L160: Units missing for *a*.

L167-170: Jachens et al. (2020) argue that this is not necessarily true; the point cloud fitting method may not reveal the “general” or average recession.

L187: It should be noted here that P and Q_{ini} are effectively uncorrelated (*r* is only 0.11 and is not significantly different from zero). This seems like an important point and worth a little more discussion. How exactly is P calculated for each recession event? Over what time period is it totaled? This should be described in the methods.

L188: What are these "presumed thoughts". I would reiterate the thoughts here or leave this phrase out.

L191: Describe how is L/G is a proxy for the interaction of landscape and climate. References?

L214: Circular logic. The authors' definition of non-linearity is already that the value of *b* is not equal to 1. Also, values less than 1 are non-linear.

L220: It would be clearer to simply say highest and lowest values of *b*. Or "the most and least nonlinear cases are…".

L230-232: But see also Sharma and Biswal (2022).

L238: "Complicates" is not a good term here. I think the authors mean to say that heterogeneity may increase with catchment area because of the possibility of including a wider range of subsurface conditions. This sentence should be rewritten for clarity.

L259: This is the first time any "Type" of catchment is mentioned. The classification of catchments into Types needs to be introduced more clearly. I would start a new paragraph and direct the reader to the upper half of Fig 9 (which will mean reordering the figures).

L271: I suggest rewriting as "The positive relationship of *b* with both H and L…."

L272: I don't understand this sentence. What are these "blocks"? How does higher H and L imply greater prevalence of such blocks?

L273-274: This is an important idea the authors introduce. Do only catchments with short AND gentle hillslopes have large riparian areas? How exactly does a larger riparian area reduce heterogeneity?

L276-277: Do these large basins have something in common other than being large? Almost all these large basins have their headwaters at the highest elevations and most of the smaller basin are on the west side. These smaller basins are also mostly in the rainshadow, whereas the larger basins receive much more rainfall. What role could these factors play?

L279: "The large deviation" of what? Please be explicit.

L303-314: I think it worth noting that *b* is treated as a constant here throughout a single recession event, though it has been empirically shown that it can change over the course of an event (e.g., Rupp and Selker 2006b; Tashie et al. 2020a). Also, groundwater hydraulic theory predicts that *b* can change over time (Brutseart and Nieber 1977). For a horizontal aquifer, this change depends on the initial conditions (Rupp and Selker 2006a) but the change in *b* happens relatively early and *b* becomes relatively steady as the recession progresses. In a sloping aquifer and/or one that is vertically compartmentalized, *b* can change over a lengthy part of the recession (e.g., Bogaart et al. 2013; Roques et al. 2022). How this theoretical idealized hillslope behavior might manifest in a complex catchment has still not been not well-described, however.

L306: This is an interesting idea that the pervasive saturated overland flow reduces the nonlinearity of recession. Two issues:
1) What field evidence is there of this pervasive saturated area?
2) I expect this saturated area is decreasing in time, possible very quickly. How would this affect *b*? Thinking along the geomorphological lines of Biswal and Marani (2010), might this not increase *b*?

L309-311: I'm not sure I follow this. How would a large rainstorm "connect" saturated zones of slow reservoirs that were otherwise not draining to a stream? What may be happening is that during large storms there is a wider range of active quickly to slowly draining sources (the fast ones being the ones activated during the large storms). This heterogeneity of sources can increase *b*. This appears to be what the authors say in the sentences following this one.

L326: Is there any field evidence for these perched storages?

L329: I wouldn't say "unpredictable". Predictability is not the issue here.

L340: I would not say "pretty diverse". Is "inconsistent" what is meant?

L346: What is meant by "higher hillslope hydraulics"?

Figure 2: The panels in column c clearer show discretization artifacts that visually hide the underlying relationship at low flows. A way to remove these artifacts was first proposed by Rupp and Selker (2006b) and modifications were made by Roques et al. (2017) and Guo et al. (2022). I suggest the authors apply one of these methods.

Figure 5: State in caption whether all stations and all events are shown in this plot.

Figure 7: Say in caption which symbols are for Type A, B, and C, basin.

Table S1: Some of these basin average drainage network gradients are very large (as high as 0.75). Hillslope gradients must be yet larger. What are the implications for subsurface flow?

Table S1: L/G is given as having units of m^2. If L has units of m and G is unitless, L/G must have units of m.

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