Responses to comments from Anonymous Reviewers 1 to 4

Key:
RC1 – Comments from Reviewer 1, **RC2 –** Reviewer 2, **RC3 –** Reviewer 3, **RC4 –** Reviewer 4.
AC – Author comments

Questions about Table 1

RC1: The number in line 418 appears to contradict what is in Table 1, which says the difference is 17.1 +/- 3.4. If both numbers are correct I think you need to be clearer about what each number is so readers know why they are different.

RC2: L24 – P11531 L6: explanations do not match with Table 1. There are several apparent typos and errors in Table 1. Section 4.3:

RC3: Table 1: Could this table be presented as a bar plot?

AC: The table itself is accurate, however the narrative reflects a previous iteration of Table 1 that subsequently changed - we apologize for this oversight as we did not update the narrative with the updated Table 1. We will amend line 418 to reflect values given in Table 1. That is, the difference in flow between the two watersheds during the flip era should be 17.1 ± 3.4 mm and not 15.7 ± 3.2 mm as currently stated. We will check Table 1 and ensure that there are no other typos, and that the narrative and the table are identical in the values the report. In response to the suggestion by RC3 to convert the table to a bar plot – we will certainly explore this idea. However, we suspect that it will be hard to capture all the information contained in the Table within a single barplot.

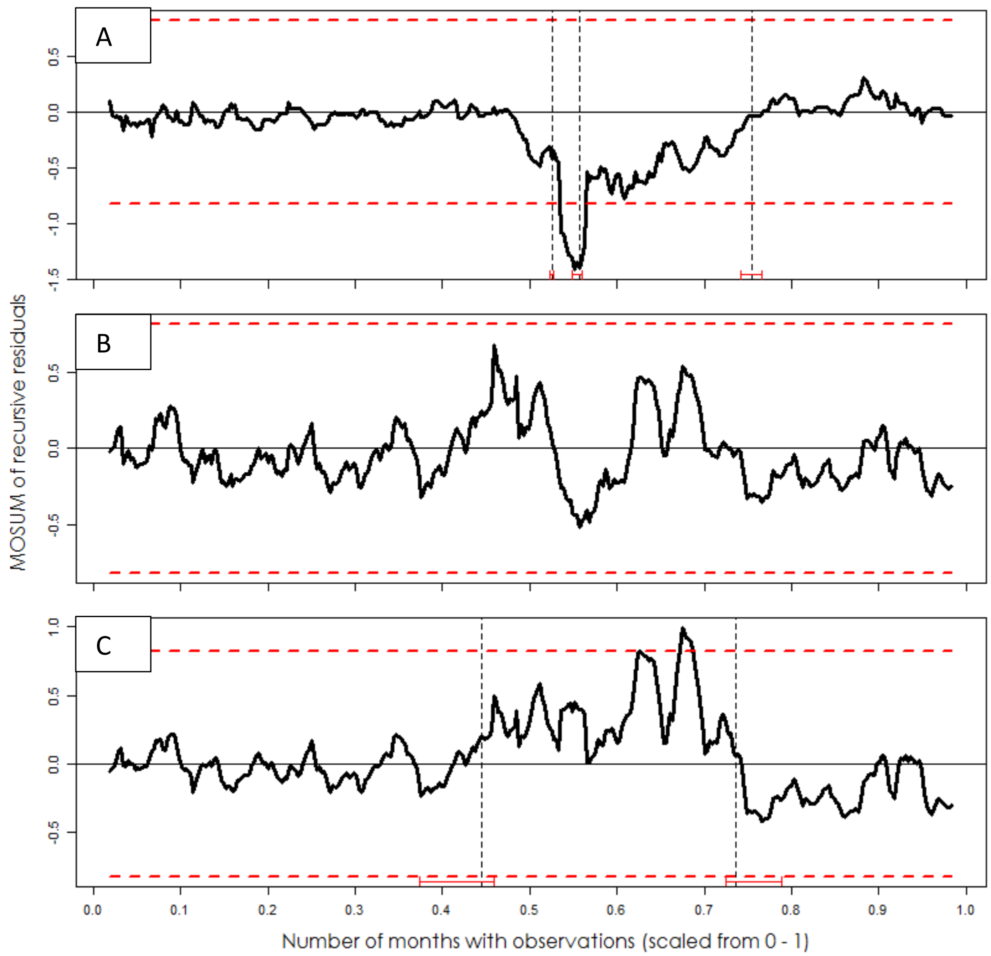
Questions about MOSUM and CUSUM

RC1: Figs 4 & 5 are confusing. In the caption on Fig 4 you say what constitutes a structural break but none of the vertical lines in either figure appear to fit the criterion. This is especially the case for the third vertical line in Fig 4 and both of them in Fig 5. This really needs either correction or a clearer description of what you are looking to say there is a structural break.

RC3: Figure 4&5: Could these figures be combined in one figure?

AC: Briefly, because one is analyzing moving sums of recursive residuals (MOSUM), the point where they cross the 95 % confidence boundary is not always the location of the structural change. A revised caption and additional explanation of the MOSUM and CUSUM calculations will be included to provide clarity of how the structural breaks are analyzed. Additionally we will combine Figures 4 and 5 so they are on a common timeline for better clarity. Also, Refer to responses to Student Reviewers #1 and #2.

<u>A proposed revised MOSUM Figure (figures 4 & 5 combined):</u>



New Caption for the MOSUM figure

Plots of moving sums of recursive residuals (MOSUM) for linear relationship between monthly flows of watersheds WS77 and WS80 (A), WS77 monthly flow and monthly rainfall (B), and WS80 monthly flow and monthly rainfall (C). A shift of the MOSUM outside the 95 % confidence intervals (long horizontal dotted lines) is indicative of a structural break in the linear relationship. The vertical dotted lines on (A) and (C) are estimated breakpoints (break dates). The corresponding small horizontal lines that cross each break date are the respective 95 % confidence intervals for each break date. Because the analysis is on moving sums, the location where the MOSUM cross the 95 % confidence boundary is not always the location of the breakpoints. Also, when the MOSUM return inside the 95% confidence boundary, it does not mean the relationship has regained the previous structural stability. There are three break dates on (A) corresponding to March 1993, March 1994, and April 2004. The first break date on (C) corresponds to June 1990 while the second break date corresponds to April 2003.

RC2: The authors used the MOSUM (moving sum of recursive residuals) to characterize structural changes in monthly rainfall-runoff relationships at each watershed. And they concluded that there was no structural change in WS77 (section 4.1) during the hurricane event, although the hurricane event resulted in 38% trees loss and 54.5% basal area loss (Table 2), as well as at least 50% monthly runoff (Q) increases (Table 1). Authors are simply saying that the 50% increase in monthly Q was within the uncertainty ranges in the simple precipitation-based rainfall-runoff model. This is a just wrong conclusion. The major problem of this simple linear model between monthly precipitation (P) and runoff (Q) is that the seasonality of Q in these watersheds was not driven by P, but by evapotranspiration (ET). Your data also show that about 70-80% of monthly precipitation is not coming as Q (Table 1). Although there would be some portion of P would be lost in groundwater by-pass at these low-gradient watersheds, significant portion of P – Q would be attributed to ET. As far as I know, the precipitation in this region is evenly distributed throughout the year; therefore the seasonality in Figure 7 was mostly driven by ET and resulting groundwater dynamics. In other word, you cannot simulate monthly Q right without considering seasonal ET component. For this reason, there are surely very wide confidence intervals in your model (Figure 5), where the uncertainty in seasonality would override all others. However, there would be no problem to apply the same approach to compare the runoff generations between WS77 and WS80 because the seasonality would be offsetting in this analysis.

AC: The focus of MOSUM on the Q and P relationship was not to accurately predict monthly flow but to demonstrate that the pre-Hugo Q and P relationship significantly shifted or changed after Hurricane Hugo (at alpha level of 0.05). Also, statistical analysis showed that monthly precipitation was a significant predictor of monthly flow (with $p < 0.001$) for both watersheds. The effect of the strength of the relationship is reflected in the large widths of the 95% confidence interval for the two breakpoints (Figure 4c).

The comments by the reviewer on seasonal effects are accurate. However, for the analysis of long-term effects of Hurricane Hugo on the structural stability of the flow-rainfall relationship, the authors chose a MOSUM moving window of 12 months to minimize these seasonal effects.

RC2: A simple solution to this problem is simulating the model at coarser time resolution. At the annual scale, this seasonality issue would not be a problem in your model. However, there will be not so many data points, which may still result in the wide confidence interval ranges. Also, interannual precipitation variability makes it hard to extract meaningful signals from the model. My suggestion for this problem is to see the patterns of annual P – Q in these two watersheds. Even though this approach has been usually applied to mountainous headwater catchments to estimate ET, I still think that this would be effective to detect the changes in hydrologic behavior driven by vegetation changes. In other words, the P – Q would not be strictly equal to ET due to groundwater component in these low-gradient watersheds, however it still give an idea regarding the temporal patterns of ET driven by vegetation dynamics assuming that the changes in groundwater components are minor.

AC: The suggestions by the reviewer outline well-established practices in hydrological change analysis. Both MOSUM and LOESS use analysis windows of 12months or more so seasonality in the flow data is accounted for – these two techniques were used purely to identify relative changes in hydrologic character or structural breaks using the entire dataset. The focus of this manuscript was to characterize relative changes between two watersheds and to introduce the readers of this journal to another method (well established in econometric analysis) for change detection in time series. We reiterate that developing a an accurate hydrologic model to predict runoff from rainfall was not the goal of this study – our goal was to describe relative changes in rainfall-runoff relationships between the two watersheds and not to explicitly model runoff for each watershed. We agree that inter-annual or inter-era variability in precipitation is an important issue, both watersheds in this study are in close proximity and were equally affected by seasonal forcing functions (rainfall and PET). Therefore, in our opinion, further analyses of this variability would not add to explaining the relative changes in hydrologic behavior between the paired watersheds. The reviewer refers to the possibility that inter-era changes in precipitation were not accounted for which might affect the results presented. However, we'd like to counter this suggestion by reiterating that this study only evaluates relative changes in hydrologic character between two watersheds that are adjacent to each other; therefore, inter era changes in precipitation should affect both watersheds equally.

Questions about the seasonality of monthly flows

RC1: Section 4.3 should explicitly state that although there are some difference in magnitude the differences are not significant. Alternatively, it appears that a few of the differences may be significant and it might be worthwhile pointing that out it true. Whatever the case, my concern is that you discuss significance elsewhere with some of the other results so making sure there is no confusion regarding Figs 7 & 8 would be helpful.

RC2: Without the information of seasonal variation in precipitation, it is very hard and impossible to interpret the Figure 7 and 8. Authors need to show that the seasonality in precipitation did not change during three eras.

AC: With regards to the comment by RC1: the original work did not test for statistical significance in seasonal flows between the two watersheds and among the three eras; instead we chose only to represent the data using monthly means and standard error bars. As per the suggestion of RC2, we will present a figure showing how little variation in measured precipitation occurred between watersheds across eras and seasons. An example of such a graph is presented in the next section.

On pondering the comments from all reviewers, we realized that the seasonal (monthly) decomposition of rainfall and ET data are hydrologically relevant at short-term time scales, the analyses detract from the overall goal of the paper - to show how the hydrologic character between two watersheds changed over decade-long time spans. In other words, while seasonal variations are important, it is the decade long changes in hydrologic character that we would like to focus on in this study. Therefore, we have decided to eliminate any reference to seasonality of flows in the manuscript, a step we believe will lead to a more concise and focused study.

Suggestion to adopt a water balance type approach

RC2: Figure 1 represents the annual (April to March) P – Q data from the famous two paired watershed experiment in Coweeta Hydrologic Lab. Note that vegetation year (April to next March) was used to minimize the storage changes (delta S) (check Ford et al. 2007 for details, published in Agricultural and Forest Meteorology). While WS18 is a long-term control watershed with an oak-hickory forest, WS17 experienced clear-cut in 1940, and regrowth suppression until 1955. White pine planted in 1956 and canopy closed around early 1970s. We can see clear changes in ET patterns. They shows about 200 mm y-1 low during the clearcut period (1941-1955), and about 200 mm y-1 high after canopy closure in WS17) compared to 3 years before clear-cut (1938-1940). Note that this approach is also effective to estimate planted pine growth during the period from 1956 to 1970.

AC: We agree that calculating vegetation dynamics in the form of ET= P-Q is a useful exercise and a way to explicitly quantify ET. However, our study differs from the Coweeta study that RC2 refers to in that simultaneous rainfall data in both watersheds 77 and 80 in our study represent temporally limited but statistically similar datasets. Simultaneous rainfall readings in both watersheds were available only from 1990 to 1997, 2001 to 2008, and 2010 to 2011. Flow data represented a much more comprehenzive dataset and was therefore the obvious focus of our work. The limited rainfall dataset represented only two years in the Pre era, 4 years of the Flip era, and 8 years of the Flop era. An analysis of this rainfall data showed that while the rainfall totals between watershed was not significant (Two Factor ANOVA-Factors: Watershed and Era), but rainfall among eras was significantly different. Additionally, Amatya et al. (2006) examined the spatial rainfall variability of historic 1964-1982 data where 5 randomly spread gauges were used on each watershed:

"*The variation among gauges in each year was very small with an average coefficient of variation (C.V.) of just 3% (Table 4) compared to the variation within years at each gauge with an average C.V. of 12% (Table 3). However, the difference between gauges was recorded as large as 155 mm in 1979 (Table 4). Most of the large differences between gauges exceeding 100 mm occurred for the years with above average rainfall. Although these data demonstrate that on an annual basis using data from a single gauge may introduce errors in rainfall of as much as 8% compared to using aerial average from five gauges for this relatively flat coastal watershed, larger errors may exist for temporal scales of months and events.*" - Amatya, D. M., Miwa, M., Harrison, C. A., Trettin, C. C., and Sun, G.: Hydrology and water quality of two first order watersheds in coastal South Carolina, in: Annual International Meeting, Paper no. 062182, 21 pp., American Society of Agricultural Engineers, St. Joseph, 2006. 11522, 11524

So given that the rainfall totals (P) between the two watersheds in our study were not significantly different based on monthly rainfall totals over the period of record, we did not think that estimating ET based on P-Q in both watersheds would provide additional information beyond that embodied by just using Q. In Figure 1 provided by the reviewer (see below), P-Q was used as an estimate of ET and graphed for two watersheds in Coweeta (P1-Q1 = ET1, and P2-Q2=ET2). A third line showing the difference between the two estimates of ET is also shown. i.e. ET1-ET2. In our study, we (will) show that P1=P2, therefore ET1-ET2=Q2-Q1. Figure 3 in our original manuscript is exactly this (monthly flow from WS80 – monthly flow from WS77)

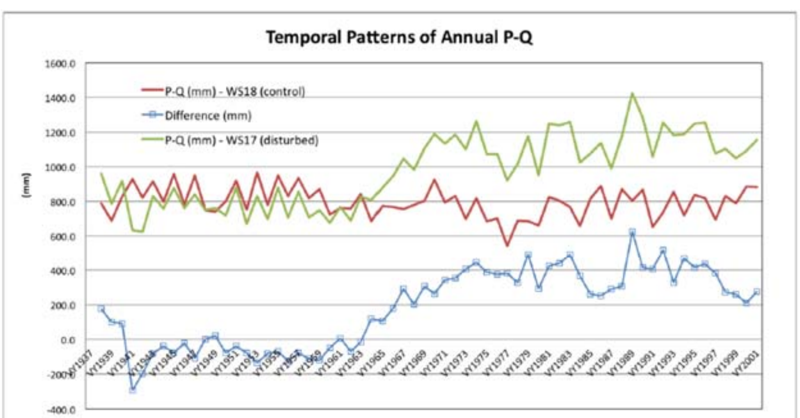
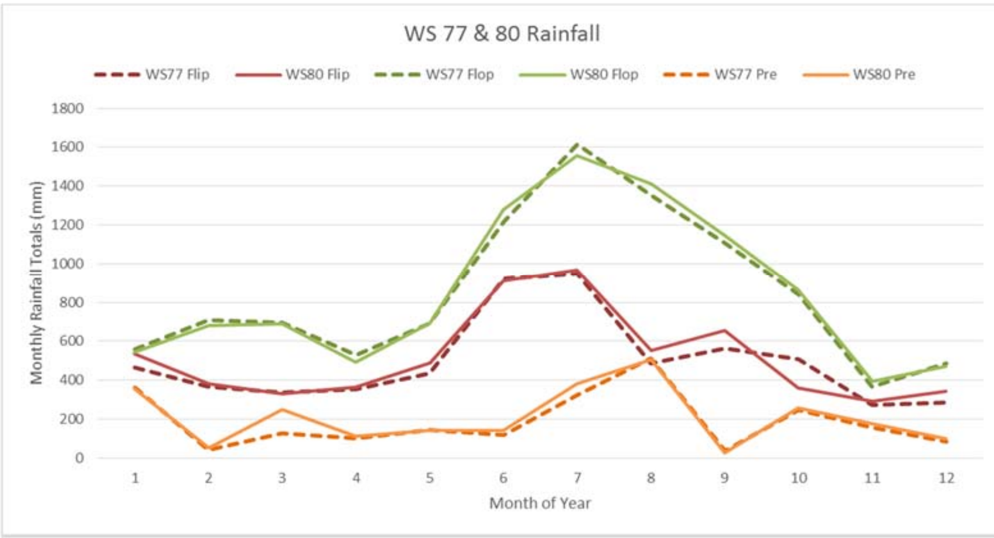


Fig. 1. Temporal patterns of precipitation (P) minus streamflow (Q) at the April-March water year period from the paired catchment experiments in Coweeta Hydrologic Lab (WS17/WS18). Details are in the text.

The reviewer is also correct in pointing out that a water balance method might have limited value where storage (surface and shallow aquifer) and low gradients prevail.

To better illustrate the similarity in rainfall between the two watersheds, we created a chart showing the monthly distribution of rainfall over the three eras. This rainfall represents the dataset of all simultaneous rainfall measurements in the two watersheds. Each point represents the sum of all rainfall that occurred during a particular month for a given era and watershed. However, we must caution that data in the Pre era comprise just two years of data – so each month represents the sum of only two values.



Evaporation, transpiration and groundwater infiltration on low gradient forested watersheds near the southeastern US coast.

RC4: On a related note, one of the concerns I have is that this manuscript falls into the trap of some of the very paired watershed experiments that it criticizes with its inability to distinguish between water losses due to transpiration and evaporation and, I would add, infiltration to groundwater. The discussion presumes that changes in the relationship between rainfall and runoff can all be attributed to transpiration differences due to changes in tree cover but without any discussion of the other factors in the water balance–evaporation and infiltration. I would imagine that changes in tree cover could have an impact on infiltration to groundwater, and because groundwater watersheds may differ from surface watersheds, the statistical analyses would also reflect changes in infiltration. Though I think the authors' attribution of changes in the regression relationships to changes in transpiration is reasonable, I do think the complete water balance in these watersheds deserves more discussion.

AC: we agree that evaporation and infiltration are possible processes that deter from what we might have attribute to transpiration.

With regards to infiltration losses in these watersheds, Harder et al. (2007) reported deep GW or seepage was negligible in these coastal forest system based on other published regional literature. As an example, a more recent study from proximal larger watershed yielded about 5-10% of annual precipitation was lost to groundwater for a moderately well drained upland site (Callahan et al., 2011). Since both the WS77 and WS80 are mostly on poorly drained soils and 35 times smaller in drainage area than the watershed Callahan et al. (2011) studied, we expect even lower groundwater recharge possibly in the order of 2-3% of the precipitation. We have also prepared a short summary of likely infiltration, evaporation and interception processes below to further elaborate our reasoning:

Infiltration: On forested sites, all rainfall infiltrates and water table elevations are generally less than 2 m deep and often less than 1 m, with capillary fringe extending to soil surface (Williams 1978). Shallow groundwater (water table aquifer) is an integral part of runoff generation. Young and Klawitter (1968) and Young et al. (1972) showed that runoff varied from 0-70% of rainfall. Water table position prior to rainfall has been shown to be the best predictor of runoff quantity (Harder et al., 2007; Williams 2007; Eshlemann et al., 1994). Forest canopy influences on the P-ET balance are reflected by the position of the water table (Trousdell and Hoover 1955, Williams and Lipscomb 1981) at any given time.

Evaporation: The roles of surface evaporation, interception and transpiration depend on both vegetative cover and soil drainage. Riekerk and Korhnak (2000) found that removing forest canopy resulted in greater water loss from cypress domes in Florida. Lockaby et al. (1994) found a relation between soil color and soil evaporation from nearly saturated soils and concluded that the albedo of the soil surface was the cause for the differences in soil evaporation in dark and light colored saturated soils when trees were removed. Energy collected by dark soils increased evaporation rates more than transpiration, while interception decreased when forests were cut, resulting in a net loss of runoff after cutting similar to the findings of Riekerk and Korhnak (2000). Alternatively on light soils cutting the forest resulted in net gain in runoff as is normally seen on well drained soils.

Interception: The role of interception may also be decreased on forests close to the coast. The role of evaporation of intercepted water and water vapor condensation on the canopy are both influenced by presence of high humidity near the coast. Hitchcock's (https://www.hobolink.com/s/23a87fa1152f941c134c93963816aab8) monitoring of hourly relative humidity has shown air saturation for up to 16 hours a day. Epps (2012) found that 5 of 10 rain gauges placed under the forest canopy received more annual precipitation that from a nearby open gauge. Epps (2012) concluded that canopy drip made throughfall measurement with stationary gauges unreliable. Near the coast, intercepted rainfall is less likely to evaporate and canopy drip may equal or exceed evaporated interception.

Roles of evaporation, infiltration and interception after Hurricane Impact: Both watersheds were well vegetated prior to Hugo, with the balance of evaporation and transpiration shifted towards transpiration and soil evaporation limited by shading. Canopy interception and crown drip varied with available energy and vapor gradient. Interception was probably more important in dry years (many sunny with low humidity days) and canopy drip during wet years (many cloudy with high humidity days).

The hurricane reduced the number of tall trees but had little impact on smaller trees and increased forest floor litter. The impact of this would be to decrease transpiration and canopy interception but possibly increase understory interception. Although there was a substantial increase in runoff, ET was still the dominant mechanism of water loss from both watersheds. A rise in the average water table would also have occurred which has the tendency to increase the incidence of the capillary fringe meeting the surface and thereby increasing surface evaporation. From 1989-1992 runoff was greater from both watersheds suggesting that the above processes were equally altered on both watersheds. One could also conclude that areas of saturated soil, subject to increased surface evaporation, were also roughly equal. During those years, the greater number of surviving pines and greater pine regeneration on WS77 do not appear to have made much difference to the rate of ET. Salvage logging on nearly half of WS77 probably decreased understory interception by removal of woody debris on that watershed, which also suggests that interception was not a major contributor to runoff differences. By 1993, the rainfall-runoff relationship on WS77 returned to near pre-hurricane levels, but excessive runoff persisted on WS80. We have no direct data on how much basal area on WS77 returned to pre-hurricane levels (other than the satellite observation of reflectance on the experimental forest reaching pre-hurricane levels by 1999) but data collected on WS80 suggest that recovery of basal area to pre-hurricane levels was not complete until 2003. It seems clear that on WS77, leaf area increased with regrowth of the pines, returning ET levels to pre-hurricane levels more quickly than on WS80. We also firmly believe that soil evaporation was reduced by lowered water tables and shading of the soil surface shifting the balance of increased ET towards transpiration.

References:

Young, CE Jr, Klawitter RA. 1968. Hydrology of wetland forest watersheds. Proc. of CUCOH Hydrol. Conference. Clemson University, March 28-29, 1968: 29-38.

Young, CE Jr, Klawitter, RA, Henderson, JE. 1972 Hydrologic model of a wetland forest. Journal of Soil and Water Conservation 27 (3):122-124.

WilliamsTM 1978. Response of shallow water tables to rainfall. P 363-370 IN Balmer WE Proceedings :Soil Moisture-Site productivity symposium USDA Forest Service Southeaster Area State and Private Forestry.

Williams, T.M., 2007. Evidence of Runoff Production Mechanisms in Low Gradient Coastal Forested Watersheds. ASABE Paper No: 072228, ASABE, St. Joseph, Michigan.

Riekerk, H., and Korhnak, L. V., 2000, The hydrology of cypress wetlands in Florida pine flatwoods: Wetlands, v. 20, no. 3, p. 448-460

Lockaby, BG, ThortonFG, Jones RH, ClawsonRG.1994. Ecological responses of an Oligotrophic Floodplain Forest to Harvesting. Journal of Environmental Quality 23: 901-906.

Epps, TH. 2012. Assessment of the rainfall response of headwater streams in lower coastal plain South Carolina. Masters thesis, Biosystems Engineering program, Clemson University. May, 2012.

Eshleman, K., J. Pollard, and A. O'Brien, 1994. Interactions Between Groundwater and Surface Water in a Virginia Coastal Plain Watershed, 1. Hydrological Flowpaths. Hydrological Processes 8:389-410.

Harder, S. V., Amatya, D. M., Callahan, T. J., Trettin, C. C., and Hakkila, J.: Hydrology and Water Budget for a Forested Atlantic Coastal Plain Watershed, South Carolina, J. Am.Water Resour. Ass., 43, 563–575, 2007.

Callahan, T.J.; Vulava, V.M.; Passarello, M.C.; Garrett, C.G. 2011. Estimating groundwater recharge in lowland watershed. Hydrological Processes. doi: 10.1002/hyp.8356.

Specific comments

RC2: P11520 L5 and P11524 L21: should be 'streamflow' P11530 L18: 'WS80' typo P11530. (AC: We will correct these).

RC2: L24 – P11531 L6: explanations do not match with Table 1. There are several apparent typos and errors in Table 1. (AC: See our response to "Questions with Table 1" above).

Section 4.3: Without the information of seasonal variation in precipitation, it is very hard and impossible to interpret the Figure 7 and 8. Authors need to show that the seasonality in precipitation did not change during three eras (AC: See our response to "Questions about seasonality of monthly flows" above).

RC2: L11533 L12: Table 9? (AC: should read Table 2)

RC2: L11534 L5: space needed (AC: ok).

RC2: L11534 L18 – L20: I don't agree to this conclusion as mentioned above. I think this is simply wrong (AC: we will attempt to clarify this. Please also refer to our responses to "Questions about MOSUM and CUSUM" above).

RC2: P11535 L15 – L17: Provides with scientific names. (AC: ok)

RC2: P11535 L4: Traditional temporal analyses of water yield from paired catchment experiments (e.g. Coweeta Hydrologic Lab) or artificial disturbance would be good references here (e.g. Kuczera curve Kuczera 1987 in Journal of Hydrology) (AC: We will include these references – thank you!).

RC2: P11535 L26 – P11536 L4: The seasonal comparisons between three eras would be right only after showing the seasonal patterns in precipitation data do not change throughout the periods. Table 1: (AC: We agree and will correct. Also, see our response to "Questions about seasonality of monthly flows" above)

RC2: Many numbers in Table 1 are wrong. Check them carefully. It seems that the change percent values (%) were calculated from the each previous era. It would have been better to calculate based on the beginning period (1969-1976). (AC: We see one typo in the narrative related to Table 1 but are not clear what the reviewer finds wrong with the table otherwise. We will ensure however that all the numbers in the table are consistent with the narrative. We will also calculate all changes as a percentage of the beginning period.

RC2: Table 2: define HWD. (AC: ok)

RC2: Figure 2: The shapes and boundaries of watersheds are not good. (We will redo Figure 2)

RC2: Figure 3: Why there are so many zero values in the graphs. It means that the monthly flows between WS77 and WS80 are exactly the same, which I think is impossible. Check the values again. Insert the vertical lines for three different eras (AC: zero values correspond to months when there are no flows in both WS77 and WS80 – typically late summer. WE should make this clear in narrative and figure caption. The reviewer is correct that otherwise this would be impossible. There was only one month of the 376 analyzed when flows in both watersheds were identical [0.01mm] we will show lines to demarcate the three eras in Figure 3)

RC4: A major concern for me deals with some of the terminology used. To a geomorphologist, "flow reversal" means a literal reversal in the direction of streams. I would advise the authors to be very careful about this terminology, referring instead to a reversal in the relative magnitude of stream flow between the watersheds. Relatedly, the terms "flip," "flop," "catastrophic shift" (in the abstract), and references to discrete, threshold like changes (as on line 10 of page 11538), all imply a dynamic that I do not believe these watersheds exhibit. The phrase "catastrophic shifts" (unfortunately overused or improperly used in many recent papers) implies the existence of alternate stable states, with sudden changes between them, which these watersheds do not exhibit. I would even argue that these watersheds don't exhibit threshold behavior. Instead, the relative difference in stream flow between the two watersheds undergoes a "reversal" because its sign changes, but the approach to each sign change is gradual and continuous (even across the sign change), due to the gradual regrowth of trees.

AC: Thank you for these great suggestions! We recognize that "flow reversal" needs to be replaced with a "reversal in relative magnitude of streamflow". We will also remove the term "catastrophic shift' from the abstract as we too agree that this might be an overly dramatic term for what is a gradual but distinctive process occurring over decadal-timespans. In the same light, we will only use the term "threshold like" in reference to the relative change in streamflow between watersheds and not in the context of the hydrologic response of a specific watershed. In other words – the threshold would only refer to the relative change in stream flow because of a theoretical juxtaposition of flows from two watersheds.

RC4: 1. In a few places in the manuscript (e.g., line 20, p. 11534), changes in runoff for the watersheds are referred to in uncertain terms (e.g., "suggests a net decrease in runoff). But aren't these analyses based on actual runoff data? Refer to the actual data in more certain terms wherever possible.

AC: The uncertainty in the language reflects the finding that MOSUM analysis showed structural changes in runoff production only in WS80, while the vegetation analysis showed regeneration potential of pine tree in WS77 (therefore net decrease of runoff production) and removal of tall/older trees in WS80 (therefore net increase in runoff production). While the results of runoff and vegetation data analyses reinforce relative flow differences between watersheds, the results also suggest mutually distinct physical processes on the two watersheds. We will restate this sentence to explicitly state that MOSUM analysis only detected change in WS80, while vegetation analysis suggest changes in both WS77 and WS80. However, the changes suggested by the vegetation analysis mutually reinforce runoff differences between watersheds – i.e. the act in opposite directions

RC4: 2. Second paragraph of Discussion: This paragraph is long difficult to wade through and would benefit from revision. I think what it is trying to say is that pine grew faster in WS 77 than in WS 80, causing runoff to return to normal sooner than in WS 80. The paragraph needs to be more clear and probably shorter.

AC: Thank you for this suggestion: we will shorten and make the paragraph easier to read.

RC4: 3. Summary: Please check the dates of transition and revise accordingly. In the first paragraph, 1993, 1992, and 1990 are all invoked as being the year of transition.

AC: The three dates range depending on the analysis used to detect change and are detailed on Pg. 11530 lines 10 through 17.

RC4: Line 8, p. 11537: Three methods of flow analysis? LOESS, MOSUM, and what was the third? Maybe I missed something, but I don't believe any results were presented for a third method of flow analysis.

AC: The three methods of flow analysis were: 1) MOSUM: rainfall-runoff relationships for each watershed; 2) MOSUM: runoff relationships between watersheds, and 3) LOESS. We will state this explicitly here.

RC4: Figure 5: Why not show actual dates on the x-axis? Having the non-rescaled years on the axis would be much more useful in trying to relate the figure to the text.

AC: The implementation of MOSUM using the "strucchange" R package automatically ignores missing data and rescales the time axis form 0 to 1 using the number of available observations. The above explanation is included in the revised manuscript. Therefore, one has to keep track of the dates corresponding to each observation number.

RC4: Editorial comments:

1. Line 8, p. 11522: add "is from" after "knowledge."

2. Line 3,p. 11532: take out the second "counts."

3. Suggested change for line 5, p. 11532: "The differences in mean count were significant, with trees counted by aerial photography (6.74 trees per plot) fewer in number than those counted by inventory (7.60 trees per plot)."

4. Line 20, p.11534: Sentence beginning with "Interestingly" should be rewritten. Try "Interestingly, WS77 showed more regeneration of pine seedlings during this period, suggesting that it would have experienced a neat decrease of runoff."

5. Line 8, p. 11537: Take out "or decrease in WS77" for conciseness.

6. Figure 3 caption: capitalize LOESS.

AC: Thank you for these editorial suggestions – we will incorporate every single one.

Suggestion to present flow and rainfall information graphically

RC2: Figure 4: I prefer the scatter plot between monthly (annual would be better) Q from WS77 and WS80. Figure 2 is an example from Coweeta Hydrologic Lab.

RC4: I also think the authors are missing an opportunity to show some first-order statistics that would help readers quickly understand the temporal changes and changes between the two watersheds before delving into LOESS and MOSUM. Namely, I would have found plots of the difference between rainfall amounts and runoff over time and for each watershed valuable in trying to understand the basic water balance. Similarly, plots of runoff for each watershed would be helpful in illustrating the "radical changes in runoff flowing Hurricane Hugo" (p. 11534, line 12) and subsequent recovery. In short, I think that through the text and figures, more of a water balance perspective is needed in this manuscript.

AC: We agree with both reviewers that a presentation of a more basic analysis of rainfall and runoff dynamics might be useful. However, two studies by Amatya et al. (2006) and Williams et al. (2012) have already presented this information in more detail. We will however do a better job of highlighting the results already published in Amatya et al. (2006) and Williams et al. (2012) so readers have a clearer sense of the range of analyses this dataset has been subject to.

The objective of this paper was to describe relative changes in runoff over decade-long time spans, and we believe that focusing on a water balance approach given the complexities introduced by seasonal dynamics (ET and rainfall) detract from the changes that the two watersheds exhibit in relation to each other over multiyear time spans. It is a fact that only in the analysis of several decades of flow data was the altered hydrologic response evident. Both the LOESS and MOSUM analyses are simply techniques that highlight or characterize the timings of these change periods, helping us to arrive at better estimates for when the "flip" era began and ended. We respectfully submit to the reviewers that given the high similarity in rainfall between the two watersheds for each period of analysis, a water balance approach (derivation of ET as P-Q) will in fact detract from the overall goal of this paper, and introduce additional detail that have already been presented by other researchers.

RC2: Figure 4 and 5: The x-axes should be linear and properly labeled. It is hard to read them. (AC: see response regarding "Questions about MOSUM and CUSUM".)

RC2: Figure 7 and 8: I think that these graphs are not effective to show what the authors try to explain. As mentioned above, it is quite hard to interpret them without showing the seasonal variations in precipitation. (AC: see response regarding "Questions about seasonality of monthly flows".)

RC3: Figure 7: Could you reduce the extent of the y axis to better visualize the differences**?** (AC: We plan to revise this figure and possibly combine to a single plot. We will make sure to scale the y-axis so as to ensure maximum detail)

RC3: Figure 9: Could you add error bars to the dots? (AC: We will error bars to show standard error associated with the average values plotted)

Questions about manuscript organization

RC1: The first paragraph of section 5 (discussion) is negative, pointing out real or potential weaknesses in your work. Why start it out with the negative? Lead with more positive discussion and include this aspect later on. Just a suggestion.

AC: Great suggestion! We will revise this paragraph to state first how the analysis we conducted reveals the strengths of the techniques we employed, and then how these strengths might overcome some of the potential weaknesses posed by the traditional paired watershed approach.

RC3: Summary vs. Conclusions? I suggest deleting the sub-heading 'Summary' and leave that text as part of the Discussion. (AC: We will eliminate the Summary section and merge with the Discussion)

RC4: One rough part of the manuscript is the Introduction. It begins by describing shortcomings of paired watershed experiments, followed by a paragraph about how runoff generation is poorly understood, followed by a paragraph about how hurricane effects are poorly understood, with little linkage between these disjointed paragraphs or to the objectives. The paper would be made more compelling and clear to readers with some simple reorganization and rewriting here. Here is what I suggest: Start with the description of runoff generation: why it is important and how it is poorly understood. Follow that with a description of how runoff generation might be studied with paired watershed experiments. Be sure to define what a paired watershed experiment is and how it might be used to improve our understanding of runoff generation. Then describe the shortcomings of paired watershed experiments, introducing the example of how the response of the two watersheds in Santee illustrates one of those shortcomings–that adjacent watersheds may respond differently to climatic forcing or extreme events. Conclude with a description of how this study improves the state of the science, which should lead nicely into the objectives. As with the Introduction, the Discussion also seems a bit disjointed with respect to the relationship between the first paragraph (about the limitations of paired watershed analysis) with the rest of the discussion. Either clear statements about how these sections of the discussion are related or use of subheadings would make for a clearer read.

AC: We thank the reviewer for her clear instructions on improving the writing. We will rewrite the introduction as well as the discussion per the recommended outline provided.

Additional figure to illustrate differences between watersheds

RC4: I would also encourage the authors to consider including a conceptual figure illustrating the differences between the watershed. I envision that the figure would have two columns (representing WS77 and WS 80) and several rows. A schematic diagram of low-density large trees for WS 80 and high-density skinny trees for WS 77 would be one or the rows. Underneath, characteristics of each of the watersheds during the Pre, Flip, and Flop period would be summarized in a few words (e.g., lower/high flows, biggest difference in flows during the summer (ET), etc.). I drew such a figure for myself as I was perusing the paper in order to more readily grasp the differences between the watersheds and found myself often referring back to it as a guide. To accommodate this and the other extra figure request, I would suggest eliminating either Fig. 7 or 8, or combining them into a two-part figure.

AC: Thank you for this great suggestion. We will consider developing a schematic that highlights key changes in rainfall, flow and vegetation between watersheds and across the three eras in the results section. As mentioned previously, we will eliminate Figures 7 and 8.