Responses to comments from Student Reviewer 2

Key:

SC2 – Comments from Student 2

AC – Author comments

General comments

SC2: First of all, part of the results are based on an analysis using MOSUM. This is done because CUSUM is supposedly less sensitive to changes in regression coefficients especially when these changes occur in the later stage, after the severe changes as a result of hurricane Hugo. To get around this issue the use of MOSUM is implemented (Chu et al., 1995). However no explanation is given why this method would be justified to use. It would be convenient if a short explanation is given on this method in which the advantages of this method over CUSUM are discussed in more detail.

AC: The sentence below is added to section 3.5: “Thus, the MOSUM is more sensitive to parameters that are temporary unstable than the CUSUM because the cumulated sums become less sensitive as the number of residuals becomes larger.”

SC2: Also more explanation is required regarding the output of MOSUM, figure 4 and 5. Here the moving sum of the recursive residuals is plotted for the linear relationship between monthly flows of watersheds WS77 and WS80. According to the caption values outside of the 95% confidence intervals indicate a break in the linear relationship. However, these points do not at all correspond to the vertical lines representing the break date. What is the difference between the break date and the moment of exceeding the 95% confidence interval? This requires a clearer explanation or alteration.

AC: Section 3.5 has been rewritten to differentiate between test for existence of structural change and determination of the number and location of breakpoints. For details refer to response to Student reviewer #1.

SC2: Continuing with section 4.1, where the results of MOSUM are described. This section implies large uncertainty as table 1 suggests that there is a 65.3% change in average monthly flow in WS77, whereas in section 4.1 is concluded from the output of MOSUM that there is no structural breakthrough and that this large increase in discharge falls within the confidence interval. This is hard to accept and makes the linear discharge-precipitation model used for MOSUM questionable. The authors already mention that the precipitation data used may not fully represent the study area. Furthermore the conclusion in this section resulting in choosing two onset dates for the flip and flop period, January 1992 and January 2004 respectively do not match the break dates in fig. 4 or fig. 5, but almost exactly match the onset dates resulting from the LOESS analysis shown in fig. 3. This gives rise to the question whether MOSUM is a good approach in determining structural changes in the case of a linear relationship between discharge and precipitation. Overall the sections describing the application and the output of MOSUM are weak and needs revision. A possible error could be that the assumed evapotranspiration cannot predominantly be explained by vegetation (Ford et al., 2007) and therefore evaporation should be implemented into the existing precipitation-discharge model to complement the hydrological balance resulting in a more realistic linear model.

AC: The strength of the flow-precipitation linear relationship is similar for WS77 and WS80 (with adjusted R2 of about 0.4). During the same period highlighted by the reviewer (1989-1991), flow on WS80 also increased by 57.1% (65.3% for WS77). Since both relationships (WS77 and WS80) are similar, the existence of structural change in WS80 and absence in WS77 is not influenced by differences in strength of the linear relationship. However, the difference in breakpoints between flow relationship (Figure 4a) and flow-rainfall relationship for WS80 (Figure 4c) is influenced by differences in the strength of the relationships (adjusted R2 = 0.80 versus R2 = 0.4). The effect of the strength of the relationship is also reflected in the differences in the intervals of 95% CI for the breakpoints (Figure 4a vs. 4c).

The primary objectives of using MOSUM were to test for existence of a structural change in the flow relationship between the paired watersheds. The second objective was to determine which of the two watersheds was greatly affected (long-term shift), and the third was to determine the location of the structural breakpoints. However, the 95% confidence intervals for the estimated breakpoints are wide and use of LOESS (only used on the paired flow relationship) provides another independent tool in determination of the breakpoints. As stated in the original manuscript, the dates used to determine the flip and flop eras is a compromise of results from the two methods.

Finally, the sections describing MOSUM application and outputs have been improved in the revised manuscript.

SC2: Secondly, the authors claim that transpiration is the major component of the total evapotranspiration process in the global water cycle. It is not clearly stated but I assume that this is mentioned to neglect the fact that the paired watershed technique cannot differentiate between water loss by transpiration and evaporation separately and therefor it is assumed that the evapotranspiration consists predominantly of transpiration based on the analysis by (Jasechko et al., 2013).

AC: No, this is not correct. If we do have the measurements of ET and its separate components like transpiration and canopy/soil evaporation on both the watersheds we could use the paired watershed approach to assess the impacts like with the streamflow. Part of the explanation and data support have been provided in the first review of student comment. Here we show another reference by Bryant et al. (2005) for a Georgia site south of our South Carolina study site where the canopy interception loss was shown to be only 18.6% and 17.7% of the precipitation for the mixed forest and lowland hardwood, respectively, during 2001-2002 study period.

In Tian (2011) simulation study for the watershed WS80, 16% of the total ET was attributed to canopy interception and 84% of the total ET was attributed to transpiration and soil evaporation, which is again a small component only. In the North Carolina study, Amatya et al. (1996) found 24% of the total ET as the canopy evaporation from interception, as expected, for an intensively managed pine forest site.

Bryant, M.L. S. Bhat, and J.M. Jacobs. 2005. Measurements and modeling of throughfall variability for five forest communities in Southeastern US. J. of Hydrol, 312, 2005, 95-108.

I think that this assumption is not supported by other empirical evidence nor by the data presented in the current manuscript. Jasechko et al. 2013 conclude that more than 80% of global evapotranspiration should consist of transpiration. This does not exclude that on catchment scale under specific ecological conditions the transpiration may be lower. This is supported by results of Kool et al. (2014) and Sun et al. (2013), while transpiration is in most cases the major component of evapotranspiration, evaporation still accounts for roughly 30–40% of evapotranspiration for the given catchment characteristics and therefor deserves independent consideration.

AC: However, this does not seem to be the case with our study site in the humid coastal forests where the related studies did not show more than 25% for intensively managed and lower for less intensively managed "natural" forests on WS77 and WS80.

By not distinguishing between transpiration and evaporation the impact of vegetation growth on brook outflow will be overestimated. This results in water loss appointed to transpiration whereas this may have been caused by evaporation which has a more equal effect on both WS70 and WS80 and thus is not caused by the recovery of vegetation in the more severely struck watershed. The exact contribution to the total evapotranspiration, coming either from transpiration or evaporation, does not affect the results, but it is important to elaborate on this before publishing to improve the strength of this manuscript.

AC: We believe the above information and other data/results we provided in response to Anonymous Reviewer 2 address this concern. Please refer to the "Response to Anonymous Reviewers" document.

Specific Comments from SC2

A lot of values mentioned in section 4.2 are not in agreement with the values in table 1. The 45% change in mean monthly flow in WS77 and WS does not correspond to the values of 65.3% or 57.1% in table 1. to the value of 17.1 3.4. Also the 4.2 mm per month more than WS80 does not correspond to the value of −3.9 1.1 in table 1. If the given numbers in the text are correct an explanation is required why these are different than the values in table 1. The value of 15.7 3.2 does not correspond. (AC: thank you for pointing these out – the table is accurate but the narrative needs to be updated. In the Response to Anonymous Reviewers document we have highlighted issues with correspondence between Table 1 and the narrative and have committed to correct these typographical errors.)

Page 11521, lines 3–4: Add parentheses around references. (AC: Thank you)

Page 11521, line 16: "to be related to soil water" (AC: Thank you)

Page 11522, line 8: "temperate knowledge comes from a 1938 hurricane" (AC: Thank you)

Page 11523, lines 23–24: "wind speeds of 49 m s−1 were measured in Sumter, SC, at 139 km from the coast." (AC: Thank you)

Page 11524, line 2: Add a comma after WS77. (AC: Thank you)

Page 11524, line 3: Add a comma after WS80. (AC: Thank you)

Page 11524, line 21: WS80 instead of WS 80 (AC: Thank you)

Page 11525 line 8: Remove "then" (AC: Thank you)

Table 1: "Flop 2004–2011" instead of "Flop 2004–1911" (AC: Thank you)

Figure 1: Higher resolution image would be better. (AC: Thank you – we plan to redo Figure 1)

Figure 4 and 5: Same size of axis labels (AC: Thank you – we plan to combine Figures 4 & 5 and add a common x-axis. See an example in the Response to Anonymous Reviewers document)