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Interactive Comment

Interactive comment on "Hurricane impacts on a pair of coastal forested watersheds: implications of selective hurricane damage to forest structure and streamflow dynamics" *by* A. D. Jayakaran et al.

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Review on 'Hurricane impacts on a pair of coastal forested watersheds: implications of selective hurricane damage to forest structure and streamflow dynamics'

Note for the authors and editor

The following review was written by one of the students of the MSc programme Earth and Environment at Wageningen University. As part of the course Integrated Topics in Earth and Environment, students are asked to prepare a review of a scientific paper. I supervised this review process, and submit this comment on behalf of the student that produced it. The manuscript by Zaroug et al. was one of the manuscripts that was selected for this exercise. The review is written as an official review in order to comply with the course guidelines , but it should be considered by the authors as a regular comment which they can use to improve the manuscript. I hope that this comment will positively contribute to the review process and that it will help the authors to revise their manuscript for possible publication in HESS.

Summary

The manuscript by Jayakaran et al. presents an analysis of the impact of hurricane Hugo on the rainfall-runoff relationship and vegetation dynamics of a pair of watersheds in South Carolina. Using long-term streamflow records, unfortunately containing some gaps, in combination with data on rainfall, the magnitude and timing of the earlier observed reversal in relative flow magnitudes is quantified. In addition to that, the potential influence of the vegetative composition, which showed a shift as a result of the selective impact of the hurricane, on this process of reversal was studied using tree inventory data. The subject being addressed in this study is of great relevance as it provides understanding of the long-term effects that occur after a storm, which are often not included in hydrological studies. During this long period the hydrological response of two paired watersheds relative to each other are studied, in which also the influence of (evapo)transpiration is taken into account.

The results of this study provide valuable information on the impact of vegetation on rainfall-runoff relationships, which are subject to changes over a period of about 14 years as a result of hurricane Hugo, which can for instance be included in the calculation of the probability exceedence of a certain discharge. It would be interesting if eventually the influence of disruptors like hurricanes can be included in models to predict streamflow. I think this makes the paper interesting, as it can contribute to our knowledge on the long-term effects of hurricanes on hydrological processes and the role of vegetation dynamics. However, I have a few suggestions and minor comments which I believe, can improve the clarity of the paper. I therefore recommend a minor revision before publishing.

General comments

Evaporation from the land surface can have different origins: through transpiration from plants' stomata, evaporation from bare soil and evaporation from intercepted water. The authors claim that the observed changes in runoff can be attributed to transpiration rather than evaporation. Interception doesn't play an important role in the short term, but its impact shows to be apparent on the longer time scales (Gerrits, 2010). Especially for aerodynamically rough vegetation like forests evaporation from interception is thought to be a considerable fraction (Klaassen et al., 1998). Moreover, when modelling the rainfall-runoff response, what is wanted to be achieved eventually, interception is an important process to include (Savenije, 2004).

In my opinion the statements made by the authors about increased transpiration being the cause of decreased runoff found in WS77 are not supported by their results. The authors assumed solely from Jasechko et al. (2013) that transpiration must be the biggest driver in total evapotranspiration rates. However, Jasechko et al. (2013) states that every individual catchment does show a unique evaporation trend as they all have a unique climatology and hydrology. In addition to this, the method used by Jasechko et al. (2013) can be disputed as water from lakes is used to determine the isotopic composition. However, it is not exactly known what the sources are of this lake water. Therefore I think it is rigid to just assume, based on this article alone, that transpiration is the largest component of the total evapotranspiration of the studied catchments. And as said before, several other studies show that especially in a forested catchment the fraction of evaporation from interception can be of considerable importance to include in the hydrological analysis.

I would suggest to reconsider the terminology used and to also include the changes in interception evaporation as a possible explanation given its importance in temperate forests. Furthermore, it would be good to also cite other papers about the partitioning of evapotranspiration which are specifically focussing on forested catchment (e.g., Bryant et al., 2005, Klaassen et al., 1998).

A second general comment concerns the technique of moving sum of recursive residuals (MOSUM) used by the authors to determine changes in runoff. This technique is not often used in hydrological studies and the authors should therefore elaborate more on how the method has been applied, why they have chosen it and what the possible constraints and advantages of this method are (e.g. that a moving-estimates test is more sensitive to parameters that are temporarily unstable than for instance the CUSUM test because in the latter, the cumulated sums are used which become less sensitive to parameter changes as the number of residuals becomes larger (Chu et al., 1995)).

In addition to that, to me it remains unclear how the authors dealt with the periods of missing data in figures 4 and 5 which result from using the 'strucchange' package in R implemented by Zeileis et al. (2012). It is mentioned that non-linear axes are used to deal with the data gaps, but this seems not to be the right terminology. The axis itself is linear, but the data within is not linear due to the missing data. To clarify the figure I would suggest implementing arrows or vertical lines at the place of the two gaps of missing data. To make it even more clear the x-axes can be renamed to for instance: number of day with observations. Another more elegant option would be to add another panel which shows the original data with its gaps in order for the reader to understand the relation to the data shown. In this way the interpretation of the figures would be easier.

Furthermore, in section 3.5 it was mentioned that analysing the points on structural change, only a single breakpoint was assumed (p.11527, l.16). I would suggest to add to this explanation that one breakpoint, can generate more than one break dates (as I now had to find out myself using the package 'strucchange' in R), especially because now in section 3.5 it appears for the reader that breakpoint and break date are the same (p.11527, l.11).

All in all, I think both figures 4 and 5 need a better explanation on how the break dates are obtained because the explanation given in section 3.5 and the caption beneath figure 4 are confusing as they refer to two different 95% confidence intervals; 'the long horizontal dotted lines', but also the 'small horizontal lines that cross each break date'. Now the break dates especially of the second panel in figure 5 not seem to fit the description given in the caption of figure 4.

Another general comment concerns the interpretation of section 4.3 about seasonal streamflow trends and its accompanying figures 7 and 8. This is in my opinion only briefly discussed in the discussion section and therefore I would recommend some further elaboration on the significance of difference in magnitude found, and if this significance does hold for all months or that there is a seasonal effect, are necessary to comment on, as this is done in all other parts of the results. The authors claim in the discussion section that changes in the transpiration rate must be the cause of the differences found between streamflow of WS77 and WS80 throughout most of the year. However, alternative explanations are possible. The effect of soil moisture can be important especially when focusing on seasonal trends (Johnson and Kovner, 1956). In addition, especially when interpreting figure 7, a note should be made about the

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interception term as the relative importance of transpiration and interception do change as the forest grows older (Naranjo et al., 2012) during the Flop era in comparison to the Flip era.

In the current version of the manuscript, section 4.3 is not related to the rest of the results and its significance is not clear. But it could be an interesting section as Brown et al. (2005) found that there are found impacts on seasonal yield and flow regime due to vegetation change, but that these impacts are unique per catchment. Therefore it would be interesting to be able to determine if there is a significant seasonal effect for these paired watersheds.

Another comment refers to how this work relates to other studies on land use effects on evapotranspiration, for instance the recent work by Naranjo et al. (2012) who studied the change in discharge and actual evapotranspiration both relative to the values found before the disturbance using K-curves. This paper shows another way to determine structural changes even at longer temporal scales. It could be interesting to see if a same relation can be found when creating such a K-curve with the data the authors used, and to note how it will evolve on the longer time scales (100 years) and then to compare the first few years with the results from the LOESS analysis done.

The last general comment refers to the introduction where the authors mention the limitations of the paired watershed approach, but they do not justify why this approach is valid for their application. Did they have no other options regarding the data they have got, or is it an appropriate method for the aims they want to achieve?

Specific comments

p.11521, line 16; which is believed to be related to soil water and depression storage.

p.11522, line 8; most of the temperate knowledge is from a 1938 hurricane...

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p.11524, line 2; WS77, the treatment watershed, (first comma should be added).

p.11530, lines 8-9; ...altered the reported break dates. The lines above suggest that the break dates of the MOSUM analyses of watershed-specific rainfall-runoff relationships is meant (figure 5), but it is not stated clear. It could be interpreted in more than one way.

p.11533, line 4; 4.3 $\mathrm{mm}^2\ \mathrm{ha}^{-1}$ should be 4.3 $\mathrm{m}^2\ \mathrm{ha}^{-1}$.

p.11533, line 12; Table 9 should be Fig 9.

Table 1; In the column ‘Years’ the end date of the Flip era should be 2003 and the end date of the Flop era 2011.

Figure 1; The locations of the weather stations are not clearly visible.

Figure 6; In the description of the second box seasason should be season.

Figures 7 and 8; When printing in black-white, they are not readable anymore. A suggestion is to use different line types.

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Interactive Discussion

Discussion Paper

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