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Discussions

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".

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 Jayakaran 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 describes the recovery of two watersheds from the indirect hydrological effects of hurricane Hugo in 1989. The main conclusion of the manuscript is that there is a temporal difference in watershed outflow as a result of different vegetation recovery times after the hurricane. The manuscript covered both topics of hydrology and ecology and was generally well written. The amount of published research on hurricane effects on vegetation is large. However, the indirect hydrological effects of hurricanes on runoff generation have not been studied in depth. Therefore this manuscript provides a valuable contribution to the existing ecohydrological literature. I therefore recommend minor revisions. My suggestions will be discussed in more detail below.

General comments

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.

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.

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.

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).

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. 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.

Specific comments

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. The value of 15.7 $\pm$ 3.2 does not correspond to the value of 17.1 $\pm$ 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.

Page 11521, lines 3–4: Add parentheses around references.

Page 11521, line 16: "to be related to soil water"

Page 11522, line 8: "temperate knowledge comes from a 1938 hurricane"

Page 11523, lines 23–24: "wind speeds of 49 m s^{-1} were measured in Sumter, SC, at 139 km from the coast."

Page 11524, line 2: Add a comma after WS77.

Page 11524, line 3: Add a comma after WS80.

Page 11524, line 21: WS80 instead of WS 80

Page 11525 line 8: Remove "then"

Table 1: "Flop 2004–2011" instead of "Flop 2004–1911"

Figure 1: Higher resolution image would be better.

Figure 4 and 5: Same size of axis labels

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11519, 2013.