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Hydrology and Earth System Sciences
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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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

Jayakaran et al. have studied the magnitude and timing of changes including a reversal in relative streamflow-difference between two paired watersheds that were on the path of hurricane Hugo in 1989, and examined the selective impacts of the hurricane on the vegetative composition of the forest. The authors relate these impacts to their potential contribution to change watershed hydrology through altered evapotranspiration processes. The authors illustrate the important effects of different forest management practices on the hydrology of managed forests. Little is known about these effects and these relations are often not included in hydrological models. Therefore the paper makes a valuable contribution to the hydrological literature in this field of research.

The study presents some interesting long term hydrologic and ecological data. While some of the data are of debatable quality, the results are still substantial enough for publication. This research showed that there was a significant transformation in the hydrologic character in the investigated watersheds. I think it is an interesting article for HESS readers but I have a few corrections and some minor revisions that I think will improve the paper.

General comments

I have some questions about the inventory of the trees using aerial photography. If I understand correctly, all trees with a stem size in the 5 cm diameter class and larger were tallied in 1991 and included in the survey, these were assumed to be alive prior to hurricane Hugo. A series of georeferenced aerial photos of WS77 and WS80 taken in the winter of 1983 were compared to data of the 1991 tree inventory to estimate the possible error in the tree inventory totals. In the results is described that the mean count was not significantly different between the two inventories, but the aerial photography showed fewer pine trees per plot than those counted by tree inventory data. And subsequently is stated that with the tree inventory data a few more trees were included than with the aerial photograph interpretation. But with aerial photographs the stem sizes are not visible, so how can you be sure that all trees above the 5 cm diameter class are included? Why were winter photographs used for the aerial photograph interpretation? Winter photographs have serious downsides for vegetation inventories. Summer photographs would have been better for determining the species and to see the foliage as was done in the research of Valinger et al. (FAO, Forestry department).

In section 3.5 is described that the MOSUM test (moving sum of recursive residuals) was implemented in R to determine which watershed's hydrologic regime shifted due to hurricane Hugo and thus changed the historical hydrologic relationship between the two paired watersheds, which is part of the main goals of this article. Therefore I was surprised to see that the way the structural change point in the monthly streamflow and the corresponding 95% confidence interval was explained without giving much detail. For the reader would it be better if the MOSUM test was explained more clearer in the article. It is not a very common used method in the HESS articles or in many other scientific articles. Figure 4 is also difficult to read. In section 3.5 is stated that only one singe break date is assumed by hurricane Hugo as major climatic event, therefore I do not understand why figure 4 describes three different break dates. The vertical dotted

lines are the estimate change dates with corresponding 95% confidence intervals. But nowhere is explained why the vertical dotted lines are placed at those locations.

I also have a few general comments on the discussion by the authors. Personally, I would not start with the negative part of a research in the discussion, i.e. the recent criticisms of the paired watershed technique by Vogl and Lopes, 2010. For a positive image of this research it would be better to discuss the recent criticisms later on.

In the discussion the authors state that the changes in discharge can be attributed to transpiration without studying this or supporting this statement strongly with literature. The exact distribution of observed changes in evapotranspiration over transpiration, soil evaporation and interception evaporation is not relevant for the results. Following this, why would transpiration be the only explanatory process? Canopy and forest floor interception also important in forests (Gerrits et al. 2010). And Pypker et al. (2006) points the importance of understory vegetation in the interception process. The in situ processes differ a lot on the catchment scale. Therefore I would like to point out that the reference to the letter of Jasechko et al. (2013) in the introduction is less relevant for this article. Jasechko et al. uses isotope effects of transpiration and evaporation from a global dataset to demonstrate that the transpiration is the major component of the total evapotranspiration processes. The study area used for this paper (two paired watersheds in coastal Southeastern USA) is not a part of the global dataset used and the relative input per terrestrial vegetation type in not equal in this research.

I recommend to include an extensive paragraph in the discussion to the different evaporation processes and to reconsider or remove the statements about transpiration in the introduction.

To conclude, I think the paper is a good addition to the information in scientific literature about the impact of hurricanes on streamflow and vegetation in forested watersheds; and it makes a good contribution to the series of scientific articles on the effect of landuse management on hydrology.

Detailed comments

P11523 L7–9. Missing words: 'While the effects of hurricanes on tropical forest have been examined for up to seventy years, most of the temperate knowledge comes from a 1938 hurricane that struck Harvard Forest (Foster, 1988a,b).'

P11533 L4. 4.3 mm^2 ha^{-1} is incorrect, should be replaced by 4.3 m^2 ha^{-1}. P11533 L12. "Table 9" should be figure 9. P11533 L15. 0.13 m^2/tree should be changed to 0.13 m^2 tree^{-1} to be consistent.

The wrong years have been notated in table 1. Change years 1992–1903 into 1992–2003 and years 2004–1911 to 2004–2011.

The shapes of WS80 are not consistent with each other in figure 1 and 2.

The second bar of figure 6 shows the effect of "Dormant seasason prescribed burning" on mean monthly differences. I assume this should be the effects of Dormant season prescribed burning. The fourth bar is missing a label.

In figure 7 the mean monthly flows are labelled "Mean Monthly Flow (mm)" in the axis. But in figure 8 the mean monthly flow is labelled as "Montly Flow (mm)".

Figure 9. The reference to the upper and lower graph has been mixed up in the caption. The upper graph shows the effect of Hugo on different species in terms of tree numbers. The lower two panels show a change in average tree size.

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