Hydrol. Earth Syst. Sci. Discuss., 10, C5804–C5810, 2013
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Hydrology and Earth System Sciences
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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.

Anonymous Referee #2

Received and published: 9 December 2013

General comments

The manuscript by Jayakaran et al. (Title: Hurricane impacts on a pair of coastal forested watersheds: implications of selective hurricane damage to forest structure and streamflow dynamics) addresses the difference in emergent hydrologic behavior between two headwater catchments in coastal southeastern USA after a hurricane Hugo, mostly driven by different vegetation regeneration patterns in the watersheds. This paper is well written and investigates a topical issue in the relationship between

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ecologic and hydrologic processes. The main conclusion of the paper is that the contrasting hydrologic recovering behavior between two watersheds are attributed to different regeneration patterns with selective hurricane damages, based on the long-term hydrologic records and ecological data. The paper was relatively well-written from both ecologic and hydrologic points of view, and I also really enjoyed reading it. I recommend a major revision with one critical question and several suggestions, which I would leave on authors' and editor's choice.

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.

A simple solution to this problem is simulating the model at coarser time resolution. At

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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.

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.

I understand that the mass-balanced method would be quite not applicable these watershed due to low gradients and wetlands, which makes it hard to close the mass balance. But, I still think that this is a best way to compare the hydrologic behavior with vegetation dynamics. With this method, authors do not need to use 'believe' or 'appear to' in the manuscript to relate emergent hydrologic behavior with vegetation dynamics.

Specific comments

HESSD

10, C5804–C5810, 2013

P11520 L5 and P11524 L21: should be 'streamflow' P11530 L18: 'WS80' typo P11530 L24 – P11531 L6: explanations do not match with Table 1. There are several apparent typos and errors in Table 1. 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. L11533 L12: Table 9? L11534 L5: space needed. L11534 L18 – L20: I don't agree to this conclusion as mentioned above. I think this is simply wrong. P11535 L15 – L17: Provides with scientific names. 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). 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: 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).

Table 2: define HWD.

Figure 2: The shapes and boundaries of watersheds are not good.

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.

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.

Figure 4 and 5: The x-axes should be linear and properly labeled. It is hard to read them.

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.

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

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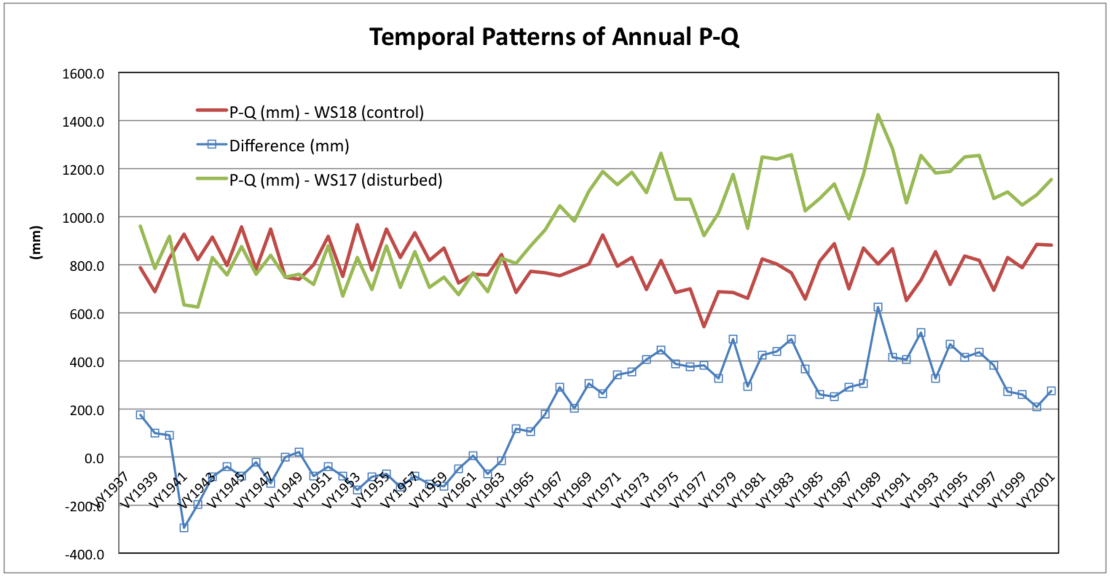

Fig. 1. Temporal patterns of precipitation (P) minus streamflow (Q) at the April-March water year period from the paired catchment experiments in Coweeta Hydrologic Lab (WS17/WS18). Details are in the text.

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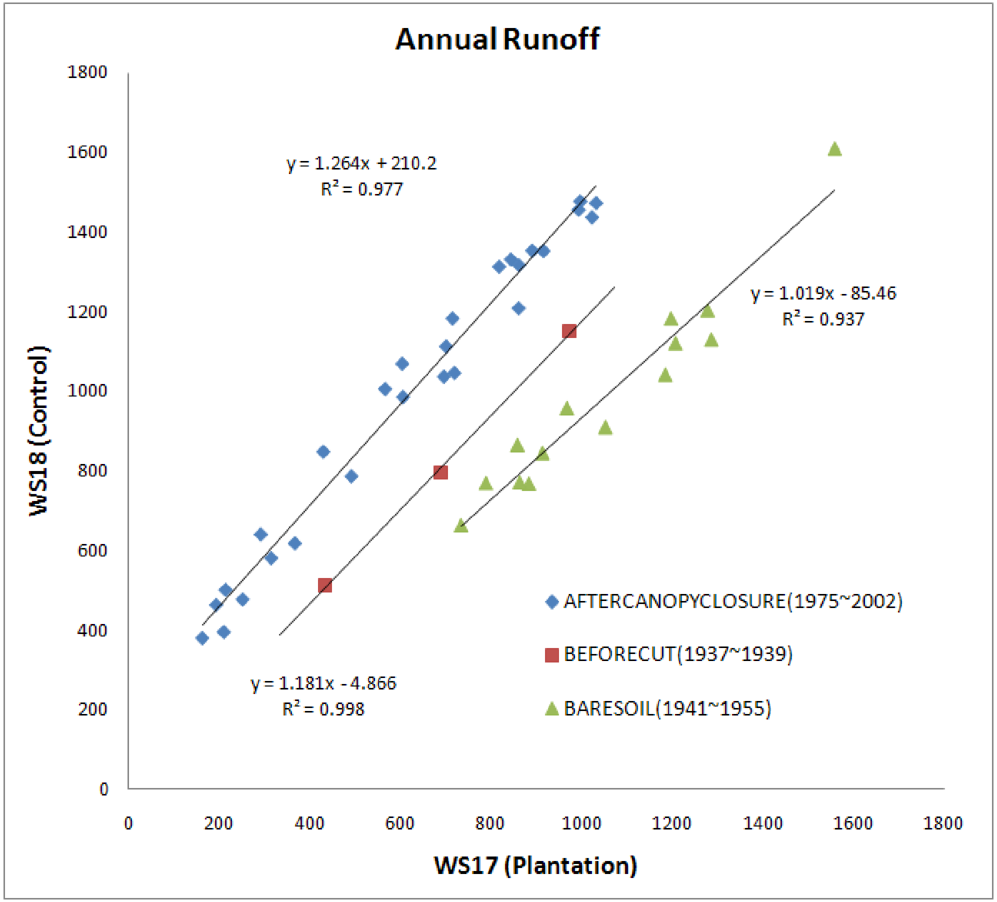

Fig. 2. Annual runoff generation between WS17 (disturbed) and WS18 (control)

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