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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Modeling the distributed effects of forest thinning on the long-term water balance and stream flow extremes for a semi-arid basin in the southwestern US" *by* H. A. Moreno et al.

**Anonymous Referee #1**

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This manuscript contains an interesting attempt to understand the sensitivity of water balance to changes by forest thinning occurred over the period 1990âĂŘ2010 in the Tonto Creek basin. There are not many of this type of analysis in literature especially using process-based hydrological models. The analysis gives insights on the relative importance of changes in vegetation structures and soil hydraulic conductivity in controlling the hydrological response. Interesting results are that vegetation removal is the most important factor determining distributed changes in fluxes and storages of water, more so than hydraulic changes in soil. Despite these interesting points, the study



presents several criticalities, the most important is that the used model performed poor (even though the author stated that the model is able to reproduce the distinct hydrologic patterns) (see major comment 1), the second concern is the lack of a more detailed discussion of the assumptions and limitations of the study and their possible effects on the obtained results (see major comment 2).

1) My major concern is related to the rather poor confirmation of the tRIBS model performance in simulating the hydrography. In common, a NSE value of 0.66 is not good for a model application. Furthermore, I disagree that the changes in water balance should not depend on the magnitude of a given variable. The less than satisfactory results in reproducing the discharge and snow water let me wonder how much we can trust the final results of the numerical sensitivity analysis. While I strong believe that in many hydrological studies, especially when models are used for virtual experiments, the skills of the model in reproducing streamflow is not so significant, since it is the overall credibility in representing hydrological dynamics which matters. In this specific case, I'm not sure the results presented have enough scientific confirmation to be regarded as robust results. There, I suggest more evaluation (e.g., compared with observed evapotranspiration, soil moisture, and groundwater) should be added to help understand the performance of the processes in the model. 2) Overall, the manuscript does not highlight or discuss the numerous assumptions and deficiencies which are likely affecting the final results. For instance, vegetation phenology is not simulated by the model. All these limitations which could be among the causes of the rather poor performance of the model need at least to be explicitly mentioned and discussed. In the section of model overview, the description of the model is too broad. As the tRIBS is a well-developed model, not all the components of this model should be introduced; instead, the parameterizations about vegetation and soil hydraulic conductivity and how they affect the water balance should be introduced emphatically. At least, the parameterization about the parameters in Table 2 should be introduced in detail. Moreover, more uncertainties should be discussed to address the possible deficiencies in the results. 3) Additionally, the results and discussion are not well organized. In the section,

all the changes in the components in water balance are equivalently reported, in terms of inter-annual trends, seasonal pattern, spatial distribution, soil column water balance with contrasting solar aspect, stream flow shifts and extreme event probability. This organization is hard to follow because the focus is not prominent. As I learn from the introduction, the most important component of water balance is river discharge. Thus, I suggest to organize the results as: 1) first, as a start, show the changes in discharge; 2) second, interpret the reasons why discharge changes by analyzing the changes in other components. If necessary, flow diagram can be presented. I also suggest to add the surface runoff and subsurface runoff to give more insights on the changes in total runoff.

Minor comments: (1) Fig 2. I suggest to add a map of US and point out the location of this basin. (2) Fig 3. I think the relative change is more intuitive. (3) P 10834, L20. The post-treatment scenario was obtained by applying probabilistic distribution. I'm wondering how significant this probabilistic distribution affects the simulated results. Is it necessary to apply different post-treatment scenarios? (4) Section 3.1. The climate, such as mean annual precipitation, temperature, runoff etc., should be introduced. (5) P 10837 L22. Where is Appendix A2? (6) P 10847 L10. Is "Temp" temperature? (7) I think the conclusion is too long. More concise information is required.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 10827, 2015.