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Interactive Comment

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 #2

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Overall Review

The manuscript presents an analysis of the sensitivity of the hydrological response of the Tonto Creek (AZ, USA) to the effect of forest thinning using a process-based hydrological model (tRIBS). The model is forced with 20 years of historical climate and different scenarios consequential to a forest restoration project (forest thinning). Specifically, beyond the change in vegetation structure and LAI the authors account for potential reductions in soil hydraulic conductivity. The manuscript is well written with a detailed description of the methodology and I particularly appreciate the rigor in various aspects of the hydrological modeling analysis, including the preparation of the model inputs, the check of model result consistency, and the analysis of different types of results in the hydrological response (mean, seasonality, extremes, and different hydrological periods). Conversely, to several previous study on a similar tropic, the authors provide process-based explanations of the presented results, which made the article much more scientifically sounds and interesting. Even though it is specific for a given catchment, I believe the implications of this study have a wider generality. I found most of the manuscript very clearly presented, but I have few minor comments, especially related to suggestion for re-organizing the content of certain paragraphs and shortening Sections 4.3 and 5. A more relevant comment I have is related to how vegetation is considered in the present model (see comment 1 below).

1) As far as I understood (PP 10840 Line 24-25 and Appendix B) the tRIBS model is used with all vegetation properties (LAI, albedo, canopy radiation transmittance roughness, etc.) being as static fields. In this context, changes due to forest thinning are substantially prescribed by the authors (PP 10843 LL 6-8) and vegetation cannot respond over time for instance trees cannot resprout, seedlings cannot grow or much more simply LAI cannot adjust and respond to the new conditions after the thinning. The lack of vegetation dynamics is a limitation that the authors are aware of but it is dismissed very quickly in the conclusions (PP 10855 LL 26-28). I believe this issue should be discussed much more thoroughly referring to literature (e.g., using tRIBS-VEGGIE (Ivanov et al 2008) would have relaxed some of these assumptions) and cautioning some of the findings (for instance the summary of PP 10848 LL 1-6 and point 1-5 in Section 5). There is emerging evidence in literature that even massive forest mortality events did not translate in large hydrological or carbon fluxes responses as it would have been expected (e.g., Gough et al 2013; Biederman et a 2014; Reed et al 2014). It is true that the authors found changes in the order of 10% or less except for headwater catchments, anyhow the discussion about the lack of vegetation dynamics is quite important for this manuscript. With this, I am not asking for any additional analysis but just for a more extensive treatment of the issue. Due to the complexity of the model and of

the non-linear relations shaping the hydrological responses, even the outcome of the current analysis is still interesting and difficult to predict a priori, so I definitely see the merit of the analysis. For instance, you found that the major hydrological differences following a decreasing in LAI are related to change in snowpack/snowcover rather than decreased transpiration, which is mostly compensated by evaporation (PP 10854 LL 5-13). I believe this place your analysis in a sort of safer zone, because vegetation dynamics and re-growth is more likely to affect transpiration than snow-dynamics at least in the first years.

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Minor Comments

PP 10829 LL 7. But see also Biederman et al. 2014

PP 10829 LL 20. If ET decreases, base flow can potentially also increase, as you find later for some scenario.

PP 10833 LL 10-20. See also Fatichi et al 2014. I see common points with your study. They also prescribed scenarios of decreased soil hydraulic conductivity due to management practices and they also provide mechanistic explanations of how management affects hydrological budget across various scales.

PP 10834 LL 4 and LL 15. Please use SI units (it is a scientific journal) and not “acres” or “ft2 ac-1”.

PP 10838 LL 12-13. I think the sentence is wrong and that you mean: melt water can either infiltrate or run off and eventually is routed down-slope to the channel as surface or subsurface runoff.

PP 10838 LL 23. Could you please clarify how Penman-Monteith equation makes use of the energy balance in the model, and how the energy balance is computed (PP 10837 LL 24-26). Penman-Monteith equation is typically used exactly to avoid solving the energy budget since due to assumptions in its derivation Penman-Monteith

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equation does not depend on surface temperature and surface humidity anymore. If you solve the energy budget and you know Latent Heat, then there is no need of Penman-Monteith equation anymore. This aspect needs clarification.

PP 10841 LL 9. Do you mean "dynamic steady-state"? Is one year sufficient to spin-up groundwater? I would expect a much longer period is needed. Is this because the initial guess is already so good?

PP 10842. Equation (4). Please check the expression, this does not seem to be the variance Xjsim should rather be the average of observations.

Section 3.5 Do you really have only 1 streamgauge and 1 snow-pillow in 1900 km2? This does not allow any check of internal consistency of hydrological dynamics, which must rely on the model structure only. I think this aspect must be stated explicitly.

PP 10843. LL 8, there is a typo.

PP 10844. LL 8-16. This paragraph should be regarded as method (watershed description) rather than a part of results.

PP 10846. LL 11-14. I think this paragraph and Fig. 9a 9b should be rather in the method section, which described the watershed characteristics and inputs.

PP 10847 LL 23-25 and PP 10848 LL 1-6. These sentences would fit better in the discussion section.

Figure 11 and Equation (6) to (8) should be an integral part of model description rather than within the result section. Furthermore, there is something weird in Equation (8), if the subscript "f" refer to post forest-thinning why only few terms have the subscript? Otherwise Eq (8) is just identical to Eq (6). This part needs clarification.

Section 4.3. Why did you select 16 basic computational elements to illustrate differences in hydrological budget components related to aspect (Table 4 and 5) rather than plotting hydrological response for all the elements of the catchment as a function of as-

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pect, slope etc.? I think it would have been more synthetic and effective in supporting your discussion. Is just due to how the model store results?

Section 4.3. Given the fact that differences between N and S exposed hillslope are not so evident and that many other confounding factors (precipitation and other climate forcings) play an important role, I wonder if Section 4.3 cannot be shortened and simplified. There is the risk that the reader is lost in all the numbers and comparisons of Table 4, 5 and Figure 12, for a result that is not so essential to your overall analysis.

PP 10851 LL 13. Just to avoid any potential misunderstanding could you please state that Q1 correspond to the low flow and Q4 to the high flows.

PP 10852 LL 5 and PP 10854 LL 19. I would refrain from using the word “disaster”, I think we are not doing a good service to science using these words without strong reasons; even a change of 10% is likely not going to make a change from a non-disaster to a disaster.

PP 10855 LL 11-25. This entire paragraph is very repetitive with what has been already stated in Section 5, I would suggest merging with the previous statements and shortening this section.

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References

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