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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.

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General comment:

We appreciate the comments from this anonymous reviewer and truly believe these can significantly improve the current status of our manuscript. We consider that both major and minor changes can be included in the submitted document to achieve publication status. In the following paragraphs we provide responses to the major and minor comments in sequential order.

Responses to Referee #1

Major comment #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."

Responses to comment #1:

We agree with the reviewer that in order to provide confidence in the final results, a model must demonstrate ability to reproduce historic behavior in key forecasting variables. However, we consider that a single evaluation metric like the Nash Sutcliffe model Efficiency (NSE) coefficient may not fully capture the entire ability of the model to reproduce hydrologic mean and variability of processes and their correlation structure (Gupta et al 1998, 2009, 2011; Boyle et al. 2000; Vrught et al. 2003). This is the reason why we we added two additional skill metrics(1) Mean Squared Error and (2) Correlation Coefficient. A complementary view to these three scores provides a broader meaning of the calibrated model in terms of the mean and variability of Q and SWE. On one side, NSE values greater than zero imply that model predictions are better than the historic mean. Unfortunately, the presence outliers, significantly skew

NSE values to lower scores due to the large differences between observed and simulated values. On the other side, it is pertinent to mention that the overall quality of hydrologic simulations is largely tied to the quality of hourly precipitation inputs whose uncertainties propagate basin-wise as has been shown by different authors (Michaud and Soroshian, 1994; Bardosy and Das. 2008; Borga et al. 2006).

We must mention that the calibration process of our model was an intensive and extensive part of the study that used high performance computational resources (Arizona State University's supercomputer) during weeks of execution. However, the propagation of uncertainties from the weather forcing, specifically rainfall, is an obstacle to achieve better calibration scores, particularly at the temporal scales we presented the results (daily values, during 20 years). Thus, we consider re-doing a this procedure would not necessarily conduct to better results on the prediction skills of the tRIBS model, unless we assure a higher accuracy in precipitation records. This is still a challenging situation due to the presence of a very low number of rain gauges in the region. This said, we believe our model still captures fairly well mean and variability of historic stream flows and snow records, particularly if we have into account the simulation of spatially-distributed values during 20-years.

During the development of this study we checked all possible sources of ground information to provide confidence to the model results. Unfortunately, the only information we could find were data records of one Snowtel station and one USGS stream flow gauge that we incorporated into our model verification process. No evapotranspiration measurements were found inside this watershed divide (e.g. eddy covariance tower, evaporative tank or lysimeter). Additionally, no soil moisture sensors have been deployed so far in this area to compare with our model. The only possible output we could find results for was groundwater depth from MODFLOW simulations carried by the USGS in 2010 with groundwater table depths modeled for years 1990, 1992,1993,1999, 2000, 2002, 2003 and 2005. Precisely, our model was initialized with the one of the groundwater outputs from this model (i.e. 1990) In this sense the com-

parison would be model to model outputs between tRIBS and the USGS- Modflow for different years (e.g. 1992, 1993, 1999, 2000, 2002, 2003 and 2005). At this point we are hesitant to introduce this new comparison for model verification as the USGS model outputs do not necessarily represent the ground-truth.

Major comment #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."

Responses to comment #2

We agree with the reviewer that our manuscript must make emphasis on model assumptions and deficiencies, so that results and uncertainties can be taken with cautious judgment. Trying to deal with this, we allude to them in the Summary and Conclusions section. For example in line 3 of page 10855 we stated:

"The tunning and evaluation procedures both provided appropriate skill scores for stream flows and snow water equivalent, despite some discrepancies introduced by model forcing, initial conditions and structural errors. While calibration and validation coefficients are not optimal, model performance offers the possibility of quantifying changes introduced by forest thinning, independent of the model structural and parametric uncertainty, as results are primarily presented relative to model simulations made with 2006 vegetation conditions, which we adopted as current reference case."

Also on page 10855, line 26 we state another model limitation:

"Our model does not consider dynamic changes in vegetation physiology, incremental changes in thinned areas, re-growth and effects in sediment and pollutants load to streams and reservoirs. Additional studies are necessary to investigate the effects of increased local groundwater recharge induced by less surface storage (e.g., Int, SW) and the shift in water residence times from surface to groundwater storages. Also, further studies should investigate the effects of deforestation on erosion, sediment transport and organic pollutants in such semi-arid systems."

Nonetheless, as the reviewer suggests, we agree to create a new section called "Model Results Assumptions and Limitations" immediately before the "Summary and Conclusions" section. In this new section we will introduce key aspects to consider during the interpretation of the results of this study, such as:

1. The low NSE values, relation with precipitation uncertainties and potential influence on the model results. Caveats when interpreting results from this modeling sutdy based on a gauge-corrected precipitation product with a limited number of on-the-ground rain gauges.

2. The effects of only considering a static vegetation condition, fully ignoring plant phenology, re-growth, competition and mortality. We will discuss the possible hydrologic effects of not introducing these processes in the modeling based on specialized literature (e.g., Brooks and Vivoni, 2008; Voepel et al. 2011; Edburg ey a. 2012; Gough et al 2013; Biederman et al 2012 & 2014; Reed et al 2014). The discussion will include both extreme and mean hydrologic conditions and how can they be influenced by not considering this dynamics into the current modeling.

3. The effects of not considering the recovery of hydraulic conductivity during post-treatment time due soil recuperation and vegetation processes.

We will also include caveat sentences where key conclusions are reached along the manuscript, alluding to the fact that results are presented in terms of a reference, simulated case that might not fully represent reality.

We agree with the reviewer that the Distributed Hydrologic Model section is too long. We propose to shorten it and rather expand subsection 3.5 on Calibration and evaluation strategy where we will enhance our model parameter description and significance for the tRIBS model and hydrologic processes we are seeking to represent.

Major comment #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."

Responses to comment #3

We agree with the reviewer that the most important component to simulate is stream flow. Based on this precept, an alternative organization of the manuscript staring with the stream flow shifts and following with complementary sections that explain those changes would be more convenient. Thereby, the new organization of the Results and Discussion section would be: 4.1 Streamflow Shifts and Extreme Event Probability, 4.2 Effects of Forest Thinning on Mean and Variability of Basin-Scale Water Balance Components, 4.2.1 Interannual trends, 4.2.2 Seasonal shifts and emerging hydrological patterns, 4.3 Distributed Hydrological Effects of Forest Removal, 4.3.1 Runoff and soil moisture, 4.3.2 Evapotranspiration, 4.3.3 Snow, 4.4 Soil Column Water Balance in Hillslopes with Contrasting Solar Aspect. The Figure 1 explains better this new results organization.

Minor comment #1

"Fig 2. I suggest to add a map of US and point out the location of this basin."

Response to minor comment #1

We will make an new inset to Figure 1 showing the location of the study region within the US.

Minor comment #2

"Fig 3. I think the relative change is more intuitive."

Response to minor comment #2

Yes, we agree. We will replace Figure 3(b) with a new map showing the relative basal area density change, respect to the pre-treatment conditions.

Minor comment #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?"

Response to minor comment #3

We consider that rather than correctly informing about the origin of the forest thinning plans, this sentence confuses more on the work done by Hampton et al. 2011. Therefore, we propose this modified sentence: "The post-treatment scenario was obtained from the Four Forest Restoration Initiative implementation plan (http://www.fs.usda.gov/4fri). The reader is referred to Hampton et al. (2011) for more

details on the tree density criteria and projections."

Minor comment #4

"Section 3.1. The climate, such as mean annual precipitation, temperature, runoff etc., should be introduced."

Response to minor comment #4

We agree. This is how this new paragraph would look like (Section 3.1 starting from line 12):

"Precipitation is bimodal with a mean annual value of 481 mm/y, with the largest amounts during winter months due to frontal storm systems and a secondary rainy period during summer, coincident with the largest evapotranspiration rates, via monsoon-driven precipitation. The mean annual temperature and runoff in the region have been estimated as 17.9 °C and 79.8 mm/y (Arizona Department of Water Resources, 2010). The VTS provides...."

Minor comment #5

"P 10837 L22. Where is Appendix A2?"

Response to minor comment #5

There is a typo. Appendix A2 will be replaced by Appendix B

Minor comment #6

"P 10847 L10. Is "Temp" temperature?"

Response to minor comment #6

The word "Temp" will be replaced by "temperature (Temp)" to keep consistency with the name convention showed in Figure 9.

Minor comment #7

“I think the conclusion is too long. More concise information is required.”

Response to minor comment #7

The “Summary and Conclusions” section is long due to the fact that a short summary was included at the beginning which makes this section heavy. We propose to remove all material corresponding to the summary. Some other material from the Conclusions section will also move to the new proposed sub-section on Model Results Assumptions and Limitations right before the Conclusions section. This two changes will make it more concise.

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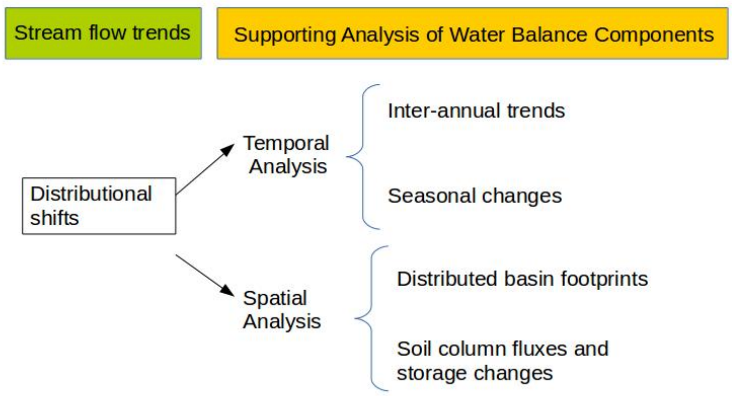
Stream flow trends
Supporting Analysis of Water Balance Components
Distributional shifts
Temporal Analysis
Inter-annual trends
Seasonal changes
Spatial Analysis
Distributed basin footprints
Soil column fluxes and storage changes

Fig. 1.