Hydrol. Earth Syst. Sci. Discuss., 12, C6417–C6429, 2016
www.hydrol-earth-syst-sci-discuss.net/12/C6417/2016/

Hydrology and Earth System Sciences
Discussions
Open Access

HESSD
12, C6417–C6429, 2016

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.

H. A. Moreno et al.

moreno@ou.edu

Received and published: 28 January 2016

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 issued by this reviewer in sequential order.

Responses to Referee #2

Major comment #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."

Responses to comment #1:

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. Specifically, when ignoring vegetation dynamics and phenology as a result of re-growth, species development, competition and mortality. It is also important to remark this caveat when mentioning some of the findings in this manuscript as the reader is fully aware of the limitations of the results. In this sense, when possible, we want to warn the reader about the implications of the simulated outputs and the level of uncertainties involved. This manuscript presents an step-function case of forest restoration that is expected to cause drastic and permanent hydrologic changes, assertion that is not necessarily true but that can evidence short-term post-fire hydrologic shifts. This is also an opportunity to encourage future work considering vegetation dynamics using tRIBS-Veggie or any other hydrologic model that allow vegetation processes.

So, we propose to create a new section called "Model 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 and chiefly:

1. The effects of only considering static vegetation conditions, fully ignoring plant phenology, re-growth, competition and mortality. The influences of not considering key biochemical processes like photosynthesis and primary productivity, plant respiration, tissue turnover and stress-induced foliage loss, carbon allocation, phenology and plant recruitment. We will discuss the 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 et al. 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.

HESSD

12, C6417–C6429, 2016

2. Precipitation uncertainties and influence on the model calibration skill scores. Caveats when interpreting results from this modeling article based on a gauge-corrected precipitation product with a limited number of on-the-ground rain gauges

3. The effects of not considering the recovery of hydraulic conductivity along time due soil elasticity and vegetation processes.

Finally we will include caveat sentences where key conclusions are reached along the manuscript. For example, after the following lines:

- PP 10840 Line 24-25 and Appendix B - PP 10843 LL 6-8 - PP 10855 LL 26-28 - PP 10848 LL 1-6 and point 1-5 in Section 5

We expect to add on the potential uncertainties with these results, particularly for the condition when vegetation re-establishes and/or thinning operations are maintained or intensified in the region.

Minor comment #1

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

Response to minor comment #1

Biederman et al. 2014 on the effects of Pine mortality on snowpack will be added as a reference.

Minor comment #2

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

Response to minor comment #2

This is a general finding in paired basin studies and most of our simulated cases. However in some specific cases, when other controlling factors like basin area or rainfall depth play an important role in regulating base flows, groundwater contributions be-

HESSD

12, C6417–C6429, 2016

come significant and thus thinning might lead to an increase in base flows. We will refer to articles supporting contrasting arguments.

Minor comment #3

“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”

Response to minor comment #3

This is indeed a pertinent reference to add to this study.

Minor comment #4

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

Response to minor comment #4

Units will be changed to SI units. So 2.4x106 acres will be replaced by 9.712x109 m2. 120 ft2ac-1 and 58 ft2ac-1 will be changed by 0.002755 m2m-2 and 0.00133149 m2m-2, respectively.

Minor comment #5

“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.”

Response to minor comment #5

This is correct. The sentence will be changed to this new statement.

Minor comment #6

HESSD

12, C6417–C6429, 2016

“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 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.” Response to minor comment #6

Penmann Monteith in the model is used separately to compute latent heat flux. The surface energy balance in tRIBS is computed from the short and long wave radiation components that are simulated accounting for geographic location, time of year, aspect and slope of the element surface (Bras, 1990). Then, the combination equation (Penman, 1948; Monteith, 1965), gradient method (Entekhabi, 2000), and force-restore (Lin, 1980; Hund Islam, 1995) method are used to estimate the latent, sensible, and ground heat fluxes at the land-surface. An optimum is sought in terms of the soil surface temperature that leads to the energy balance. Soil water content in the root zone and top soil layer constrains evapotranspiration from vegetated surfaces and bare soil. A species-dependent parameterization of stomatal conductance allows for diurnal variation of transpiration flux.

Minor comment #7

“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?”

Response to minor comment #7

Yes. The model initial condition provided by the elevation of the groundwater table was taken from the Northern Arizona Regional Groundwater-flow model (Pool et al. 2011) conducted by the USGS. The estimations from this model were verified using

HESSD

12, C6417–C6429, 2016

Interactive Comment

wells and piezometers within the study region so that the initial groundwater condition was as realistic as possible. After the 1-year spin-up period, the groundwater levels approached to a dynamic steady state with only slight fluctuations during subsequent days. In the manuscript we will add the word "dynamic" to the steady state condition to reflect for this fact.

Minor comment #8

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

Response to minor comment #8

This is correct!. Xjsim should be replaced by the mean of observed values Xmeanobs

Minor comment #9

"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."

Response to minor comment #9

Yes. Unfortunately this basin is poorly gauged. If you refer to the Arizona Water Atlas (http://www.azwater.gov/AzDWR/StatewidePlanning/WaterAtlas/CentralHighlands/documents/Volume_5_TON_final.pdf) you will find that there is only one active snow station operating since 1973 (Promontory SNOTEL). The other station was installed in 1973 but unfortunately was discontinued in 1989, before our study period. In terms of stream flow gauges there were three of them operating since 1964, 1965 and 1913 and discontinued in 1975, 1985 and 1940. The only survivor flow gauge is Tonto Creek near Roosevelt that was installed in 1940 and still in operation. We will briefly mention that this is the only readily available, operative information to calibrate and validate our model.

Minor comment #10

"PP 10843. LL 8, there is a typo."

Response to minor comment #10

Yes. Appendix A2 will be replaced by Appendix B

Minor comment #11

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

Response to minor comment #11

We agree. Will be moving this paragraph to section 3.1 Study Region and Watershed Characteristics

Minor comment #12

"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."

Response to minor comment #12

"We agree. Will be moving these sentences and Figures 9a 9b to section 3.1 Study Region and Watershed Characteristics."

Minor comment #13

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

Response to minor comment #13

We agree. We will move both of these sentences to the Summary and Conclusions section.

Minor comment #14

HESSD

12, C6417–C6429, 2016

“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.”

Response to minor comment #14

We agree. Will move the Figure, equations 6-8 and corresponding description to the “Design of Numerical Experiments” subsection. Regarding the sub-index f, this only represents the net outflow of water from the control volume, regardless of pre or post treatment condition. In other words, it’s only used to represent the difference between inflows and outflows in the saturated GWf= (GWin-GWout) and vadose zones θf=(θin-θout).

Minor comment #15

“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 aspect, 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?”

Response to minor comment #15

This could have been one approach to exploring the effect of solar aspect on the column water balance. However, the way we designed this comparison obeyed to the fact that aspect is not the only variable influencing water fluxes. Other variables like precipitation, elevation, forest density, slope degree, also influence those fluxes. By picking those pairs manually, we are ensuring a more fair comparison between elements of similar characteristics other than aspect, so we could isolate this effect from other effects.

Minor comment #16

“Section 4.3. Given the fact that differences between N and S exposed hillslopes 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.”

Response to minor comment #16

This section has already been reduced several times before submission, but we could, perhaps, omit Figure 13 and its corresponding seasonal analysis as Figure 12 might be conveying most of the results at the inter-annual scale.

Minor comment #17

“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.”

Response to minor comment #17

Yes. The sentence will be changed to: “Hourly time series from the reference and simulated cases are classified by hydrologic period (winter, pre-monsoon, monsoon, and all months included) to understand the probability distribution shifts that forest thinning produces on quartiles (Q1 , Q2 , Q3 , Q4) where Q1 and Q4 correspond to low and high flows respectively, and low order statistical moments (μ, σ) of long-term (20 years) simulations (Fig. 14)”

Minor comment #18

“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.”

Response to minor comment #18

HESSD

12, C6417–C6429, 2016

Agree. We will change these sentences to:

"...need for decision making oriented towards water preservation during dry conditions and mitigation or adaptation of the negative effects of floods on urban settings and ecological communities."

"This shift can increase the risk of negative flood related effects directly downstream of the treated areas"

Minor comment #19

"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."

Response to minor comment #19

We agree. This entire paragraph will be merged/removed so that this entire Summary and Conclusions section is shortened.

References:

Biederman JA, Harpold AA, Gochis DJ, Ewers BE, Reed DE, Papuga SA, Brooks PD. Increased evaporation following widespread tree mortality limits streamflow response. Water Resour Res 2014, 50:5395–5409. doi:10.1002/2013WR014994.

Bras, R.L., 1990. Hydrology: an Introduction to Hydrologic Science, Addison-Wesley/Longman, Reading, MA/London.

Brooks, P.D., and Vivoni, E.R. 2008. Mountain Ecohydrology: Quantifying the Role of Vegetation in the Water Balance of Montane Catchments.Ecohydrology. 1(3): 187-192.

Edburg S.L., Hick J.A., Brooks P.D., Pendall E.G., Ewers B.E., Norton U., Gochis D.J., Gutmann E.D., Meddens A.J., 2012. Cascading impacts of bark beetle-caused tree mortality on coupled biogeophysical and biogeochemical processes. Frontiers in Ecol-

HESSD

12, C6417–C6429, 2016

ogy and the Environment 10, 416-424

Entekhabi, D., 2000. Land Surface Processes: Basic Tools and Concepts, Department of Civil and Environmental Engineering, MIT, Cambridge, MA.

Fatichi S, Zeeman MJ, Fuhrer J, Burlando P. Ecohydrological effects of management on subalpine grasslands: from local to catchment scale. Water Resour Res 2014,50:148–164. doi:10.1002/2013WR014535.

Gough CM, Hardiman BS, Nave LE, Bohrer G, Maurer KD, Vogel CS, Nadelhoffer KJ,Curtis PS. Sustained carbon uptake and storage following moderate disturbance in a great lakes forest. Ecol Appl 2013, 23:1202–1215.

Hu, Z., Islam, S., 1995. Prediction of ground surface temperature and soil moisture content by the force-restore method. Water Resour. Res. 31(10), 2531–2539.

Ivanov VY, Bras RL, Vivoni ER. Vegetation hydrology dynamics in complex terrain of semiarid areas: 1. A mechanistic approach to modeling dynamic feedbacks. Water Resour Res 2008, 44: W03429. doi:10.1029/2006WR005588. Lin, J.D., 1980. On the force-restore method for prediction of ground surface temperature. J. Geophys. Res. 85(C6), 3251–3254.

Monteith, J.L., 1965. Evaporation and environment. Symp. Soc. Exp. Biol. 19, 205 –234. Penman, H.L., 1948. Natural evaporation from open water, bare soil and grass. Proceedings of the Society of London Series A193,120–145.

Pool, D. R., Blasch, K. W., Callegary, J. B., Leake, S. A., and Graser, L. F.: Regional groundwater-flow model of the Redwall-Muav, Coconino, and alluvial basin aquifer systems of northern and central Arizona, US Geological Survey Scientific Investigations Report 2010- 5180, v. 1.1 , Tech. Rep. 2010-5180, US Geological Survey, Reston, VA, 2011. 10840, 10841, 10879

Reed DE, Ewers BE, Pendall E. Impact of mountain pine beetle induced mortality on forest carbon and water fluxes. Environ Res Lett 2014, 9:105004. doi:10.1088/1748-

9326/9/10/105004.

Voepel, H., B. Ruddell, R. Schumer, P.A. Troch, P.D. Brooks, A. Neal, M. Durcik, and M. Sivapalan, 2011. Hydrologic controls on catchment-scale vegetation productivity, Water Resources Research, doi:10.1029/2010WR009944

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