SUBJECT-MATTER EDITOR'S COMMENTS  
Subject-Matter Editor: Kitzberger, Thomas  
  
Comments to the Author:  
  
This a valuable study on an important question concerning the watershed-scale benefits (fire resilience) and co benefits (water regulation) of 40-50 year post-suppression wildfire management. The modest changes in landscape element transitions, landscape structure, forests structure and soil moisture compared to a more productive nearby watershed measured in a previous study are very interesting and important from both the conceptual and fire management perspective. One important (and somewhat hidden) message of the study is that success of fire restoration efforts based on fire reintroduction are highly context (fire history, productivity) dependent. Clearly more productive systems may have shifted during suppression more than unproductive ones, thus, it is somewhat logical to expect that fire reintroduction may conversely show stronger effects in the former than in the latter.  
  
Despite the importance of this dataset I find in accordance with Rev 3 the manuscript to be overly long and descriptive of the SCB situation (also reflected in the rather neutral ms title) and suggest a major re framing and refocusing into a more conceptual ms which could help capture a wider audience. This means synthesizing more the information related to Objectives 1-3 and emphasizing and expanding objective 4 which in my opinion provides the richest and most informative results. As a consequence of change of focus the discussion should be shorter and more oriented towards the responses of systems of different productivity to fire management practices expanding a critical evaluation of wildfire management policy in less productive forest ecosystems.   
  
I have attached a pdf with moderate and  minor comments

REVIEWERS' COMMENTS  
  
Reviewer: 1  
  
Comments to the Author(s)  
  
Overall, this is a very well-written, straight-forward paper describing the effects of managed wildfire on vegetation and water resources. The story is not very dramatic- very little change took place- but I think it’s a good contribution to the literature. My one main question is whether the authors thought about considering fire severity in their spatial analysis? Besides that, I have only a few minor comments.  
  
Line 35: Just reading the abstract, it’s unclear why you would compare with a nearby wetter watershed experiencing similar fire management. This becomes more clear reading the paper.  
  
Lines 65-67: Minor point, but in most places managed fire is only allowed to proceed if specific criteria are met.  
  
Lines 80-82: Is there a way to rephrase this so it’s more clear? I had to read this several times. Maybe give examples of vegetation types and high/low soil moisture, and then expand on the second half of the sentence too.  
  
Lines 82-84: same here- if you could give more detail, the reader wouldn’t have to go back and read the Boisramé paper. What specifically changed compared to historical cover?  
  
Line 100: Why 1973?  
  
Line 102: Why 1970?  
  
Figure 1: Fires 1973-2003 do not show up on the figure. Also I wouldn’t make the weather station and the fires 1952-1972 the same color.  
  
Lines 169-170: Would be helpful to briefly explain sparse meadow vs. dense meadow.  
  
Line 173: the photographs are not really from before the first fires- you removed several small fires from your analysis that burned before 1973.  
  
Figure 2 and elsewhere: Wouldn’t it be just as easy to put times burned: 2-4 or 2+ in the legend? Also, I wonder if it would be interesting to zoom in on any of the places where more changes occurred, for example in the NE corner of the basin. This could be an area where looking at fire severity might be interesting to explain why some areas experienced more change than others.  
  
Lines 557-574: What about the increase in shrubs across the plots? I think the authors should discuss why they think this is happening in the discussion.  
  
  
Reviewer: 2  
  
Comments to the Author(s)

Review of “Forest vegetation change and surface hydrology following 47 years of managed wildfire”

Overview:

The authors describe the effects of managed wildfire in the Sugarloaf Creek Basin (SCB) during the 1971-2018 period on 1) forest density and composition, 2) vegetation cover changes, and 3) soil moisture. They also compare these results with the Illilouette Creek Basin (ICB), which has experienced similar management during this period following changes in National Park Service wildfire management policy. Findings include little change in vegetation cover types and soil moisture in the SCB compared to the ICB. Largely low-intensity fires resulted in very little overstory mortality and no significant reductions in mid- and lower-story tree density. Vegetation type transitions that did occur were typically forest conversion to shrub or sparse meadow classifications. Soil moisture exhibited slight increases in areas experiencing forest fire, though at levels much lower than those observed in the ICB.

General comments:

The authors are to be commended for conducting this much-needed analysis of the effects of fire management in the SCB and compare it to the ICB. This will be an important contribution to future fire management in the western United States. The paper would benefit from a more focused discussion of their results centered on what the authors believe to be their central points. In particular, there should be more of a discussion on the effect of fire severity and time since last fire on their results. While there is brief mention of fire severity, there is no quantitative description with respect to each fire nor to fires in the ICB. Next, the fact that the two most recent large fires occurred in 1985 and 2003 must play a role in the results. The time elapsed since these fires is more than sufficient to erase any soil moisture effects and to permit significant regrowth. While the authors hint at the effects of increased fire suppression since 2003, their data may permit quantification of the impact of that suppression. This may not be possible, but it would be a very important point to make if possible. If not, then a discussion of what is needed to do this would be helpful for the community. Finally, the role of fire management differences between the SCB and ICB is hinted at but should be discussed more directly and concisely when considering differences in outcome in each basin.

Specific comments:

Title: “Surface hydrology” generally refers to water on the ground surface such as lakes and streams. Consider “shallow subsurface hydrology” or “subsurface hydrology”.

Line 89: remove “of” so that first phrase is “ These results suggest a promising co-benefit”

Line 132: Including proportions of each fire that burned in high, moderate, low, and low/none severities would be helpful, along with a comparison to fire in the ICB during the same period.

Line 144: Figure 1 is missing the 1973-2003 fire perimeters.

Lines 305-306: These kinds of rain gauges miss substantial amounts of snow fall because it creates a cone of snow over the gauge. Is the snow melt recorded by the gauge added to the estimates of snow water equivalent from the photos?

Line 320: It is important to point out that cumulative soil moisture as defined here is not a measure of how much soils have received. The metric as it is described is responsive to pulsed input, not steady-state input, as there must be a change in VWC to qualify as an increasing in cumulative soil moisture gain. Hence slow snow melt under heavy snow pack or quick snowmelt through porous sandy soils will likely not be captured by this method.

Line 398 Figure 5: Does this figure apply to all forest plots or just those in *Abies magnifica* plots?

Lines 464-468: Consider adding a sentence to clarify that cumulative shallow soil water gain is not the same thing as cumulative soil water infiltration. While the definition is clear in Appendix B, it seems appropriate to remind readers that they are different especially because plants probably respond to the timing of and cumulative infiltration of surface waters to soils rather than episodic inputs estimated by the soil water gain metric.

Lines 509-517: A comparison of fire severity between the SCB and ICB is warranted here. The two basins burned very differently in this respect.

Lines 516-517: Are these two fires reversed with respect to percentile? One would expect the hotter fire (Williams) to be a lower percentile. Here and in the two subsequent paragraphs, there should be a concise discussion of how/why fires may have burned differently in the SCB and ICB. Are there management differences, fire behavior differences due to terrain and vegetation or both?

Lines 535-537: Roche et al (2018) also shows that fires in the Kings watershed recovered faster (in terms of evapotranspiration) than those in the wetter and cooler American watershed. This may be important in the discussion of evaluating the effects of wildfires in 2017, 13 years after the last major fire in the basin and 33 years after the major fire prior to that one.

Lines 552-556: This seems a central point when it comes to management. How does this compare to the ICB? In the conclusion of the paper, it is stated that differences in fire severity and productivity likely account for the different responses in the basins. This deserves more focused attention in the discussion section, especially with respect to fire management, to make this point stronger.

Line 613. Should this refer to Figure 6, not 8? Figure 8 is measured not modeled soil moisture.

Lines 650-653: It is not clear from this statement, how we might apply findings at ICB to SCB without using imagery or forestry data? Perhaps it is more important to emphasize that this work applies a refined set of tools for evaluating the effectiveness of managed fires and the watershed and forest characteristics that are important to consider.

Lines 656-658: There is some evidence in this paper that differences in management between ICB and SCB may have played a major role (see earlier comments). What is the influence of management? It seems that there is data available to examine this effect.

Line 659: “watersheds” not “watershed”

Appendix B, Line 16-26: How large are the vegetation patches associated with each weather station as compared to those in ICB? What is the confidence that the measurements actually represent the patch vegetation type rather than some other factor such as soil depth, slope, or proximity to an adjacent vegetation type?

Appendix B, Lines 27-36: It is not clear how snow melt or increases in shallow soil water were used to gap-fill the precipitation record. Were site photos used to estimate when and how much snow fell and then estimate the amount of precipitation using the density conversion of 0.4 swe/snow depth? What is the basis of this density conversion? When was soil water gain used to estimate precipitation?  
  
  
Reviewer: 3  
  
Comments to the Author(s)  
  
This work looks at the long-term vegetation and soil moisture characteristics and response to managed wildfire, where fire suppression efforts were relaxed in a watershed in the Sierra Mountains.  The authors, show that in this particular site, vegetation and forest structure change due to the reintroduction of fire was moderate.  They further argue the observed and modeled soil moisture gains from the re-introduction of fire was also moderate.  This is framed in comparison to another nearby but more productive site, also with 40-50 some years of managed wildfire.  This is a topic of great interest and is timely work.  Especially given the long-term perspective to vegetation, forest structure and hydrology of re-introducing fire into ecosystems.  Overall, I thought to presentation, albeit long with excessive use of appendices to be of interest.  I am also impressed with the authors use of historical data and management applications.   
However, I have several criticisms to the work that should be addressed before publication.   
  
First, I am not convinced that the spatially distributed soil moisture observation methods are robust enough to make sweeping conclusions about changes in soil moisture, as surrogate for overall hydrologic conditions following 47 years of managed wildfire.  Though I must admit that the results are what would be expected (that soil moisture gains are not as strong as in more productive sites), I would say at the very least that the measurements and associated analysis only ‘suggest’ that soil moisture change is moderate compared to the ICB site and are not really conclusive evidence.  I am particularly suspect of the application of the RandomForest package as a way of recreating past soil moisture conditions based on spatially extensive but temporally very sparse measurements.  I am also suspicious of the neglect of covariates that constitute first principle soil hydrologic properties, such as porosity, hydraulic conductivity, infiltration capacity, residual saturation, and any discussion of unsaturated soil flow.  Perhaps a more in-depth presentation on how the RandomForest package was implemented is warranted, but even so without the inclusion of more mechanistic soil moisture properties in the analysis, I would say that the work is not very conclusive regarding soil moisture changes at the site.  
  
Second, I recognize that vegetation cover change could certainly have an effect of soil moisture, but I am a bit surprised not to see much discussion on the mechanisms of how vegetation cover change could affect soil moisture, specifically through changes in transpiration, interception and surface energy balances. Also, are rooting depths considered for the different vegetation types? In my opinion a mechanistic link to why vegetation cover and soil moisture is needed.   
  
Lastly, and despite my concerns about the soil moisture analysis, I think there is a very important message and concept that is a bit hidden.  In my opinion, the big message to this work, especially when comparing ICB to SCB is that restoring fire to more productive sites, i.e the ICB site will result in large ecohydrological shifts in comparison to restoring fire to less productive sites which will result in comparatively smaller ecosystem sifts. This is because during fire suppression, more productive sites have more growth potential to move the system away from the fire induced equilibrium, whereas less productive sites have less growth potential to drift away from the fire induced equilibrium.  Therefore, when restoration efforts are imposed there is less system change to recover from.  I think this message is something that a reviewer would get in this paper, but not somebody who doesn’t spend the time picking through the manuscript or only quickly glances at the title, abstract, and conclusion sections.  While it may not be my place to tell the authors what to write about, it is my opinion that the authors should present this work as a way of measuring the response in comparison to system productivity, especially with the way the SCB is compared to the ICB site.  This would greatly improve the impact of the manuscript.   
  
Additional comments:

See comments in attached PDF also

L37:  Perhaps add, ‘observed’ to ‘…response of soil moisture.’  
  
L41-42:  I didn’t see much particular support for the statement that ‘Future fires in SCB could be managed to encourage greater tree mortality’. Seems like to make this statement management goals need to be stated or known.  
  
L73-77:  Of note there is a bunch of hydrologic research that is actively trying to quantify hydrologic changes due to fire.  But the uniqueness of the manuscript here is the several decade perspective rather than the immediate affect.   See: Silva et al., Int.J.Wildfire (2006); Stoof et al., HESS (2012); Ebel WRR, (2013); Cardenas & Kanerek, J. Hydro. (2014); Wine and Cadal ERL, (2016); Atchley et al., VZJ, (2018).  And see Kinoshita and Hogue ERL, (2015) for at least a 10 year perspective.   
  
  
L80-82:  Not quite following the intent here.  Are you saying that because vegetation and hydrology are related, any change in hydrology is because of vegetation change? Or does hydrology determine vegetation?  
  
L100-108:  These questions don’t seem to entirely sync with the paragraph in the conclusion section.  There focus is on soil moisture and how the site change compares to the ICB site.  
  
Figure 1) I’m not seeing the overlapping fire perimeters since 1973.  
  
L272-276: Why not included actual soil hydrologic properties that in large part determine soil moisture, infiltration, and water movement?  Porosity, hydraulic conductivity, van Genuchten type parameters.  While not exactly hydrological data, soil texture can loosely be related to these via pedotransfer functions.   
  
L272-276: Also, were there any vegetation cover density measurements taken?  The reason soil moisture is linked to vegetation is via transpiration and interception.  Functionally this is done by estimating leaf area index either through lidar or densitometer measurements.  These types of measurements or covariants would further link both vegetation type and structure to soil moisture.     
  
L409-410:  In dry conditions variability in soil moisture is generally determined by local soil properties, likely in the case you’ve observed it is from deference’s in residual saturation. See Grayson et al., WRR 1997; Famiglietti et al., J Hydrol 1998; Atchley and Maxwell, Hydrogeology J. 2011.  
Famiglietti JS, Rudnicki JW, Rodell M (1998) Variability in surface moisture content along a hillslope transect: Ratlesnake Hill, Texas. J Hydrol 210:259–281. doi:10.1016/S0022-1694(98)00187-5  
  
Grayson RB, Western AW, Chiew FHS (1997) Preferred states in spatial soil moisture patterns: local and nonlocal controls. Water Resour Res 33(12):2897–2908. doi:10.1029/97/WR02174  
  
L429-446:  It is not clear how the random forest model was trained and validated.  It seems like it was trained and validation in space rather than time, but then it was applied over time, to link to the changes in vegetation over time.  If that is the case, then I would argue it is not a real robust method for estimating soil moisture changes in time.  However, I may have miss understood how this was done, in which case I would ask for clarity.

Gabrielle: It is true that the random forest model was trained in space rather than time. This is necessary, since there are no soil moisture measurements available from the fire-suppressed period. Therefore, we use a space-for-time substitution by relating soil moisture to vegetation cover and other covariates. I will clarify in the text that the model in effect shows soil moisture under different vegetation scenarios, which is related to (but not exactly the same as) changes in time.  
  
L441-443: Is this because SCB is an already dryer site compared to ICB?  What would be the relative changes in VWC compared to the absolute changes?  
  
L463: ‘Figure B2’. Do you mean Figure B3?  
  
Figure 8:  The color differences between depths are not easy to differentiate.  
  
Line:546-547:  What do you mean by ‘fire sensitive?  I’m under the impression that lodepole pine benefits from fire, at least reproductively.     
  
L594: ‘lodgepole pine’. Seems to switch from scientific to common names.   
  
L594-600:  This is a real interesting research line, that in my opinion should be highlighted way more.   
  
L605-607:  Without considering how hydrologic flow works, both in saturated and unsaturated conditions, it’s a stretch to say that the deeper spatially distributed soil moisture will always reflect what is going on at the near surface and act like the weather station observations.  This is especially so given that that spatial measurements where only taken at a few points in time and what the characteristic rooting depths of the vegetation is.

Gabrielle: This is somewhat true. We should change the wording to something like “high correlations … suggest that surface soil measurements can reflect relative changes in deeper soil moisture. However, we do not have enough deep moisture measurements to determine the uncertainty in this relationship between deep and shallow moisture.” However, we did already acknowledge the limitations of this approach later in the paragraph, as well as pointing out that it is backed up by our findings in ICB.

L608-610:  Mechanistically this could happen for several reasons.  1) a reduction in deeper root uptake between forest and shrub/wetland stations and therefore transpiration,  and 2) a change in the surface energy balance where more light and evaporation happens off the top soil in shrub and wetland.  It would be interesting to know what the evaporative demand, or percent cover for each vegetation type is.   
  
  
L656-658.  In my opinion, this is the really cool angle to this work.  I’d like to see it highlighted more.  
  
Appendices:  There are a lot of Appendices, I think some of that information appears to be more appropriate for supplementary information.