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

*Response: We appreciate the Editor’s thoughtful synthesis of the reviewer comments. In accordance with the Editor and Reviewer 3’s suggestions to refocus the manuscript on the contrast between the two basins, we have taken extensive steps to do this, both in our writing and in the incorporation of an additional analysis using LANDSAT-derived NDVI data to demonstrate a clear contrast in productivity between the basins. Additionally, we further discuss the potential importance of differences between the two basins in fire frequency. We have shortened and focused the discussion around these two concepts as requested, while retaining additional important information to help the reader understand the factors in play behind the relatively moderate responses we observed in each of our three primary areas of inquiry: The forestry plot analysis, the vegetation patch analysis, and the soil moisture analysis. A comprehensive response to reviewer comments follows.*

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.

*Response: We appreciate the reviewer’s positive comments. With respect to the question about fire severity we agree that incorporating it is a great idea, and as we found out doing so helped interpret our findings (or lack thereof). One of the problems we encountered was that 10 of the 18 fires in our study (Table A1) burned before 1984, when the LANDSAT program enabled remotely sensed measurements of burn severity. Additionally, three of the post-1984 fires were quite small and were not mapped for fire severity. We did include percent and area burned at high severity in Table A1 for the five larger post-1984 fires. Based on this information and similar information we added from Illilouette Creek Basin (ICB) for comparison, we significantly added to and edited the second paragraph of the Discussion section. This comparison elucidated the large discrepancy in both total burned area and percent high severity between SCB (much less) and ICB.*

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.

*Response: Thank you for the comment, which ties into a number of larger critiques. We believe our expanded focus on the basin comparison is now evident throughout, including the abstract where we have modified the following sentence to read “by comparison to a nearby watershed with higher vegetation productivity and greater fire frequency, the managed wildfire regime at SCB caused relatively little change”.*

Lines 65-67: Minor point, but in most places managed fire is only allowed to proceed if specific criteria are met.

*Response: We agree. This sentence already mentions that certain criteria may preclude allowing the fire to burn.*

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.

*Response: This has been changed to: “Field measurements show that vegetation type is a strong predictor of soil moisture, with dense meadows generally indicating the wettest soils while shrubs and sparse meadows indicate the driest soils”*

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?  
 *Response: This has changed to “Such models suggest that the fire-induced changes to vegetation cover in ICB (less forest cover, but more meadows and shrublands) are associated with an overall increase in water storage and plant available water resources.”*

Line 100: Why 1973?

*Response: 1973 was the first major fire in SCB since fire suppression began, and also the first year aerial photography is available. We have added language to the previous paragraph to clarify this.*

Line 102: Why 1970?

*Response: 1970 was the year when the plots were surveyed. We have added language to the previous paragraph to clarify this.*

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.

*Response: There may have been an issue with image transparency with respect to the fires from 1973-2003 that will be resolved in the final version. We have changed the color on the weather station points.*

Lines 169-170: Would be helpful to briefly explain sparse meadow vs. dense meadow.  
 *Response: We have added clarification that sparse meadow is “areas dominated by bare ground, with sparse shrub and/or herbaceous cover” and dense meadow is “wetlands and other areas of dense herbaceous cover.”*

Line 173: the photographs are not really from before the first fires- you removed several small fires from your analysis that burned before 1973.

*Response: We have changed this to “first* large *fires.”*  
  
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.

*Response: We have made the change in the figure legend (this is now Figure 5, formerly Figure 2). In the other figures (3 and 4 in the current manuscript), no forestry plots burned more than 2 times, so the number “2” on the axes is appropriate. To the author’s broader point about drivers of burn severity, as described above we do not have data on burn severity from fires prior to 1984, which includes the fire that burned in the northeast corner of the watershed.*

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.

*Response: With respect to our discussion of the twice-burned forestry plots, we have added the sentence “The increase in shrubs at all burn frequencies (Figure 4) was expected, as the dominant shrub species of Arctostaphylos and Ceanothus in this system have fire-cued seed germination (Safford and Stevens 2017).”*

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.

*Response: We appreciate the reviewer’s positive feedback and useful suggestions here. The suggestions to incorporate burn severity and time since fire were very good. As stated in our response to Reviewer 1 on incorporating burn severity: “…doing so helped interpret our findings (or lack thereof). One of the problems we encountered was that 10 of the 18 fires in our study (Table A1) burned before 1984, when the LANDSAT program enabled remotely-sensed measurements of burn severity. Additionally, three of the post-1984 fires were quite small and were not mapped for fire severity. We did include percent and area burned at high severity in Table A1 for the five larger post-1984 fires. Based on this information and similar information we added from Illilouette Creek Basin (ICB) for comparison, we significantly added to and edited the second paragraph of the Discussion section. This comparison elucidated the large discrepancy in both total burned area and percent high severity between SCB.” In that same paragraph in the Discussion we added text on time-since-last fire, which for a good portion of the study area was over 30-40 years.*

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”.

*Response: We agree that “surface hydrology” is somewhat misleading here. We have decided to use the term “soil water” instead.*

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

*Response: Thank you for catching this error. It has been corrected.*

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.

*Response: See our response to the Reviewer’s General Comments.*

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

*Response: This was an issue with image transparency that will be resolved in the final verison.*

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?

*Response: We used estimates of SWE from the photos to fill in periods when snowmelt was not captured by the precipitation gauges. Therefore, snowmelt for a given day is either measured by the gauge or estimated from photos, but not both. It is possible that this method still resulted in slight underestimates of snowfall in cases where not all snowfall was captured by the gauge, but this is expected to be a small error compared to the total volume of precipitation. We added more detail in Appendix B to clarify this process.*

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.

*Response: We agree that steady-state infiltration is not going to be calculated in the cumulative soil moisture gain metric and therefore changed the sentence to reflect this: “However, in saturated wetland sites and during periods of steady-state infiltration, cumulative water gain cannot be calculated”. However it is unlikely that there are extensive consecutive 10 minute intervals in the soil moisture record that reflect steady-state conditions. Based on our analyses of soil moisture time series (Figure 9), we believe that there is at least a temporary increase in VWC whenever infiltration is occurring under unsaturated conditions.*

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

*Response: These data apply to all plots across vegetation type, as in the current Figure 3. We have updated the figure caption (currently Figure 4) to reflect this.*

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.

*Response: We added the following sentence to clarify the cumulative soil water gain metric: “Cumulative soil water gain reflects any detectable increase in VWC of shallow soil, however it does not always reflect change in storage or availability of water for vegetation uptake.”*

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.

*Response: We agree, and incorporated burn severity as previously explained. Thank you for pointing this out.*

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?

*Response: Good points – the percentiles are indicative of the range of maximum temperatures observed in all major California fires, therefore lower percentiles are “cooler fires” (the 99th percentile of maximum temperature in this dataset is the hottest fire). We believe the revised discussion now mentions multiple instances of different potential fire behavior between the basins, due primarily to differences in productivity (e.g. Lines 569-580).*

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.

*Response: We have added the following discussion point to line 628 to address the possibility that the lack of change is related to the long time since fire: “It is possible that fire might have greater impacts on soil moisture at shorter time scales; our hydrologic data collection all took place at least a decade following the most recent fire, which could be sufficient time for ET processes (which impact soil moisture) to recover to pre-fire conditions (Roche et al. 2018).”*

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.

*Response: The discussion section has been extensively edited to focus more on the differences in fire severity and productivity between the basins. We have also added more information about differences in the number of recent fires between the basins.*

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

*Response: This was supposed to refer to Figure 7 in the initial manuscript submission (the figure showing fire-caused changes in modeled soil moisture), and we thank the reviewer for catching this. However, now that our current figures have changed due to manuscript reorganization, Figure 8 is now the correct figure.*

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.

*Response: The point of this section was that it would be incorrect to assume that the degree of change in ICB was reproduced in SCB, despite their many similarities (both watersheds have had the same fire management strategy for the same amount of time, and they are similarly-sized Sierra Nevada catchments at similar elevations). We have updated the sentence in question to now read “While the nearby ICB is similar to SCB in size, elevation and forest types- as well as the amount of time they have been managed under a wildland fire use policy - assuming similar fire-related changes in SCB would have overestimated fire-driven change in vegetation and in water availability, highlighting the importance of the place-based field and imagery datasets that we used in our analysis here”*

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.

*Response: Again we don’t have the data to comprehensively address this, but in the discussion we do emphasize the importance of continuing to burn these watersheds, which we believe ICB has done with greater effectiveness than SCB.*

Line 659: “watersheds” not “watershed”

*Response: Thank you, this has been corrected.*

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?

*Response: All weather stations are located at least 30 m from the nearest edge of their respective vegetation patches (this information has been added to new line 25 in Appendix B). We chose the locations in order to have three fairly homogeneous patches that were close to each other, which meant we could not always choose the largest patch available. The shrub plot in ICB is in a much larger patch than in SCB (~250m and 100m across, respectively), but otherwise the patch sizes are similar. We know that soil type is similar between all station locations (except for the shallowest wetland soils) from measuring soil samples, as discussed on Appendix B lines 52-58. We also know that all soils reached a depth of at least 1 meter. Each set of 3 stations have similar slope, aspect, elevation, and TWI, though the forested sites are slightly steeper (We have added Table B1 to Appendix B to show this). There are definitely some subsurface differences (e.g., the wetland sites are associated with subsurface flow paths that accumulate water), but all sites were forested prior to being burned.*

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?  
 *Response: We appreciate the reviewer’s call for more clarity here. There are two periods where snowmelt was used to gap-fill precipitation record; the first period was January -Feb 2017 and the second period was May through June of 2017. Using CDEC snow surveys, snow density does range 25-55%, thus the approximation of 0.4 swe/snow depth. However, we went back and refined density values in our analysis. For January/February, snow density at Rowell Meadow (closest snow survey to weather stations) was 30% and for May/June snow density was measured 52%. Snow melt was only used to gap-fill values in SCB for these two periods. Shallow soil moisture was used to identify periods of soil water inputs but was otherwise not used in gap-filling itself. In SCB, at most 10% of the precipitation record was gap-filled using snowmelt, of which only 2% had non-zero snowmelt values. As a result of implementing these changes, values in Table 1 and Figure 9 of the main paper and Table B2 of the Appendix are slightly different, though the story is unchanged.*

*To keep Appendix concise, we did add additional information about gap-filling but left out some of the details from the above response: “When all three stations were missing precipitation data (only the case in SCB), we first identified periods of snowmelt using increases in shallow soil moisture and then gap-filled these periods using snowmelt (as determined by a decrease in snow depth observed from cameras). Snow depth was converted to SWE using snow density measurements taken at Rowell Meadow (station RWM, cdec.water.ca.gov), a nearby snow course: .30 cm water/cm snow in January/February 2017 and increased to .52 in May/June of 2017. All predictions were rounded to the nearest 0.1 inch (2.54 mm), the smallest increment in the rain gauge.”*

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.

*Response: We appreciate the reviewer’s positive assessment and respond in detail to the criticisms below and in the revised manuscript.*

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.

*Response: We do not have verifiable information on soil properties across the full watershed. Therefore, we had to rely on other variables. We have added text to explain this: “While information on soil type may have increased this model’s accuracy, we did not include soil properties since we did not have verifiable basin-wide soils data that would have allowed us to upscale the measurements to the rest of the watershed. Since random forest is a statistical model, rather than a physically-based model, it does not require information on physical soil parameters in order to represent soil moisture, as long as the covariates used are correlated with soil moisture state.” The responses to specific comments below further explain how we have added more clarification of how the model was used and validated.*

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.   
 *Response:* *We have added a description of how vegetation change alters hydrology: “Reductions in forest cover due to fire caused a combination of reduced interception, reduced transpiration, and altered snowpack dynamics, which drove the soil moisture increases (Boisrame et al. 2019).” In a similar vein, we have added an explanation of why drier watersheds may be less hydrologically responsive to vegetation change: “… drier conditions may make the SCB less hydrologically-responsive to wildfire-induced changes. This is because any additional water that becomes available (e.g., due to reduced interception and less competition for water in a fire-thinned forest) in a water-limited forest is likely to be taken up by the already water-stressed vegetation rather than contributing to increased streamflow or soil moisture.”*

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.

*Response: As emphasized by the editor in chief, we agree that this is a helpful suggestion, and we have accordingly structured a major revision around reorienting this paper to focus on the differences between the basins and the ways in which those differences (namely in fire history and climate/productivity) could drive the differences we observed. Most notably, we have incorporated an explicit analysis of basin productivity (Figure 2) to justify these explanations. We greatly appreciate the reviewer’s helpful suggestion.*

Additional comments:

L37:  Perhaps add, ‘observed’ to ‘…response of soil moisture.’

*Response: Our observed soil moisture only shows us the soil moisture for the last few years. In order to estimate long term changes due to the altered fire regime, we had to use the observations to fit a model that could extrapolate the measurements in space and time. Therefore, it would not be accurate to say “observed” here.*

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.

*Response: Fair point, we have used more specific language here about increasing mortality in areas likely to see hydrological benefit.*

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.

*Response: We absolutely agree. There is a large and important body of literature that relates to the hydrological responses of catchments post fire. We concur that the majority of this work addresses the immediate consequences of fire for watershed hydrology, with a smaller body of work tracing the longer-term process of watershed recovery from fire disturbance. We would argue, however, that the importance of this study is slightly distinct from the duration of the post-fire analyses. What is unique here is that we are considering the effects of a change in fire regime - a sustained change in the disturbance pattern – over multiple decades. To our knowledge this is one of very few studies exploring this topic. We have added the following new text: “Although there is a well-established literature in fire hydrology (e.g., Stoof et al. 2012, Ebel 2013, Wine and Cadol 2016, Atchley et al. 2018), studies that explore longer-term hydrological responses (e.g. over decadal scales) are rare (but see Kinoshita and Hogue 2015). The sites in question here allow the investigation of not only a longer-term set of hydrological responses to fire, but more interestingly again, the responses to a change in fire regime and the imposition of multiple disturbance events on a catchment.”*

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?

*Response: These lines have been edited to be clearer. For the purpose of this section of the paper, it does not matter whether hydrology determines vegetation or vice versa, it only matters that they are correlated and one can therefore be used to predict the other.*

*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.  
 Response: We note the subtle re-organization of research questions here and the general increased emphasis on ICB comparisons throughout the paper; in general we believe some disconnect between the research questions and the Conclusions is acceptable; the first three questions highlight what we set out to investigate, and the fourth question and Conclusion emphasize one of the most interesting findings to emerge from this research. We believe each of the three specific research questions are extensively addressed in the Discussion section and the Conclusion section rightly emphasizes what we perceive to be the most interesting findings.*

Figure 1: I’m not seeing the overlapping fire perimeters since 1973.  
Jens will fix

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.

*Response: We do not have verifiable information on soil properties across the full watershed. Therefore, we had to rely on other variables. We have added text to explain this: “While information on soil type may have increased this model’s accuracy, we did not include soil properties since we did not have verifiable basin-wide soils data that would have allowed us to upscale the measurements to the rest of the watershed.”*

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.     
 *Response: It is true that vegetation density is linked to soil moisture. However, we do not have watershed-wide estimates of vegetation cover/density. Even if we could estimate this from remote sensing, we would not have corresponding remote sensing data for 1973 (the year of our pre-fire vegetation map). Since a large part of the goal in relating soil moisture to vegetation was to be able to estimate pre-fire soil moisture and compare it to contemporary conditions, it is not useful to us to include variables that cannot be used for fire-suppressed conditions. Further, the coarse-scale vegetation categories we used here make it easy to apply these methods to other basins using commonly available data (e.g. aerial and satellite imagery).*

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  
 *Response: Thank you for this excellent list of references. We believe that a deeper discussion of the seasonal changes in soil moisture is outside the scope of this paper, since it does not answer any of our main scientific questions, but we will use this information in a future paper which focuses on soil moisture dynamics in relation to climate.*

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.

*Response: The methods section explains how the model was trained and validated. We have added more text to the methods section to clarify how we used the model to estimate changes due to fire: “To estimate soil moisture levels in the absence of fire, we modeled soil moisture on the same 40m grid, with the same covariates, except that we set times burned and fire severity to zero, time since fire to 100 years, and replaced 2014 vegetation cover with 1973 vegetation (since this vegetation represents the watershed’s state after years of fire suppression). We then compared these two modeled soil moisture datasets - one with “unburned” conditions and one using contemporary vegetation and fire histories – in order to quantify the change in soil moisture due to fire. This technique assumes that only a negligible amount of vegetation change between 1973 and the present is due to causes other than fire, which appears to be supported by the fact that the largest patches of changed vegetation occur in burned areas (Figure 5d).”*

*We also added text in the methods section to clarify how we aggregated the point measurements of soil moisture and extracted model covariates from various maps.*

*The model is not truly applied over time. The model is used to compare what the soil moisture is now to what it would likely be if there had been no fires in the watershed (leaving vegetation cover as it was prior to 1973).*

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?  
 *Response: No, the ICB changes occurred across the same range of soil moisture values as are shown in Figure 8. We have added the relevant figure from Boisrame et al (2018) to the supplemental materials for comparison (Figure D6).*

*We chose to focus on absolute changes, rather than relative changes, because relative changes exaggerate the importance of small changes to dry soils, even when these changes don’t actually add enough water to make a difference to vegetation or water movement.*

L463: ‘Figure B2’. Do you mean Figure B3?  
 *Response: Yes, we have changed this, thank you.*

Figure 8:  The color differences between depths are not easy to differentiate.  
 *Response: The lines were made thicker and the light orange was changed to be lighter. Thank you for the feedback, we hope the graph is now more readable.*

Line:546-547:  What do you mean by ‘fire sensitive?  I’m under the impression that lodepole pine benefits from fire, at least reproductively.    *Response: We have clarified to say that lodgepole pine is an example of a species more easily killed by fire (what we meant by “fire-sensitive”), which we saw increase even in twice-burned plots. This is a counterintuitive result which we attempt to explain in subsequent sentences.*

L594: ‘lodgepole pine’. Seems to switch from scientific to common names.   
 *Response: Good catch, we have reverted to scientific name here.*

L594-600:  This is a real interesting research line, that in my opinion should be highlighted way more.   
 *Response: We are glad that you find this line of thought interesting. However, without a more in-depth study of tree dynamics at the edges of meadows in both watersheds, we do not believe that we have enough data to elaborate further at this point.*

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.

*Response: The reviewer is correct that we cannot claim that deeper soil moisture always reflects the surface conditions. However, the similarity to patterns observed in ICB suggest that this is not simply a one-time phenomenon. We have altered the wording to be more clear about the limitations and uncertainties associated with this type of comparison, such as changing “show” to “suggest,” and adding “However, there is high uncertainty regarding the changes to deeper soil water storage, since we cannot determine how broadly these relationships between deep and shallow soils extent beyond the weather station locations.”*

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.

*Response: These are interesting hypotheses, however we do not have the data readily available to test them, and we believe the scope of the manuscript is already broad enough to limit the addition of additional investigations beyond those have already added to this point.*

L656-658.  In my opinion, this is the really cool angle to this work.  I’d like to see it highlighted more.  
 *Response: This has been one of the main revisions of our manuscript and we have added more text in multiple locations to further discuss the lower productivity and fire activity in SCB compared to ICB, as is described in responses to other comments.*

Comments Imported from PDF, which we believe was submitted by Reviewer 3:

Figure 1:

the legend Fires 1973-2003 without outline color should be removed from the main panel. Also, re: the Streamflow Gauge on the Kings River: It's just outside the range of Figure 1, but you could potentially zoom out a little to show it.”

Figure 2:

yellow color of burned once is hard to see in panels c and d, please change. please change in legend to "2-4". eliminate the legend NO change to MC and no change from MC as it represents background and does inclued other cover types (e.g. granite to granite, shrub to shrub, etc).

*Response: We have made all the suggested changes; this is now Figure 5. Thank you for these recommendations.*

Figure 3:

please reorder cells in order to have MC in the first row and col and early successional types in rows/cols 2, 3 and 4, this will ease the interpretations to the reader, also the year prefix is not necesseary for labeling each row/col. Also last sentence should read "to 2014"

Conclusion:

I would go back to the question about benefits (fire resilience) and co-benefits (water regulation) to critically analyze the value of managed wildfire policy on less productive watersheds

*Response: We believe the sentence in the Conclusion “In SCB the lower overall productivity and the lesser proportions of high severity fire effects relative to ICB led to greater stability in vegetation over time and a more muted hydrological response to managed wildfire in SCB” addresses this concern.*

Appendices:  There are a lot of Appendices, I think some of that information appears to be more appropriate for supplementary information.

*Response: We believe the Appendices are equivalent to supplementary information and are not sure of the reviewer’s point here; we are happy to work with the editor to modify the presentation of our supplemental material.*