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?