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Hydrology and Earth System Sciences
Discussions



# *Interactive comment on* "Application of MODIS snow cover products: wildfire impacts on snow and melt in the Sierra Nevada" *by* P. D. Micheletty et al.

**Anonymous Referee #2**

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Manuscript Summary: The authors examine wildfire impacts on fractional snow covered area using remote sensing (NASA's MODIS imagery). They evaluate two snow covered area remote sensing products to determine which product more effectively captures fractional snow covered area in a landscape with mixed forest cover subject to wildfire. Comparison with an unburned area with similar hydroclimatology allows separation of climate impacts (i.e. a paired watershed approach) from wildfire disturbance impacts. Major conclusions from this work are (i) there is a larger fractional snow covered area post-fire, (ii) complete melt takes place 9 days earlier (on average) after wildfire, (iii) the MODSCAG product was better than the MOD10A1 product for frac-



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tional snow covered area estimation post-wildfire, and (iv) and that vegetation canopy recovery in this burned basin takes longer than the 5 year monitoring period.

Overall, I find the manuscript clearly written and technically strong. The results will be of wide interest to scientists and engineers in the hydrologic community and help shape future studies.

I have included some questions and suggestions that I feel will strengthen an already excellent manuscript.

Major Points of Review:

1.) I have a few questions regarding the burn severity metric used in this work. The authors chose to use soil burn severity, which is more commonly associated with assessment of runoff increases from rainfall. Yet their basin is snow-dominated, in terms of runoff and precipitation. Soil burn severity is also qualitatively classed. Why not use the change in the Normalized Burn Ratio (dNBR) derived from Landsat data, which is readily available? dNBR is on a quantitative scale (not arbitrarily classed for the purpose of rainfall-derived runoff) and is also known to reflect changes in canopy structure and is less sensitive to soil burn severity. Have the authors considered that, since the major shifts in energy balance that drive fractional snow covered area changes are caused by canopy removal/alteration, an alternate burn severity metric might reveal more quantitative nuances in snow processes after wildfire? Since the daily fSCA is already disaggregated to match the 30 m pixels of the soil burn severity map for the Moonlight Fire, this might not be too tough to try for a future analysis, although it may be beyond the scope of this manuscript?

2.) For the MODSCAG estimation, the non-snow endmembers are taken from a library of field and laboratory measurements (not from this site, P7520, L5-25). Are there any burned vegetation or wildfire-impacted soil endmembers in this library? If not, it would seem that the substantial albedo effects from wildfire might adversely bias the MODSCAG results? For example, on P7530, L12-15, it is stated that the MODSCAG



HESSD

11, C3710–C3713, 2014



product has a higher linear correlation to soil burn severity and shows larger increases in fSCA compared to the other MODIS product (MOD10A1) because of the pixel mixing analysis. Is this pixel unximing in MODSCAG (and the fSCA analysis) not affected by wildfire members (if they aren't already accounted for in the spectral library)?

3.) The water resources implications of earlier melt and changes in snow covered area are given as primary motivations for this work. Certainly no one would argue that the Western US is highly dependent on snowmelt from mountainous areas that are vulnerable to wildfire. It would be helpful, if it is feasible, to have some perspective on what a 9 day (on average) earlier melt means in the Moonlight area. If there is a larger fSCA (as shown in Figure 8) and potentially more SWE and runoff (although that's beyond the scope of this work), do the changes in snow cover attributes matter substantially when the water is stored in a reservoir? Beyond the water quality issues that the authors have shown in previous work, do these shifts in snow cover substantially impact water availability to municipal and agricultural consumers? In particular, a reader looking at the large variability in snow season length in Table 2 might conclude that 9 days seems small relative to natural climatic variability. Is it more that wildfire concurrent with a drought is the "straw that breaks the camel's back" for water resources? This comment is more seeking clarification than a criticism.

Minor Suggestions/Comments P7515, L15: This isn't totally clear, do you mean that even small impacts in forest structure can have large impacts on water resources? The Milly et al. (2008) citation muddles this sentence up some. Suggest rewording for clarity.

P7516, L19-25: This paragraph does an excellent job of clearly laying out the focus of the manuscript for the reader.

P7517, L 3-4: I think this needs a citation to whatever publication has demonstrated this. Or rephrasing into a topic sentence for the paragraph followed by whatever publication has shown this statement to be true to a specific significance level.



HESSD

11, C3710–C3713, 2014

P7517, L14-15: If slope aspect is approximately evenly distributed, then it doesn't seem like "dominant" is the best word to use. Also, I suggest replacing "slope" with "aspect" on L15.

P7520, L23-25: The RMSE is given as 5%, it is not clear if this is for grain size or snow covered area?

P7534, L18: Replace "colleges" with "colleagues"

Figure 4. I think it should be clarified in the caption that the Tmax, Tmin, and precip data are based on PRISM estimates and not actual measured data.

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