Airborne Lidar and machine Learning Reveal Decreased Snow Depth in Burned Forests
Koshkin and Marshall
Review of egusphere-2025-4081

This article aims at quantifying the impact of forest fire on the seasonal snowpack distribution in the Sierra Nevada in the western US. It leverages a time series of 115 high-resolution snow depth maps measured with airborne lidar between 2015 and 2024, fire and topographical data to generate predictive model of the snow depth. The impact of fires is further evaluated by applying the models to different scenarios assuming fires and lack of fires.

The topic is interesting and the problematic is well adressed, using a large amount of state-of-the-art dataset. The article is well-written, the conclusions seem valid and are clearly presented. Figure 3a, for instance, is a great illustration of the varying impact of fires across the snow season.

My comments below should be easily adressed. I would only recommend to improve the figures and to clarify a bit the method. I understand that using the specific terms is a very effective way to communicate the methods to an expert audience but this might not be the case for most readers of the Cryosphere (see comments about L181-187).

Suggestions of text modification are in **bold**.

L24-L37 Could these paragraphs be reorganized ? I would keep the first paragraph geographically general about snow and fire (global scale), and only introduce the region of interest in the second one. The focus on western US arrives too early and a bit as a surprise (L27 « While recent work... »)

L25 order chronologically the citations

L41 « a 1150 % increase in area burned from 1984 to 2020 » is this an increase in yearly area burned ? Otherwise hard to grasp since the previous metric (13%) is for a time period (1984-2020).

L43 « compared to **the 2000-2019 average** » ?

L46 Varhola et al. (2010) is not very specific for fire disturbance. Maybe there is a more adapted reference ?

L56 « (←»

L107 « the occurrence of at least one wildfire between 2015 and 2024 » are fires defined by a minimal burned area ?

L108 to avoid () close to each other : « **in** 2021 (... », « **in** 2023 (... »

L115 Consider adding a Study site section with some informations about the topography, forests and fires type.

L120 « procure » is not completely clear here for a non-native speaker. Could it be replaced with « order » ?

L122 « 2e17 km² » There might be a unit problem here and in Table 1 (basin of the order of 10^{15} km²). The whole Earth area is ~10^8 km².

L123 « is less than 8 cm **over flat terrain** »

L133 « Previous studies have shown snowpacks recover from post-fire impacts within 10 years following a fire » Isn't it the vegetation which recovers within 10 years ? It sounds as if the snowpack is recovering, independently of the vegetation.

L143 « Pixels ~~were filtered to those~~ with **tree cover lower** ~~greater~~ than 10 % ~~tree cover~~ **were excluded** to constrain the analysis to forested areas »

L163 « Areas that burned prior to 2015 were treated as unburned. » I guess this is a valid approximation but it seems inconsistent with L133-134 where it is stated that the recover period is 10 years. Maybe it can be justified that most of the recovery occured within 5 years ?

L171 « a daily SWE product » provide some details about the resolution and the scale of the product.

L179 «)~~}~~ »

L181-187 This paragraph is a bit hard to grasp for a non-expert in machine learning which might be the case for many in the Cryosphere readers.

If « parameters » and « hyperparameters » are the same thing, could only one term be used ?

L183 « the parameters were selected for the model with the lowest cross-validated root mean squared error » => « the parameters **of** the model with the lowest cross-validated root mean squared error **were selected** » ?

A workflow figure showing the different steps would help.

L181 « before running the XGBoost model **on the complete areas/domain/grid points/pixels**» ? Otherwise I am confused about the hyperparameters optimization step : are there no model calculated at that step ?

L182 « 5-fold cross-validation out-of-sample comparison, » could this be also explained with less lingo ?

L183-184 « CV RMSE » further in the text « CV-RMSE »

L184 « under null conditions » idem, less lingo, more terms specific to this case (fire, burned...)

L184-187 « The hyperparameter optimization was applied to… » Should this be moved the first time hyperparameters optimization is mentionned in the previous paragraph ?

L190 « canopy cover was set to 10 % » Is this empirical or is there a justification?

L190 « burn severity was set to 4 (high burn) » I would need to know if this is a reasonable hypothesis (most fires have this severity) or if this is an extrem case scenario to measure sensitivity. Then, it could be discussed (shortly) how lower intensity fire would impact the snow depth. Maybe remind too this hypothesis in the abstract and conclusion.

L205 Isn't the CV-RMSE correlated with the absolute snow depth value (larger CV-RMSE with thicker snow depth)? This could partially explained the difference in CV-RMSE between burned and unburned areas.

L206 Spell « IQR » once in the text.

L274 Is it realistic to have forest at these elevations ?

L284-285 « high elevations remained consistently negative across both seasons » How is the distribution interpreted for month -3 in Figure 5b. At that time, all the distributions seem similar, high-elevation do not seem especially negative.

L337 « This seasonal contrast is further <u>explained</u> by elevation. » I am unsure about the term « explained ». Could it be more accurate to say « enhanced », « correlated » ?

L347 « This contrast could be an artifact of the definitions of high elevation as Koshkin et al.

(2025) » Please provide more details about the differences in high-elevation definitions.

L390-392 Cite the key RMSE of these studies.

L397 « complex dataset » be more precise : what type of complexity ?

L424 You might want to link this idea to Raleigh et al. (2025) which advocates for the use of adding few in situ stations at key locations.

Raleigh, M.S., Small, E.E., Bair, E.H. *et al.* Snow monitoring at strategic locations improves water supply forecasting more than basin-wide mapping. *Commun Earth Environ* **6**, 665 (2025). https://doi.org/10.1038/s43247-025-02660-z

L436 «**The** decrease in snow depth post-fire » Otherwise it sounds like an hypotetical decrease.

L446 missing a « w » at the very end of the link.

Figure 1. Background image could use some improvement : at least a different color for ocean and land ?

State what do the white part in the basins show. Is the legend right ? Low elevations seem masked.

The legend is too big. Maybe split the map in 3 : one small panel overview showing the whole region with some geographical landmark (ocean, countries, ranges…), one panel over Feather and American and one panel over the other basins.

Figure 2. In the legend : « Burn Burn No Burn. » ? a, b, c are way too high.

Aestethic detail but it is disturbing that the boxplots do not have the same width.

Figure 3. Maybe it is not necessary to show the median, many bins only have one or a few points. At least, change the symbol of the median.

Having the same y-scale for all plots would help compare between basins.

Figure 4. It is really not easy to get information out of this figure. The colorbar is too large and the maps too small. Reduce the space between the maps, for instance by shifting the north in the maps to make the basins aligned vertically. Or provide a zoom on regions of interest and larger maps in supplement. Idem for S4.

Figure 5. Put the median line in white on the dark boxes.

« (a) Interquartile range » is for the boxes. What do the lines show ?

References

L572 Missing a doi for Koshkin et al. (2025).