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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Developing a representative snow monitoring network in a forested mountain watershed" *by* Kelly E. Gleason et al.

Anonymous Referee #2

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General Comments

Gleason et al. present a case study detailing how a binary regression tree (BRT) model can be used to identify major statistical classes of snow accumulation based on readily observable physiography. This regression tree classification/ model subsequently is used to inform a detailed snowpack monitoring array/ network. The specific application is in the MacKenzie River basin in the Cascade Mountains of Oregon, an area subject to strong elevation gradients in snowpack accumulation and potential quite susceptible to climate change. The result of this effort was the establishment of the Forest Elevational Snow Transect (ForEST) which has been operational for five years.

The case is made that an objective approach like the BRT is preferable to errors as-

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sociated with a subjective siting based on heuristics or experience. Specifically, the drivers are: elevation, land cover, percent canopy cover, slope, NDVI, and latitude. Not terribly surprising, but nice to see objectively defined. Although technically sound, the paper could do a better job of closing the circle on a compelling story or novel contribution. Discussion of climate change, monitoring, and site selection are mixed making it difficult to determine exactly what is the contribution of this effort beyond adding another site at slightly higher elevation. A clearer analysis of how the BRT guided siting resulting in a more representative/ predictive/ useful network in a climate change scenario is needed. For example, how can others use this approach without physically validated SWE distributions? Should we expect this classification to hold elsewhere or even here under climate change? Indeed, should we move the discussion beyond April 1st to better inform water resource management? That date is a compromise after all and it very well may be that a more representative/ useful for water resources monitoring network is not wedded to that date (heresy I know). I can't say which of the above (or others) are most worthwhile avenues to pursue, but after reading the manuscript several times, I am left with the sense that something is missing.

Specific Comments

Although there is a certain level of objectivity in the BRT, I suggest that the modeling approach carries its own set of biases first in the physical model which must scale point observations to a 3-D snow cover while only the wind redistribution part of the model operates in 3-D including non-local effects. A number of papers suggest that edge effects of vegetation on energy balance as well as remote topographical vs local vegetation shading influence snowpack mass and energy balance. These are difficult to include, especially at the scale of this exercise, but they clearly may be relevant for future scenarios. Instead, the strength (of using the BRT and physical model) is being able to accurately evaluate these assumptions by making them explicit, rather than suggesting they don't exist. No model is perfect.

Second, the statistical approach used in the BRT assumes stationairity in the pro-

cesses from year to year for your comparisons as well as for future predictions under climate change. This leaves me confused by the 50% difference in BRT vs physical model SWE for 2012. How was the BRT initialized for 2012? Did you force the tree structure to replicate that from 2009 or did you let the model form its own structure? The observation that patterns persist is nice, but it seems that there is information in the differences between the two estimates of SWE volume.

I suggest showing the tree and order/ strength of nodes. This would strengthen the presentation of the BRT relative to the table.

Figure 3 – At first glance (and second) it seems that only one of the new locations is in the area of volumetric SWE accumulation. In other words, one location is too low in elevation and there is a large volume higher in elevation that your highest site. Just eyeballing it perhaps 30 to 40% of SWE is above that location.

Figure 5 - I find this confusing - the classes open, forest, and all don't seem to make sense. perhaps you are trying to communicate too many messages? area, volume, and controls all in one figure with no indication that the volume differences between forest and open are due to area or depth

Figure 6 why show max and minimum as it muddies the difference between location and year.

Technical comments

Page 1, Lines 22-26 – It seems that the current ForEST network of sites is the result of the BRT modeling exercise, if so, say that directly rather than back in to it as currently written

Page 4, Line 25 and on to next page - Unclear how NLCD, 30m LANDFIRE, and 250m NDVI were used (what data from each source) and/ or aggregated to reach 100m spatial resolution. Specify this up front so the reader doesn't need to go back and forth between results and methods

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Land cover is variously referred to as forest or open, and is synonymous with veg class, correct? This should be clarified.

Table 2 – what are units?

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