Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-317-AC2, 2016
© Author(s) 2016. CC-BY 3.0 License.

Hydrology and Earth System Sciences
Open Access
Discussions

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

Kelly E. Gleason et al.

kellyegleason@gmail.com

Received and published: 5 November 2016

Dear Reviewer,

Thank you for your comments and recommendations for the revised manuscript, they were very helpful in presenting this research in a more robust and defensible way. In order to "close the circle" and tell a more compelling story, we have made multiple changes to the revised manuscript. We focused the paper solely on the objective approach to improve snow observational network design, and therefore omitted the evaluation of the SNOTEL network under climate change. We acknowledge the limitation in the initial analysis conducted in 2010 which was based on data from 01 April 2009, with the assumption it represented maximum snow accumulation across the basin during an

average snow year. To improve upon this in the revised manuscript we used data from the five days centered on the date of actual peak SWE in the McKenzie River Basin for an average year 2009, an above average year 2008, and a below average year 2005. Evaluating the BRT-derived snow classes from three years of SWE data enabled us to use a more robust analytical approach including omission and commission statistics of overall classification accuracy.

Interactive comment on "Developing a representative snow monitoring network in a forested mountain watershed" by Kelly E. Gleason et al. Anonymous Referee #2 Received and published: 15 September 2016

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 associated 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.

In the discussion we acknowledge the many potential sources of uncertainty in this method, in the following statement, "This objective method is a useful tool in classifying snow characteristics across the landscape to determine representative locations for intelligent snowpack monitoring particularly in physiographically complex landscapes. Although it is an improvement over more commonly used heuristic approaches to site selection, the method incorporates uncertainty as a result of compounding statistically-, physically-, and spatially-based models which justifies caution in implementing these estimates in management decisions. However, the method meets assumptions of non-

parametric data analysis, is performed with relative ease, and if data are available for the research basin of interest, it can be well validated. As even physically-based models incorporate inherent empirically-based historically-derived assumptions, there is also uncertainty in using this approach to represent future spatial variability in snow accumulation."

Second, the statistical approach used in the BRT assumes stationairity in the processes 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?

Yes, good point. We reran the BRT models using peak SWE for three years of data to develop a more robust comparison of the spatial distribution in BRT-derived snow classes between years. In the revised manuscript we used the average year SWE data to build an optimal BRT model, then used the same parameter conditions with SWE data from an above average year and below average year and let the model form its own structure. We then used omission and commission statistics to compare the accuracy in the spatial distribution of these BRT-derived snow classes between years.

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.

In the revised manuscript, we simplified the final BRT model to just two predictive variables to reduce multi-collinearity between variables and prevent overfitting the final model. It is a good suggestion to show the BRT tree for clarity, however now that the final BRT model has been simplified, we think it is redundant to show the associated tree.

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.

To address comments on the stationarity assumptions and to simplify the overall manuscript we have excluded the discussion of climate change throughout the revised manuscript. We acknowledge that the ForEST station locations are not distributed in space, but span the parameter space in the spatial variability of SWE across the MRB for an average year, above average, and below average year. There is a large area of SWE above our highest site which was not feasible for us to monitor.

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

We have modified this figure in the revised manuscript, and hope it is easier to understand. Also we included a discussion of how interrelated the volume differences between forest and open areas are a function as both area and depth. The following has been included in the revised manuscript in the caption for Figure 5, "In the mid-elevations, the forest vs. open distinction is statistically important in distinguishing snow classes. In the high-elevations, above treeline, only elevation significantly drives variability in snow accumulation. Mean SWE increases but volumetric SWE decreases as the land area decreases at the highest elevations."

Figure 6 why show max and minimum as it muddies the difference between location and year. We think it is important to show the variability as well as the average snow accumulation between sites and across years.

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 We included the following statement to address this comment, "The Forest Elevational Snow Transect (ForEST) is a result of the BRT modelling and represents combinations of forested and open land cover types at low, mid, and high elevations."

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

We included the following statement in the methods section to specify how the data were aggregated, "All spatial data were masked to the McKenzie River Basin and converted to the same projection and spatial resolution: NAD83, UTM Zone 10, and a 100-m grid cell size. Spatial data were processed using ArcGIS 10.1 using bilinear interpolation for continuous data and nearest neighbour interpolation for discrete data."

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

Land cover is used throughout the revised manuscript, and veg class has been omitted. Table 2 – what are units?

Table 2 has been removed from the revised manuscript.

Thank you very much for our considerate review of our manuscript.

Sincerely, Kelly Gleason

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-317/hess-2016-317-AC2-supplement.pdf

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-317, 2016.

Table S1. Accuracy assessment matrix comparing the BRT classes derived from the normal snow year 2009 with those from the high snow year 2008. Overall there is less error in the lowest and highest elevation BRT classes, whereas the mid- elevations there is more error between models. Many classes were reassigned when the BRT model was rerun between years, underestimating the accuracy of the overall spatial variability between models.

BRT Class																						
2009	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Comission
2008																						error (%)
1	55402	6035																				10
2		16467																				0
3		369	22960																			2
4		52		3930																		1
5					9879																	0
6					5486																	100
7						3232	3232															50
8								4667														0
9									2524													0
10								2053		4007												34
11										5276	5740											48
12									486			2900										14
13											1965	339	5421									30
14													5252	4338	617							57
15														13692	1948	719						88
16																10260	14155					58
17																		23580				100
18																			5931	705		100
19																				1850		100
20																				1057	1025	51
21																					2039	0
Omission	0	28	0	0	36	100	0	31	16	57	26	10	49	76	24	7	100	100	100	71	33	
error (%)																			Overall accuracy			63

Fig. 1. Supplementary Table 1_Accuracy Accessment

Table S2. Accuracy assessment matrix comparing the BRT classes derived from the normal snow year 2005 with those from the high snow year 2008. Overall there is less error in the lowest and highest elevation BRT classes, whereas the mid- elevations there is more error between models. Many classes were reassigned when the BRT model was rerun between years, underestimating the accuracy of the overall spatial variability between models.

BRT Class																						
2009	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Comission
2005																						error (%)
1	55402	22923	22960	3930	15365	3232	6013	3365	2243													59
2								3355		9283	5840											100
3									767			2900										100
4											1965		9212	12939								100
5														5091	757	3973						100
6												339	1461		1808	879						100
7																3718						100
8																	2194					100
9																	3622					100
10																	2697					100
11																	3702					100
12																	1815					100
13																		7239				100
14																		4776				100
15																		4045				100
16																		2347				100
17																		3253				100
18																		1923	512			21
19																			3857			0
20																			1562	3612	421	35
21																					2643	0
Omission	0	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	92	35	0	14	
error (%)																		Overall accuracy				28

Fig. 2. Supplementary Table 2_Accuracy Accessment