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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.

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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 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 #3 Received and published: 27 September 2016

SUMMARY OF THE PAPER

This paper (1) investigates which physiographic factors influence modeled spatial SWE distributions on 1 April in the McKenzie River Basin of the western Oregon Cascades and (2) demonstrates how this knowledge can be used to locate new snow studies sites in an objective way for resolving physiographic influences on SWE. The work is motivated to inform observational network design in snow-dominated watersheds where forest change and climate change present challenges. They use binary regression trees (BRT) to predict 1 April SWE in an average year based on predictors such as elevation, forest cover, NDVI, and latitude. This is a unique application of BRT because they use 1 April SWE output from a spatially distributed physically-based model (SnowModel) at the watershed scale, whereas most previous BRT snow studies have been at smaller scales and with observational data. The analysis examines 20 snow classes from BRT in average snow years in current and future (+2 C degree) conditions. The study compares differences in SWE in forest and clearings at different elevations, as sampled in the ForREST network. I think the most major contribution of the paper is that it demonstrates a method for utilizing physically-based model output to improve observational network design. The method is novel and the results should garner decent interest from the community. I think the writing/figures are of especially high quality. This paper should be published in HESS after addressing a variety of major and minor

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comments (below).

MAJOR SPECIFIC COMMENTS

1. The most glaring weakness of the analysis is that it does not address collinearity of the predictors anywhere. Certainly some of the predictors in the BRT co-evolve in space. For example, forest cover decreases with elevation (seen in Figure 3). How can one disentangle the unique influence of covarying predictors within the adopted regression framework, let alone assert which predictors dominate SWE (Page 7, Line 12)?

In order to reduce the multi-collinearity of the predictors and prevent overfitting the model we excluded predictors which explained less than 1% of the variability in SWE to develop a more parsimonious model. Elevation and land cover type explained 93% of the variability in SWE, which justified excluding the less important more correlated variables. We have confidence in this final model as it captures different patterns of influence from elevation and land cover to the spatial variability in snow accumulation between years. Across the basin, SWE increases predictably with elevation, but in the most forested regions of the river basin (mid-elevations) land cover also drives variability in SWE. Open areas tend to accumulate more SWE than forested areas at the same elevation, and forested areas tend to have more variability in SWE accumulation than open areas across elevation gradients. We have included the following statement to address this comment, “The BRT model identified elevation, land cover, NDVI, insolation, percent canopy cover, slope, and wind as significant explanatory drivers of the spatial variability of peak SWE (all selected variables had p-values < 0.05 and are listed above in order of significance). Although elevation and land cover were the dominant predictive variables where the other physiographic variables each explained less than 1% of the variability in peak SWE. In order to reduce the multi-collinearity between related variables and reduce the risk of overfitting the model, we simplified the final model to only include elevation and land cover. The final BRT model was validated using data for an independent set of 20,000 randomly selected grid cells from within the

MRB. The final parameters developed in this optimal tree for peak SWE in an average year 2009, were used to develop equivalent BRT models using peak SWE input for an above average year 2008, as well as to peak SWE during a below average year 2005."

2. The analysis implies that current observational sites may not be representative of snow conditions in a future climate and that physically-based model outputs are valid irrespective of climate conditions. However, SnowModel (and other models with the physically-based label) do have embedded routines/parameterizations that are empirical in nature and tuned to historical conditions (e.g., atmospheric longwave radiation). There is a general lack of discussion about the reliability of models in projecting changes outside of historical conditions. These approximations of the real-world are further muddied here because the study is advancing a model (BRT) of a model (Liston/Elder SnowModel).

To focus the paper, we have removed the evaluation of SNOTEL sites under future climate change, and therefore removed the implication you mention "that physically-based model outputs are valid irrespective of climate conditions". We also included the following in the discussion at page 9, line 22, "As 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."

3. Comparing basin SWE on 1 April for the current climate and a warmer +2 degree climate may be misleading/inappropriate, as the basin may be well into the melt season by 1 April in the warmer climate. 1 April is historically significant only because it has been (on a mean basis) near peak SWE timing. Arguably, the date of peak SWE will advance earlier in the year with climate warming. So analyzing 1 April in a future warmer climate is like analyzing a date in mid- or late- April in the current climate, and we might say that SNOTEL sites are unrepresentative of basin conditions once melt conditions have advanced to that date in late April. However, that is not a fair comparison, as the SNOTEL sites may have been more representative of mean conditions earlier in the season (i.e., near peak conditions). To address this potential issue, the

authors should consider not only the spatial distribution of SWE but also the temporal evolution. Are the SNOTEL sites more representative of basin SWE at an earlier date (e.g., March 15) in the warmer climate?

We have removed this analysis in the final manuscript as discussed above in the general comments.

MINOR SPECIFIC COMMENTS

1. The “Future year (1 April 2012)” terminology versus +2 degree C year terminology is inconsistent and confusing at times. How can April 2012 be “a future year” when it is now (in 2016) well in the past (e.g., Page 6, Lines 19-20)? This needs better explanation. Also, please consider revising the language throughout the manuscript.

We have removed this analysis in the final manuscript as discussed above in the general comments. 2. The “high inter-annual variability in SWE” is offered as a reason for differences in SWE volume from BRT vs. SnowModel in the future scenario (page 8, line 4). However, this does not make sense, given that only average years are considered in the analysis, effectively precluding any influences of inter-annual variability. The authors go on to contradict the above assertion about inter-annual variability in the discussion: “This method could be improved by including more years of input data to fully capture the inter-annual temporal variability in the spatial distribution of SWE.” Please revise.

To make the analysis more robust in this revised manuscript we included additional years of data for an above average year and a below average year to run the BRT model, in addition to the average snow year data we used in the first analysis. This has enabled us to use more robust quantitative evaluation of model accuracy between years using omission and commission statistics. We included the following describing these results, “The final optimal BRT model from the normal snow year (2009) applied to the high snow year (2008) demonstrated an overall accuracy of 63%, whereas the BRT model applied to the low snow year (2005) demonstrated an overall accuracy of

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26% (Table S1, S2). The BRT model performed well across the low and high elevations, where errors of omission and commission were generally lowest (Table S1, S2). Although across the mid-elevations which consist of a patchwork of forest harvest and fire disturbance, were the areas with the greatest error between the BRT models. The high elevations above tree line, were the most consistently classified areas with low error between BRT models. The high error across the mid-elevations was due at least in part to the renumbering of classes when the model is rerun for each year, and therefore these statistics may underrepresent the accuracy of the BRT-model in predicting overall spatial patterns of physiographically derived snow classes between years. The BRT-modelled snow classes captured the spatial variability in SWE across the MRB relative to elevation and land cover during an average, above average, and below average snow year and were used to objectively inform the site selection of a snow monitoring network."

3. Was this analysis actually conducted prior to the installation of the ForEST network in November 2011? Or is this a retrospective analysis to test the representativeness of the established network? The connection between the presented work and the design of the ForEST network is never really made clear.

This distinction has implications for the title and tone of the manuscript. Currently, the manuscript implies that the analysis was used to inform the design of the ForEST network (page 10, lines 5-7). The current title is appropriate if the analysis with April 2009 was conducted first. However, if this is a retrospective analysis of the adequacy of the network, then the title may be better stated as "Testing the representativeness of a snow monitoring network in a forested mountain watershed".

Yes, this analysis was conducted prior to the installation of the ForEST network in 2010. Now I realize the need to emphasize the main narrative in this research, presenting the integrated objective method for informing snow observation network design. In the introduction and throughout the paper we have emphasized the connection between the presented work and the design of the ForEST network. We have augmented this original analysis in the revised manuscript by including two additional years of data, but this does not alter the conclusions from the original analysis.

TECHNICAL CORRECTIONS

- Page 2, Line 28: Add "currently" before manages (the number of SNOTEL stations changes in time). Sentence changed to, "The NRCS currently manages approximately 858 Snowpack Telemetry (SNOTEL) stations across the western US (http://www.wcc.nrcs.usda.gov/snotel/SNOTEL_brochure.pdf)."

- Page 4, Line 9: "In the heart of" is somewhat colloquial; consider rephrasing this sentence.

Sentence changed to, "The McKenzie River, located in the western Oregon Cascades, is a major tributary of the Willamette River (Figure 1)."

- Page 4, Lines 21-22: There is some overlap between these variables and at this point it is unclear how they are uniquely distinguished. For example, incoming solar radiation will vary with slope, aspect, and vegetation, all of which are variables listed here. Is there something unique about "solar radiation" that you should list it here? Does it vary with atmospheric conditions? Please clarify.

There is clear multi-collinearity between the correlated variables, however we used all of them initially in the model to select which were the most powerful predictive variables. Solar radiation was initially included because mid-winter snow melt events are very common in the warm maritime snowpacks and we thought perhaps solar radiation (a strong driver of snow ablation) would be more significant than slope and aspect alone. Although we have only included the two most dominant explanatory variables in this final analysis to prevent overfitting the BRT model.

- Page 5, Line 16: Presumably the model was run at a sub-daily time step (necessary for physical models), but the model provided outputs on a daily basis. Please rephrase.

The SnowModel input data were developed from model runs at a daily time step using data which were collected hourly and integrated to the daily time step. The specific methods for these model outputs are described in this paper referenced in the manuscript, Sproles, E. A., Nolin, A. W., Rittger, K., and Painter, T. H.: Climate change impacts on maritime mountain snowpack in the Oregon Cascades, Hydrology and Earth System Sciences, 17, 2581-2597, 10.5194/hess-17-2581-2013, 2013.

- Page 5, Line 24: Please provide more information about how finer resolution spatial data (e.g., 10-m elevation, 30-m land cover data, etc.) were aggregated to 100-m, and how coarser resolution spatial data (e.g., the 250-m NDVI data) were resampled/downscaled to 100-m.

The following information was included, "All spatial data were converted to the same projection and spatial resolution: NAD83, UTM Zone 10, and a 100-m grid cell size using bilinear interpolation for continuous data and nearest neighbor interpolation for discrete data. Spatial data were processed using ArcGIS 10.1."

- Page 5, Line 24: You already cited the maker/city of ArcGIS, so I am unsure if you need to do it again. Yes thank you, this change was made.

- Page 5, Line 27: Did you use the publically available locations of the SNOTEL sites? The publically available coordinates are imprecise.

No, we used locations obtained from the Oregon NRCS resource managers.

- Page 6, Lines 2-4: Again, I question the independence of the physiographic predictor variables.

We have addressed the multi-collinearity of the physiographic predictor variables by only using the most powerful explanatory variables and excluding all other potentially correlated variables with weak predictive capacity even if they were considered significant in the original model.

- Page 6, Line 21: Add "a" before "set".

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This change was made, thank you for catching this error.

- Page 6, Line 23: Revise to say “and public lands where the presence. . .”.

This change was made.

- Page 6, Line 27: Did you test for normality? Perhaps include the skew and kurtosis. There is a bit of a skew toward higher SWE volume at the higher elevations, which is why I ask.

We have included the skewness and kurtosis values for the SWE distribution across the elevational gradient in the McKenzie River Basin.

- Page 7, Lines 1-2: Consider including a separate SWE volume line in Figure 3 for the climate change scenario. This will provide another way of showing the shift toward higher elevations above the SNOTEL sites (in addition to the spatial plots in Figure 2).

We have excluded this analysis for this revised manuscript.

- Page 7, Line 3: Is this SWE range measured or modeled at the SNOTEL sites? Please state.

This analysis and associated results were removed from this revised manuscript.

- Page 7, Line 11: Please include units on the RMSE.

RMSE units now included.

- Page 7, Line 12: How much variance did elevation explain? Please quantify.

The following statement was included in the revised manuscript, “Elevation explained the most variance in modeled SWE across the basin, and is the primary driver of all snow classes (2009 BRT model with only elevation; R2 = 0.91, p-value < 0.01).”

- Page 7, Line 15: Recommend using a different word than “believed”. Also, it is possible to test the influence of the Three Sisters – just exclude those points in the BRT anaylsis and compare the resulting regression trees.

Latitude explains less than 1% of the variability in the final BRT model and therefore was removed to prevent overfitting of the final model.

- Page 7, Line 20: Should this be 6%? 1.05/0.99 = 1.061 or 6.1%.

These statistics are now changed because we used input data from the actual date of peak SWE instead of using assuming peak SWE was 01 April.

- Page 7, Lines 24-25: Check the sentence: “Although these areas. . .. Above 1791 m.” This does not appear to be a complete sentence.

The sentence was changed to the following, “Deep snowpack at the highest elevations only cover a small aerial extent of the MRB, which resulted in decreasing contribution of total basin-wide SWE above approximately 1700 m during the average and above average snow years. In contrast, during the low snow year, the highest elevation classes contributed the most to total basin-wide SWE (Figure 5).”

- Page 8, Line 1: Please clarify which model when you state “greatest error in the model”. I think it is the BRT model. Also, the use of the term “error” implies that the SnowModel output is “truth” in the comparison, which may be tenuous. Consider using some language like “difference between models” in this context.

We included three years of input data in this revised manuscript and compared the BRT models using omission and commission statistics to compute overall accuracy between years. The language has been changed throughout the manuscript to evaluate “differences between models”. We included the following statement, “The final optimal BRT model from the normal snow year (2009) applied to the high snow year (2008) demonstrated an overall accuracy of 63%, whereas the BRT model applied to the low snow year (2005) demonstrated an overall accuracy of 26% (Table S1, S2). The BRT model performed well across the low and high elevations, where errors of omission and commission were generally lowest (Table S1, S2). Although across the mid-elevations which consist of a patchwork of forest harvest and fire disturbance, were

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the areas with the greatest error between the BRT models. . .”

- Page 8, Lines 22-28: This is more appropriate for the discussion section, not the results section.

This was moved to the discussion section which now includes the following, “The ForEST network contributes to the existing SNOTEL network to explicitly investigate snow-vegetation-climate interactions across the range of elevations and forest types in the watershed. The ForEST network is unique in that the monitoring site locations were selected based on statistical classification and geospatial analysis, rather than subjective methods that may incorporate bias. The paired forest-open land cover site selection process alone is not unusual, and has already led to important understanding of key sub-canopy snow processes (Storck et al., 2002; Golding and Swanson, 1986), but here, it has been further validated using coupled physically-based spatially-distributed snow model input data and non-parametric BRT statistical modeling. After five consecutive years of snow monitoring, we have created a valuable and detailed dataset of snow accumulation, snow ablation, and snowpack energy balance that spans the spatial variability in forest and open land cover types across an elevational gradient. The inter-annual consistency in patterns of snow surface energy budget and snow-vegetation interactions across the elevational gradient of the ForEST network suggest that the data are representative of key snow accumulation processes in the MRB (Figure 6). “

- Page 9, Line 12: Add “a” before “key role”.

This change was made.

- Page 9, Line 20: Improper semi-colon usage. You can safely remove it, or break the sentence into two here.

This change was made.

- Page 9, Line 23: Replace “does incorporate” with “incorporates”.

This change was made.

- Page 9, Lines 23-26: This is a long and overly complicated sentence. Please rephrase and/or revise into shorter sentences.

The following sentence has been included instead," The paired forest-open land cover site selection process has already led to important understanding of key sub-canopy snow processes (Storck et al., 2002; Golding and Swanson, 1986). But here, it has been further validated using coupled physically-based spatially-distributed snow model input data and non-parametric BRT statistical modelling across a forested montane watershed."

- Page 10, Line 19: If a hypothesis is validated, is it still a "working hypothesis"? The word choice is puzzling here.

The sentence has been changed to the following, "However in the rugged and densely forested mountain regions of the western Cascade Mountains where there are few alternatives to modeling spatially distributed SWE, this approach provides a validated hypothesis to guide representative and objective snow monitoring efforts."

TABLE AND FIGURE COMMENTS

- Figure 2 caption: Replace "in shown" with "is shown".

This change was made.

- Figure 2 caption: Please define the units of SWE.

This change was made.

- Figure 2: If April 2009 is an average year (page 5, line 19) and the climate change scenario is a 2 degree C perturbation to an average year, why is the maximum SWE lower in April 2009 (4.31) than in the climate change scenario (5.03)?

This analysis was omitted from this revised manuscript.

- Table 1: What is the logic of the organization of snow classes in Table 1? It generally goes from low to high elevation, except the 977 to 1199 elevations are not in order. Please rectify.

Table 1 has been simplified by elevation and includes all three years of BRT-derived snow classes.

- Table 1: Should snow class 1 read “977-1199” instead of “977-199”?

This change was made.

- Table 1: Consider showing statistics with each snow class to record how well the regression works in that group.

Table 1 is already very busy, and would we prefer to not include additional information that may make the table more difficult to interpret.

- Table 1: What is the purpose of having a binary vegetation class (forest vs. open) and forest canopy cover (CC) predictor variables? Would it not be more straightforward to just include CC and let the BRT tell us when/where the binary distinction dominates the SWE response?

To define a parsimonious and interpretable final BRT model we have only included elevation and land cover in the final BRT model. Land cover type was only slightly more predictive than NDVI in the BRT model, which may be because BRT models tend to optimize categorical variables. Also the slight variability between the BRT classes in the ranges of a continuous variable like NDVI or % canopy cover is confusing when comparing between years. Therefore we decided to only include the binary land cover data in the final BRT model that is clearly defined across all snow classes.

- Table 1: In some (but not all) cases, there is an overlap in the elevation. Is a location at 1426 m elevation in the open in snow class 11 or snow class 13?

The previous snow classes have been slightly redefined because we are using data from the actual date of peak SWE instead of 01 April. There is no overlap in the elevation between classes.

- Figure 3: Please use a superscript for cubic km on the left y-axis.

This change was made.

- Table 2: It is unconventional to have negative standard deviation or coefficient of variation. Please make these positive. Also, are the CV numbers correct? They should be the SD/Mean, but that does not appear to be the case here.

Table 2 was omitted from the revised manuscript. Instead of comparing CV numbers we are using omission vs commission statistics to evaluate the accuracy of spatial variability of SWE between years.

The results of this accuracy assessment are discussed in the text, and included in the supplementary tables 1 and 2.

- Table 2 caption: Please include the units of SWE differences here.

Table 2 was omitted from the revised manuscript.

Thank you very much for your consider 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-AC3-supplement.pdf

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

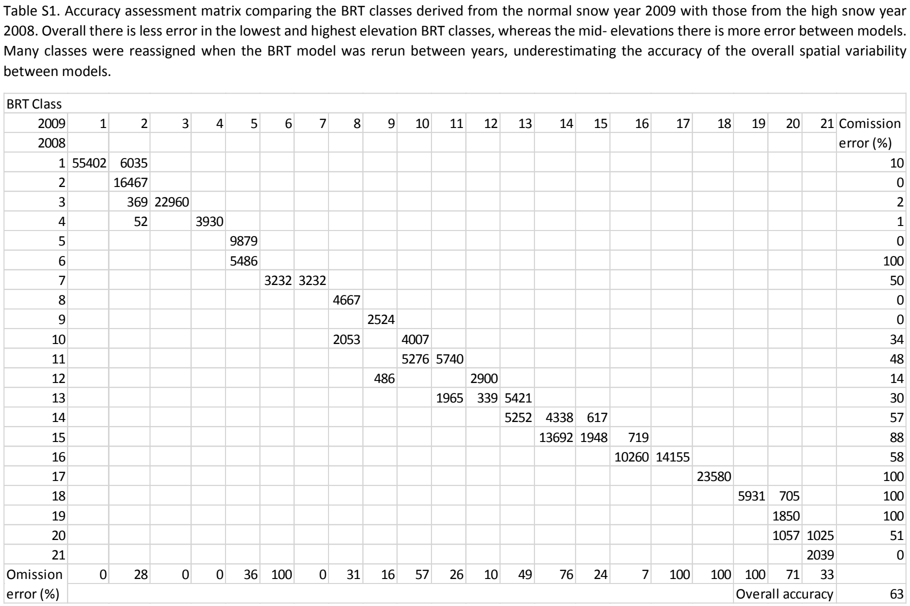

Table S1. Accuracy assessment matrix comparing the BRT classes derived from the normal snow year 2009 with those from the high snow year 2008. Overall there is less error in the lowest and highest elevation BRT classes, whereas the mid- elevations there is more error between models. Many classes were 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. Supplemental Table 1_Accuracy Assesment

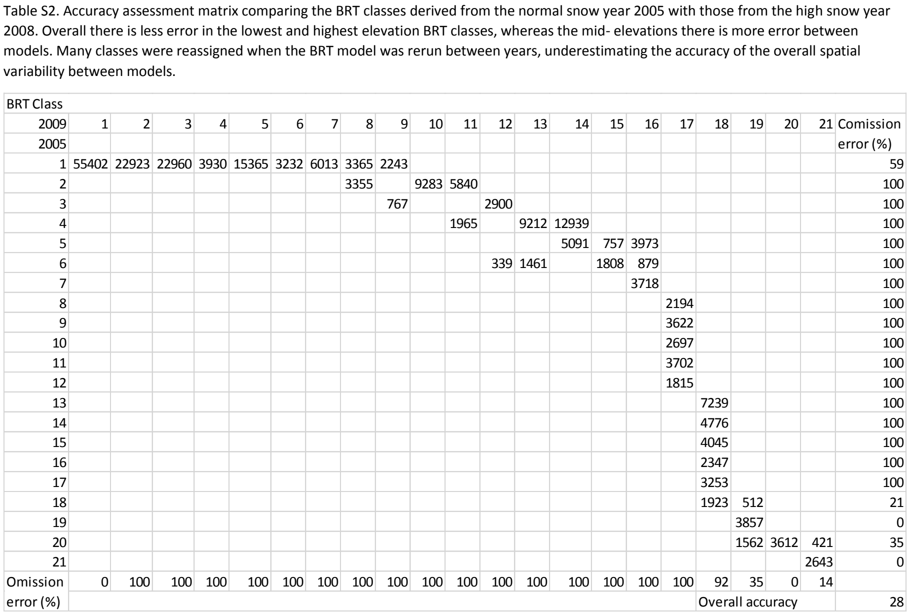

Table S2. Accuracy assessment matrix comparing the BRT classes derived from the normal snow year 2005 with those from the high snow year 2008. Overall there is less error in the lowest and highest elevation BRT classes, whereas the mid- elevations there is more error between models. Many classes were 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. Supplemental Table 2_Accuracy Assesment

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