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

Anonymous Referee #3

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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 (Snow-

Model) 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 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)?

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).

3. Comparing basin SWE on 1 April for the current climate and a warmer +2 degree

C 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?

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.

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.

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".

TECHNICAL CORRECTIONS

- Page 2, Line 28: Add "currently" before manages (the number of SNOTEL stations changes in time).

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

- 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.

- 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.

- 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.

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

- Page 5, Line 27: Did you use the publically available locations of the SNOTEL sites?

The publically available coordinates are imprecise.

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

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

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

- 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.

- 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).

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

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

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

- 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.

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

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

- 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.

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

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

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

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

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

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

TABLE AND FIGURE COMMENTS

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

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

- 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)?

- 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: Should snow class 1 read "977-1199" instead of "977-199"?

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

- 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?

- 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?

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

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

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