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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.

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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 #1 Received and published: 7 August 2016

The authors present a comparison of a binary regression tree (BRT) statistical model, trained using a distributed snow model (SnowModel), to spatially locate similar snow classes around a watershed which guides the siting of meteorological stations (6 stations at three sites). Two snapshots of spatial snow distribution are used: 2009 (training data) and 2012 (evaluation data) in order to evaluate the BRT and demonstrate its utility for met station siting. This concludes with the claims that it improves the basis for site selection over a physically based model due to the uncertainty propagated by parameter selection (i.e. nested sub-models) in physically-based models. As the manuscript is currently written, there are some substantial issues to respond to as well as a few minor suggestions:

Your comments highlight the need to clarify a few key points in the revised manuscript that may have been initially misinterpreted. For example we conclude that the presented method of site selection is an improvement over more commonly used heuristic approaches, but because the method couples physically-based, statistically-based, and geospatial models there is uncertainty particularly in predicting future conditions. Here is the revised paragraph in the discussion section which addresses your comment above, "We developed a snow monitoring network representative of the spatial variability of SWE relative to physiographic landscape characteristics across the MRB for an average, above average, and below average snow year; using a coupled BRT statistical classification model, a spatially distributed physically-based SnowModel, and a geospatial selection model. 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."

1. I don't see how this is novel science from the perspective of BRT applications. The authors provide six citations in the introduction to similar BRT work and explicitly mention in their conclusions that it is not an advance over Randin et al. (2014).

We present a novel method in designing an objective and representative snow monitoring network, which promotes the opportunity for novel science. In the introduction we acknowledge research which has used the BRT to evaluate snow accumulation at small scales, or snow covered area at broad scales, however no previous work has coupled physically-based model output with non-parametric statistical models to improve snow monitoring network design. In the introduction we include the following paragraph explaining how this method goes beyond any previous work using BRT modeling, "Landscape characteristics have been used to predict snowpack conditions at hillslope scales using non-parametric binary regression tree (BRT) statistical classification models (Molotch et al., 2005; Anderton et al., 2004; Erxleben et al., 2002; Winstral et al., 2002; Balk and Elder, 2000; Elder et al., 1998). Larger scale BRT approaches have also been conducted using remotely sensed snow-covered area and

interpolation methods (Molotch and Meromy, 2014; Molotch and Bales, 2006b). However, no study to date has used landscape characteristics in conjunction with modelled and validated physically-based and spatially distributed SWE data to understand physiographic drivers of snow accumulation at broad scales (watersheds > 1000 km2) or to identify optimal locations for snowpack monitoring. Additionally, most of the research on the physiographic relationships to snow processes has been done in cold-dry continental snowpacks where mid-winter melt events are infrequent and wind redistribution is substantial (Molotch et al., 2005; Erxleben et al., 2002; Winstral et al., 2002; Balk and Elder, 2000). Much less is known about how physiographic conditions influence the temperature sensitive snowpacks in the forested maritime basins of the Pacific Northwest."

Also in the conclusions we include the following paragraph, which specifically describes the novelty of this research," By quantifying the spatial variability in the key drivers of natural resource distribution, researchers can focus on sensitive areas which may not be identified through traditional site selection means. The use of validated model outputs as a predictor of the spatial variability in snow-vegetation interactions is not new (Randin et al., 2014). The novelty of this research stems from the application of the method, where by the coupling of a traditional BRT classification process with a validated physically-based spatially distributed model, we improved observational network design in a forested montane watershed."

2. This work demonstrates that a statistical BRT model that is not temporally responsive to a warming climate (i.e. in the same way that SNOTEL data provide temporally static statistical relationships to discharge), performs worse than the distributed physically-based model (SnowModel). Table 2 shows this performance difference is by an order of magnitude in the mean values for medium and low elevations. Hence the assertion in the conclusions that there is still a place for simple approaches is undermined. From the presented methodology of the BRT model it seems this is not a simple approach, and in a watershed where a physically-based model can (and has)

been deployed, it offers no improvement. While there may be uncertainty in many parameterizations and process representations of physically based models, at least they will be responsive in outputs to changing input in a warming climate (especially relevant to the pacific north-west region).

To simplify the manuscript and focus on the novelty of our method, we have removed the evaluation of the SNOTEL network under a warming climate. Also, in order to make the validation of this approach more robust in the revised manuscript, we have included three years of input data and allowed the BRT model to build its own structure from an average snow year, an above average snow year, and a below average snow year. By including these additional years, we were able to use omission vs commission statistics to determine the overall accuracy of the models between years of current snow conditions. We hope this clarifies some of the confusion mentioned in the above comment. We do not suggest that a BRT modeling approach is more robust than a physically-based model in predicting snow volume across a watershed. We present a relatively simple method using coupled models to classify the snowpack (a normally continuous variable) across a complex watershed to guide objective snow monitoring network design. We have also included the following statement in the discussion, “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.”

3. The claim of a predictive system (whether BRT or a physically based model) as a tool for advancing the siting of met stations is very site specific and doesn’t provide wider scientific advancement. Local watershed knowledge of potential site access, elevation and forest/open areas would likely provide just as much information required as a complex statistical BRT style analysis. While this style of statistical analysis may have been useful to justify the location of met sites in the MRB watershed, in itself, it doesn’t justify either a methodological or scientific advance in HESS.

Using an objective method of site selection is rarely used, and we suggest is an ad-

vancement over more heuristic approaches which require institutional knowledge that may not exist in remote rugged watersheds. Although the result of this analysis may not be particularly surprising, it is a useful method for objectively validating our assumptions about “representativeness” of any particular monitoring site location. For example, our final station locations may appear clustered in physical space, but from using our method we are confident the locations span the parameter space of the key drivers influencing the spatial variability of snow accumulation across the watershed. We present this method with the hope that more scientists will objectively distribute future monitoring locations based on actual data instead of going on “gut feeling”.

4. The benefits of a BRT approach remain poorly quantified. In the abstract, elevation, vegetation type and vegetation density are defined as the significant drivers of SWE distribution. As we already know this is important in montane environments this does not come as a surprise, however, not providing any statistical quantification of the relative significance (nor on the main body of text) means such a major concluding statement adds little to the current body of work in the literature. We hope we have clarified in the final manuscript the benefits of this coupled approach to objective site selection. As stated above, the results of this analysis are not surprising and validate already known assumptions about snow-vegetation interactions in montane watersheds. However what is novel in this method is that it uses statistically derived relationships to classify the spatial distribution of snow by its primary drivers to improve observational network design. Also, because we included three years of input data in the final manuscript, we include a more robust statistical validation of the BRT model between years.

Minor comments:

Abstract: this could be condensed substantially. Ln 9-14 and 24-27 could be shortened/removed. No quantified results are presented. The reader is left unaware how representative (i.e. quantified) this BRT model actually is.

As suggested, we have condensed the abstract to focus more simply on the method of site selection, and included a basic statistic of the final BRT model in the revised manuscript. The revised abstract now reads, "A challenge in establishing new ground-based stations for monitoring snowpack accumulation is to locate the sites in areas that represent the key processes affecting snow accumulation and ablation. This is especially challenging in forested montane watersheds where the combined effects of terrain, climate, and land cover affect seasonal snowpack. We present a coupled modelling approach used to identify a parsimonious set of monitoring sites in a forested watershed in the western Oregon Cascades mountain range. We used a binary regression tree (BRT) non-parametric statistical model to classify peak SWE based on physiographic landscape characteristics in a normal year, an above average year, and a below average year. Training data for the BRT classification were derived using spatially distributed estimates of SWE from a validated physically-based model of snow evolution. The optimal BRT model showed that elevation and vegetation type were the most significant drivers of SWE in the watershed (R2 = 0.93, p-value < 0.01). Geospatial elevation and land cover data were used to map the BRT-derived snow classes across the watershed. Specific snow monitoring sites were selected randomly within the BRT-derived snow classes to capture the range of spatial variability in snowpack conditions in the McKenzie River Basin. 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. After five years of snowpack monitoring, the ForEST network provides a valuable and detailed dataset of snow accumulation, snow ablation, and snowpack energy balance in forested and open sites from the rain-snow transition zone to upper seasonal snow zone in the western Oregon Cascades."

Pg 1, Ln27: The idea this paper tests the MCB snow network within a projected warming climate (from 2009 to 2012) suggests something that is not adequately delivered by this paper. As suggested this evaluation of the MRB SNOTEL network within a projected warming climate (from 2009 to 2009 + 2° C) was removed from this revised

manuscript to focus on the objective method of site selection to capture the spatial variability in SWE during current years. We have also included three years of data to evaluate the accuracy of the model between years in the revised manuscript, but this is not intended to suggest anything about climate change.

Pg 4, Ln 7 & 19 – don't need 'Description of the' in either sub heading.

This change was made.

Pg4, ln 12 – 'which' is grammatically correct after a comma rather than 'that'.

This change was made.

Pg 4, Ln 25-27 – following Winstral et al., (2002) and subsequent papers by Winstral et al., was this used to calculate redistribution of snow (especially above tree line) in drifts which are very important hydrological areas to get SWE correct in a watershed?

Yes, these methods were used to calculate the upwind contributing area to calculate redistribution of snow in drifts across the landscape. Although snow redistribution is not as important in the warm maritime low elevation snowpacks characteristic of the Pacific Northwest, than in drier higher elevation continental snowpacks, we still felt it important to include wind as a driver of spatial variability in SWE. We have included the following sentence to clarify your question in the revised manuscript, "Upwind contributing area data, which captures the variability in snow deposition as a result of wind redistribution for each cell throughout the watershed (Winstral et al., 2002), was calculated following Molotch et al., (2005)."

Pg 5, Ln 18-21 – while Sproles et al. (2013) is often cited, as this is such a key foundation to this work it needs greater explanation in this paper – in particular how the future SWE conditions are calculated, and especially the change to precipitation rates and phase (rain/snow) as well as temperature. A fairly detailed paragraph describing the methodology used for the modelled input SWE data was included in the data sources section of the methods. For the sake of brevity, we would prefer to cite Sproles et al 2013, and not reiterate what has already been published. We include the following paragraph in the methods, "Modelled and gridded SWE data across the MRB (Figure 2) were provided by Sproles et al., (2013). These data were developed using a physically-based spatially distributed snow mass and energy balance model, SnowModel (Liston and Elder, 2006). SnowModel uses micrometeorological and topographic data to distribute snow across the landscape accounting for climatic, topographic, and vegetation variability. The model was modified by Sproles et al., (2013) to account for rain/snow precipitation phase partitioning, and snow albedo decay in forested landscapes. This model was calibrated and validated using data from the four SNOTEL sites, meteorological data from the HJ Andrews Long Term Ecological Research site and National Weather Service stations and Landsat fractional snow covered area data over the sampling period 1989-2009 (Sproles et al., 2013). The model was run at 100-m spatial resolution on a daily time step. We used modelled peak SWE data as the predicted variable in the BRT model. Sproles et al., (2013) showed that 2009 was considered an average snow year so we used peak SWE from 2009 (five days centred on 04 April 2009) as our reference year. Additionally we used peak SWE from 2008 (five days centred on 24 April 2008) as an above average snow year, and peak SWE from 2005 (five days centred on 20 April 2005) as a below average snow year."

Pg 5, ln 24 – can more be said about issues of up-scaling (aggregation) and downscaling (disaggregation) of different data sets?

We have included additional information about the method used in scaling the input data following, "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."

Pg5, Ln 25 – why concentrate on areas defined as 'bulk' rather than fully spatially distributed models? Locating big drifts, often above tree line, are key to understanding the timing and magnitude of discharge. This seems to have been neglected under this

BRT model.

The bulk snowpack was evaluated for current and future (+ 2° C) conditions to quantify the area on the landscape where the majority of the snowpack lies from the physically-based spatially-distributed modelled SWE data. Although we have removed this analysis from this revised manuscript to focus on the novel method of coupling physically-based and statistical models to improve observational network design.

Pg 6, Ln 5-10 – The way that SnowModel is combined or used to evaluate BRT is presented in a very confusing fashion. Where is the independent data to evaluate BRT?

Within the CART statistical software, we have reserved 20,000 random cells within the modelling domain to test the final BRT statistical model. The modelled SWE data was used as the dependent variable in the BRT statistical model, and we hope this is now less confusing in the revised manuscript. We have included this additional information in the statement, "Within the CART software, the final BRT model was validated using reserved data from an independent set of 20,000 randomly selected grid cells from within the MRB."

Pg 6, ln 12 – 20 BRT snow classes? Wasn't one removed due to logistics and finance? This adds confusion to the methods.

We have revised our analysis in this revised manuscript in a few key ways including, a) using peak SWE instead of 01 April, and b) pruning the optimal model to just the two main drivers to prevent overfitting and multi-collinearity brought up by reviewer #3. In the revised manuscript, the optimal model is defined by 21 BRT snow classes, none of which were removed. We hope this is clear in this revised manuscript. Pg 6, ln 14-16 – Why were lower elevation extents removed? This is done without any quantification nor real justification.

The BRT model did not set bounds on lower elevation limits for SWE, although SWE did not exist below approximately 600 m in the modelled data as well as anecdotally from observations. As stated in the manuscript, “Because the BRT-model did not determine a lower elevation limit on snow extent, we excluded areas with an elevation less than 600 m to prevent over-prediction of snow-covered area below elevations where it was observed in the modelled data.”

Pg 6, ln 16-17 – what proportion of the basin was removed? Why do this if it is a SWE contributing area to discharge, why would this cause over prediction?

This removed 26.5% of the area in the basin during the evaluation of the volume of SWE in the BRT classes, although this area held 0.068% of the SWE in the basin (or 0.0009 km3 of 1.48 km3 SWE). If we had not set a lowest extent of snow covered area in the basin, it would be the equivalent of drawing a regression line beyond the range of the input data. We set this boundary were it was observed in the modelled data to prevent extrapolation of the model beyond the bounds of the input data.

Pg 6, ln 21 – add ‘a’ between ‘create’ and ‘set’.

This change was made.

Pg 6, ln 24 – why is 500m threshold applied? In practice one would expect field locations for met sites to be closer or further away from transport links depending on local conditions (i.e. how potential met site locations have always previously been evaluated).

Your question highlighted a typo in the manuscript which we have revised, as well as included additional information for clarity. The following statement was included in the revised manuscript, “To prevent contamination from the road network, but still define accessible site locations, we also identified areas within 100-500 m of a snowmobile-accessible road.”

Pg 7, Ln 8 – the ‘final’ BRT model. How many BRT models were evaluated? The rest of this paragraph has already been discussed and is providing repetition.

In the revised manuscript, we developed the optimal model based on an average year snowpack, which was then paired down to a more parsimonious final optimal model, which was then applied to an above average and below average snowpack. In the revised manuscript, we have rewritten the methods for clarity, including the following text in the analysis section, “An optimal tree was produced to minimize the standard error of the model, which was then pruned down to the simplest tree possible within one standard error of the optimal tree, and so each terminal node represented at least 1% of the variability in peak SWE. The resultant tree identified 21 terminal nodes that characterized the spatial variability in SWE through combinations of independent drivers into 21 BRT-derived snow classes (Table 1). 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 multicollinearity between related variables and reduce the risk of overfitting the model, we simplified the final optimal model to only include elevation and land cover. Within the CART software, the final optimal BRT model was validated using reserved data from 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 for peak SWE during a below average year 2005.”

Pg 7, Ln 14 – why does latitude matter?

Good point, particularly over a 3000 km2 watershed it should not matter, and therefore we have removed it from the analysis in this revised manuscript.

Pg7, Ln 15 – why does aspect not matter? Especially for snowmelt rates, this goes against conventional wisdom.

In this study we sought to classify the spatial distribution of snow water volume on the landscape, and therefore focused on the peak snow accumulation across the watershed. Aspect should matter, as well as insolation and slope, during snow ablation, but we did not expect it to be important in accumulation processes. Because we focused on snow accumulation, we did not expect aspect to be an important driver in the spatial distribution of peak SWE.

Pg 7, Ln 18 – why were BRT and SnowModel not used in conjunction with each other. When both are available it is confusing that they are not used together to optimize estimation of SWE distribution.

We coupled SnowModel, BRT, and a geospatial model to classify snow across the watershed. They are used in conjunction and hope we clarified this issue in the revised manuscript.

Pg 7, Ln 20 – BRT estimation of mass should be good in 2009 as it is tuned with SnowModel, but poor prediction of SCA (64% SCA over prediction) suggests it's not getting SWE right for the right spatial reasons (i.e. at low elevation).

We were previously using the static BRT-derived snow classes based on 2009 SWE and applying them to 2012. This method implies there is no inter-annual variability in SWE, and therefore does not properly evaluate the accuracy of the BRT model between years. In order to improve upon this, we included three years of SWE input data to develop three equivalent BRT models, and using omission and commission statistics we evaluated the overall accuracy of BRT models between years in the revised manuscript. We don't expect the BRT model to predict actual SWE volume on the landscape, but to predict the spatial distribution of similar SWE characteristics across the landscape, and we believe we have achieved this goal in this revised manuscript.

Pg 7, Ln 23 – Increasing elevation does not increase accumulation, it is increases with elevation (i.e. not a cause in itself).

We have included the following statement to address this comment, "Snowpack accumulation increased with increasing elevation, resulting in a greater mean SWE per unit area at the highest elevations. Although 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)."

Pg7, Ln 26-31 – could this information be put into a table?

This information has been refined and added to Table 1 in the revised manuscript.

Pg 8, Ln 1 – comma needed after 'Whereas'.

This change was made.

Pg 8, ln 4-5 – How does BRT adapt to changes in winter precipitation inter-annually? If it can't, what advantages does it have over running SnowModel?

I hope this confusion has been clarified in this revised manuscript. We used the BRT model to classify the modelled SWE output of continuous data based on physiographic landscape characteristics. In order to address how the BRT model adapts to interannual variability in SWE, we included an above average snow year, and a below average snow year in the analysis of this revised manuscript.

Pg 8, Ln 7 – SnowModel derived estimates were NOT captured well by BRT. They were an order of magnitude different at low and medium elevations. Need a much better quantified argument to justify this.

We aim to capture the spatial variability in SWE characteristics across the landscape, not the actual volume of SWE. In the revised manuscript, we have rerun the BRT models using three years of data to more robustly evaluate the spatial variability in snow classes across the landscape between years.

Pg 8, Ln 13 – Need to provide more about how accessibility is determined as a criteria.

These criteria have been described in the methods section including the following statement, “To create a set of feasible locations for the in situ snow monitoring network we evaluated the accessibility of locations within the MRB. Using a GIS-based binary selection model, we masked out all private lands and public lands where the presence of endangered Northern Spotted Owl prevented permitted access. To prevent contamination from the road network, but still define accessible site locations, we also identified areas within 100-500 m of a snowmobile-accessible road. From these accessible areas, the final sites were then randomly selected from each of the dominant BRT-derived snow classes within the seasonal snow zone.” Also to clarify this in the results section we included the following statement, “The geospatial selection model identified 16 of the 21 classes as being accessible (following criteria explained in the above methods) during winter.”

Pg 8, ln 19 – six met stations is a bit misleading, rather there are three sites, each with adjacent open/forest met stations.

Although the six met stations are paired by elevation and appear to be in the same site on the map, they are approximately 1 km from each another. They are grouped by elevation but distinct in the land cover characteristics. The following statement has been included which clarifies that the six stations are grouped by three elevation ranges and two land cover types, “Within the area covered by these 16 classes, random site locations were selected within the six most abundant classes across the MRB to capture low, medium, and high elevations, with forested and open land cover classes. The resultant Forest Elevation Snow Transect (ForEST) monitoring network site locations were thus objectively selected to sample across the range of spatial variability in SWE. The ForEST network, composed of six meteorological stations and snow survey transects, was deployed in November 2011, and continues to provide high quality snow and climate data to evaluate snow-forest-climate interactions in the MRB (Figure 4).”

Pg 8, Ln 17-26 – this isn’t a scientific result unless you then go on to do something with these met data.

The ForEST network of snow monitoring stations is the result of the coupled modeling approach, and therefore we believe it should be included in the results section. We have moved any qualitative evaluation of the network to the discussion section. We could fill an entire manuscript evaluating the met data, but in this manuscript we aim to focus on the novel method of objective site selection.

Pg 8, Ln 26 – how has this been stringently validated with the BRT model?

We aimed to objectively distribute a representative snow monitoring network using this coupled modeling approach. Instead of heuristically deciding where stations should be located, we used physically based SWE data and a non-parametric statistical model to define the spatial variability across the watershed. We hope in the revised manuscript we have clarified this and include the following statement in the discussion section, “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, the assumptions driving paired site selection process 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.”

Pg 8, Ln 26-28 – Consistency in the pattern of measured snow course SWE doesn’t corroborate energy balance and snow-veg interactions.

We have removed this statement to allow the reader to form his/her own conclusions about the measured data resulting from this project.

Pg 9, Ln 15-16 – This study doesn’t explicitly demonstrate the impact of timber harvest/ fire disturbance impact on SWE distribution.

We have included the following statement in the discussion to clarify this point, “By distinguishing snow classes based on forest vs. open land cover across a range of elevations, this study emphasizes the watershed-scale control that vegetation and particularly land cover change relative to timber harvest (and potentially fire disturbance) has on snowpack accumulation in the maritime western Oregon Cascades."

Pg 9, Ln 20-21 – If BRT and SnowModel are coupled (as stated) then what does this combination give us that SnowModel doesn't give us as a stand-alone product? This is not providing added information on hydrological response units (HRU), it is not a new idea in snow hydrology (e.g. CRHM), and doesn't provide an obvious robust advancement in inter-annual transferability.

We use this coupled approach to classify snow characteristics across the landscape and to improve upon traditional methods of observational network design. The Snowmodel output data is continuous by nature, and doesn't provide any guidance for which specific locations in a watershed may be representative of greater landscape scale processes. We did not expect to advance scientific knowledge, but to provide an objective technique for distributing point based monitoring locations which represent the spatial variability across the watershed.

Pg 9, Ln 26 – Yes, inter-annually transferability really needs to be more robustly tested by this methodology, rather than one 1 April snapshot in 2012. Currently this evaluation/validation has not been sufficiently done with independent data.

We included three years of data from the actual date of peak SWE in this revised manuscript to provide a more robust evaluation of the accuracy of the BRT models between years.

Table 1 – What percentage of SCA was above 1546m (was it 40%)? If these data were rejected can this be demonstrated that this is not a problem? While thin SWE and scour is likely in Alpine areas above tree line drifts in these areas can contribute substantially to the timing of increased discharge through melt-out.

We aim to capture the spatial variability in SWE characteristics across the watershed to improve observational network design, not to accurately capture any watershed discharge characteristics. We do need more observation stations at higher elevations, although within the resource limitations of this study we were restricted to locations we could reach with a snowmobile and must accept the related uncertainty.

Table 2 – no units. Can low, medium and high be classified? Which sites were the forest and open sites – can these be related to a map or specifically described?

Table 2 has been omitted from this revised manuscript in lieu of more robust accuracy assessment tables included in the supplementary tables.

Fig 2 – put yellow circles in legend. Cite Sproles in caption (see previous comment about more explicit explanation of future precipitation scenario in Sproles data).

Figure 2 has been omitted from the revised manuscript as described in the above text.

Fig 3 – I am surprised that mean SWE by elevation increased above tree-line, would have expected some thinning of SWE due to scour, can this be explained? The hypsometry of the basin would be a very useful (essential?) addition to this figure.

As mentioned above, snow redistribution is not as important in warm maritime snowpacks as it is in cold dry continental snowpacks, and therefore we are not surprised by this result. In order to make the connection between volumetric SWE and mean SWE across the elevation gradient we have included the hypsometry on Figure 3.

Fig 4 – relate snow classes to the Table otherwise they make no sense.

Table 1 has been altered in the revised manuscript to explicitly describe each snow class for each year.

The BRT snow class numbers in Table 1 match those used in the legend of Figure 4 in order for readers to make the connection between the statistics and spatial distribution of each snow class.

Fig 5 – How does forest and open relate to the ‘all’ classification? What is additional to ‘all’ other than forest and open? Why is mean SWE so different to SnowModel? Which year is this for? Don’t put descriptive results in caption, put them in the main body of the text. Caption says it’s statistically important, where is this statistical analysis?

The information for each BRT-derived snow class is now consistent between Table 1, Figure 4 and Figure 5 in the revised manuscript. The BRT model distinguishes snow classes across the middle elevations into forest vs. open land cover types, but only by elevation across the high elevations. All land covers includes both forest and open land covers as opposed to forest or open land covers. Mean SWE here is defined for each BRT snow class, where as in Figure 3 mean SWE is defined for each 100 m elevation band. We hope these questions are addressed in the revised manuscript. The BRT model selected statistically significant drivers of SWE across the landscape and is the analysis we refer to here, although these descriptive results have been removed from the figure caption.

Fig 6 – This is just measured SWE, how is it use to quantitatively evaluate the new modelling framework? Need to define the high, mid and low elevations in the caption. Error bars seem to be the range rather than any calculation of error.

We are not evaluating the modeling framework using these measured SWE data, but we present the measured SWE data to show there are consistent differences in peak SWE between forests and open areas that seem to evolve across the elevational gradient. We have included the elevations within the caption, and also state in the caption that, “Error bars indicate the maximum and minimum measured SWE (cm) from 2012, 2013, and 2014.” We are unaware of another name to refer to these bars and hope they are clearly defined in the revised manuscript.

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

Please also note the supplement to this comment:

http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-317/hess-2016-317-AC1-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. Supplemental 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. Supplemental Table 2_Accuracy Accessment