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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.

Anonymous Referee #1

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Developing a representative snow monitoring network in a forested mountain watershed Kelly E. Gleason, Anne W. Nolin, and Travis R. Roth

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:

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).

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).

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.

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.

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.

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.

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

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

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.

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

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.

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?

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

Pg 6, ln 14-16 – Why were lower elevation extents removed? This is done without any quantification nor real justification.

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?

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

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).

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.

Pg 7, Ln 14 – why does latitude matter?

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

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.

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).

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

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

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

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?

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.

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

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

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

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

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

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

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.

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.

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.

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?

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).

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.

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

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?

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.

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