

Interactive comment on “Climate change impacts on maritime mountain snowpack in the Oregon Cascades” by E. Sproles et al.

E. Sproles et al.

eric.sproles@gmail.com

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Responses to reviewer comments Reviewer comments are shown in italics and responses are shown in regular font. In cases where reviewers made similar comments, the comments and responses have been grouped and indicated by topic heading for clarity.

Reviewer 1:

Comments on the elevation band of the meteorological stations:

Reviewer #1 - First of all, the authors rightly note that the limited elevation range for which snow observations are available from the SNOTEL network is a major concern,

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especially when it comes to the upper reaches of the study basin which reaches elevations of over 3000m. Unfortunately, the climate data used in the study to run Snow-Model has very similar problems with the highest station being located at 1509m and, therefore, still within the rain snow transition zone defined in the study for the MRB. I realize that there probably isn't anything the authors can do about the lack of data in the higher regions of the basin. However, I think that more discussion is needed about the confidence of the authors in the interpolated distributed climate data for higher elevations especially since much of the discussion centers around these parts of the basin.

Reviewer #2 - SWE point measurements are all located within a narrow elevation range, therefore using them in order to calibrate the model could lead to poor results at low or high elevations.

We agree that a wider range of elevation measurements, especially at higher elevations would have benefitted this study. Unfortunately access to areas above 2000 m during the field season was not logistically feasible. These data deficiencies also support one of the supplemental goals of this study – identify where new field measurements would augment the existing network.

With regard to the representativeness of the measurements used in the model, this range of elevations represents a majority of the basin area. Hypsometrically 74% of the area of the McKenzie River Basin is encompassed by the elevation ranges of the monitoring sites (430 – 1512 m), and 85% of the basin lies below the highest elevation site of 1512 m (see Table 1 and Figure 1 in the supplemental documents). Because we spent considerable time and effort in successfully validating the interpolated input data from the MicroMet module of SnowModel, we have confidence in the MicroMet simulations that provide the meteorological drivers for the energy balance and snowpack evolution models above 1512 m. MicroMet is a well-established model that has provided robust results in a range of climatic and topographic conditions (Liston and Elder, 2006). We effectively validated the model over a range of elevations extending

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from areas dominated by rain and into areas dominated by seasonal snowpack. We used the best data available and supplemented the data with field observations. A brief discussion of these details is now included in the updated manuscript (lines 634 – 644 of the updated manuscript).

Reviewer #1's comments regarding the rain-snow transition zone identify a point that could be clarified. The manuscript described the rain-snow transition as extending up to 1500 m, however this statement may be unintentionally misleading. The elevation zone between 400 -1200 m represents the rain-snow transition zone, in which rain dominates at the lower elevation with increasing proportions of snow at higher elevation (at 1200 m). In this rain-snow transition zone, snow may accumulate and melt several times throughout the winter (Tague et al., 2008). Above 1200 m, the seasonal snowpack seasonal has distinct accumulation and ablation periods. We apologize for any ambiguity and have clarified this in the updated manuscript (lines 75 – 77 of the updated manuscript).

To reiterate, we agree that higher elevation stations would benefit this and future studies. We note in the introduction that a point-based monitoring network is not representative of the spatial distribution of snowpack across the basin (lines 93 – 98 of the updated manuscript). The methods moved forward with the best data available, and the information gained from this study leveraged efforts to overcome similar challenges in the future. For instance, results helped the National Resource Conservation Service site a new long-term monitoring site for weather, snowpack and reservoir management (<http://www.or.nrcs.usda.gov/snow/maps/sitepages/22e10s.html>) and new higher-elevation field sites. Additionally, one of the premises of the study was to improve our understanding of snow water storage at the basin scale. These results, for the first time, quantified volumetric snow water storage for the McKenzie River basin.

Comments on using daily data to drive SnowModel:

Reviewer #1 - How was concluded that e.g. 12 h and 18 h temperatures were “too

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warm” and therefore too much rain was simulated. Was there any data or field observations of precipitation that fell at such times to support this assumption or was this conclusion based solely on goodness of fit between the model and the data. Then the question would be as the model only used daily time steps, how was it decided that these two temperatures were “too warm” while the 6 h temperature was “too cold”. I think some further explanation might be needed here.

Reviewer # 2 - The model is driven by daily data, triggering this discussion whether one should use data measured at midnight, 6am, midday, etc (see page 13046). But it misses a discussion on how appropriate it is to rely on daily measurements for representing the highly dynamic snow pack evolution, which is clearly heavily influenced by diurnal cycles. It appears that driving the model with daily means causes a significant bias (underestimation) in estimated SWE, which shows in turn that daily resolution is problematic (see page 13046, line 5).

Reviewer #3 - The choice of nighttime temperature should be further developed in the paper, as it is not intuitive why this approach is necessary for fitting the modeling calculations.

We thank the reviewers for identifying a topic requiring clarification. While hourly meteorological data can provide important diurnal variations, the project's goals were focused on quantifying watershed snowpack storage across multi-decades in order to encompass a range of climatic conditions. Hourly meteorological data for the higher elevation SNOTEL sites were available only beginning in WY 1999. Using the hourly forcing data would have significantly decreased the number of years available for the study by nearly 50% (a full decade). Daily data were adequate for this study because we did not focus on the sub-daily/diurnal snowpack dynamics.

Reviewer #2 introduced valid concerns regarding whether daily measurements represent the dynamics of snowpack evolution. The maritime snowpack of the McKenzie River Basin (MRB) does not have a strong diurnal signal because there is little diur-

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nal variability in air temperature. For example, we calculated the coefficient of variation (CV) for hourly air temperature in WY 2007 and found that 86% of all days had a CV value that varied by only $\pm 2\%$ (please refer to Figure 2 in the supplemental documents). Multiple years of unpublished field data measured by Nolin and Sproles for unrelated projects supports this observation. These data show that in snowpacks up to three meters deep, the temperature gradient is typically about 1°C . This (and other) maritime regions have snowpacks that are warm, nearly isothermal, and highly sensitive to increased temperature; hence the importance of studies such as this to demonstrate the accumulation and ablation sensitivities of maritime snow.

The decision to apply meteorological data at 00h was questioned by the reviewers. The midnight temperature and precipitation data from the NRCS SNOTEL sites are the only values that are subject to quality assurance/quality control (<<http://www.wcc.nrcs.usda.gov/nwcc/sitenotes?sitenum=619>>). At the NWS sites, meteorological data were available through the study period (1989 – 2009) at 00h, 06h, 12h, 18h and with daily means values of T_{air} . Test iterations of the model were run with individual inputs for each of these times and results were compared to independent data for goodness of fit and Nash-Sutcliffe Efficiency (NSE) values. The data acquired at 00h provided the best goodness of fit and NSE values. We apologize for any ambiguity and have clarified this in the updated manuscript (lines 173 – 198 of the updated manuscript).

We strived to minimize model tuning so we used published values for albedo, albedo decay, and rain-snow temperature partitioning rather than use them as tuning parameters for a better fit with input data from other times of the day. Our validation of spatially interpolated model input and the high Nash-Sutcliffe values for model outputs supports our level of confidence that these results were obtained for the right reasons.

Reviewer #1 commented that the terms “too warm” and “too cold” may unintentionally describe measured comparisons. We apologize for using terms that introduce ambiguity, and we have removed them.

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We agree with the reviewers that ideally, the hourly meteorological forcing data would be available over a longer time period. However, our study's focus was on snow water storage at the watershed scale and over multiple decades. Our methods are in agreement with our stated goals (DeWalle and Rango, 2008). The updated manuscript addresses the reviewer comments in the Methods section (lines 173 – 198 of the updated manuscript), and in the Discussion section (lines 640 – 643 of the updated manuscript).

I assume that this comparison used the partitioning equation (eq.1) introduced on the following page. The transition temperature zone used in the study is -2 to $+2^{\circ}\text{C}$ based on an older study from 1956. More recent studies (although most not in this climatic region) have often found transition temperatures that were several degrees warmer. Was there any analysis done on whether such a higher transition temperature might improve model simulations or might allow the use of all recorded daily temperatures? Was there any sensitivity analysis on the transition temperature performed?

There were two transition schemes tested during this study. The USACE (1956) study used a straightforward linear partition. Dai (2008) derived a more sophisticated hyperbolic tangent function to describe the rain-snow partition. We tested both methods and results were virtually identical. The USACE linear partition provided higher computational efficiency so we proceeded with this approach (lines 245 – 247 in the updated manuscript).

Comments on albedo and albedo decay:

Reviewer #1 - For the albedo the authors point out that albedo in the forest decays faster than in the open. However, the albedo routine implemented distinguishes only between non-melting and melting conditions and apparently uses the same decay function for un-forested and forested sites, while only using different albedo ranges for forested versus un-forested sites. Is there a possibility to use different decay functions according to land cover to account for the quicker decay at forest sites?

Reviewer #2 - The issue with the initially fixed snow albedo has been properly identi-

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fied, but calibrating the albedo evolution with time in order to reach the best possible agreement between the modeled SWE and the SWE point measurements transforms the albedo into a global tuning parameter.

We applied the Strack et al. (2004) albedo decay functions which have validated with field data in Canadian Boreal forests. We did not use albedo as a tunable parameter. We apologize for any ambiguity and have included the citation in the updated manuscript (lines 249 – 266 in the updated manuscript). It is noteworthy that the inclusion of an albedo decay function improved model results considerably during the ablation period.

The initial albedo values for forested and unforested conditions are based upon measured albedos from Burles and Boon (2011). When this study was conducted, we did not have validated albedo values for our study area and proceeded with the same decay function for forested and unforested sites. Development of new albedo decay functions lie outside the scope of this paper. We understand that applying a single albedo decay function may be a potential source of error and address this point in the updated discussion (lines 624 – 628 in the updated manuscript).

p. 13038 Include the size of the MRB basin in the abstract to show readers what the authors consider “regional scale” We have included the basin size in the abstract.

p. 13048, line 21 I think you mean Eq. 1 not 2. Also, you may want to note that Tair in the equation has the unit of Kelvin. We have corrected this in the updated manuscript.

p. 13055 Please provide more details on the locations of your field measurement sites, especially things like elevation, land cover, exposition, slope etc. and the actual data measured vs. modeled SWE. Figure 4 has been added to the updated manuscript that provides details of the field measurement sites. The NSE for field sites was also added to the revised manuscript in Results section.

p. 13057 and 13058 The losses of SWE in the future climate model runs versus the

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present day climate runs are sometimes given in km³, and sometimes in m which might be a little confusing. Could you maybe add “mm SWE” as a familiar unit to those numbers to provide the reader with a better understanding of the impact of these changes, especially since you specify annual precipitation in the region in mm on p. 13040 Thank you for this valuable suggestion. We now report SWE in mm throughout the text.

p. 13058 The last paragraph on this page explains some of the results in Figure 7 as a result of shading from surrounding topography. Is this form of shading even adequately included in the model especially at the resolution of 100 by 100 m at which the model is run in the current study? Topographic changes in the study area are not gentle undulations but distinctive transitions in geology and topography. The expressions of topography on snowpack are consistent in Figure 7, in maps of snowpack, and through field observations over multiple years.

p. 13061 The authors state: “Losses in SWE and declining snow duration will impact years with high, low and average snowpack and will change the statistical representation and human perceptions of what a high, low and average snowpack represents”.

Yet the discussion on the impacts of climate perturbations on snowpack focuses solely on results spanning the entire reference period. Would it be possible to also show some results (i.e. % loss of SWE or shift in snow covered days) for high, low, and average winters separately as was done in the model calibration and validation section? Additional sub-figures of years with high, low, and medium snowpack years have been added to Figure 3.

Table 4: “station will noted by an asterisk” ??? This has been corrected.

Fig.3 shows very clearly the impact of the warmer temperature on the evolution of the snow cover over a whole winter. Maybe some additional figures showing all (or at least more) reference years for one location could be added to further visually illustrate the impact of the climate change over the entire winter period more clearly. Additional

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sub-figures of years with high, low, and medium snowpack years have been added to Figure 3.

Fig. 5 Caption: An explanation of what is illustrated in the lower map should be added, while the sentence: "The upper elevations are not affected as significantly as the lower elevation snowpack." should probably be removed and added to the text when discussing Figure 5. The authors thank the reviewer for this recommendation. Based upon this insight and a recommendation from Reviewer #3, we now represent the SWE results by elevation band in both the present and +2°C scenarios in Figure 6.

Direct labeling on Figure 7 We appreciate the feedback on making the figures more user-friendly. However, we feel that the direct labeling on the figures and the explanation in the caption adds clarity and therefore we have retained the direct labeling.

Reviewer 2:

The model is driven by daily data, triggering this discussion whether one should use data measured at midnight, 6am, midday, etc (see page 13046). But it misses a discussion on how appropriate it is to rely on daily measurements for representing the highly dynamic snow pack evolution, which is clearly heavily influenced by diurnal cycles. It appears that driving the model with daily means causes a significant bias (underestimation) in estimated SWE, which shows in turn that daily resolution is problematic (see page 13046, line 5).

These comments were collectively addressed with reviewer #1's comments. Please refer to pages 3 - 5.

The model is also based on a single snow layer description (page 13044, line 21) when it has been shown repeatedly (e.g. SNOWMIP) that at least three layers should be considered to avoid biases in dynamic snow behaviour.

We appreciate the reviewer's concerns about a single layer model. The maritime snowpack in the McKenzie and the Western Cascades of Oregon is almost entirely isother-

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mal. Multiple years of unpublished field data measured by Nolin and Sproles for unrelated projects support this observation. As mentioned previously, these data show that in snowpacks up to three meters deep temperature gradients are typically about 1°C for the entire snowpack. Similarly, Sturm et al. (1995) found temperature gradients of -0.04 to -0.07 °C cm⁻¹ in maritime snowpacks. Liston (2013) indicates that differences in melt onset and snow disappearance dates between a correctly formulated single layer model and a multi-layer vary by about a half-day in locations with stronger snowpack temperature gradients than in the MRB. SnowModel was developed after the SNOWMIP project and includes an enhanced snowpack evolution model that was not assessed in the original SNOWMIP research. SnowModel addresses changes in snowpack by accounting for changes in density during the accumulation and ablation period. This includes rain-on-snow events that are common throughout the winter in this region, and have a considerable effect on snowpack evolution (Marks et al., 1998). Therefore we feel that applying a one-layer, validated model effectively represents the internal dynamics of the snowpack in the McKenzie River Basin.

The issue with the initially fixed snow albedo has been properly identified, but calibrating the albedo evolution with time in order to reach the best possible agreement between the modeled SWE and the SWE point measurements transforms the albedo into a global tuning parameter.

These comments were collectively addressed with reviewer #1's comments. Please refer to pages 5 – 6 of this document.

The calibration of the temperatures for the accumulation phase and the ablation phase (performed separately, see section 2.1.3 page 13049) has been evaluated on the SWE but should instead have been performed by comparing with some reference temperature measurements.

We agree with the reviewer that the calibration and validation process should be applied to the first order controls of temperature and precipitation. Our methodology took

C7207

exactly this approach. We first calibrated and then validated temperature and precipitation simulations to measured values throughout the basin. Only when temperature and precipitation simulations were satisfactory did we explore the calibration and validation of snowpack. Calibration was not performed directly on SWE (lines 224 – 230 in the updated manuscript).

The remote sensing data that is used in this calibration is very sparse and this could be especially problematic in the ablation phase: a small timing error could lead to a large spatial discrepancy in gentle terrain while in steep terrain the comparison would be very un-challenging.

We agree with Reviewer #2 that the frequency of remote sensing data for model assessment is sparse. Cloud cover dominates winters in this region and the Landsat instrument has an 16-day repeat cycle. Combined, these factors limit the availability of unobscured Landsat data. We concur that the application of remote sensing data in steep terrain is challenging. While more remote sensing data would be of benefit to the study, we used the best data available. The combination of high temporal resolution ground-based validation data and spatially extensive (though temporally limited) space-borne snow data provides the highest degree of model assessment possible. Moreover, this study helped to identify areas of the basin where enhanced ground-based monitoring would aid future modeling efforts.

SWE point measurements are all located within a narrow elevation range, therefore using them in order to calibrate the model could lead to poor results at low or high elevations.

These comments were collectively addressed with reviewer #1's comments. Please refer to pages 1 and 2 of this document.

The model validation was performed directly on SWE

We agree with the reviewer that the calibration and validation process should be applied

C7208

to the first order controls of temperature and precipitation. As previously mentioned, our methodology took exactly this approach, first validating model simulations of temperature and precipitation to measured values throughout the basin. Only when these controls were satisfactory calibrated and validated did compute the evolution of SWE. Calibration was not performed directly on SWE (lines 224 – 230 and lines 295 – 303 in the updated manuscript).

A timing issue that shifts some precipitation to one day later ends up producing a more than two-fold over estimation of SWE. This is both surprising and worrying since a timing issue would slightly degrade the results but should not lead to such a massive change in SWE.

This too was of concern when reviewing the initial results. We would like to clarify that the mistiming of precipitation inputs created a double-count of precipitation inputs distributed across the study domain. The fact that the simulated SWE overestimated by roughly a factor of two indicates that the model performed well given the doubled precipitation inputs (lines 393 – 404 in the updated manuscript).

Discussion of how long wave input is obtained or estimated, despite this parameter playing a major role in the energy balance.

We would like to thank the reviewer for bringing this salient point to our attention. Snow-Model uses the method of Iziomon et al. (2003) to compute the longwave radiation balance (lines 139 – 141 in the updated manuscript). In MicroMet, the incoming longwave radiation is computed using the Stefan-Boltzmann equation:

$$Q_{li} = \epsilon_{cl}\sigma T^4$$

where, T is the temperature in Kelvin, σ is the Stefan-Boltzmann constant, and ϵ_{cl} is cloud emissivity. MicroMet uses elevation, cloud cover, vapor pressure and cloud cover to distribute cloud emissivity. Additionally SnowModel adjusts incoming longwave radiation by using forest canopy and leaf area index.

C7209

Emitted long-wave radiation is calculated in the EnBal submodel using the following equation:

$$Q_{le} = -\epsilon_{surf}\sigma T^4$$

where, ϵ_{surf} is surface emissivity of the snowpack (held constant at 0.98).

Net longwave is computed as: $Q_{lw} = Q_{li} + Q_{le}$

What is the added value of the model that has been used compared to simple degree day models or more complex multi-layer energy balance models.

A simple degree-day model does not explicitly account for slope, aspect, and land cover—all of which are important in the MRB. Over 30% of the MRB has a slope greater than 20°, and aspect varies greatly by location (refer to Figure 3 in the supplemental documents). Land cover in the MRB ranges from forest canopy to broad exposed volcanic landscapes. This project required a spatially explicit model that accounts for complex topography and land cover. SnowModel is a well established, physically-based model that successfully simulates snowpack evolution. Because SnowModel is physically-based it accounts for slope and aspect in calculating the energy balance. Additionally the role of land cover (e. g. canopy interception, sublimation, and unloading) is included in the simulations of snowpack evolution through its canopy representation in the sub-models MicroMet, EnBal, SnowTran, and SnowPack. These aspects would be lost in a simple degree day model causing both accumulation and ablation to be incorrect.

We have previously addressed the advantage of using a single-layer versus a more complex multi-layer model. SnowModel effectively addresses changes in snowpack characteristics during both the accumulation and ablation periods. These calculations include rain-on-snow events that are common in the study area, and that can play a considerable role in snowpack dynamics (Marks et al., 1998). A multi-layer model adds unneeded complexity whereas a correctly formulated single-layer model

C7210

captures energy and mass balance transfer with less computation. A multi-layered snowpack model is needed for simulating snow layer differences for metamorphosis and avalanches, which lie outside the scope of this research.

This study presents significant advances by quantifying for the first time the watershed-scale volume of snow water equivalent in the McKenzie River Basin (MRB) across multiple decades. It is not possible to quantify watershed-scale SWE with the existing monitoring network, and requires a validated modeling approach. Because SWE is the most climatologically sensitive hydrologic component in this watershed, we developed a clarified understanding of snowpack sensitivity to projected temperature increases. Although beyond the scope of this work others have shown that the MRB is an important contributor to regional base flows during drier summer months (Hulse et al., 2002; Jefferson et al., 2008; Tague et al., 2008), emphasizing the added-value of this study.

We respect the reviewers concerns regarding some of the methods used in this study. While all models have inherent strengths and weaknesses, they are often a balance of pragmatic realism (Beven, 1999). The constraints of limited data did not provide meteorological forcings at all elevations of the MRB. Despite these constraints we successfully simulated spatially distributed precipitation, air temperature, and SWE with mean NSE values of 0.97, 0.80, and 0.83, respectfully. We are applying our findings to improve field measurements in the basin that will ultimately aid future model-based studies in this watershed and region. We respect the comments and concerns of the reviewer but contend that the model, methods, and added benefits from this study clearly outweigh any deficiencies.

Reviewer #2 specific comments:

In the abstract, at page 13038, line 10: maybe giving the projected temperature change could be a good idea. This has been included in the updated abstract (lines 11 – 12 in the updated manuscript).

In the abstract, at page 13038, line 13: maybe a reference to SnowModel would be

C7211

appropriate. This has been included in the updated abstract (lines 7 – 8 in the updated manuscript).

Page 13039, line 1: please consider referencing figure 1. A broader context map has been included in Figure 1, and is referenced in line 35 of the updated manuscript.

page 13039, line 17: consider showing the "mountain West" on the figure 1 map (does it mean, the mountains on the West side of the area or is it a specific place?) This has been included in the updated manuscript (line 35 in the updated manuscript).

page 13040, line 18: is there no clear trend on the precipitation? General Circulation Models (GCMs) show both increases and decreases in projected precipitation. Our approach of using $\pm 10\%$ was intended to show the potential sensitivity of snow accumulation to changing precipitation not to provide a precise application of GCM projections. We have modified the text to clarify this point.

The text that read: "quantify the watershed-scale response of snow water equivalent to increases in temperature and variability in precipitation."

has been modified to: "quantify the watershed-scale response of snow water equivalent to increases in temperature and increases/decreases in precipitation." (lines 126 – 130 in the updated manuscript).

page 13041, lines 2-3: please rephrase Done (lines 75 – 77 in the updated manuscript).

page 13042, lines 25-29: you are contradicting yourself with respect to lines 7-11 next page! Thank you for identifying this contradiction, and we have clarified the point (lines 109 – 122 in the updated manuscript).

page 13043, lines 14-15: please keep in mind that this depends on the landscape: a smooth landscape is well suited to remote sensing while a rugged one might still require a spatial resolution that is not yet available Good point. We have mentioned this in our error discussion with regard to the remote sensing assessment (lines 628 – 632 in the updated manuscript).

C7212

page 13044, line 12: is it necessary to list both Colorado, Idaho and Wyoming? The updated text provides an abbreviated description of SnowModel, and does not reference geographic locations.

page 13044, line 20: please rephrase Reviewer #3 also requested that this section listing the sub-models be condensed and this is now reflected in the updated manuscript (lines 132 – 151).

page 13047, line 19: is there a reference for this report? We would like to thank the reviewer for this oversight. We now cite the following reference: United States Army Corps of Engineers: Snow Hydrology: Summary report of the snow investigations, U.S. Army Corps of Engineers, Portland, Oregon, 437, 1956.

page 13048, lines 10-11: please rephrase in a more objective way This has been included in the updated manuscript (lines 249 – 266 in the updated manuscript).

page 13048, lines 23-25: consider rephrasing, starting with α_t is... (more logical) Thank you for identifying this point of confusion, we included this suggestion in the updated manuscript (line 258 in the updated manuscript).

page 13052, line 12: this is not very clear at first, consider replacing "+/-" This has been included in the updated manuscript (lines 368 – 380 in the updated manuscript).

page 13052, line 22: rephrase This has been included in the updated manuscript (lines 383 – 390 in the updated manuscript).

page 13053, line 17: is it a two fold over estimation of the SWE time series (instantaneous values) or accumulated values? As mentioned earlier, the precipitation data for that year double-counted inputs, which in turn produces model simulations with a two-fold over estimation. The revised manuscript provides an improved description of this the double count and its implications on SWE accumulation (lines 393 – 404 in the updated manuscript).

page 13056, line 25: what is the exact definition of "retaining a seasonal snow-pack"?

C7213

Thank you for identifying this point of confusion. We have changed the text to read "retains a distinct accumulation and ablation period."

page 13059, line 2: "of" is missing Corrected.

page 13059, lines 9-12: one has the feeling when reading the paper that the whole model was calibrated for SWE using the set of stations (which has a direct impact on the local air temperature), the albedo and the precipitation partition, contradicting what is said here. The comment on page 13054, line 12 that when comparing the interpolated temperatures and the point measurements, the interpolations ended up 2 degrees too high confirms this.

We respectfully disagree with the reviewer on this comment. The model was calibrated first for precipitation and temperature (lines 269 – 276 in the updated manuscript). Only when model performance for these first order controls was optimized we compute SWE. While RMSE values were greater than anticipated, air temperature had an R2 of 0.85 and 98% of all simulations were within a 95% confidence interval (lines 327 – 333 in the updated manuscript).

page 13063, line 26: I guess "worship" is not what is intended here

We understand the reviewer's point regarding the use of the word "worship" in describing a role of water as a resource. While this notion of worship is often not the primary focus of hydrologic research, water does serve an important role in culture and religion. For example water is used as holy water, to wash before prayers, and in baptisms. The inclusion of the word worship helps provide added contemplative value to water. <http://watercitizennews.com/water-and-religion/> http://www.unesco.org/water/wwd2006/world_views/water_religions_beliefs.shtml

page 13070: grouping stations by model forcing would improve readability We thank the reviewer for this suggestion. We organized this table by elevation, and feel that this allows information to be found in a hierarchical format and kept in its original format.

C7214

page 13073: in the table comment, "swill" is a typo Corrected.

page 13077: the fits between measurements and modeled values are sometimes pretty bad (CENMET, Santiam Junction, Upper Lookout Creek) We agree with reviewer #2 that the fits between measured and modeled values at CENMET, Santiam Junction, and Upper Lookout Creek are not as robust as the other SNOTEL sites. Unfortunately the automated snow pillows within the HJ Andrews Experimental Forest (CENMET and Upper Lookout Creek) have experienced technical issues and have not been regularly calibrated. Despite this, the data from Upper Lookout Creek had a mean Nash-Sutcliffe of 0.88 for SWE.

The underperformance at Santiam Junction may be partially attributed to the physical characteristics of the site (lines 423 – 426 in the updated manuscript). This site is adjacent to a state highway, an Oregon Department of Transportation facility, and an airstrip which combined, make it more exposed to wind than the nearby natural forest setting. Additionally the Oregon Department of Transportation facility houses large piles of road cinders used for winter road traction. On field visits Sproles and Nolin have repeatedly noticed dark particles from road cinders accumulating on the snow near this station. Despite these challenges Santiam Junction had a mean Nash-Sutcliffe of 0.74 for SWE. Field measurements were conducted on the opposite side of the airstrip (site S4 in Figure 4 – updated figures), about 0.5 km west of the SNOTEL site and show a strong fit to the modeled SWE. We have included these field measurement metrics in the updated manuscript (lines 450 – 452 in the updated manuscript).

Reviewer #3 comments:

Abstract. Contains too much background, introductory material; a shorter abstract that get to the main finding of this research would make it easier for the reader to understand what the authors did and found. The result of their calculations is there; but it gets lost in the background. We have incorporated these into the revised manuscript.

Introduction. This section is too long on the contextual and fails to motivate the methods

C7215

used. A different introduction would serve this paper better. We would like to thank the reviewer for this comment and have abridged the introduction to reflect the reviewer's suggestions.

Study area. This section is an extended background and introduction to the McKenzie River basin and region, and is not needed at this point in the paper. It (study area) also provides more introductory material giving the authors' views of certain aspects of snow data and calculations using those data. It should be eliminated. If some fraction of the material is relevant to interpretation of the results, then it should be incorporated into the discussion section. A very brief paragraph giving salient features of the basin relevant to the snow-storage calculations could be put in the methods section. We appreciate the reviewer's feedback regarding the content of this section, as it provides a valid critique. Based on the suggestions of the reviewer, we have made this section more concise. This includes condensing study area information and its salient features into a single, short paragraph.

We feel that some of the information on snow data and calculations using this data are required to better understand the motivation for this study. For example, identifying the deficiencies in the present snow monitoring network support the relevancy of this model-based study.

Research methodology. This section should be called methods; methodology is the wrong word. Changed as suggested.

It is appropriate to offer a summary of the approach here, and this should directly follow the questions posed in the introduction to be most effective. It should directly flow from the last paragraph of the introduction so the reader gets the what, why and how of the research in going from the intro into this statement. Changed as suggested.

It also needs to indicate what data used, not just state what calculations were done. We thank the reviewer for this insight, but feel that Table 1 effectively summarizes the input data used in the study.

C7216

Modeling the snowpack. This section is a list of various sub-models that the authors used for the current calculations and the input/output variables. Collapsing this with the next section would help the reader understand what data are driving the calculation, in context. The revised manuscript now combines a condensed version of the sub-models with model input data.

Model input data. This section could be more effective if it was limited to a straightforward description of the data used and any modifications to the data that were needed in order to use it for the current research. At present it is a somewhat diffuse description of model data requirements, characteristics of various datasets and results. We apologize if this section is somewhat diffuse to the reviewer. The revised manuscript now provides a more direct description and a table of the required data inputs to make this section more clear and succinct.

We have included a revised and abridged description of the Barnes Objective Analysis technique. We feel that this is important because the spacing between meteorological inputs proved to be critical for improved model results using this weighted interpolation technique.

The choice of nighttime temperature should be further developed in the paper, as it is not intuitive why this approach is necessary for fitting the modeling calculations. As described earlier in our responses to reviewers, we have added further explanation (lines 188 – 198 in the updated manuscript).

Model modifications. This section, while needed to describe the calculations, could be presented in a short paragraph. We appreciate the comments of the reviewer on this section. However, comments from the other reviewers requested more information specific to this section. The updated manuscript will address all three reviewers' comments into a concise section.

Model calibration. This section needs to say how and not just what calibration was done. What parameters were adjusted, and was there a systematic or intuitive ap-

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proach? We would like to thank the reviewer for identifying this omission. We used an approach that varied tunable parameters systematically. We qualitatively evaluated outputs to identify potential model deficiencies. For instance while peak SWE was correct, initial results showed the albedo process occurring too slowly. This suggested that a static albedo was potentially dampening the shortwave albedo signal. The updated manuscript includes a brief, but detailed description of how and what parameters were used in the calibration of the model (lines 269 – 276 in the updated manuscript).

At some point in the paper the authors could explore why Minder et al came up with such surprising low surface lapse rates. The values from the calibration in this current work are much more in line with what has been observed elsewhere. The reviewer introduces a legitimate point of discussion. However, we feel that the focus of the research is on simulating snowpack, and that a comparison of lapse rates lies beyond the scope of this research.

Remote sensing calibration. What is the importance of snow under canopy in the current analysis, versus what snow is detected by Landsat? The manuscript should address this. The updated manuscript now includes a brief discussion on the difficulties associated with measuring fractional snow covered area (fSCA) when snow is partly obscured by forest canopy (lines 628 – 632 in the updated manuscript). It may be of interest to Reviewer #3 that SnowModel simulates canopy interception, sublimation, and unloading onto the snowpack.

Remote sensing calibration. This section needs to indicate what was calibrated, i.e. did this assessment result in changes to model parameters? Much of the discussion currently in this section is peripheral. The initial calibration of the model focused on precipitation and temperature. Once the calibration of these first-order controls was completed, snow extent was compared to fSCA data. As noted previously, the original version of SnowModel had a static albedo and a fixed rain-snow temperature threshold. Prior to the implementation of the albedo decay function and rain-snow partition, there was an overestimation of modeled snow extent compared to Landsat data. How-

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ever once these modifications were incorporated into the model, spatial agreement improved considerably. We did not use these improved parameterizations as tuning parameters. This model improvement makes sense conceptually. The fixed rain-snow temperature threshold simulated 100% of precipitation to fall as snow when air temperature was 2°C or colder, and lead to an overestimation of snow. Compounding this overestimation was a fixed albedo that underestimated the net shortwave radiation critical to the melt process. The rain-snow partition would proportion less precipitation falling as snow, and the albedo decay would hasten the melt process. These improved model parameterizations consequently improved the simulated SWE (as compared with data from monitoring stations) and the spatial extent of snow (as compared with Landsat).

Model assessment. i) What do the points on Fig 2 represent? Daily precip and night-time temp for some subset of the study period? The reviewer is correct, these are daily precipitation and temperatures for the validation years. This detail has been clarified in the updated manuscript.

Model assessment. ii) The results section could benefit from a succinct description of the results, referring to the appropriate figures and tables, before getting into an interpretive discussion of why calculations at some measurement sites fit observations better than others. This insight has been included in the updated manuscript (lines 383 – 393 in the updated manuscript).

Model assessment. iii) It would be appropriate to focus the presentation of results on just the period of snow accumulation and melt, as the aims of the paper to estimate the distribution of snowpack water content. It is not really clear to the reader what time periods or seasons the authors are presenting in the figures. Good point. We have incorporated this in the updated manuscript (updated Figure 2).

Model assessment. iv) At what elevations is precip snow versus rain dominated, and what is the transition? Overall, the rain-dominated region is about 400-800m tran-

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sitioning to a rain-snow mix from about 800-1200m to a snow-dominated regime at elevations above 1200m. This varies from year to year and storm to storm. We are currently preparing a second follow-up paper focusing on the relationship between rain and snow. Thus, we have not included these details in this manuscript.

Model assessment. v) There is really insufficient presentation of the evaluation using the Landsat data, and it is not apparent that these data influenced the model calibration. The paper would probably be better off without these data. There is also the issue of vegetation influences on snowcover, which are not addressed in this study and may be a dominant factor in trying to evaluate the calculations. We welcome the insight of the reviewer and will include more detail regarding the presentation of the Landsat data evaluation. More detail on this topic was discussed in the Remote sensing calibration section of Reviewer #3's comments.

However, we respectfully disagree with the reviewer that this component should be omitted from the manuscript. Relying solely on point-based calibration of SWE provides location specific insights into model accuracy. Including the remote sensing component helps promote spatial accuracy. We agree with the reviewer that there are issues of vegetation influences on snowcover, and address why the degree of false positive (13%) is expected (page 13056, lines 4-8). For brevity, we did not include a detailed summary of model assessment of remote sensing data, but did provide the citation to a comprehensive analysis in the manuscript (Sproles, 2012).

Model assessment. vi) What are the elevation characteristics of the spatial snowpack estimates, mean and standard deviation? This would be a good addition to Fig 4, and would much more readable than shades of dark blue on a map. We would like to thank the reviewer for this suggestion. The updated manuscript includes an addition to Figure 6 (the maps), similar to that of Figure 7. The updated sub-figures shows SWE (x-axis) and elevation (y-axis). Mean and standard deviation have been included in these new sub-figure and on the map in Figure 6.

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Sensitivity to changes. i) Summarizing changes by elevation band on a map would be interesting and again would give the reader a much better feel for the sensitivity than just shades of dark blue or red on a map. Thank you for the suggestion. Please refer to Figure 6 in the updated figures.

Sensitivity to changes ii) The main message would be much clearer if the focus was just on the temperature increase and the +/- 10% precip changes omitted. Alternately, the authors can pose an additional objective and further develop the rationale for studying this magnitude of temperature change. A better approach would be to use the precipitation record for the period used in this study, which exhibits more than +/- 10% interannual variability. We apologize if the message regarding the snowpack's sensitivity to perturbed temperature was not clear. We feel that including the $\pm 10\%$ variability is relevant, as discussed in lines 504 – 514 of the updated manuscript and shown in Figure 6. These results show that snowpack in the McKenzie River basin is governed primarily by temperature, and that projected temperature increases of 2°C will have more of an effect than years of above or below average precipitation.

Sensitivity to changes iii) The elevation shift in the rain/snow transition was how much for the 2°C temperature warming, given the variable monthly lapse rate? We found that the elevation shift in the rain/snow transition to be approximately 260 m with a 2°C temperature increase. This and similar metrics will be discussed in greater detail in a follow-up paper focused on the relationship between rain and snow.

Sensitivity to changes iv) The interpretation of Fig 7 would fit better in the discussion. Thank you for this suggestion. Figure 7 has been incorporated into the Discussion section.

Discussion. i) Omit the first paragraph, and if relevant state as a conclusion. This paragraph has been included in the conclusion.

Discussion. ii) Most of the 2nd paragraph is statements of the obvious and it could be cut to a brief statement of metrics of accuracy. Thank you for this suggestion and more

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concise version has been included in the revised manuscript (lines 544 – 557 in the updated manuscript).

Discussion. iii) Is the suggestion in the 3rd paragraph really feasible? Is this a hypothesis, or is this known? We feel that the suggestion to compute dynamic lapse rates is feasible with a dual pass approach. The first pass through the meteorological station data would establish the lapse rate and the second pass would use the new lapse rates in the Barnes Objective Analysis method to spatially distribute temperature data. It is relevant because this would allow an individual storm's lapse rate characteristics to be included in the model. A dynamic lapse rate will also help during stable conditions when cold air pooling may be important. Daly (2010) demonstrated that lapse rates are often decoupled from expected values in the steep Western Cascades because of cold air pooling.

Discussion - Impacts of climate perturbations. i) The 1st paragraph seems to be backing away from the questions posed in the intro and indicating that this work is not a good estimate of snow, only some suggestions on how to go about estimating snow. Is this what the authors really want to convey to the reader We would like to thank the reviewer for this suggestion. The updated manuscript now includes a more assertive statement about the study results.

Discussion - Impacts of climate perturbations. ii) A figure summarizing snowpack water content by elevation for representative years with current and +2C would greatly facilitate this general discussion. Thank you for this suggestion. Figure 6 shows mean snowpack by elevation and with +2°C is now included in the updated manuscript and has the same axis format as in the original Figure 7.

Discussion - Impacts of climate perturbations. ii) What confidence is gained by these detailed calculations that would not come from a simpler estimation of present elevation-averaged snowpack and snowmelt, and then applying a 2°C elevation change using an average lapse rate? Please refer to reviewer #2 's comments on pages 10 –

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11.

What is more important about the detailed calculations done as part of this study, getting the lapse rate right, doing a detailed parameterization of the energy balance using a spatially distributed algorithm, or the methods of evaluation and calibration? The detailed parametrization of the energy balance components served as a primary driver of simulating snowpack evolution, and thus was an important component of the detailed calculations. This helped ensure that simulations were right for the right reasons. Evaluation and calibration complimented the parametrization of the energy balance. From the authors' perspective the development of a dynamic lapse rate that better represents an individual weather system remains one of the primary recommendations for to improve model results.

Conclusions. The present conclusions section should be replaced with a paragraph that answers the questions posed in the introduction. We have included a new paragraph as suggested (lines 686 – 696 in the updated manuscript).

References included in the responses to reviewers:

Beven, K.: How far can we go in distributed hydrological modelling?, *Hydrol. Earth Syst. Sci.*, 5, 1-12, 10.5194/hess-5-1-2001, 1999. Burles, K., and Boon, S.: Snowmelt energy balance in a burned forest plot, Crowsnest Pass, Alberta, Canada, *Hydrological Processes*, 25, 3012-3029, 10.1002/hyp.8067, 2011. Dai, A.: Temperature and pressure dependence of the rain-snow phase transition over land and ocean, *Geophys. Res. Lett.*, 35, L12802, 10.1029/2008gl033295, 2008. DeWalle, D. R., and Rango, A.: Principles of snow hydrology, Cambridge University Press, 2008. Hulse, D., Gregory, S., and Baker, J. P.: Willamette River Basin planning atlas: trajectories of environmental and ecological change, Oregon State University Press, Corvallis, OR, 2002. Iziomon, M. G., Mayer, H., and Matzarakis, A.: Downward atmospheric long-wave irradiance under clear and cloudy skies: Measurement and parameterization, *Journal of Atmospheric and Solar-Terrestrial Physics*, 65, 1107-1116, 2003. Jefferson,

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A., Nolin, A., Lewis, S., and Tague, C.: Hydrogeologic controls on streamflow sensitivity to climate variation, *Hydrological Processes*, 22, 4371-4385, 10.1002/hyp.7041, 2008. Liston, G. E., and Elder, K.: A Meteorological Distribution System for High-Resolution Terrestrial Modeling (MicroMet), *Journal of Hydrometeorology*, 7, 217-234, 2006. Liston, G. E.: personal communication, 2013. Marks, D., Kimball, J., Tingey, D., and Link, T.: The sensitivity of snowmelt processes to climate conditions and forest cover during rain-on-snow: a case study of the 1996 Pacific Northwest flood, *Hydrological Processes*, 12, 1569-1587, 10.1002/(sici)1099-1085(199808/09)12:10:11<1569::aid-hyp682>3.0.co;2-l, 1998. Sproles, E. A.: Climate change impacts on mountain snowpack presented in a knowledge to action framework PhD, Water Resources Graduate Program, Oregon State University, Oregon State University, 2012. Strack, J. E., Liston, G. E., and Pielke Sr, R. A.: Modeling snow depth for improved simulation of snow-vegetation-atmosphere interactions, *Journal of Hydrometeorology*, 5, 723-734, 2004. Sturm, M., Holmgren, J., and Liston, G. E.: A Seasonal Snow Cover Classification System for Local to Global Applications, *Journal of Climate*, 8, 1261-1283, 1995. Tague, C., Grant, G., Farrell, M., Choate, J., and Jefferson, A.: Deep groundwater mediates streamflow response to climate warming in the Oregon Cascades, *Climatic Change*, 86, 189-210, 10.1002/hyp.8067, 2008. United States Army Corps of Engineers: Snow Hydrology: Summary report of the snow investigations, U.S. Army Corps of Engineers, Portland, Oregon, 437, 1956.

Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, 9, 13037, 2012.

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Table 1:

Elevation Range (m)	Area (km ²)	% of McKenzie River Basin
0 to 500	460	15.0
501 to 1000	959	31.3
1001 to 1500	1170	38.2
1501 to 2000	431	14.1
2000 to 2500	37	1.2
Over 2500	7	0.2
430 to 1512	2733	74.4

Figure 1:

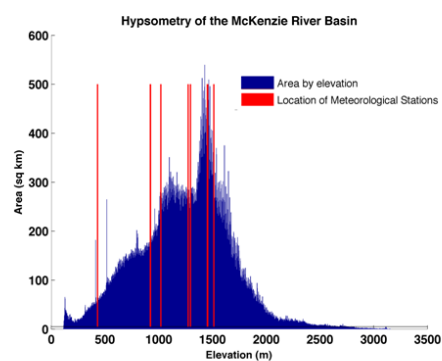


Fig. 1.

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