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Hydrology and Earth System Sciences Discussions

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

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This paper presents an interesting study about the impacts of changing climatic conditions on the maritime snowpack, an important component of the hydrologic water balance in many parts of the world and especially and a component that will be greatly affected by climate warming. The authors use a well established calibrated and validated snow model along with predefined changes in air temperature and precipitation to illustrate the effects changing climatic conditions might have on maritime snowpacks. The study is technically sound, interesting and well within the scope of the journal. There are however, in my opinion, a few minor concerns mainly about the implementation of the model that should be addressed.

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, 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 SnowModel 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.

I also have some concerns about the modifications the authors made to the model. One of the problems needed to be solved was the portioning of the precipitation into rain and snow. On p. 13046 the authors state:

"however the mean (daily) temperatures underestimated the amount of snow throughout all of the calibration years. SNOTEL sites in the MRB have temperature data recorded at 0 h (midnight), 6 h, 12 h, and 18 h throughout the reference period. We tested the model using temperature data from each of these times and achieved the most accurate model results when using data acquired at midnight. This makes sense for several reasons. Temperatures at 12 h, and 18 h were too warm and so precipitation was partitioned as rain rather than snow. The pre-dawn 6 h temperatures were cold causing the model to overestimate the proportion of snowfall. The midnight temperature values provided the correct rain-snow partitioning in the model."

While the explanation for leaving out the data from certain times makes some sense, I don't quite understand how it was concluded that e.g. 12 h and 18 h temperatures were "too 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.

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°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?

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 unforested and forested sites, while only using different albedo ranges for forested versus unforested sites. Is there a possibility to use different decay functions according to land cover to account for the quicker decay at forest sites? What land cover did the sites have that were used to calibrate the decay gradients? How sensitive is the model simulation to albedo decay? A more detailed discussion would be helpful here in my opinion.

Specific comments

p. 13038 It would be helpful to include the size of the MRB basin in the abstract to show readers what the authors consider “regional scale”.

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.

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.

p. 13057 and 13058 The losses of SWE in the future climate model runs versus the present day climate runs are sometimes given in km^3, 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

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?

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 focusses 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?

Table 4: “station swill noted by an asterisk” ???

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.

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.

Figure 7: The explanatory comments included in the Figure and in those the Figure caption ("The snowpack between. . .") should in my opinion be removed and also included in the results or discussion section.

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