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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.

Anonymous Referee #2

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This paper addresses an interesting topic with a special focus on maritime snowpacks. It also represents a good mix of point measurements, remote sensing and modeling and therefore could be a good complement to previously published studies. The paper is well written and well structured. However, major problems arise from the modeling approach.

It seems that several serious shortcomings of the model have been addressed by tuning some parameters or the inputs instead of properly extending the model to represent what is missing. For example, 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 measure-

ments 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). 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. The issue with the initially fixed snow albedo has been properly identified (page 13047, line 14), 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 (page 13048, line 17). Similarly, 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. Moreover, the remote sensing data that is used in this "calibration" is very sparse (page 13051, lines 22-26) and this could be specially 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 unchallenging. This actually could also apply to the SWE point measurements: these 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.

Because the calibration of the model is performed directly on the SWE, one has the feeling that the model is tuned to produce a good fit, using the number of input stations, the albedo and the precipitation partition as tunable parameters. This is exemplified by the paragraph on page 13053: 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 all points to a model that is insufficient for the aim of this study. It is generally agreed that extrapolation as necessarily done when doing climate change calculations should not be based on heavily calibrated models. Another worrying clue that the model is not appropriate is that there is no discussion of how long wave input is obtained or estimated, despite this parameter playing a major role in the energy balance.

Overall, one has the feeling that this paper is well written and well structured but based on an inadequate modeling approach, which is not state of the art. The model that has been used lacks several features that would be found e.g. in competing multi-layer energy balance models. Based on this fundamental weakness, I recommend either the rejection of the paper or a significant extension to show 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.

Specific comments:

- in the abstract, at page 13038, line 10: maybe giving the projected temperature change could be a good idea
- in the abstract, at page 13038, line 13: maybe a reference to SnowModel would be appropriate
- page 13039, line 1: please consider referencing figure 1
- page 13039, line 17: consider showing the "mountain West" on the figure 1 map (does it means, the mountains on the West side of the area or is it a specific place?)
- page 13040, line 18: is there no clear trend on the precipitation?
- page 13041, lines 2-3: please rephrase
- page 13042, lines 25-29: you are contradicting yourself with respect to lines 7-11 next page!

- 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
- page 13044, line 12: is it necessary to list both Colorado, Idaho and Wyoming?
- page 13044, line 20: please rephrase
- page 13047, line 19: is there a reference for this report?
- page 13048, lines 10-11: please rephrase in a more objective way
- page 13048, lines 23-25: consider rephrasing, starting with alpha_t is... (more logical)
- page 13052, line 12: this is not very clear at first, consider replacing "+/-"
- page 13052, line 22: rephrase
- page 13053, line 17: is it a two fold over estimation of the SWE time series (instantaneous values) or accumulated values?
- page 13056, line 25: what is the exact definition of "retaining a seasonal snowpack"?
- page 13059, line 2: "of" is missing
- 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.

- page 13063, line 26: I guess "worship" is not what is intended here
- page 13070: grouping stations by model forcing would improve readability
- page 13073: in the table comment, "swill" is a typo
- page 13077: the fits between measurements and modeled values are sometimes pretty bad (CENMET, Santiam Junction, Upper Lookout Creek)

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