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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 #3

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This manuscript presents a case study estimating the extent to which an increase in temperature will diminish snowpack storage in the McKenzie River basin in Oregon, using data for the 20-year period 1989-2009. The main data used to estimate basin-wide snowpack storage includes records from the met stations and SNOTEL stations in the basin, plus topographic data. A detailed, spatially distributed snowmelt model was also used as a framework for the calculations. The paper could benefit from a sharper focus on the main questions, a concise presentation of results, and interpretation of those results in the context of uncertainties that are apparent given the sparse data available as a basis for the calculations. It would be helpful if the authors could make the case for why this method gives better results than a simple downscaled climate model (it probably does) , or a simple elevation-averaged snowpack/snowmelt calculation (not

clear that it does). Comments and suggestions to make the paper more reader friendly follow.

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.

Introduction. This section is too long on the contextual and fails to motivate the methods used. A different introduction would serve this paper better. 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 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.

Research methodology. This section should be called methods; methodology is the wrong word. 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. It also needs to indicate what data used, not just state what calculations were done. 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.

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. 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. Model modifications. This section, while needed to describe the calculations, could be presented in a short paragraph.

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 approach? Calibration metrics. 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. 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. 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.

Model assessment. i) What do the points on Fig 2 represent? Daily precip and nighttime temp for some subset of the study period? 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. 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. iv) At what elevations is precip snow versus rain dominated, and what is the transition? 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. vi) What are the elevation characteristics of the spatial snowpack estimates, mean and standard devision? This would be a good addition to Fig 4, and would much more readable than shades of dark blue on a map.

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. 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. iii) The elevation shift in the rain/snow transition was how much for the 2oC temperature warming, given the variable monthly lapse rate? iv) The interpretation of Fig 7 would fit better in the discussion.

Discussion. i) Omit the first paragraph, and if relevant state as a conclusion. ii) Most of the 2nd paragraph is statements of the obvious and it could be cut to a brief statement of metrics of accuracy. iii) Is the suggestion in the 3rd paragraph really feasible? Is this a hypothesis, or is this known? iv) 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? ii) A figure summarizing snowpack water content by elevation for representative years with current and +2C would greatly facilitate this general discussion. iii) 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 2oC elevation change using an average lapse rate? 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 methos of evaluation and calibration?

Conclusions. The present conclusions section should be replaced with a paragraph that answers the questions posed in the introduction.

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