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Hydrology and Earth System Sciences Discussions

Interactive comment on "Has spring snowpack declined in the Washington Cascades?" *by* P. Mote et al.

Anonymous Referee #1

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General Comments:

Mote et al.'s paper, "Has spring snowpack declined in the Washington Cascades," summarizes recent work that warming temperatures have led to a decline in snow in the Western United States and then examines specific issues related to how much the snowpack has declined in the Washington Cascades. The paper clearly illustrates how the precise magnitude of snowpack decline calculated depends on how the available data is sampled. For example, the early snow records in the Cascades only sampled high elevation stations, which have been relatively insensitive to long-term warming. Thus, trends calculated from only the longest records show smaller snowpack declines than trends calculated from shorter records but more stations. The paper also illustrates how a hydrologic model (VIC) and observations represent snow at fundamen-

tally different scales. While this makes it difficult to directly compare model simulations and observations, the trends represented by the two are generally comparable, and the model can help fill spatial gaps to make estimates at times and places when measurements are not available. Thus, models and observations show that while individual sites and locations exhibit much variability in response to climate change, temperature and precipitation alone account well for regional average snowpack changes.

The paper presents a lot of details related to conclusions already reached in the published literature (e.g., Mote et al. 2005 and Hamlet et al. 2005). The motivation for the paper under review (specifically, why it is needed in addition to the already-published work) should be more clearly presented in the paper. Some readers may be aware of the recent public debate about snowpack changes in the Cascades (http://seattletimes.nwsource.com/html/localnews/2003618979_warming15m.html or http://www.realclimate.org/index.php/archives/2007/03/pnw-snowpack/), but some readers may not. The paper under review is clearly a scientific analysis in response to this debate. While I appreciate the authors' dedication to remaining neutral and impartial throughout the paper, I think that a brief mention in the Introduction about the confusion arising from different methods of analyzing long-term trends would help readers understand the motivation for writing this paper.

Overall, the paper is well-written and should be published with minor revisions. The statements and conclusions are well-supported by the evidence. The paper sometimes feels repetitive and seems to be stating the obvious (Section 5) and thus, would benefit from being shortened and including fewer figures. Specific comments follow.

Specific Comments:

1) I would make the title more specific, such as "How much has spring snowpack declined in the Washington Cascades in past 30 to 80 years?" I think the idea that it has declined isn't debated, and this paper goes much beyond that.

2) The abstract is very well written.

HESSD

4, S680–S685, 2007

3) P 2075, lines 4-7: Statement that the solar radiation feedback is considerably weaker in the winter depends on where one is looking. For example, in the Sierra Nevada, there is significant solar radiation feedback in the winter, whereas in the Washington Cascades so much further north, there should not be. The lack of large-scale declines in Dec and Jan are probably more due to these months not having many areas with temperatures in the crucial 0 to 5 C isotherm range, whereas during the spring, large areas lie within this crucial temperature range.

4) P 2075, line 13-4: were there long-term declines at 75% of the sites over both the “since 1950” and the “since 1960” periods, or did the number of sites showing declines change between these time periods? This is unclear here.

5) P 2075, line 18, specify that Bales et al. (2006) used the VIC forcing dataset to make these calculations.

6) P 2076, lines 4-6, Snow courses were also selected to have above average snow accumulation in order to maximize their forecasting potential for summer streamflow. Might want to add citations here: Farnes, P. E., 1967: Criteria for determining mountain snow pillow sites. Proc., 35th Western Snow Conference Proceedings, Boise, ID, 59-62.

7) P 2078, lines 11-12: Should state how this is different from what was done in Mote et al. 2005 and why a more in depth focus is warranted.

8) P 2078, lines 17-18: Look at and potentially cite Molotch and Bales 2005 here: Molotch, N. P., and R. C. Bales (2005), Scaling snow observations from the point to the grid element: Implications for observation network design, Water Resour. Res., 41, W11421, doi:10.1029/2005WR004229.

9) P 2079, lines 2-3: I would expect the mean values between the VIC grid cell and the snow course to be different, but I would expect the trends to be similar. Can you elaborate on why the trends are likely to be different.

HESSD
4, S680–S685, 2007

10) P 2080, line 5-12: Specify that in the Cascades the snow is wet and dense. Therefore, the effects of blowing snow and melt delays due to snowpack ripening are much less than in continental snowpacks. (The latter makes Cascades snow more sensitive to warming temperatures.)

11) P 2081, lines 17-18: Are these numbers new, or is this the same as in the prior papers?

12) P 2082-2086: Much of section 5 seems like it should be intuitive and obvious - I would advise shortening this entire section.

13) P 2085, line 9: Where did the factor 1.5 come from? Is this based on PRISM, or was this computed in calibration of the hydrologic model? Do you have a reference for this?

14) P 2085, lines 22-23: Good point!

15) P 2087, line 1: Look at the Molotch and Bales paper (cited above) in relation to this.

16) P 2087, lines 4-7: Good point.

17) P 2088, lines 1-7: Good summary.

18) P 2089, lines 1-4: What justification is there for hypothesis 2? This does not seem intuitive.

19) P 2091: Shorten the discussion overall.

20) P 2098, Figure 2: The “o1615m v -16m” is confusing - can you find a clearer way (maybe without the o and v) to clarify what these elevations mean.

21) P 2102, Figure 6: This figure does not seem necessary and could be deleted.

22) P 2104, Figure 8: This figure does not seem necessary and could be deleted.

23) P 2107, Figure 11: This figure does not seem necessary and could be deleted. If

HESSD
4, S680–S685, 2007

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the authors do feel the need to keep this figure, the caption need to better identify what the graph shows.

24) P 2108, Figure 12: This is a good graph, and I recommend keeping it. Label the subplots (a) and (b), and clarify how the second panel plot was determined (using VIC?). Also, the caption reads “thousands of feet” when the axes seem to be in meters.

25) P 2109, Figure 13: You state that VIC values have been scaled to the mean observed SWE - how were the values scales? What was the offset or multiplicative factor?

Technical Corrections (typing errors etc.):

1) p 2074, line 22: insert comma after year: “every year, and”

2) p 2074, line 26: insert comma: “(Lemke et al., 2007), and”

3) p 2078, line 21, insert “may occur” - “storm or melt event may occur between the observation”

4) p 2078, line 26: change “rain gage efforts” to “rain gage. Efforts”

5) p 2079, line 27: change “2005, which see for details” to “2005, see for details”

6) p 2082, lines 9=10: awkward wording

7) p 2083, line 25: How is figure 7 different form the work already published? If not different, perhaps it does not need to be included here.

8) P 2088, lines 13-14: “and that the period since 1997” - fragment - please clarify this sentence.

9) P 2088-2089: Weird capitalization patterns in the listing of hypotheses and corollaries. Please be consistent.

10) P 2089, line 17: change “trend” to “trends”

11) P 2090, line 10: add comma: “NPI regressed out, the trend changes”

HESSD

4, S680–S685, 2007

Interactive Comment

12) P 2099, Figure 3: Label the panels “(a)” and “(b)”

13) P 2100, Figure 4: Label the panels “(a)” and “(b)” and specify what the little arrow is in the top panel. I would also switch the order of these – put the bottom one on the top and make it be (a) because the graph is easier to make sense of in that order.

14) P 2103, Figure 7: Label the panels of the graph (a,b,c) and label the solid line in the top graph “corr(SWE,P)” to be similar to the label “-corr(SWE,T)”. I don’t think the bottom two panels are necessary. In the caption, identify the lines by “solid” and “dashed” instead of “top” and “bottom” for better clarity.

15) P 2106, Figure 10, change “Storage efficiency”, to “Storage efficiency,”

16) P 2106, Figure 10, instead of “top” and “bottom,” identify as “circles” or “o’s” and “v’s”

17) P 2110, Figure 14: Label the panels “(a)” and “(b)” and keep the vertical axis the same in both plots - the difference between the trends is hard to see with the different vertical scales.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 2073, 2007.