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Hydrology and Earth System Sciences Discussions

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Interactive Comment

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

P. Mote et al.

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We thank the anonymous reviewers and especially Dr. Mark Stoelinga for their careful reviews. We have taken most of their suggestions. In particular, we eliminated and combined some figures to reduce the total number of figures from fourteen to ten and shortened section 5. In several cases where a reviewer suggested revising a sentence or adding more text to clarify, we removed the sentence, judging it superfluous (e.g., Reviewer 1 suggestion #10)

Addressing specific points

========= Reviewer 1 ================

We added some discussion in the introduction about the recent public controversy on this topic, which this paper (and the unique format of HESS-D) addresses.

#1. The idea that it has declined is, in fact, debated. See above. And we felt that adding text to the title detracts from the clarity, because the starting point is only one of several issues that complicate the calculation of declines.

#6. deleted sentence - we didn't want to cite old grey literature and the point was tangential.

#9. this was explicitly stated in reasons 4&5 of the passage in question.

#10. We removed the sentence because it was superfluous.

#11. They are new.

#12. OK. sections 4 and 5 have been combined, reorganized, and trimmed.

#13. Provenance of the factor 1.5 explained in text now.

#14, 16, 17. thank you

#15. ref added

#18. This hypothesis was advanced by one of the discussants in the February dialogue. Some climate models suggest large increases in winter precipitation associated with global warming, though not enough to produce more snowpack in the Cascades.

#19. The discussion on pages 2088-2091 has been trimmed somewhat.

#20. Now explained in caption.

#21-23. We have reduced the overall number of figures, though not exactly according the directions of any single reviewer.

#24. Combined with Fig 13 and panels labeled. Derivation of hypsometric curve is now explained in caption.

#25. Caption now states that VIC scaled to mean observed SWE (i.e., VIC(t)=mean(VIC)*obs(t)/mean(obs))

Technical corrections - accepted, except as noted here:

for comments #7 and #14, none of this has been shown before. We replaced the top panel is replaced by a verbal description. The scatterplot aids in interpreting what used to be Fig. 3 and is now Fig. 7b and d. comment #13, after combining with Fig. 5 we thought this order made more sense. Comment #17 we redid the plot as suggested but used the area-weighted curve from Fig. 5, so the numbers changed slightly.

======= Reviewer 2 ===================

Specific comments.

1. The attribution question was implicit in the vertical profile of trends shown in the new Fig. 4, but since it would have required more analysis to demonstrate this point satisfactorily, we have dropped it.

2. Both Reviewer #2 and the editor would like to see more discussion of measurement error and model uncertainty. There is, however, no independent way of evaluating the uncertainty in each of these with available data. The comparison in Fig. 2 and Table 2 is the best we can do. We know that neither approach is perfect, but the close corraboration of both the spatial average in Fig. 5 and the relationship of trends to DJF temperature (Mote et al. 2005) gives us some confidence that, properly aggragated, they are probably both reasonable estimates of the actual trends. The uncertainty in these estimates is not known and cannot be further quantified without another independent source of data. Only one aerial marker was included in our study (Little Meadows and Cloudy Pass had too much missing data), and the correlation with VIC is worse than the median but not in the bottom five.

3. We have added a description of the sub-diurnal implementation of the snow model.

Technical corrections

4. Three facts are listed, so "the facts that" is grammatically correct

5. Reworded.

6. Have added text to clarify. Data are 93.3% complete, and indeed ranking by correlation does not correspond to ranking by distance.

7. Fixed.

8. They are % changes over the period indicated. This was a point of disagreement even among the authors - while there are good scientific reasons to present the rates of change as %/time, the real challenge we face in this paper is satisfying policymakers who cite these results internally and in public, and they want a summary like "15-35% since mid-century".

9. fixed

10. fixed

11. Good suggestion. Done.

======= Reviewer 3 ===================

We have shortened the paper and reduced the number of figures. We took the technical suggestions, except the one for pg 2082 which we felt was already sufficiently clear –

======= Stoelinga ===================================

1) Because in previous works we reported trends since 1950 (a date chosen solely for data completeness) and were accused of cherry-picking, so we wanted to show the effects of starting point on the trend computed. Using proper area-weighting increases the magnitude of the computed decline, so a figure of 35% is entirely reasonable for certain starting years.

2) Additional efforts to account for PDO were shown in the paper by Hamlet et al.

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(2005). And we are baffled why Stoelinga says we have previously used 1950-1995: the shortest period we have published was 1950-1997, owing to the constraints of the VIC simulation at the time.

3) The point about streamflow trends is important and worth examining, but there is no reason to expect a one-to-one correspondence between the declines in April 1 SWE and streamflow: summer streamflow=summer precipitation + snowmelt + groundwater. Had we space to include detailed analysis of observed and simulated water balance, we could address this point more clearly.

For most Cascades rivers, the correlation between March-April-May temperature and June-July-August streamflow is about -0.6 (see http://www.climate.washington.edu/streamflow/sensitivity/). Since, as we show in our paper, the long-term trends are dominated by temperature changes and precipitation introduces nearly-white noise, June-August flows (or June-August flows as a fraction of annual total flow) are clearer indications of the role of temperature.

Another issue with the streamflow trends Dr. Stoelinga reports is the choice of starting point. For precipitation, the 1920s and 1930s were the driest period of the instrumental record, whereas the 1997–2004 period was among the wettest periods of the instrumental record. It is therefore remarkable that April-August streamflow in these rivers decreased at all. We performed an analysis of June-August streamflow for a total of six rivers on both sides of the Cascades, and found an overall decline of 27% for 1944-2005.

4, 5) We thank Dr. Stoelinga for the careful scrutiny of the figures: he caught two errors and raised a good point about streamflow. First, he is correct that one of the percentages reported in figure 3 was indeed incorrect, owing to a coding error, and changing the way rounding is handled in the software changed some of the other percentages slightly. Second, the area-elevation curve in Fig 12b was also incorrect - a single line of code was two lines out of place, so the area-weighting was effectively

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applied twice. The correct median elevation is 1243m, so his estimate from the graphs was remarkably good. This did not affect the results presented in Figure 13, where the area-weighting was applied only once.

6) The domain is now specified in the text, and differs from Stoelinga's in such a way that a 65m difference in mean elevation is not surprising.

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

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