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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.

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Editor comments

The paper can clearly be published in HESS after consideration of a few points. A point-by-point reply to the reviewers and the other discussion contributions would be appreciated, but at least the following points should be addressed in the revised version.

First, the motivation of the paper and the added value compared to earlier publications need to be specified. A reference to the apparently ongoing debate on the issue would be very appropriate indeed, and its motivation can well be linked to this (see also rev

1). Also, the introduction could explain the overall set-up of the paper a bit better by introducing the major question of what is the likely cause of the observed already in the first section of the paper, and the goal of the VIC simulations to interpolate/correct for the incomplete data coverage in time and space. The goal of a manuscript to list 'challenges' (2075, l22) is a bit unclear.

Second, in spite of the desire to be complete on the issue, I share the view of reviewers 1 and 3 that some reduction would be beneficial. There is an inconsistency in the way these reviewers propose this reduction, and I am more in favour of the comments by rev 1 than of 3 (who suggests to remove figs 5, 7 and 10, which I find quite useful). In particular I share the opinion to remove or reduce figures 8 and 11 and the discussion thereof. Figure 4 is a bit complicated and could probably also be removed.

Some other figures could use some better explanation by adding a legend with the symbol lines (apart from just describing it in the caption), make the scale similar (3a b), add labels to the panels (see rev. 1), or increase the font size of statistics (9, 13). The VIC line in 6b is reported to be copied from 3, but there is clearly an offset in the values (in fig 3 the peak is at approx. 110 cm, in 6b it exceeds 160).

I share rev. 2's opinion that little attention is paid to observation and modelling error. Probably you've addressed this in earlier publications, but the significance of the trends found definitely is easily criticized as an artefact of highly questional observation/model properties if not appropriately addressed in this paper.

About the title of the paper: it should not be casted in a question, merely just describe the contents: 'Snowfall trends in the Washington Cascades'

Finally, the 2nd hypothesis maybe emerged from the debate, but is not clear to me either, like rev 1. Either explain better how the argumentation reads, or remove the hypothesis.

The comments of M.Stoelinga seem to emerge from the earlier debate as well, and

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I would appreciate your reply to some of his points. His point 1 should be replied by expressing the changes in

In addition to the reviewers comments I have some specific remarks

- The presentation of ranked years (e.g. 2082 l.21) is lengthy and can be removed
- the use of symbols S and SWE for the regression model and true observations (2084) is a bit confusing
- The discussion in 2084 l.27 and further is also lengthy and can be removed, as is 2085 l.13-19.
- I thought the problem discussed in 2087 l.12-15 was solved by the VIC weighing, but apparently I missed something?
- The third argument mentioned at 2090 l.12 can be counterargued by the notion that every statistical ensemble contains noisy elements, that per se do not prove the representativity of the sample.
- The reference to Zhang et al (Detection of human influence on twentieth-century precipitation trends; Nature 2007) could be added to refer to the detection of 'regional' attribution of precipitation changes

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