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Discussions

Interactive comment on "The sensitivity of modeled snow accumulation and melt to precipitation phase methods across a climatic gradient" *by* Keith S. Jennings and Noah P. Molotch

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This well written paper analyzes a key question for snow hydrology, which is the impact of precipitation phase algorithms on snow water equivalent (SWE) modelling in different climates. The paper studies four more or less different methods of precipitation phase computation (each with different portioning parameters) and assesses the impact of the methods on different snow accumulation and melt metrics, obtained with the model SNOWPACK at five different locations in the US. The methods are based on temperature thresholds and on bilinear regression. The analysis gives an answer to

the general question of how important it is to carefully choose the precipitation phase method for different climates.

A drawback of the study is that it is purely simulation-based and does not use observed SWE data to push the study further. In fact, with the observed SWE data and SNOWPACK, it might have been possible to estimate actual daily or hourly snow accumulation amounts and compute best parameter values for the studied precipitation phase methods at the selected stations. This way, it would have been possible to judge how critical deviations from these best estimates would be at the different sites. In other words, this would allow to answer questions like "how critical is it to have a 1°C error in the air temperature threshold at a warm site as opposed to a cold site"? "How important is it to use dew point or wetbulb temperature at warm sites versus at cold sites?"

This having said, the study is nevertheless worth publishing and interesting for the readers of HESS. Below some general and detail comments.

General comments

I would not say that a study tests 12 different methods if only a few methods are tested with different parameter values; this oversells the study in the abstract. I would in fact say that the study tested four different methods: based on air temperature (with different 50% thresholds and different transition ranges, some of the ranges being 0), based on dew point and wet bulb temperature and based on binary regression.

A key analysis of the paper is the one of "Climatic controls on precipitation phase method sensitivity".(section 4.4); it analyzes how the results vary with air temperature. Air temperature sensitivity is, however, built into each method in a different way. In the case of daily snowfall fraction: the fact that it shows the highest standard deviation for air temperatures between 0 and 4 C simply expresses the fact that several methods use thresholds in this range. The result would look different if the thresholds were between -2 and 2 C. This should be better reflected in the discussion of of the results.

In general, the conclusion that precipitation falling in the range 0 – 4 C explains much of the variation observed across the methods comes from the choice of the threshold values. Without actual comparison to observed data, the results are hard to generalize. Why is there no comparison to actual SWE-derived thresholds?

Furthermore, when reading the results section where actual SWE curves are presented for the first time, it is a little disappointing to see that all studied sites show a typical seasonal snow cover with significant accumulation over many weeks. The most sensitive sites would typically be the ones where the snow cover might build up several times during the winter.

Detailed comments

- The abstract does not mentioned what types of methods have been tested nor whether they have been compared to reference data or which method performed best
- Introduction: it would have been interesting to shortly discuss how /where precipitation phase is actually observed; as far as I am aware of, actual precipitation phase observations are crucially missing at most places.
- Introduction: the manuscript focuses its discussion on snow-hydrological models. How do meteorological forecast models determine the limit (elevation) of snow fall? Completing the literature review with this respect would complete the picture
- P. 2: "In general, warmer sites are more sensitive to precipitation phase method selection in terms of annual snowfall fraction variability, though it is less certain how this variability translates into divergences in simulated snow accumulation and melt. " This statement is given without reference. In what is the apparently previously known result different from your own findings?

- Study sites: It might be useful to know the variability of the daily air temperature around the seasonal mean (ie. the anomalies, obtained e.g. by fitting a sine curve to air temperature as in the work of Woods, 2009. It is this variability that will tell something about the probability of switching from accumulation to melting conditions and about a site sensivitiy to the chosen temperature threshold.
- Methods: it is not clear at this stage that all stations always show a seasonal snow cover (significant accumulation over several weeks), which is important for the concept of "peak SWE" to be meaningful
- the current definition of snowmelt rate is probably over sensitive to spurious shifts from a primary to a secondary SWE peak, which could reduce the melt duration sensibly; how could this measure be made more robust? Similar comment applies to the peak SWE date that is discussed in the results section. Is this measure useful? Minor modifications of SWE accumulation can switch the SWE peak date between a spurious primary or secondary peak (Figure 4 suggest that stations with two peaks might exist, but I might be mistaken).
- P. 14 "meaning a significant proportion of water was simulated to have run off using one precipitation phase method versus being stored in the snowpack". This not well formulated since rainfall does not necessarily run off. It can infiltrate and recharge the groundwater.
- Section 4.4: Here, standard deviations are calculated across the results of all 12 computation methods. Standard deviation does not seem to be a good measure to quantify the variability of values that do not come from an actual sample of a given process but of values pertaining to different methods. (Besides: how are standard deviations obtained? First per method and then averaged over all methods?)

Woods, R.A., 2009. Analytical model of seasonal climate impacts on snow hydrology: Continuous snowpacks. Advances in Water Resources, 32(10): 1465-1481. DOI:10.1016/j.advwatres.2009.06.011

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