

Review comments – “Improved snow simulations through improved representation of vegetation conditions ...” by Gupta et al.

This paper was sent to me after a first round of reviews, to which the authors have responded. It's unfortunate that I didn't see the paper earlier but so be it. The “bottom line” from Reviewer 1 was “[the paper] currently does not sufficiently highlight its main contributions, and substantial revisions are required”, and from Reviewer 2 it was “I am not sure whether the study delivers on what was promised in the title”. Unfortunately, there were then pages and pages of detailed comments from each reviewer, which the authors have dutifully attempted to address. However, in my view the paper still suffers from Reviewer 2's assessment -- it doesn't offer insights into the performance of the Noah-MP snow model with respect to snow-vegetation interactions that can be deconvolved from model forcings. Frankly I think the authors need to go back to the drawing board. The stated aim certainly merits a good paper -- the interaction of snow and vegetation (primarily forests) is key to understanding the hydrologic response of watersheds in the Western U.S. over time scales from hours to days (e.g., floods) to seasons to decades (e.g., climate change). In its present form though, the paper doesn't address that need.

One basic problem with the paper is that it convolves the effects of uncertainty in the model forcings at the large scale of the Sacramento-San Joaquin River basins with the interaction of snow and vegetation. There is no relevant data at that scale that get at snow-vegetation interactions, so they quickly are into evaluating the quality of the different data sets used for model forcing and assessment. In my view, that should be the topic of a separate paper. I will say that our own limited experience with AORC was discouraging – my impression is that the developers were overly focused on high spatial resolution, and didn't adequately recognize that the actual data sources they are forced to use (and this is critical for precipitation) are gauges with average separation 10-20 km – not close to the 1 km spatial resolution of the data set. NLDAS-2 has a more realistic spatial resolution (1/8 degree), but it has a host of its own problems, including a major shift in the stations that it ingests in the early 2000s when it became semi-operational. In my view, PRISM is probably better than the other two, primarily because it makes direct use of high elevation stations (primarily SNOTEL). Exactly what AORC does in terms of orographic adjustments to precipitation and temperature, I'm not sure. Again, while these issues need to be addressed, they're really the topic of a separate paper. But as it is, I don't take away a message as to how well or poorly Noah-MP is handling snow-vegetation interactions, as there's no attempt to segregate model and data error.

Given the absence of large-scale observations, the obvious alternative is to use what's available at the process scale. The best alternative in my view would be to run the model at

SnoTel sites, using the precipitation and temperature observed at the sites. This takes away most of the problem of convolving forcing and model errors. I should note that this strategy is not without challenges, as SnoTel sites are generally in clearings, although some of the clearings may be small enough to reduce the magnitude of this problem. The other issue though is that the authors probably were poorly advised in their choice of watersheds, as there are relatively few SnoTel sites in the Sacramento-San Joaquin watersheds. California DWR operates its own snow pillow network, which is somewhat similar to SnoTel, but I don't believe most of those sites observe precipitation. That said, I am surprised that the paper has no reference to the work of John Pomeroy or Pascal Storck, both of whom collected data on snow interception in and under forest canopies (basically by weighing the trees). Those data are reported in a 2009 WRR paper by Kostas Andreadis (also not referenced). By now, there likely are other data sets that could be used to understand performance of the Noah-MP snow-vegetation module.

The other major problem I have with the paper has to do with their results from the Noah-MP dynamic vegetation model. They say little about the model itself, but it appears that they're just reporting results with that switch on or off in the model. In any case, what they report makes no sense to me (their table 3 in particular, and the big differences in SWE for dynamic vegetation on and off). I Googled "What fraction of high elevation forests in California are conifers?" What I got back (sourced from USFS) was "At higher altitudes and montane belts (like the Sierra Nevada, Cascades, and Klamath ranges), vegetation is highly dominated by needle-leaved, cone-bearing trees." That matches with my observation that deciduous forests (e.g., oak) are almost entirely at lower elevations, where snow is rare. I don't know what's in the Noah-MP parameterization for snow, but I suspect that it's based on LAI, which interacts with snow in two major ways. The first is snow interception, which is strongly related to LAI. The second is attenuation through the canopy of wind and solar radiation that reach surface snowpacks; both are strongly related to LAI. So my guess is, the dynamic vegetation model somehow is changing LAI, either seasonally or over longer time scales. But why? Seasonally, conifer LAI is essentially constant. Over longer time scales, yes trees do grow and die, and there's fire. They don't say anything about this though (and in particular how well the model matches observed variations in vegetation properties over their domain). I strongly doubt that whatever the model is doing matches even remotely what vegetation observations show. My overall recommendation that the paper be rejected could be based on the dynamic vegetation results alone, notwithstanding the other critical problems I've identified above.

I do want to re-emphasize that the topic of the paper is a worthy one. Noah-MP is the hydrologic core of the National Water Model, which increasingly is finding its way into both research and operations, at least in the U.S. In the Western U.S. in particular, watershed

hydrology is mostly strongly linked to snow, and in turn to vegetation. This has long been recognized and was the basis for a lot of work years ago at USFS experimental watersheds, and later pre-2000 (especially in the Pacific Northwest) motivated by concerns about effects of logging on flooding. The authors appear to be unfamiliar with that literature, and I'd urge them to take some time to bring themselves up to speed. Then, I think they need to rethink their experimental design, so that the core of the paper is in fact linked to the title of the paper.