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The Cryosphere Discussions

Interactive comment on "Using machine learning for real-time estimates of snow water equivalent in the watersheds of Afghanistan" *by* Edward H. Bair et al.

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Overall, this is a very well written paper, and a solid contribution to the field. I would recommend it for publication following revisions.

There are two major issues that need strengthening before publishing.

1. The accuracy of the reconstruction method, which is treated as "truth" for the region's snowpack, needs to be more carefully assessed. In particular, all of the citations demonstrating the method works refer to the Sierra Nevada. Given what is known about the sources of weakness in the reconstruction method (e.g., cloud cover on the actual

date of disappearance, precipitation occurring after the date of peak SWE, errors in the atmospheric forcing terms), which issues are likely to be more or less problematic in Afghanistan compared to California and how are these errors likely to propagate to accuracy of the reconstructed SWE? (I realize that some issues may be hard to pin down, but the authors should be able to make some statements about the degree to which weather models are poorer in this region and the degree to which this will impact the reconstruction.) I would also like to see more analysis regarding potential issues with the daily-reset cold content of the snowpack in particularly cold regions. This could be examined by running a model in the traditional forward sense (with meteorological data from this region, fully accounting for multi-day accumulated "cold content") and comparing it to a model run in reconstruction mode.

2. The introduction nicely makes the connection that Afghanistan's water supply is susceptible to year-to-year variations in snowfall and that some way of making seasonal predictions of the snow available for runoff is very important. This paper demonstrates a way of doing this. However, the paper needs to clearly make the connection of how the errors inherent in the proposed method (order of 20%) compare to the errors in the current system. For example, what is the interannual variability in snowpack? How wrong would a water manager be if he/she just presumed mean runoff from snow? What methods are currently used for such a forecast, and what are their errors? (Are there any citations on this?) I'm guessing that 20% error is better than the current situation, but the actual numbers (or a best guess to the actual numbers) should be presented in the discussion and conclusions.

Some more minor issues include:

1. Given that only fSCA and mean reconstructed SWE had predictive power, why not test a simpler model with just those terms? How does that compare with the full set? Also, given the conclusion that only those variables mattered, why does the conclusion say that an operational system would need to ingest Passive Microwave data? Does that make a difference that warrants the effort?

2. The discussion should also address the implications of combined error from the forward prediction (which was trained on reconstructed SWE) and the errors in the reconstructed SWE (which the one point check suggests may be biased low 20%). How large might these combined errors be, and combined, are the expected errors still better than a baseline assumption of an average year?

Note, I am also providing an annotated manuscript to the editorial office and the authors, which marks in the text where the issues summarized here arise.

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