

Interactive comment on “Correcting basin-scale snowfall in a mountainous basin using a distributed snowmelt model and remote sensing data” by M. Shrestha et al.

Anonymous Referee #2

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This paper described a novel technique for model calibration that makes use of both remote sensing information and discharge data. This is an important step in the integration of remote sensing methods for water resource management, especially in watersheds without instrumentation, such as the one shown here.

This review came up with the following minor comments/revisions:

1. Page 6, Line 1+: “This estimates the snowfall that would have likely occurred that minimizes the difference between observed and simulated discharge at the basin outlet and the difference between MODIS-derived and simulated snow cover pixels in the

C6292

basin, given the physics of the model.” This is the nugget of the entire paper and a description of what is being done. I think this could be stated more clearly.

2. Page 6, Line 4+: “. . .Yagisawa Basin. . .where water use in spring is completely dependent on snowmelt discharge.” It would be better to here some more quantitative descriptions of the water supply by season, role of groundwater, versus surface storage, etc. If there is any groundwater contributing to discharge, that might undermine some of the methodology.

3. Page 8: Line 7+: “The basin partly supplies drinking water to 29 million people in the Tokyo metropolitan area.” This statement would be more powerful (and convincing as a study motivation) with a number attached such as “the basin typically supplies X% of the Tokyo metropolitan water supply.

4. Page 8: Section 2.2 Study area. Why was this small basin chosen over all of the others? There appear to be no precipitation observations in the basin at all to ground validate the MODIS data, for example. Potential biases in MODIS are discussed, but ground observations would have strengthened the conclusions of the paper.

5. Page 10: Section 2.2.4 MODIS snow cover area. The MODIS is available daily. Why are the authors using the 8 day product for a model that runs hourly?

6. Page 12: Line 1-2: “attained by running the model several times, until hydrologic equilibrium was reached.” Please be more specific. The model was run X number of times until equilibrium was reached as defined by Y and Z.

7. Page 14: Section 3: Results and discussion. Why was 2002-2003 chosen as the calibration period?

8. Figure 7: the Watershed boundaries and topography seem inconsistent.

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C6293