

Geosci. Model Dev. Discuss., referee comment RC1  
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## Comment on gmd-2021-407

Anonymous Referee #1

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Referee comment on "SnowClim v1.0: high-resolution snow model and data for the western United States" by Abby C. Lute et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-407-RC1>, 2022

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Lute et al. present simulations of snow over a large domain for reasonably long periods and at high spatial resolution. To achieve this, they have made some quite severe approximations to limit computational expense. Neglecting forest effects seems like a limitation for water resource applications in the western US. Simple canopy models, and snow models with a few layers to allow a more physical representation, need not be very expensive. Has code optimization to offset the cost of improved model physics been considered? MATLAB is convenient for parallel computation, but otherwise might not be the best language for this. Having said that, the authors have adopted a particular development strategy and reporting the results is worthwhile.

I was able to access the code and run the MATLAB Online example easily (a quickstart guide might have saved me a couple of minutes).

I suggest some corrections and clarifications, identified by line number:

24

There are too many to list, but the abstract should give some idea of what kind of "snow metrics" are offered.

52

Should note that this is earlier but slower snowmelt

106

Equation (1) is not the surface energy balance because  $G$  is not a surface flux. I would describe it as the energy balance of the snowpack.

127

Note that separate visible and near-infrared radiation fluxes are not supplied as inputs; the albedo is simply averaged between these bands. The illumination angle dependence should only be applied to the direct-beam component of incoming radiation.

133

Equation (3) is incorrect. If the emissivity is not 1, the upward longwave radiation includes a reflected fraction of the downward radiation (Kirchoff's law).

179

Constant G is not a very common (or realistic) feature of modern models. Etchevers et al. (2002) included an experiment with fixed G as a sensitivity study, but did not report the results.

189

Iterative solution of the surface energy balance to find snow surface temperature is not the only possibility. Best et al. (2011, <https://gmd.copernicus.org/articles/4/677/2011/>), for example, linearizes the surface energy balance equation to find a surface temperature solution without the expense of iteration. In either an iterative or linearised solution, the aim is to find a surface temperature that is consistent with the surface energy balance. I don't see how this can be achieved with the pragmatic but ad hoc approach in SnowClim.

281

How were the 210 m and 1050 m resolutions selected? How many points are there in this domain?

330

SNOTEL sites may not be representative of larger areas. Could this calibration skew the model performance?

<https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2005WR004229>

362

Any ideas why model performance would improve from 12 to 24 hour timesteps?

369

There is no evaluation of the large-scale snow simulations for the western US. MODIS snow cover extent products would have a convenient resolution for this.

Figure 2

There are a lot of sites to balance in the calibration, but errors up to +/- 50 days in duration and +/- 50% in maximum SWE even after calibration seem large for practical applications. Other datasets used to demonstrate poorer agreement in the discussion were not calibrated to SNOTEL sites.

Table 1

Given temperature, pressure and one of dewpoint temperature, relative humidity and specific humidity, the other two can be calculated – they are not all required forcing data.

Table 2

As it is abbreviated as Q, I guess that the WRF mixing ratio output is taken as the SnowClim specific humidity input (they are not the same thing, but will have nearly identical values).

Table 5

The monthly SWE and snow depth are minimum, mean and maximum.