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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Mass balance and hydrological modeling of the Hardangerjøkulen ice cap in south-central Norway" *by* Trude Eidhammer et al.

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Eidhammer and colleagues present a new coupled modelling tool for atmospheric, glaciological and hydrological simulations, where they have integrated the snowpack model Crocus into WRF-Hydro. The authors apply the model to a multi-year, very-high-resolution simulation of Hardangerjøkulen and evaluate its performance with respect to a variety of observations. The integration of Crocus provides an important improvement in the representation of glaciers compared with the Noah-MP land surface model that will increase the utility and reliability of WRF for simulations of glacierized regions. While a small number of previous efforts have been made to improve glacier physics



in WRF, this work includes a novel linkage to detailed hydrological processes and a thorough evaluation over a multi-year time period. The manuscript is well and concisely written, and I recommend its publication in HESS after minor revisions.

Minor comments:

1. The introduction inadequately contextualizes the authors' work with regards to our previous efforts to improve the representation of glacier physics in WRF and their applications. In addition to Collier et al. (2013), there are two more relevant references:

- Collier, E., Maussion, F., Nicholson, L. I., Mölg, T., Immerzeel, W. W., and Bush, A. B. G.: Impact of debris cover on glacier ablation and atmosphere–glacier feedbacks in the Karakoram, The Cryosphere, 9, 1617–1632, https://doi.org/10.5194/tc-9-1617-2015, 2015.

- Aas, K. S., Dunse, T., Collier, E., Schuler, T. V., Berntsen, T. K., Kohler, J., and Luks, B.: The climatic mass balance of Svalbard glaciers: a 10-year simulation with a coupled atmosphere–glacier mass balance model, The Cryosphere, 10, 1089–1104, https://doi.org/10.5194/tc-10-1089-2016, 2016.

2. The authors state that glacier ice in Noah-MP cannot melt several times (Lines 134, 303, 397, 445), however my understanding of this LSM's treatment of glaciers is that the subsurface at glacierized grid points is defined as a fully saturated and initially frozen soil. This "soil ice" can and does melt, sometimes entirely. If my understanding is correct, does this treatment differ in WRF-Hydro, or is drainage of glacier melt not accounted for in the hydrological part of the model?

3. The authors provide relatively few details about the WRF simulations and could consider adding a table with basic information (e.g., grid dimensions, timesteps, physics options, any special settings) to increase the reproducibility of their study. On a related note, was WRF-Hydro/Glacier run with or without a PBL scheme?

4. Line 172: Could the authors comment on the impact of using a reanalysis with

∼80-km grid spacing to directly force the outer WRF domain with 3-km grid spacing?

5. Line 188: Why was the model evaluation performed only for the 1-km domain? It looks like the 100-m domain may contain at least the Finse AWS. If so, I suggest the authors also provide a brief evaluation of near-surface variables from this domain, since these data directly force the glaciological and hydrological components.

6. Line 219: The manuscript has quite a few figures. I think the authors could remove Figure 6 and provide the R2 and mean bias in the text. Although simulated wind direction is evaluated, biases and their implications for the results are not discussed elsewhere, so Figure 7 may also be unnecessary.

7. Section 3.1: I suggest moving the model evaluation to the results section. In addition, please describe issues with the measurements and missing data (e.g., Lines 307-312, missing data at Finse visible in Figure 5) in the methods.

8. Line 247: Could the authors provide the dates they used for calculating climatic mass balance in the text or a table? How do the results compare when using the same dates as the observations?

9. Line 293: Could the authors discuss why the winter balance simulated by Noah-MP has, in general, a smaller bias at higher elevations?

10. Line 352: How were these two locations selected for comparison with MODIS?

11. Line 387: What do the authors mean by "lack of groundwater in these specific WRF-Hydro/Glacier simulations"?

12. Line 401-403: Why was Crocus not used to simulate the 14.7% glacierized area in Finseelvi?

13. Line 413: Where can the reader see that the streamflow significantly diverged?

14. Line 422: Please elaborate on model calibration in the methods section.



Technical comments:

Line 50: Please add "e.g.," to the list of citations.

Line 212 "time period"

Line 213 "do not"

Line 216: "were captured"

Line 236: Remove "surface" or change to "glacier surface mass balance"

Line 238: What does "(nve.no/hydrologi/bre)" mean?

Line 285: Please indicate which locations were used for measuring the summer mass balance.

Line 290: "redistribution of snow"?

Line 309: "stakes"

Lines 323 to 325: I suggest removing "slightly" since differences reach 20+%.

Figure 1: Please add a spatial scale.

Figure 2, bottom panel: It would be helpful to add the location of Finse, so that it's clearer where the station is relative to the study glacier.

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