Response to Interactive comment By Emily Collier

First, we want to thank Emily Collier for the kind review of our manuscript. The comments and suggestions have helped improve our paper. We have tried as best we can to respond to the comments and we have (or will) follow most of the suggestions. The original reviews are in black, and our responses are in blue.

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 ap- plications. 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.

Thank you for advising us on these two more papers. We have included them into the paper.

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?

In Noah-MP the glacier is represented with a two-meter layer of ice at the bottom of the column (it is not frozen soil, but 100% water). This layer can melt and refreeze, but it does not run off the grid (so will not contribute to the hydrology). This is true both for regular WRF and in WRF-Hydro.

We now state this in the paper: When snow is accumulated, Noah-MP uses a three-layer snow model to represent the evolution of the snow pack. However, when the seasonal accumulated snow melts off in the summer, the underlying surface for albedo purposes is assumed to be old snow (snow packed glacier), while not allowing for areas of bare ice.

Furthermore, the glacier is also represented in the soil layer with a two-meter layer of ice/water at the bottom of the column. This layer can melt, and refreeze, but this layer does not provide runoff to WRF-Hydro. "

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?

We will add a table with the settings used in the WRF simulations (this was also suggested by the other reviewer). The WRF-Hydro part was run without a PBL scheme since WRF-Hydro was not directly coupled with the atmosphere.

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?

We followed the procedure following Liu et al (2016, Continental-scale convection-permitting modeling of the current and future climate of North America, *Clim. Dyn*), where it is stated that "Tests showed that one-way nesting WRF, at 4-km grid spacing, with the ~75 km reanalysis was an adequate configuration without the need for a coarse grid that intermediates the ERA-Interim data and the WRF domain. "

Basically, the area of interest must be sufficient large enough for mesoscale spinup. Now, our domain of interest (Domain 2) is perhaps slightly closer to the boundary than what is in Liu et al. However, our model results are quite reasonable, thus we do not think that the jump from ~80 km to 3 km introduce a large issue. We will add a comment regarding this in the paper.

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.

We did not run WRF on the 100 m domain. The 1 km output (wind, temperature etc) were interpolated to the 100 m domain (line 185 in original document) so we could force the WRF-Hydro part with wind, temperature from the 1 km domain. The other reviewer had some questions about interpolating from 1km to 100 m, and you can see the response in the reply for reviewer #2

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.

We will consider remove these figures. In response to the other reviewer, we might add another figure, thus these two figures might be worth removing. Thanks for the suggestion.

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.

Agree. We will move the evaluation section to the result section and we will also address the missing data.

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?

For the end of the winter season, we used the end dates when observed. For the end of the summer season, we used the time when the SWE was the lowest. The reason for this is that the observations might be taken when snow has already started to accumulate and observations are adjusted for this. When we use the same date as observations, the comparison is worse.

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

Currently we are do not know the exact reason for the higher bias with Crocus and we added this sentence:

"The reason for the slightly larger bias for Crocus is not known at this point. "

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

We picked one location representative of the ablation area (Northwest location) and one location representative of accumulation area (Top of the glacier location). Furthermore, we used 9 model grid points.

We added this sentence:

To investigate different regions of the glacier (accumulation versus ablation area), we picked two different locations of the glacier.

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

We did not run with the groundwater module on for these simulations. We will clarify this in the manuscript.

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

In hindsight we could have done it. In the initial and boundary files we only added the specific glacier area that is needed to run Crocus over Hardangerjøkulen. We should have added the 14.7% glarier area too.

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.

Figure 15 shows that the streamflow starts to diverge more in mid-July 2018. We will add the data and reference to Figure 15 in the text. We will also note in the methods section that WRF-Hydro is not calibrated.

Technical comments:

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

Done

Line 212 "time period"

Done

Line 213 "do not"

Done

Line 216: "were captured"

Done

Line 236: Remove “surface” or change to “glacier surface mass balance”

Done

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

This should have been https://www.nve.no/hydrologi/bre. However, we should rather use a proper citation here, and will use e.g. Andreassen et al., 2016 instead.

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

We will add the locations in the figure

Line 290: “redistribution of snow”?

Done

Line 309: “stakes”

Done?

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

Done

Figure 1: Please add a spatial scale.

We will do that

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.

We will do that