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Discussions

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

Anonymous Referee #2

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This paper presents the development of a new glacier component of the WRF-Hyrdo simulation platform. It relies on the multi-layer snowpack scheme Crocus that has been implemented in WRF-Hydro. Crocus is used to simulate in WRF-Hydro/Glacier the continuous evolution of snow and ice layers at pre-identified glacier points. In this paper, WRF-Hydro/Glacier is evaluated for the Hardangerjøkulen ice cap in Norway. Model outputs are compared to a large set of observations: (i) measurements of winter, summer and net glacier mass balance, (ii) snow depth measurements from a Ground Penetrating Radar (GPR), (iii) albedo measurements from MODIS and (iv) discharge measurements at two locations. The evaluation revealed improved performances compared to the default version of WRF-Hydro. In particular, the evolution of

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Discussion paper

surface albedo is better represented during the ablation season leading to better estimation of summer mass balance and improved discharge estimations for a partially glacierized catchment.

The new modelling system described in this study presents a large interest for the mountain hydrology community and constitutes an important improvement for WRF-Hydro. My main comments about the study concern (i) the downscaling of meteorological variables in WRF-Hydro, (ii) the evaluation of winter precipitation, (iii) the comparison between simulated and observer glacier mass balance and (iv) the impact of the parameterization that represent the effect of wind-induced snow transport on snowpack properties in Crocus. These questions need to be clarified prior to publication in HESS. They are listed below as general comments followed by more specific and technical comments.

General comments

1. In this study, the authors tested an offline configuration of WRF-Hydro/Glacier at 100-m resolution. This configuration is driven by an atmospheric forcing obtained with the WRF atmospheric model running at 1 km resolution. The downscaling from 1 km to 100 m corresponds to a simple bi-linear interpolation as explained at L 183-185. No correction as a function of the elevation difference between the 1-km grid and the 100-m grid is applied for example for temperature. Effect of slopes, aspects and shadowing on incoming shortwave radiation are not taken into account as well and wind speed is not corrected as a function of local topography. This leads to a "smooth" atmospheric forcing at 100-m resolution and ultimately to snowpack simulations that cannot capture the variability of snow accumulation and melt over the glacier as illustrated on Fig. 10 and 11. This absence of small scale variability is not only explained by non-simulated lateral redistribution processes in Crocus. Therefore, the absence of appropriate meteorological downscaling to sub-kilometre resolution in WRF-Hydro/Glacier should be at least discussed by the authors in terms of impact on simulated glacier mass balance.

2. In the paper, the evaluation of WRF winter precipitation at high-altitude exposed stations is influenced by a large wind-undercatch impacting the measurements of winter precipitation at these stations. This is well illustrated on Fig. 5. Station data were not corrected for wind-undercatch by met.no. As mentioned by the author, this limits the relevance of the comparison between model output and observations. However, winter precipitations are a key component of the glacier winter mass balance and it would be very interesting to propose in the paper an improved evaluation of WRF winter precipitation at high-altitude stations. Since wind speed measurements are available (at least at Finse), it would be very interesting if the authors could propose their own corrections of wind-undercatch using correction functions taken from the WMO SPICE project (https://www.wmo.int/pages/prog/www/IMOP/intercomparisons/SPICE/SPICE.html) and detailed information on the precipitation gauge used at Finse and at the other locations (type of gauge, type of shield, . . .). They could also quantify the uncertainties associated with these corrections. Another solution would be to use the reference precipitation data from the Haukeliseter experimental site who was the Norwegian site that contributed to the WMO SPICE project. In particular, Haukeliseter is equipped with a Double Fence precipitation gauge. High-quality precipitation data were collected at Haukeliseter during winter 2016/2017 (Schirle et al., 2019). They may be also available for the other winters covered by this study. Haukeliseter is located south of the Hardangervidda and east of Roldal and must be located within the WRF 1-km domain based on Fig 3.

3. The authors compared on Fig. 8 the simulated and observed winter, summer and net mass balance. However, they did not clearly explain in the current version of the paper (L 284-289) how the simulated winter and summer balances were computed. For the summer mass balance, did the authors extract the simulation results at the 3 to 5 locations used to compute the observed mass balance and then interpolated the results? The same question raises for the simulated winter mass balance. In addition, on Fig. 8, the elevation dependency of the observed winter mass balance (Fig 8a, d, g, j) and snow depth associated with the winter mass balance (Fig 8b, e, h, k) look

different. However, the authors explained at L 241-243 that the observed winter mass balance was derived from snow depth and the unique snow density measurements over the glacier. As a consequence, we would expect a similar elevation dependency between winter mass balance and snow depth. However, for example, in 2015 the observed snow depth showed a decrease between 1400 and 1600 m which is not present in the observed winter mass balance. This point should be clarified by the authors.

4. In the configuration of Crocus used by the authors in this study, two parameterizations are activated to represent the effect of wind-induced snow transport on snowpack properties: (i) a param. that simulates snow compaction and fragmentation of snow grains for surface layers during blowing snow events and (ii) a param. that computes mass loss due to blowing snow sublimation. At L 164-166, the authors insisted on the importance of these two parameterizations for accurate simulations of glacier mass balance. However, in the rest of the paper, the effects of these two parameterizations are never quantified. For example, how does the compaction parameterization affect the quality of the simulated snow density over the glacier? In addition, the authors suggest that the blowing sublimation parameterization in Crocus explain the spatial variability in snow depth and SWE over the glacier (L 315-320). However, it is not clear that it can explain the local differences in snow depth and SWE. Indeed, the atmospheric forcing driving Crocus in WRH-Hydro/Glacier is rather smooth (cf General comment 1) and may not create a large variability of blowing snow sublimation from one grid cell to another on the 100-m grid. I recommend the authors to compare the results of simulations with and without the blowing sublimation parameterization. One winter would be certainly sufficient.

Specific comments

Abstract L 16: The transition between the first and the second sentence of the abstract is not clear at the moment. It would be interesting to add here a sentence that explains why Crocus is suitable for glacier modelling. The fact that a multi-layer snowpack scheme can be used for direct surface mass balance simulation of glacier is not necessarily clear for a reader who is not familiar with Crocus.

Abstract L 17-18: The two different resolutions for WRH-Hdyro and WRF-Hydro/Glacier are rather confusing. Maybe mention atmospheric simulation on one hand and offline surface simulations on the other hand.

Abstract L 19-20: A sentence is missing in the abstract to explain that the study is carried out over a glacier in southern Norway. It is only mentioned in the title.

P2 L 38-40: I recommend the authors to add one or two citations for this sentence.

P2 L 48-49: the authors should add relevant citations for the application of statistical downscaling for glacier studies. In addition, physically based downscaling methods have also been developed to obtain better distributed meteorological forcing in regions of complex terrain (e.g., Jarosch et al., 2012; Fiddes et al., 2014).

P2 L 54-55: I agree with authors that regional "atmosphere-only" models do not typically include a detailed representation of glacier and their impact on streamflow generation. However, atmospheric forcing from these regional models are often used to drive more detailed models such snowpack models or hydrological models in offline mode for impact studies. I recommend to the authors to mention this approach in the introduction.

P 2 L 59: The authors should better explain this "link" between an atmospheric model and a detailed hydrological model. Indeed, are they talking about offline simulations or online simulations with feedbacks of the surface on the atmosphere dynamics? When used in offline mode as it is the case in this study, WRH-Hydro does not really differ from a more classic hydrological model such as MESH (Pietroniro et al., 2007) that takes its atmospheric forcing from an external system (GCM, RCM, ….), downscale them and use them to drive hydrological simulations.

P3 L 75 and P4 L 103: Crocus has a user-defined value for the maximal number of snow layers. The value of 50 corresponds to the default value in the model. I recommend the authors to clearly explain it. Especially since they then use 40 layers in their implementation of Crocus in WRF-Hydro.

P 3 L 76-78: Note that the default version of Crocus still uses a classic bucket-type approach for liquid water percolation in the snowpack. It does not solve Richards equations and ignore preferential flows contrary to the SNOWPACK model (Wever et al., 2015, 2016). Preliminary developments have been tested in Crocus but are not available in the version of Crocus used in this paper (D'Amboise et al., 2017).

P 3 L 78-79: the reference to the paper by Reveillet et al. (2018) is missing. I also recommend the authors to refer to the work of Gerbaux et al (2005) which used the original version of Crocus.

P4 L 98-99: This sentence is not clear and should be rephrased. Indeed, the "age of snow" is not directly used in the prognostic equations for the time evolution of microstructural variables In Crocus. It is used in the albedo parameterization to compute the decay of albedo in the UV and visible band and will indirectly impact the evolution of microstructural variables. Compaction is not also directly impacting the evolution of microstructural variables.

P4 L 100-101: A description of the distinction between snow and ice albedo is missing in this paragraph. It would be interesting if the authors could use a similar description as Reveillet et al. (2018) (see Sect. 3.1.1 of this paper). The authors should also mention how the aerodynamic roughness are treated for snow and ice surfaces.

P 4 L 101-102: the justification "Due to the prognostic calculation of snow grain properties, . . ." is not clear and should be rephrased.

P4 L 106-108: did the authors consider using Crocus to represent snow over land in NOAH-MP?

P4 L 109-111: Are the authors imposing a constant-temperature of 0 degC for the ground below the glacier or are they imposing a zero-heat flux between the deepest Crocus layer and the ground below as in Gerbaux et al. (2005)?

P5 L 126-137: It would be useful to have this paragraph at the beginning of Section 2 so that the reader could better understand how glaciers are represented in the default version of WRF-Hydro and what are the associated challenges. This would provide a very relevant justification for the use of Crocus over glaciers.

P 5 L 149-150: initializing all the layers with pure ice may influence the accuracy of the snowpack simulations when snow starts to fall on the glacier. Indeed, as mentioned by the authors, it forces the model to merge layers as soon as new snow is added. Instead, the initialization proposed by Revuelto et al. (2018) used 6 initial layers to represent the ice and a maximal number of layers set to 50 so that new snow layers can be created as soon as snow is falling on the glacier without forcing the immediate merging of ice layers underneath. This difference should be mentioned in the paper and its impact briefly discussed.

P 5 L 150: what are the snow grain properties for ice layers used in the paper?

P 6 L 162-167: I recommend the authors to move this paragraph at the end of Section 2.1 when they are mentioning the absence of lateral snow redistribution due to wind in Crocus. This would help the reader to better understand how WRH-Hydro/Glacier accounts for the impact of wind-induced snow transport.

P 6 L 167: it would be interesting to add here a brief description of the configuration of the routing model. Is the routing simulated at the same resolution as Crocus (100 m)? How were derived the routing parameters? It is also important to mention here that no calibration was applied to the routing model.

P 6 L 170: a table summarizing information about the simulation domains (size, number of grid points, ...) would be useful. This table could also include information on the 100-m simulation domain.

P6 L 171: what is the height above the ground of the lowest atmospheric prognostic level?

P 7 L 183: how is computed the rain/snow partitioning in WRF-Hydro/Glacier?

P7 L 188: which method it used to obtain the simulated values at the location of the AWS from the output of WRF 1 km? Nearest-neighbor interpolation, bilinear interpolation? Did they author consider the elevation difference between the simulated station elevation and the actual station elevation when selecting the stations used for model evaluation?

P8 L 216-217: At this stage of the paper, it is not clear that the wind direction and the wind speed are well simulated by WRF. This information is mainly confirmed by Figure 7 which is presented later.

P 9 L 239: at how many locations is measured the mass balance of the glaciers?

P 9 L 247-249: it is not clear why the authors did not use the actual dates when the observations were gathered to compute the simulated mass balance, contrary to Vionnet et al. (2019). Could they add a justification?

P 9 L 256-257: it is not clear to me how a device transported at 15-20 km/h (approx. 4.1-5.5 m/s) with a sampling interval of 1 s can generate a 1-m spacing between datapoints. Note that I am not familiar with GPR postprocessing, so ignore my comment if the 1-m spacing is obtained from data postprocessing. P 10 L 273: snow albedo in the version of Crocus used in this paper also depends on the snow age. Indeed, the snow age is used to parameterize the influence of light absorbing impurities on snow albedo in the UV and visible range. A more recent version of Crocus explicitly simulates the direct and indirect radiative impacts of light-absorbing impurities in snow (Tuzet et al., 2017). P10 L 277: what is the size of the two river catchments considered in the study?

P 12 L 335-347: Figure 10 shows that the GPR provided an excellent coverage of the glacier for winter 2017. It would be very interesting to use these data to compare the simulated and observed elevation-snow depth relation and to compare the simulated and observed variability of snow depth per elevation bands. This would complement well Fig. 8.

P 12 L 349: the evaluation of the simulated albedo is based on a comparison at two representative points selected over the glacier. This choice should be justified in the method section when the authors are describing the MODIS albedo data. Indeed, at L 273-276 the authors mention that MODIS data are available at 500 m resolution over the glacier. This suggest that the full MODIS albedo dataset would be used for model evaluation.

P 15 L 440: I am not sure that the term "excellent" can be used to qualify the WRF winter precipitation. Indeed, the authors have shown that they cannot be directly evaluated due to wind-undercatch at the high-elevation stations which are the most relevant for this study. Maybe the authors should make here the link between the WRF winter precipitation and the winter mass balance simulated by WRF-Hydro/Glacier.

P 16 L 454-455: the author should mention here that the model at 100-m resolution cannot capture the spatial variability of the snowpack on the glacier due (i) the absence of proper meteorological downscaling and (ii) the non-representation of lateral snow redistribution. It is mentioned later in the conclusion as a perspective, but it should appear in the bullet list containing the major conclusions of the study as well.

P 16 L 467: information on the code availability would be very interesting for the readers. It could potentially serve a basis for Crocus users who want to implement the model in their own land surface model.

Technical comments

Text

P2 L 35: "ablation-season"?

P2 L 38: a parenthesis ")" is missing

P4 L 94: I recommend the authors to use “initially developed” instead of “specifically developed” Indeed, since the 90’s, Crocus has been used in many different applications (https://www.umr-cnrm.fr/spip.php?article268&lang=en).

P4 L 99: use “metamorphism” instead of “metamorphosis”

P 4 L 100: Âń Vionnet Âż instead of “Vionett”.

P5 L 144: It is surprising to have a Section 2.1 without a Section 2.2.

P5 L 147: use kg m^{-3} instead of kg/m3

P 5 L 208: use m s^{-1} instead of m/s

P15 L 427: maybe add “and ice” after “physical properties of snow”

P21 L 598: the reference to Willemet (2008) is not included in the text.

Table

Table 1: what is the signification of the values appearing in bold in the table?

Figure

Figure 7: different scales are used for the observed and simulated wind data on the wind rose. Using the same scale would allow q more direct comparison between model and observations. In addition, could the authors provide simple errors metrics such as bias and RMSE for wind speed at Finse?

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