Response to Interactive comment by Anonymous Referee #2

First, we want to thank the Referee for the review of our manuscript. The excellent comments and suggestions have greatly 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.

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 Nor- way. 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 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 meteorolog- ical variables in WRF-Hydro, (ii) the evaluation of winter precipitation, (iii) the compar- ison between simulated and observer glacier mass balance and (iv) the impact of the parameterization that represent the effect of wind-induced snow transport on snow- pack 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 bilinear interpolation as explained at L 183-185. No correction as a function of the elevation difference between the 1km 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

It is true that we are missing some small-scale variability. However, it should be noted that the area of interest is on a plateau with large open areas (see picture below). Yes, there are some shading, which is mainly prevalent at very low sun angles. We therefore believe that the bilinear interpolation from 1 km to 100 m is not of a too large concern for this specific case. In a rugged terrain, such as in the Alps, the bilinear interpolation might be a larger problem. The slope of the glacier is not that steep either, with and increase with about 100 m per km.

We added this text in the manuscript

We note that we did not account for variability in terrain in the re-gridding process. Thus the atmospheric forcing is still "smooth" as regards to a 100 m grid. However, the region of interest (Hardangerjøkulen and surrounding *terrain) is an open mostly flat area and we therefore believe that for this specific case, disregarding the variation in terrain does not have much impact on mass balance calculations.*

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.

Indeed, Haukeliseter is located in the WRF 1-km domain and it is a great suggestion to use data from this location to evaluate modeled winter precipitation. We have data from 2014 and 2015, and we will look into obtaining the data from 2016/2017 as well. As regards to conducting a wind correction at Finse based on findings from the SPICE project, this is something we will look into. The precipitation gauge at Finse is a single – Alter -shield.

Although the SPICE project has allowed for suggested transfer functions to correct for the under-cathment we note this finding in Smith et al 2020 "Evaluation of the WMO-SPICE transfer functions for adjusting the wind bias in solid precipitation measurements" Hess that they state:

"Although the application of transfer functions is necessary to mitigate wind bias in solid precipitation measurements, especially at windy sites and for unshielded gauges, the inconsistency in the performance metrics among sites suggests that the functions be applied with caution."

Just as Haukeliseter is a windy location, so if Finse. Thus it is worth looking into using published transfer functions from Haukeliseter. Thus we will take the suggestion into consideration while updating the manuscript, but with the notion that transfer functions should be applied with caution.

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.

For the modeled winter and summer balance we take the average balance for each grid point within a certain elevation level (40 m)

We have added this sentence:

The modeled winter (and summer) balance is plotted as averages of all grid points over Rembesdalskåka within intervals of 40 m

Regarding the difference in plottet SWE versus height, the SWE values were taken from the values shared in the reports by Andreassen et al., (2016), (2017), (2019) and Kjøllmoen et al., (2018) (see Figure 1) while the snowdepth was taken from the original data. One of the authors of these reports stated that the data in the reports were an arithmetic mean of height and SWE withing 50 m as a base for subjective smoothed SWE curves. In the manuscript we will be consistent with which data we use for the SWE and snowdepth (if we end up showing both). Note that there are only 6 observation points below 1600 m, so the variations are larger in this region (as seen in the snowdepth curve in the manuscript.

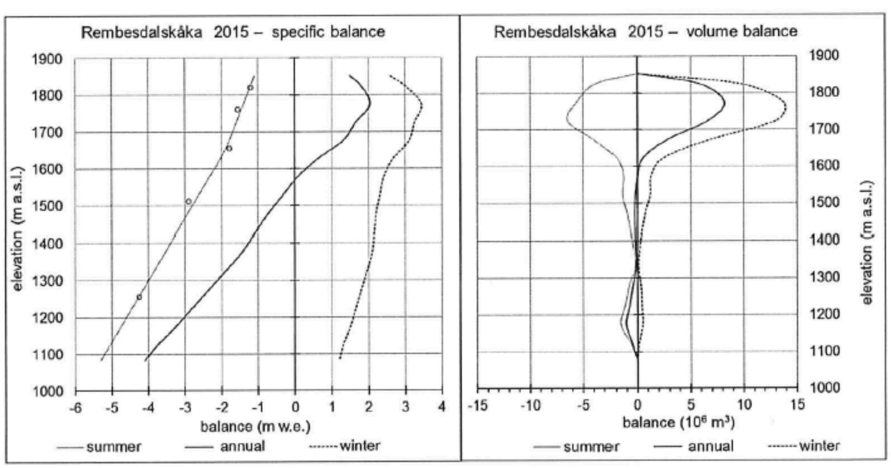

Figure 6-11
Specific (left) and volume (right) winter, summer and annual balance at Rembesdalskåka in 2015.
Specific summer balance at five stakes is shown (○).

Figure 1. Example of SWE versus height from the Reports by Andreassen et al., (2016), (2017), (2019) and Kjøllmoen et al., (2018)

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 simula- tions with and without the blowing sublimation parameterization. One winter would be certainly sufficient.

We actually did include a section describing the sublimation of blowing snow issue and had a figure. However, to reduce the paper and number of figures, the editor suggested we initially remove this section. We will go over the manuscript again and either add the figure or rewrite the text and give some examples.

The modeled wind did have some variations over the glacier on the large scale with the north east part of the glacier experiencing the highest windspeeds (Figure 2). This is also where you have the largest differences in mass balance (see figure 3 where the left plot show without sublimation due to wind drift and the right shows with).

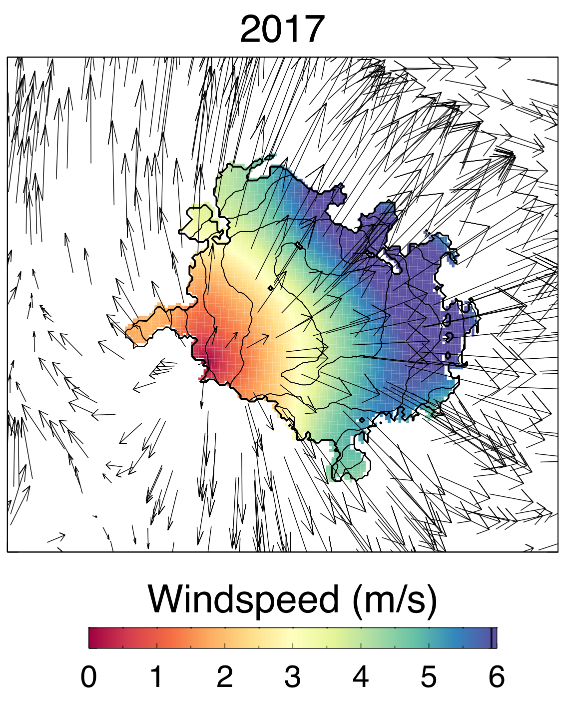

Figure 2 Average wind speed winter 2017

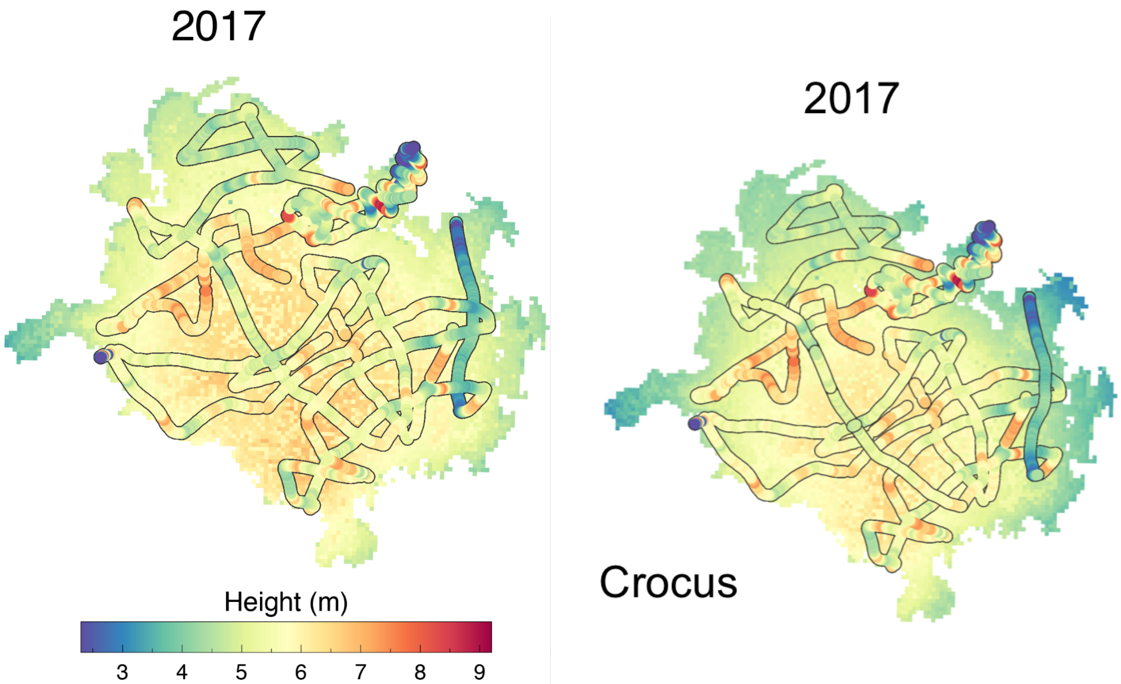

Figure 3 Modeled versus observed snow thickness. Left plot shows without sublimation of blowing snow and right plot shows with sublimation of blowing snow.

Specific comments

Abstract L 16: The transition between the first and the second sentence of the ab- stract 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.

We have moved the part of the abstract that describes why Crocus is suitable for glacier model further up in the abstract.

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.

The sentence now reads:

"WRF atmospheric model simulations were downscaled to 1 km grid spacing to provide meteorological forcing data to the WRF-Hydro/Glacier system at 100 m grid spacing for surface simulation."

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.

The sentence now reads:

"To evaluate the new system (WRF-Hydro/Glacier) over a glacier in Southern Norway," P2 L 38-40: I recommend the authors to add one or two citations for this sentence.

We are adding the citation of Ayala, A, Pellicciotti, F, and Shea, JM (2015), Modeling 2 m air temperatures over mountain glaciers: Exploring the influence of katabatic cooling and external warming. J. Geophys. Res. Atmos., 120, 3139– 3157. doi: 10.1002/2015JD02313 and citations within this paper.

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).

The papers by Machguth et al., 2009; Kotlarski et al., 2010; Van Pelt et al., 2012 already describe the statistical downscaling they are conducting; thus we do not think there is a need to add additional citations for statistical downscaling for glacier studies.

We will add the suggested citations of Jarosch et al. and Fiddes et al.

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.

We agree, and we will add a few sentences regarding this topic

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.

This is correct, and we are reformulating

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.

Sentences now read:

"It is a physically-based model, in which the snow depth can be divided into a user defined maximum levels and where the default maximum is 50 layers."

"In the Crocus model, it is possible to divide the snow into a user defined maximum numbers of dynamically evolving layers."

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).

Thanks for this comment. We are removing the statement of retention since Crocus still uses a classic bucket-type approach.

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.

Thank you for drawing attention to Reveillet et al. (2018). We have added this paper to our citations:

"The Crocus model was first used for glacier mass balance by Gerbaux et al (2005) and recently used for glacier surface mass balance studies within the French Surfex model by Reveillet et al. (2018), Revuelto et al. (2018) and Vionnet et al. (2019). "

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.

Thank you for the clarification. We have removed "age of the snow"

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.

This is a good suggestion and we will add some more information regarding the distinction between ice and snow albedo and the values for snow and ice roughness (we used the same ratio between ice and snow as in the original code). For ice albedo we ended up using the values used in Surfex (and not the ones from Gerbaux et al, as used in Reveillet.

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

This sentence is begin removed

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

Currently the fluxes between the glacier surface and the ground is not incorporated (the engineering of passing fluxes in SURFEX compared to Noah-MP is somewhat different and is not yet incorporated). Under glacier surface it is assumed that the temperature is the same as soil and therefore there are no fluxes added. We would like to incorporate the fluxes between Crocus and the soil soon to be able to use Crocus over the entire domain.

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)?

We are imposing a constant-temperature of 0 degC, as the heat-fluxes between the soil and surface is not yet incorporated. We removed the statement that there are no fluxes of heat pass trough and instead state that no fluxes have been incorporated between the glacier and the ground below.

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.

Agree. We will move this paragraph (or parts of it to the beginning of Section 2)

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.

The reviewer is correct, and in future studies the glacier should be initialized in less layers than the maximum layers. Note, we allow 2 months for spin-up time before using October 1st for calculating winter balance. At this time the glacier has started to merge. By January 1 the glacier is merged down to about 6-8 layers and remain as such for the rest of the simulation. Will add a short discussion regarding this point in the paper.

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

We use the Brun 92 scheme. We are not sure where to explicitly find the grain properties for ice layers in the code, but Snowgrain1=99 and Snowgrain2 = 0.003415 in our output files.

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.

We have followed the suggestion and moved the paragraph.

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.

We will add a brief description on the routing model (WRF-Hydro). However, instead of adding this description in section 2.1, Crocus initialization, we will add it to section 3, Experimental Description. And yes, routing is at 100m, the same resolution as Crocus.

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.

This was suggested by the other reviewer as well, and we will add a table summarizing the simulations

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

The lowest model level height is about 25 m

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

The rain/snow partitioning in WRF-Hydro/Glacier is the same as one of the Noah-MP options, which is called the Jordan Scheme. Snowfraction (SF) = 0 above 2.5C. SF = 0.6 between 2 and 2.5C. SF = 1 below 0.5C. Between 0.5 and 2C, SF is a linear function. We will add a description regarding the rain/snow partitioning.

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 interpolaion? 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?

We used nearest neighbor. We did not consider elevation difference. For most of these stations, the model elevation in the grid point is higher than the observation elevation, and at times about 150 m higher. The Finse elevation difference between model grid point and actual elevation is only 16 m.

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.

We will move up the discussion regarding wind direction and speed at Finse. However, we might remove the figure (and only describe the results) due to suggestions from the other reviewer to reduce the number of figures.

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

For the winter balance, all the green dots in Figure 1 is used (about 60 locations). For the Summer balance, there are 4 locations.

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?

For the winter balance we did. For the summer balance we did not because the observations are adjusted for summer surface, not when observations are conducted. We will clarify the text.

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 data- points. Note that I am not familiar with GPR postprocessing, so ignore my comment if the 1-m spacing is obtained from data postprocessing.

Thanks for point out this error. We checked our survey coordinates, and there is indeed a GPR trace every ~1 m; it is instead the GPS system which records a positional datapoint every 1 s. The wording has been corrected in the manuscript:

"The GPR systems were towed behind a snowmobile, at ~15-20 km/h. The interval between successive GPR recordings is ~0.2 s, giving a distance sampling interval of ~ 1 m (regularized to exactly 1 m in processing). A GPS system was also mounted on the snowmobile, recording positions every 1-2 s, to locate the GPR recordings"

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).

We will include this information either on the line referred to in this comment, or earlier in the manuscript when discussing the albedo

P10 L 277: what is the size of the two river catchments considered in the study?

The catchment size of Fineseelvi is about 16 km^2 and Middalselvi is about 12 km^2. We will add this information in the manuscript.

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.

We will consider this suggestion. However, we have gotten several suggestions to reduce the number of figures (by the editor and the other reviewer)

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.

This is a good point. We will add a clarification in the methods section about how we use MODIS to compare with the model.

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.

We will rephrase the sentence, reflecting the reviewer's comment.

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.

We will mention the non-representation of lateral snow redistribution. Regarding the absence of proper meteorological downscaling, since the region is not very complex, we believe for this study, the downscaling is proper.

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.

Currently we are implementing the Crocus into the National Water Model (a version of WRF-Hydro). So at the time just before publishing we plan on deciding where to add the code for users to use and refer to this site.

Technical comments
Text
P2 L 35: "ablation-season"? done
P2 L 38: a parenthesis ")" is missing done

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). done

P4 L 99: use "metamorphism" instead of "metamorphosis" done
P 4 L 100: Ân´ Vionnet Âz˙ instead of "Vionett". done
P5 L 144: It is surprising to have a Section 2.1 without a Section 2.2. Agreed, and we will move the text before Section 2.1 into a new Section 2.1 and moce Section 2.1 to Section 2.2 We will do the same with Section 3.1 (since there are no section 3.2

P5 L 147: use kg m^{-3} instead of kg/m3 done
P 5 L 208: use m s^{-1} instead of m/s done
P15 L 427: maybe add "and ice" after "physical properties of snow" done
P21 L 598: the reference to Willemet (2008) is not included in the text. This is removed
Table

Table 1: what is the signification of the values appearing in bold in the table? The bold font represents the case with the highest correlation for each year and each location. We will mention this in the text

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?

We completely agree. The author tried to make the scale the same, but with the software used, it was not possible. According to the other reviewer, we might remove this figure. If now, we will try and create a figure with similar scale. We will add bias and RMSE for the wind speed.

References

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