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Interactive comment on "Forcing the SURFEX/Crocus snow model with combined hourly meteorological forecasts and gridded observations in southern Norway" *by* Hanneke Luijting et al.

Anonymous Referee #2

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This paper presents an evaluation of the ability of the snowpack model Crocus to simulate snow depth and snow cover in southern Norway for two winters using different atmospheric driving data: (i) short-range high resolution weather forecast generated by the AROME MetCoOP system and (ii) gridded datasets for precipitation and temperature derived from observations. The authors propose an evaluation of model results using snow depth observations collected at 30 stations across the simulation domain and MODIS snow cover data at 500-m grid spacing. Daily snow depth variations are considered as in Quéno et al. (2016) to discuss more in details the physical processes

responsible for differences between simulated and observed snow depth.

The subject of this paper is interesting for the snow and mountain hydrology community because of the growing use of high-resolution weather forecast to drive snowpack models in mountainous terrain (e. g. Bellaire et al., 2011, 2013; Bernier et al., 2012; Carrera et al., 2009; Horton and Jamieson, 2016; Quéno et al, 2016; Vionnet et al, 2016). The analysis of results presented here is similar to the studies by Quéno et al (2016) and Vionnet et al. (2016) and reveals consistent and interesting model behavior between the French and the Norwegian mountains. My main comments about this study concern (i) the comparison between the different atmospheric driving datasets, (ii) the interpolation of AROME forecast on the 1-km grid used for Crocus simulations, (iii) the selection of stations for model evaluation and its impact on the analysis of model results and (iv) the originality of this work compared to other studies using AROME and Crocus in the French mountains. These questions need to be clarified prior to publication in TC. They are listed below as general comments followed by more specific and technical comments.

General comments

The comparison between simulated snow depth and snow cover using different precipitation and temperature forcing is interesting and illustrate well the strong impact of these variables on simulated snowpack evolution. However, the authors only present the results of snowpack simulations and never compare for example the precipitation forcing in their two experiments; how they differ for different elevation ranges or distance to the sea. Such comparison would be really useful to better understand the differences obtained in the simulated snowpack evolution. For example, Figure 9 shows that the snow cover remains longer at high-elevation in AROME-Crocus compared to GridObs-Crocus. Is it explained by lower precipitation at high-altitude in the GridObs forcing compared to the AROME forcing? This is not mentioned in Section 3.3 and in the conclusion and should be added to the paper.

AROME forecast at 2.5 km are interpolated bilinearly on the 1-km grid used for Crocus simulation. No downscaling is performed to account for the differences between the interpolated terrain height from the 2.5-km grid and the actual terrain height in the 1-km grid. This can potentially lead to large errors in region of complex terrain. For example, the phase partitioning simulated by the AROME cloud microphysical scheme is only valid at the elevation of the grid cell on the 2.5 km grid. A first order correction using a simple lapse rate is required to adjust the phase if the elevation difference is large. Overall, I recommend the author to include simple terrain adjustment routines in their AROME-Crocus simulation to use a meteorological forcing valid on the 1-km grid where are performed the snowpack simulation. This can be done using very simple methods such as the ones used in Bernier et al. (2011).

Section 4.1 shows that the authors include in their analysis stations with large differences between the model and the actual height at station location (up to 450 m). They did not make a selection of stations based on a maximal value for the difference between the model and the actual height. In their studies, Vionnet et al (2016) and Quéno et al (2016) used for example a maximal elevation difference of 150 m in absolute value. In this paper, 13 stations among 30 correspond to this criteria. What is the impact of these large elevation differences on the evaluation of model results? For example, for the stations with an elevation difference above 250 m, what is the impact on the evaluation of snow cover duration? What about the wind speed simulated at these stations and used to determine the occurrence of blowing snow days? As mentioned in my previous comment AROME forcing are only interpolated bilinearly on the 1-km grid. Therefore, altitude differences between the station height and the elevation in the interpolated terrain at 1 km from the 2.5-km grid can be potentially even larger. The authors only mention the elevation differences in the discussion (Section 4.1). I think this should be mentioned earlier in the paper; for example in Section 2.3.1 when presenting the snow depth observations. Overall, the effects of these large elevation differences should be better quantified.

The simulation framework and evaluation methods presented in this paper are very similar to the ones used by Vionnet et al. (2016) and Quéno et al. (2016) who used AROME to drive Crocus snowpack simulations in the French Alps and the Pyrenees. It is interesting to see that similar results are obtained in a different mountainous environment. However, the author need to better insist on the originality of their study compared to these previous work. It would have been interesting to see additional experiments. For example the authors used a succession of forecast from +3 to +8 to drive Crocus. Vionnet et al. (2016) and Queno et al (2016) combined daily forecast from +6 to +29 issued at 00 UTC to drive Crocus. The impact of these choices on model results has never been discussed and it would be an interesting contribution. Similarly, the authors mentioned at the end of their paper (P 19 L7-8) the potential importance of blowing snow sublimation for the high-altitude part of their domain. I recommend the authors to carry out an experiment where they test the impact of the parametrization of sublimation loss during blowing snow events implemented in Crocus. The authors could discuss the impact in terms of snow cover duration and compare it with MODIS images. Overall, these additional experiments would bring interesting insights and strengthen the discussion section which is so far very similar to the discussions in Vionnet et al. (2016) and Quéno et al. (2016).

Specific comments

Abstract: The abstract is rather vague and should present some precise figures such as the overall snow depth bias for the two experiments and the number of stations used for model evaluation. A L11-13, the authors mention the assimilation of snow depth data directly into Crocus. This topic is mentioned here but never discussed in the paper. If the authors want to keep this sentence in the abstract, they need at least to discuss more the assimilation of punctual snow depth data in distributed snowpack simulations in the discussion part.

Introduction: The current introduction of the paper does not described well enough the context of the study and the scientific questions the authors are investigating. For example, the authors never mention the growing use of high resolution NWP forecast to drive detailed snowpack model in mountainous terrain and the limitations associated with these systems. In particular, previous studies using AROME forecasts to drive Crocus in the French Alps and the Pyrenees (Quéno et al., 2016; Vionnet et al., 2016) are not mentioned in the introduction. Similar studies using other models have also been carried out and are not mentioned in the text. For example, the work done by Bellaire et al. (2011, 2013) and Jamieson and Horton (2015) with the Canadian GEM model to drive the detailed snowpack model SNOWPACK. The authors should mention in the introduction how their work differs from these previous studies and what is their contribution to this field of mountain snow research.

P 2 L 21: what are the reasons behind the selection of the simulation domain in South Norway? Hydropower forecasting? Avalanche hazard forecasting?

Section 2.1: The description of the configuration of Crocus and SURFEX should be more specific. For example, the following points should be clarified: - how many layers are used in the soil models? - how are determined the soil and surface properties (clay and sand fraction, vegetation type, . . .)? - how large is the simulation domain in km and grid points? - how are initialized the soil and snowpack properties (if any snow is present) on 1st September 2014? Did the authors perform a model spin-up?

P 5 L9-10: how many stations are used to generate the gridded precipitation and temperature products in the region? In particular, are these stations covering a similar altitudinal range compared to the stations used for snow depth evaluation?

P 6 L 6: on Fig. 1, it seems that the stations are not covering the area of high elevation of the simulation domain. To illustrate this, point, I recommend the author to add on Fig. 2 the histogram of the distribution of elevation in the simulation domain.

P 6 L 17: are MODIS snow cover data not available for winter 2015/2016? It would be interesting to compare the evolution of simulated and observed snow cover for this winter as well to see if model results are consistent in between the two winters.

P 7 L 10-11: where are located the stations used to illustrate model performance? It would be interesting to see their location on Fig. 1. In particular, it would be interesting to see their locations along the West-East transect. Indeed, we can expect significant differences in terms of precipitation amount and resulting snow accumulation between the western and the eastern side of the domain due to the proximity with the ocean. In this context, elevation is not the only variable that can explain differences of snow depth from one station to another.

P 9 L 7-10: differences of snow depth between GridObs–Crocus and AROME-Crocus are low at Hemsedal II. To support their statement on the best results of GridObs–Crocus compared to AROME-Crocus at this station, I recommend the author to compute bias and RMSE of snow depth at this station for the two simulations.

P 9 L 12-13: what are the reasons behind this under-estimation of temperature? Is it associated to a large difference between the model and the actual terrain height at station location? Can the authors justified that this underestimation is responsible for an overestimation of the proportion of precipitation falling as snow? From my experience, NWP model can present a negative bias of temperature during clear nights in wintertime. However, this bias does not affect the phase of precipitation during precipitation events characterized by overcast conditions.

P 9 L 17: the beginning of winter 2015 at Midtsova is interesting and shows a net underestimation of snow depth by GridObs–Crocus. Is it associated with an underestimation of precipitation in the GridObs or with errors in the phase of precipitation?

P 9 L 29-30: it is surprising to see that the authors have selected a category that cannot be used to classify observations ([-0.5 0.5] cm). If the snow depth does not change from one day to another, what is the corresponding category? I recommend the author to use a central category that can be used to classify both simulation and observation.

P 11 Table 2: it would be interesting to see the RMSE for the different variables as well.

Maybe make two tables if the number of information is too large.

P 11 L6 : Quéno et al. (2016) used the same criteria to define blowing snow days but they used the wind speed measured at the stations instead of the wind speed in the atmospheric forcing. Can the author comment on this choice? How accurate is the forecast wind speed for the different stations used in this study? If the wind speed is measured at some stations measuring snow depth as well, it would be interesting to compare the occurrence of blowing snow days with the two wind data to make sure that forecast wind speed can be used to determine the occurrence of blowing snow days.

P 13 L 20-34: the visual comparison of snow cover patterns proposed on Fig. 9 is useful but it should be complemented by a more quantitative analysis. The author could for example compare the temporal evolution of snow cover area in the observations and in the simulations across different altitudinal bands. Similarity metrics such as the Jaccard index or the confusion matrices could be computed as done in previous studies (e.g. Gascoin et al., 2015; Quéno et al., 2016).

Technical comments

Text

P 1 L 24: remove parenthesis around Bokhorst et al. (2016)

P2 L 20, L31: the correct reference for the Crocus paper is Vionnet et al. (2012). The authors should refer to the final version of the paper and not the discussion version.

P 3 L3 and throughout the rest of the paper: units should be written kg m-2 instead of kg/m2.

P 4 L 16-17: from the 1800 UTC analysis time, are the authors using the 3-8h lead time or the 3-5h lead time? It is not clear since they mention that they use the 0-8 lead time for the 0000 UTC cycle.

P 9 L 4: “Episodes when” instead of “episodes where”.

P 9 L 33: “ the transport of blowing snow or wind-induced ablation” is not clear and should be rewritten. Maybe : “SURFEX/Crocus in stand-alone mode does not account for wind-induced snow redistribution”.

P 12 L2: when snow is present on the ground, maximal surface temperature is 0°C. Please remove “or above”.

P 18 L 21: incoming longwave and shortwave radiations are also a key component of the snowpack evolution. Therefore, I recommend the authors to remove the sentence “the two most important variables for snow modeling”.

Figure

Figure 1: the name of all the cities on the snapshots from Google Maps are not easy to read and may be removed. Google Maps may not be the most relevant background map.

Figure 3: the axis labels and the legend are too small and hard to read.

Figure 4: it would be very interesting to know the elevation of the model grid point corresponding to the station location. Such information is really relevant to analyse model results (see the general comment on this particular point).

Figure 6 and 7: the size of the markers (squares and diamonds) and of text (legend, axis labels, . . .) is too small on these figures.

References (not included in the initial manuscript):

Carrera, M. L., Bélair, S., Fortin, V., Bilodeau, B., Charpentier, D., & Doré, I. (2010). Evaluation of snowpack simulations over the Canadian Rockies with an experimental hydrometeorological modeling system. Journal of Hydrometeorology, 11(5), 1123-1140.

Bernier, N.B., S. Bélair, B. Bilodeau, and L. Tong, 2011: Near-Surface and Land Surface Forecast System of the Vancouver 2010 Winter Olympic and Paralympic Games.

J. Hydrometeor., 12, 508–530, https://doi.org/10.1175/2011JHM1250.1

Gascoin, S., Hagolle, O., Huc, M., Jarlan, L., Dejoux, J.-F., Szczypta, C., Marti, R., and Sánchez, R.: A snow cover climatology for the Pyrenees from MODIS snow products, Hydrol. Earth Syst. Sci., 19, 2337-2351, https://doi.org/10.5194/hess-19-2337-2015, 2015.

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