

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 #1

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This article, entitled "Forcing the SURFACE/Crocus snow model with continued hourly meteorological forecasts and gridded observations in southern Norway", has for its main objective to improve numerical prediction of terrestrial snow in Norway. The approach that is presented and examined is based on the use of an external land surface system, SURFEX, that includes the sophisticated snow model CROCUS, and which is driven by a combination of surface analyses (for precipitation and air temperature) and of short-range numerical atmospheric prediction.

This subject is certainly of interest. Better analysis and prediction of terrestrial snow

C1

remains one of the great challenges that national environmental prediction centers face. But unfortunately the scope and ambitions of this study are insufficient, in the sense that this kind of work has often been presented in previous papers (which are not cited), and are actually used Operations in several centers. While the results analysis is very well done, with interesting metrics and with interesting discussion, the overall impact (positive) is not Earth shattering.

Unless the authors come up with a major overhaul of the article, maybe based on some of the more specific comments below, this paper should be rejected.

Specific comments: _____

Page 2, line 25: The main objective of the paper is stated as: "The aim was to compare and combine different forcing data sets as input to the SURFEX/Crocus model and validate the computed accumulated snow amounts and snow melt pattern in both Norwegian mountains and lowlands." The authors have to realize that this type of work has been done often. Practically every snow scheme that has been developed in the last few decades have been tested at observational surface stations using atmospheric forcing that are not unlike what is used in this study. Many land data assimilation systems also use snow models forced with a mixture of observations and model predictions to produce terrestrial snow analyses. In order to make this article acceptable for publication, the authors need to describe and include these previous studies in their Introduction, and explain how exactly what they have done contributes in an original manner to advancing this body of research.

Page 4, Table 1: Was there any adaptation applied to the atmospheric forcing, e.g., for air temperature, or precipitation phase? My understanding that the atmospheric model that provided some of the forcings was at lower horizontal resolution than the external land surface model. There might be some inconsistencies in terrain height (between model and reality) that could lead to biases.

Page 7, line 8: "... is in good agreement with the observations, ..." I don't know how

C2

we could say that from Fig. 3. More generally, more discussion is needed to correctly present and understand what is shown in that figure. For instance, it seems there is a significant bias for snow depth below 100 cm. Also, it might be better to show plot the density of points rather than just the cloud of points.

Page 9, line 9: "... matches the observed pattern of increases and decreases more closely than AROME-Crocus." I disagree with that statement... or I would say instead that this improvement is quite marginal. Is it what we should expect in terms of impact for points that do not benefit of having a surface observational station?

Page 16, line 1: "The results are promising." Too vague.

Page 16, line 1: "Both experiments are capable of simulating the snow pack over the two winter seasons." Based on current and recent scientific and technological achievements in this research area, should we consider this an achievement worth being presented as a conclusion?

Page 16, line 16: "... it could still be argued that the GridObs-Crocus are best in locations of the observations that are included in the gridded dataset used to force SURFACE/Crocus." This statement by the authors is not substantiated by evidence in this paper.

Page 17, line 28: "... this could lead to an overestimation of the snow cover. This in turn would lead to an overestimation of the snow depth." How does that work? What's the physical link here, to explain this cause and effect?

General comment: The impact of precipitation and air temperature observations on the simulation could be better highlighted with "leave-one-out" experiments.

General comment: The article would gain in quality if a comparison of the results presented in this article would be compared with what is currently available (operationally) in Norway.

General comment: Has there been any tests to evaluate the impact of air temperature

C3

and precipitation separately?

Minor comments: _____

Page 1, line 8: "The results are promising." This is too vague.

Page 2, line 8: "... include more statistics to capture the physical snow processes". The word "capture" is not appropriate in that sentence.

Page 2, line 10: "grid spacing" instead of "resolution".

Page 2, line 11: "levels" instead of "layers".

Page 2, line 14: "... the atmospheric part..." this is too vague.

Page 7, Fig. 3: The text on this figure is too small, unreadable.

Page 10, Fig. 6: The two colors chosen for this figure are too alike (difficult to distinguish for old eyes like mine...)

Page 11, Table 2: The statistics presented in that table are quite interesting, but reading it is a bit tedious. I wonder if there could be another way of arranging the table.

Page 11, line 3: "... exceeds 8 m/s..." this is for the winds at what height, 10 m? This should be mentioned.

Page 12, Fig. 7: same comment as Fig. 6 concerning the colors.

Page 15, Fig. 9: the text is too small, unreadable.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-220>, 2017.

C4