Response to Reviewer 1:

P 4 l. 20: the authors mention here Crocus for the first time in the main text. They should at least say that it is a snowpack model and that they use it their study.

Done.

P4 l. 27: please better define the "uncertainty of simulations" since they can arise from uncertainties in the meteorological forcing or in the physical parameterization used in the model. Note also that the uncertainty of Crocus simulations and their consequences on avalanche hazard forecasting have been addressed in the recent study of Vernay et al. (2015) using ensemble methods.

Vernay, M., Lafaysse, M., Mérindol, L., Giraud, G., & Morin, S. (2015). Ensemble forecasting of snowpack conditions and avalanche hazard. *Cold Regions Science and T echnology*.

Done.

P 7 l. 5: the three coefficients mentioned here are used to split the incoming shortwave radiation into the three spectral bands mentioned in the paper. I suggest the authors to use a description similar to this one:
"Crocus treats solar radiation in three spectral bands ([0.3-0.8],[0.8-1.5] and [1.5-2.8] μm). For each band, the spectral albedo is computed as a function of the near-surface snow properties (microstructure). The incoming radiation in each band is then depleted as a function of the spectral albedo. The remaining energy penetrates into the snowpack and is assumed to decay exponentially with snow depth "

Done.

P 9 l. 26: the reference to Libois et al. (2014) is not correct. Indeed the default value of 109 kg/m3 does not come from the study of Libois et al (2014) but from a study carried out au Col de Porte by Pahaut (1976). The reference can be found in Vionnet et al (2012). Like the authors in this paper, Libois et al (2014) had to adapt the parameterization of falling snow density to get realistic initial snow density in Antarctica.

Done.

P 15 l. 11-27: Section 2.5 describes the choices on the uncertainties associated with each parameter. It would be very valuable to compare these choices to those made by Raleigh et al (2015). Raleigh et al (2015) define indeed several forcing error scenarios and show the large impact of these scenarios on the final results. Which scenario is used in the present study?

Our scenario is similar to the NB_lab scenario in Raleigh (2015). We have added a comment and refer to the work of Raleigh.

P 17 l. 8-9; The sentence "Although ... albedo" suggest a direct link between snow density and albedo which does not exist. The authors should either remove this sentence or explained clearly the physical processes explaining this link.

We have removed this sentence.

P 18-19. Sect. 3.2 : the authors show the large spread of their ensemble of snowpack simulations. As explained in my initial review (Reviewer 1) it would be very valuable to have a comparison of the ensemble dispersion with the model RMSE. This would allow the reader to know if the ensemble represents correctly the model uncertainty. It can be easily done on parameter such as snow depth or surface albedo.

We have compared the ensemble dispersion with the model RMSE. The ratio of the RMSE to the dispersion is 1.01 for KNG8 and 1.38 for KNG1, respectively. The dispersion is of the same magnitude as the RMSE and we can assume that the ensemble represents correctly the model uncertainty.

In their answer, the authors explain that they provide a conservative estimation (just using the accuracy given by manufacturers). In their paper, Raleigh et al (2015) generate such ensemble (scenario NB_lab) and the uncertainty associated seems to be lower that the results presented in the present paper. The authors should comment on that. Certainly in the discussion part

Actually the ensemble spread only slightly differs between the two studies. If you take the ratio of the spread and the final snow height, the two studies are indeed similar.

P 19 l. 12-13: clarify the sentence "This indicate ... erosion)". Indeed, the snowpack model Crocus does not represent wind-induced erosion and only account for the effect of wind-induced snow transport on the physical properties of near-surface snow.

We are aware that Crocus does not account for wind-induced erosion and refers to the input data correction. The phrase was unfortunately worded and has been removed.

P 23 l. 20-26 : it is not clear to understand what are the authors try to explain in this part of the discussion. At l. 18-20 they mention the strong influence of LW on SHC in summertime (especially at KNG1). Between l 20-26 they try to describe a complex feedback occurring during snowfall event. This feedback is not clear at all. Does it occur in summertime? You mention the impact of LW during snowfall that occurs mostly in wintertime. The authors should rephrase this part of the discussion.

We clarified this issue by rephrasing the concerned paragraph as follows: "This can be related to the fact that in our approach the input uncertainty range (±10%) proportionally increases with the magnitude of LW . The latter is essentially true during summer when air temperature and humidity are high. LW is further enhanced due to cloudiness and during precipitation events. Note that in the Kongsvegen area the percentage of low clouds rises over 60% from April to October (Kupfer et al. 2006). Stronger longwave radiation input leads to higher surface temperatures which induce steeper temperature gradients within the near-surface snow layers and enhance their metamorphism (settling or even melt). “

P 25 l. 5-13 : between these lines the author discuss the influence of precipitation of SEB. Firstly, they insist on the strong influence of precipitation on the albedo of the snowpack. But at l. 13 they conclude that: “the contribution of precipitation on SEB is mainly due ton interaction with LW via cloud cover”. Please check this part of the discussion for internal coherence.

We clarified this issue as follows:” During winter and spring the calculated SEB is strongly affected by uncertainties in precipitation input, which explains about 25% of the total variance. There is no indication of important interaction with shortwave radiation (missing during winter) or turbulent fluxes. Hence precipitation induced perturbation of LW is considered as the most important factor linking the variability of P and SEB. The effect is more pronounced at the upper site. At the lower part of the glacier, fresh snow events are comparatively infrequent and inefficient. During the summer in particular, fresh snow usually melts within a short period without leaving a significant impact on SEB. “

P 25 l. 20 : do the authors mean the effects of blowing snow on the surface roughness of the snowpack when they mention the “influence of wind drift”? Please clarify.

We have removed the sentence to avoid needless misunderstanding.

Response to Reviewer 2:

First, the manuscript still requires English editing. This does not present a challenge for understanding the authors but is still distracting enough that this should be fixed. Quite a few sentences should be rephrased in a more natural way and the authors should refrain from using "e.g." too often.

> The manuscript already went through a professional English language editing (Scribendi Inc.) and this revision now considers your comments, too. We have considerably reduced the use of "e.g.".

Then, some restructuring is still required. For example, in the section 2.1 there are redundancies in the description of CROCUS between the beginning and the following paragraph (lines 23 and up should be merged with lines 12 and up).

> The two sections have been merged.

In section 2.3 (reference runs), the discussion about the initial temperature profile at line 9 should be grouped with its justification starting line 20.

> The parts have been grouped together.

And finally, the whole "discussion" section is in serious need of restructuring. Although it is interesting, it is very long and lacks internal structure. Therefore, creating sub-section within section 4 would clarify its structure and help the reader keep track of which points are being discussed.

> The discussion section now contains sub-sections for each factor.

This could also help reduce somewhat the usage of acronyms that, although they have been previously defined, tend to force the reader to go back to their definitions every now and then. Special care should be taken regarding the short wave radiation discussion: what is written on page 27, lines 25-26 tend to be inconsistent with the proof that is given on next page 28, lines 3-14. The latter seems to be the right explanation (or the dominant effect) and therefore should take precedence.

> There may be a misunderstanding concerning the short wave radiation discussion. However, we think that we have appropriately stated that comments on page 27 refer to the net effect over the complete year, which does not contradict an intermittently strong effect in spring and summer.

A few more things should also be explained: even after carefully reading the paper, it is still not clear what has been modified in Crocus compared to its standard version. This should be clarified. For example, the discussion about the conversion from snow depth changes to water equivalent: is it performed within Crocus with its own routines or is it performed offline in an ad hoc re-implementation of some parts of Crocus? If this would be the case, how is the settling handled?

> To clarify we have explicitly included "offline" in the following sentence in the section 2.2 Input Data: "Snow precipitation rates were calculated offline from surface height changes measured by the ultrasonic ranger, and converted to snow water equivalent (SWE) for input to the model". The settling is handled by the default parametrization by Crocus.

Another thing that could be improved: although the reviewer is very grateful for the great explanations about the Global Sensitivity Analysis (section 2.4), the transition from SA to GSA could be better shown. One starts reading about SA and suddenly realizes that it shifted to GSA. One sentence at the right place might be enough to call the attention of the reader to what is changed in order to switch to GSA. In the same section, on page 14, it would be great to explain how are the A and B matrices filled (or link it to the discussion about the Sobol sequences), to be on the safe side.

> We have added the following sentence to highlight the difference in SA and GSA: "In contrary to the commonly used SA, GSA calculates the sensitivity measures in broader regions of parameter space by selecting appropriate distributions instead of a specific value of each parameter."
>
> We have now linked the generation of the matrices A and B to the Sobol sampling section: "The elements of the matrices A and B are generated from quasi-random Sobol sequences (see Sec. 2.6)"

Finally, the conclusion seems to stretch the results a little bit beyond my comfort zone. When the authors conclude about altitudinal trends and/or different zones on the glacier, one should not forget that this study has been performed at only two stations (and one year). Concluding about "the lower elevation station" versus "the higher elevation" is safe while generally concluding about altitude trends is more of an extrapolation.

> Thank you very much for this comment. We agree and have changed the text accordingly.

Some detailed comments:

* on page 3, line 15: what do you mean by "more and better data to constraint [...] results of the simulation"? Although it makes sense for the initialization or forcing, it is not a direct link to the results!

The sentence now reads as follows: “In general, the development and use of higher order models also induces a need for more and better data to constrain the initialization, forcing, parameterizations, and validation of the simulations.”. This also should make clear the importance of data for validation of the results.

* on page 5, line 7: there is an extra "(" in the middle of the line;

Corrected.

* on page 5, line 25-26: does Crocus use molecular conductivity or bulk conductivity?

Following Vionnet et al. (2012), snow effective thermal conductivity is expressed according to Yen (1981) which is a bulk conductivity. We do not mention these details in the text anymore.

* on page 6, line 1: there is an extra ")";

Corrected.

* on page 6, line 21: missing "t" in "budget";

Corrected.

* on page 7, line 2: misspell "respectively";

Corrected.

* on page 7, equation 3: consider defining "P";

Done.

* on page 9, line 7: there is a lonely "2" at the end of the line;

This is the reference to Table 2. Corrected.

* on page 16, line 17: idem;

We couldn’t find the misspelling.

* on page 17, lines 8-9: basically, the density compensates for the albedo parametrization short comings, right?

Yes, that's correct. However, we removed this sentence since albedo is related to the microstructure rather than to the density.

* on page 22, line 7: very unclear... what are these other directions?

We now have made clear that this was mend for quantitative mass balance studies.

* on page 22, line 27: what do you mean by "The traces"?

We have removed the wording "The traces" from the sentence.

* overall, it seems that there are no liquid precipitation measurements. Maybe this should be mentioned on page 24 in the discussion about the precipitation impact (ie in the summer, this impact won't be shown since it is not even measured)

Yes, that's indeed an important aspect. We have included the following sentence: "The lower values in summer are bound up with the fact that no liquid precipitation is measured at this site, and hence has no impact on the variability.", to make this point clear.

* on page 25, lines 3-7: please clarify.

Done.

* on page 26, lines 1-3: this is unclear.

According to comment 4, we have re-arranged the discussion section and have removed: "The impact of the specified uncertainties in the basic meteorological forcing data (U, T, RH and SW) on the considered model output metrics is small overall. On average, such uncertainties can explain more than 10% of the total variance (Fig. 4), and there is no significant difference between the two study sites. In general, SHC is less affected compared to SEB, which is reasonable considering the role of those input data in the parameterizations of the associated processes. Regarding the seasonal sensitivity patterns, however, each factor can have an episodically strong impact."

* on page 26, lines 12-13: it seems hard to believe that the impact of the grain shape due to wind drift would be significant compared to the turbulent fluxes!

The impact of the grain shape is certainly less important than the turbulent fluxes. We have removed this statement in order to avoid misunderstanding.

* on page 27, lines 4-6: please clarify: are the air temperature measurements ventilated?

We have specified the type of sensor (unventilated) in the input data section.

* on page 27, lines 7-8: what do you want to tell us?

The lines now read as: “Relevant to this study, Karner (2013) did not find significant biases between ventilated or unventilated air temperature measurements. However, this result may not be generalized.”

* on page 29, last line: what are "the remaining ones"?

The “remaining ones” are wind speed, air temperature, humidity, and pore volume. The sentence has been changed to: “Precipitation tends to have the strongest impact during the winter, while wind velocity, air temperature, humidity and liquid water holding capacity mainly impact the simulations in the summer or transitional seasons.”.

* on page 30, line 1: misspell "for"

Corrected.