Review of the revised version of paper GMD-2015-5 by Sauter and Obleitner

This paper presents how Global Sensitivity Analysis (GSA) can be used to discuss sensitivity of glacier mass balance simulations and to identify factors affecting model results in terms of snow depth and surface energy balance (SEB). The authors made an important effort to include in the new version of the manuscript most of the reviewer's comment. In particular, they apply the GSA at another site (KNG1) located in the ablation zone of the Kongsvegen glacier. They also improved the structure of the paper which is now well-structured and well written. I am now only requesting minor revisions at this time. Most of the minor comments concern the description of the model setup and the discussion.

Specific comments:

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.

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 Technology*.

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 "

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.

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?

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.

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.
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

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.

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.

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.

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.

Technical comment:
Text

P 5 l. 7 remove "("

P6 l. 21 replace "budge" by "budget"

P 22 l. 20 start a new paragraph after "… near-surface snow layer".

P 29 l. 2 : "this feature could is": correct the text.

Table

Table 3: define the sign of the fluxes (towards or from the surface) and mention how the mean are computed (from measurements or model results).