

Interactive comment on “Assessment of the uncertainty of snowpack simulations based on variance decomposition” by T. Sauter and F. Obleitner

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Note: reviewer comments are in italics and the authors' responses and manuscript revisions are in normal face.

Comment: This paper presents how Global Sensitivity Analysis (GSA) can be used to discuss snowpack model sensitivity and to identify factors affecting model results in terms of snow depth and surface energy balance (SEB). The authors apply the GSA method to punctual snowpack simulations carried out a high Arctic glacier (Kongsvegen Glacier). The topic of this paper is important for scientists involved in snowpack modelling since it proposes a technique to quantify model sensitivity including interac-

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tions between parameters. This method can be extended to other geophysical models. The results are really interesting and should be published in GMD. Prior to publication, major revisions should be made to better illustrate the reliability of the results obtained with the GSA. The description of the reference simulation should also be improved. They are listed below (General comments) followed by more specific and technical comments.

Thank you very much for reviewing this paper.

Comment: The conclusions of the authors in terms of model's sensitivity rely on the parameters uncertainties and their distribution used to generate the ensemble and described in Table 2. The authors should evaluate if their ensemble represents correctly the model uncertainty. This could be achieved comparing the ensemble dispersion (the standard deviation of the members relatively to their average) to the model RMSE (computed using observations and the ensemble average). The error can be computed for snow depth but also for other measured parameters such as albedo and surface temperature. The ensemble dispersion is expected to be of the same magnitude as the error. If the ensemble dispersion is too large compare to the error, this would mean that one or several error distributions are too large and not appropriate.

Response: The aim of this study is to estimate the model uncertainty based on the accuracy given by the manufactures. In real life applications it is not possible to reduce the uncertainty more than the specified accuracy. Hence, the analysis can be seen as a conservative estimation. The idea is to include all measurement errors and check whether a forcing factor is sensitive or not. We agree that modellers need to check for overdispersion when doing ensemble predictions. This is not the intention of this study since we are rather interested in the complete uncertainty range. We have carefully reworked the text and point out in the introduction (p4L15-p4L17) and the discussion sections (p29L11-p29L17) that the study focuses on systematic biases (given by the sensor's accuracy) in the forcing data.

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Comment: This comparison requires having a reliable estimation of the model error. One year of simulation at KNG8 may be not sufficient. Therefore, the authors should consider extending their analysis to other years. If the data are not available at KNG8, a good alternative would be to use meteorological data available at KNG6 (Karner et al, 2013). The differences between their conclusion and those of Karner et al (2013) are mentioned several times in the paper (P 2826 | 2-3, P2828 | 3-5). Applying the GSA method at KNG6 would also allow the authors to discuss more in details these differences. Extending the GSA method to other years will probably require reducing the number of members of the ensemble. The authors could follow a two-step approach. They could first present the results of the analysis of the original version of the paper (one year at KNG8 with 16000 members) and then extend the analysis to other years at KNG8 or KNG6 using an ensemble with a restricted number of members.

Response: A longer period would certainly be of benefit. Unfortunately, reliable data is not available for a longer period at KNG8. However, we have followed the suggestion of the reviewer and analysed the sensitivity pattern at station KNG1 located in the ablation zone of the Kongsvegen glacier. In contrast to KNG6, this station has a more distinct characteristic from KNG8. For both station the sensitivity indices have been estimated from 20000 ensemble members. The number of ensemble members has been increased in order to better estimate the accuracy of the indices by bootstrap sampling. We discuss in detail the differences of the sensitivity pattern in the Result and Discussion section.

Comment: The authors include the aerodynamic roughness length, z_0 , in the sensitivity analysis (Tab. 2). As mentioned in Tab 1, this corresponds to the roughness length for momentum. In land surface models, the roughness length for heat exchanges z_{0H} is generally deduced from z_0 following $z_{0H}=z_0/K$. K is a constant equal to 10 by default in SURFEX (Mascart et al, 1995). This is the case in the reference run (Tab. 1). The

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authors should explicitly mention if $z0H$ is modified or not when generating the ensemble members using the uniform distribution for $z0$. If not, it probably reduces the mean sensitivity measures for $z0$ in the GSA. In this case, it would be really interesting to account for the dependency of $z0H$ on $z0$ in the GSA. Note that this general comment may be not relevant if this dependency is already taken into account in the numerical experiments described in the paper. In any case, $z0H$ should be mentioned in Tab. 2 with the type of distribution and the range of values.

Response: The $z0H$ is modified when generating the ensemble members using the uniform distribution. However, the factor K is equal for each member. We have added the following sentence to the penultimate paragraph in Section 2.5 (p14L21-p14L22): "The roughness length for heat $zh0$ is derived from the roughness length for momentum using the relation $zh0 = z0/10$."

Comment: Section 3.1 contains the description of the results of the reference simulation. For this simulation, the authors do not consider using a spin up to generate the initial profile of snowpack properties (especially temperature) contrary to the method followed by Karner et al (2013). Are meteorological data from previous years available at KNG8 and could they be used to improve the initial snowpack following the same method as Karner et al (2013)? What would be the impact on the simulations, at least for the reference run?

Response: The vertical temperature profile in the snowpack is directly measured by a chain of temperature sensor at KNG8. However, the exact depth of the sensors could not be reconstructed for the beginning of the simulation period. The simulations start at the end of the ablation period and the snowpack temperature is close to the melting point. We have performed several simulations with various initial boundary conditions, but the effect of the initial temperature profile on the simulation results was very small.

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Comment: The description of the results (P2819 I 25 to P 2820 I 15) is rather short and could be more clearly identified using separated paragraphs for example. Then, the authors should discussed more in details the physical processes behind the model results. For example, the difference of snow temperature close to the surface may also be due to higher snowpack thermal conductivity because of the higher snowpack density close to the surface. On contrary, the sentence P 2820 I10-11 suggesting a “surprisingly” direct link between surface albedo and snow surface density should be explained.

Response: We have carefully restructured the paper and better described and discuss the results. The first paragraph of the Section “Reference run” and “Uncertainty estimation” have been moved to the methods section as suggested by the reviewers. A new chapter “Reference run setup” has been added describing the initial and boundary conditions of the reference run. Furthermore, the results section has been restructured and consists now of four subsection: (I) Reference run, (II) Integrated model uncertainty and (III) Mean total-order sensitivity indices and (IV) Temporal evolution of the total-order sensitivity indices. In the Reference Run section we examine the accuracy of the reference runs in more detail (Note, we have included a second station, KNG1, for comparison). In this revised version we focus on the total-order indices rather than on the first-order indices. The mean values and the evolution of the indices are described in the last two sections of the results chapter. The discussion has been rewritten and should not infiltrate the discussion anymore.

Specific Comments:

Comment: The title should better reflect the content of the article. In particular, it should be mentioned that the GSA method was applied at one high Arctic site, which somewhat reduces the generality of the title. Abstract The Abstract is too vague and does not allow the reader to extract the method followed by the authors and the main findings of this study. For example, it should clearly mention where the study has been

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carried out (the current version only mentions “in a high Arctic environment”) and which factors affect the results. It would also be useful to state the name of the snowpack model in the abstract.

Response: We followed the suggestion of the reviewer and changed the title to: “Assessing the uncertainty of glacier mass balance simulations in the European Arctic based on variance decomposition”. We also modified the abstract and mentioned where study has been carried out and which factors contribute to the model variance. Furthermore, we have mentioned the snowpack scheme Crocus.

Manuscript Revision: P2813, l 5, the sentence “measured input except of outgoing infrared” is not clear. Indeed, when using a detailed snowpack model such as Crocus, incoming shortwave and longwave radiations are provided as atmospheric forcing. They can be measured values such as in this study. However outgoing shortwave radiation is not a measured value suggested by this sentence

Response: We have deleted this part of the sentence.

Manuscript Revision: P2813, l 12, the terms P and M of Eq. 3 should be defined. In Armstrong and Brun (2008), the term E of Eq 3 has a different definition than the one used in this study. In Armstrong and Brun (2008), E is defined as the sublimation and evaporation rate at the surface.

Response: Thanks for this hint. Indeed, E denotes the sum of sublimation and evaporation rate and the annotation of Eqn. 3 was correspondingly corrected.

Manuscript Revision: P 2814, l 12-13, the term “roughness length for fresh snow” is not precise enough. Please specify which roughness length (see general comment 2).

Response: We have replaced this expression by “roughness length for momentum

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...”. In the Section GSA (penultima paragraph) we have specified how the roughness length for heat is derived (p14L21-p14L22).

Manuscript Revision: P 2815, I 20, adaptations of Crocus’s parameterisation for fresh snow density have been already proposed in polar environment (Dang et al, 1997, Libois et al, 2014) and could be compared to the modification proposed by the authors.

Response: We have included the work of Libous et al, 2014 in this section (p9L27).

Manuscript Revision: P 2820, I 16-29, precise the time period considered as the “ablation season” and the “accumulation season”.

Response: Ablation and accumulation periods are of different length at KNG1 and KNG8. To enable comparison of the GSA analysis we now consider a central summer (JJA) and a winter period (DJF), respectively.

Manuscript Revision: P 2821, I 1-2, Karner et al. (2013) give the 10-year average surplus as KNG6. It would be interesting to give the inter-annual range of surplus so that the reader can better realize the differences between KNG6 and KNG8.

Response: The present study is based on simulations of a one year period. Therefore, a straightforward comparison of inter-annual ranges of the energy balance calculated at KNG6 and KNG8 is not possible. Please note, however, that due to now considering an additional site (KNG1), Table 1 and related discussion allowed for extended consideration of the issue. Comparison is possible for the ablation period which is commonly defined as JJA. The main characteristic are that net radiation decreases with elevation along the glacier (KNG1/KNG6/KNG8: 83/38/13), sensible heat flux decreases (KNG1/KNG6/KNG8: 21/8/3) as well as latent heat flux (KNG1/KNG6/KNG8: 21-/2/3). But even that comparison is hampered by comparing a 1 year period (KNG1

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and KNG8) with decadal averages (KNG6), though conforming to the expectations regarding the elevation gradients along glaciers (e.g. Oerlemans J., Björnsson H., Kuhn M., Obleitner F., Palsson F., Smeets P., Vugts H.F. and de Wolde J., 1999: A glaciometeorological experiment on Vatnajökull, Iceland, *Boundary-Layer Meteorol.*, 92, 1, 3-26.)

Manuscript Revision: P2821 I 25, P 2822, I 19-20, it would be useful if the authors could precise how the ensemble members are generated. For a given member, are the perturbations of the parameters fixed for the whole duration for the simulation (for example, RH is always increased by 2).

Response: The Section “Global Sensitivity Analysis” has been extended and two new sections “Measurement error characteristics” and “Sobol sampling” have been added. At the end of the Section 2.4 (p14L14-p15L9), we have added a new paragraph describing the calculation of the sensitivity indices, sampling strategy and how the accuracy of the indices is determined.

Manuscript Revision: P2827, I 25, the impact of PVOL on Crocus simulations has been previously discussed in Gascon et al. (2014) and could be mentioned in this discussion part. The conclusion is written as a single paragraph and would be more clear if the authors could divide it into several paragraphs. Table 1: The definitions of ALB0.3, ALB0.8 and ALB1.5 are erroneous. These 3 parameters refers to the spectral albedo for surface ice for 3 spectral bands and not to the absorption coefficient for 3 spectral bands. These parameters are not used to compute snowpack albedo (see Vionnet et al (2012) for more details concerning the computation of snowpack albedo in Crocus). They are only used to compute albedo when the snow density is above the ice density threshold (830 kg m⁻³ in this study).

Response: Thank you very much for this reference. We have referenced the this

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article in Section “Crocus model setup” (p8L3) and in the discussion part (p29L4).

We have completely rewritten and shortened the conclusion section so that no paragraphs are necessary.

The definitions of the spectral albedo for surface ice has been corrected.

Manuscript Revision: P2810, l 19, (and after), the authors use sometimes “snowpack model” or “snow model”. Please be consistent. I personally recommend “snowpack model” to avoid confusions with the modelling of snow in the atmosphere as a micro-physical specie.

Response: We follow the recommendation of the reviewer and use the term snowpack model throughout the text.

Manuscript Revision: P2810, l 19, (and after), use Crocus instead of CROCUS.

Response: We have changed the expression.

Manuscript Revision: P2812, l 4, remove “of” between “the” and “snow grains”

Response: We have removed the “of”.

Manuscript Revision: P2816, l 10, use “snow depth variations smaller than...” rather than “ snow depth smaller than ...”

Response: Changed accordingly.

Manuscript Revision: P2817, l 7, define SD

Response: Changed accordingly.

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Manuscript Revision: P2818, l 6, the sum goes from “i to k” rather than from “i to r”

Response: Changed accordingly.

Manuscript Revision: P 2819, l 20, “shortwave radiation” is not used to evaluate the reference simulation

Response: Thats true, we have removed “shortwave radiation”.

Manuscript Revision: P2819, l 23, the date of the snow pit profile is 6 April “2011” instead of “2010”.

Response: Changed accordingly.

Manuscript Revision: P2820, l 4-5, Fig. 3 shows the vertical profile of temperature and not the “vertical temperature gradient”.

Response: Changed accordingly.

Manuscript Revision: P 2820, l 18 (and after), energy fluxes are in $W m^{-2}$

Response: The unit has been changed throughout the text.

Manuscript Revision: P2181, l1, close the bracket after Fig. 1

Response: Changed accordingly.

Manuscript Revision: Table 2: remove “

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Response: The “%” has been removed.

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