

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: The results demonstrate that large uncertainties in modeled snow depth can emerge from relatively conservative input uncertainties. Longwave radiation uncertainty had a strong control on all four model outputs while precipitation uncertainty had only a substantial control on snow depth uncertainty. The other factors were typically less important (with sensitivity indices usually less than 0.25), although during episodic wind storms, uncertainties in wind speed and aerodynamic roughness increased in

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importance. The authors do not generalize their results for other locations but suggest their approach can be applied in other locations and for other models. I think the study demonstrates the value of variance decomposition sensitivity analysis for understanding how input uncertainty matters when modeling cryospheric processes at a high latitude glacier. A growing number of studies in hydrology and earth sciences are applying variance based sensitivity methods to better understand model behavior, and thus this topic is timely and relevant. Despite this potential, I think there are a number of areas where the paper needs to be improved before being considered for publication, and so I recommend that the authors consider these suggestions for strengthening their contribution.

Response: Thank you very much for your detailed comments and constructive criticism of the original manuscript. We have carefully revised the paper and considered your suggestions to improve our contribution.

Comment: First, I would like to gently make the authors aware that my colleagues and I have recently presented a very similar framework for assessing the impact of forcing uncertainty on modeled snow variables with variance based global sensitivity analysis. I refer the authors to this paper (Raleigh et al., 2014) and include the citation at the end of my review. Our contributions are different in that mine focuses more on how specific error characteristics (i.e., error types, probability distributions, and magnitudes) in the model forcings matter to the outputs, and I examine different sites and a different snow model. However, I note there are some similarities between our studies in terms of the experimental setup (e.g., I also consider a scenario of specified measurement uncertainty) and some results (e.g., the importance of longwave uncertainty). In any case, I suggest that it might be appropriate to consider the connections between the results of our independent experiments in your discussion section.

Response: We have to admit that we were not aware of this paper at the time when we came up with the draft. We are really sorry that we have overlooked this paper.

Indeed this is a very interesting work and take up for the very first time this relevant topic. We think your study and our work complement each other very well. It is exciting and encouraging to see that some results are in line with our findings. Whenever possible we refer to your paper and contrast findings in the results and discussion section.

Comment: There are numerous grammatical problems and awkwardly phrased sentences throughout the manuscript. These were distracting, though I was usually able to determine what the authors meant. I recorded many of these issues in the “Technical Corrections” section (see below), but I certainly did not catch all problems. The paper would thus strongly benefit from a more thorough grammar and language review to ensure the English usage is correct and clear throughout the manuscript.

Response: The manuscript has been proof-read by a professional editor. We hope all the grammatical problems awkwardly phrased sentences have been corrected, now.

Comment: The organization of the paper needs more attention. Specifically, the results section is actually a mix of methods, results, and discussion and would therefore benefit from careful restructuring. As an example of these elements, you can see aspects of methods (e.g., page 2821, lines 6-27) and discussion (e.g., page 2820, line 2; page 2820, lines 7-8) infiltrating into the results section. There needs to be a more clear division between sections to present a more logical exposition of the analysis.

Response: We have carefully restructured the paper. The first paragraph of the Sections “Reference run setups” and “Uncertainty estimation” have been moved to the methods section as suggested by the reviewer. The “Reference run setups” chapter describes 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

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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.

Comment: The global sensitivity analysis (section 2.3) needs to be described in more detail. While the conceptual equations are provided (equations 6-9), it is not clear how the variances are actually calculated for the first-order and total-order sensitivity indices, and whether any bootstrapping was conducted to assess the confidence in the indices. Because there are several methods available for the variance calculation (see Saltelli et al., 2010), this needs to be clarified. Also, it would be helpful to include more detail in section 2.3 about how the sampling was done, how the errors were selected and assigned, and information about the convergence rates of the sensitivity indices.

Response: The Section “Global Sensitivity Analysis” has been extended accordingly. At the end of the Section we have added a new paragraph (p14L1 – p15L9) describing the calculation of the sensitivity indices, sampling strategy and how the accuracy of the indices is determined. We have performed new simulations for both stations (KNG1 and KNG8) with N=2000 base samples. This results in 20000 model runs for each station. The accuracy of the sensitivity indices was assessed from 1000 empirical bootstrap samples. Similar to Raleigh et al. (2015) we estimated the 95% confidence regions for each total-order index (see Figure 4).

Comment: It is not clear why the authors considered a mix of meteorological forcings and just two model parameters in their uncertainty/sensitivity analysis. Why were only the aerodynamic roughness and maximum liquid water hold capacity parameters considered, and not the other parameters in Table 1?

Response: The user provides the meteorological input data as a NetCDF file. Relevant model parameters are given by a separate option file. This file contains parameter such as roughness length, pore volume, initial temperature profile, land surface categories, initial grain types, and a few more internal snow parameters. The roughness length and pore volume are the only two parameters in this file which are relevant for the calculation of the surface fluxes. We had to change the code of Crocus so that these parameters can be provided by the NetCDF file in order to perform the Monte-Carlo runs. Other relevant parameters are defined within the model code and it turned out to be difficult to include them in the input file. Furthermore, including more parameters would have gone beyond the scope of the computational possibilities. We agree that more parameters need to be considered for a more complete variance analysis.

Specific Comments:

Comment: Page 2812, Lines 4-5: What are these “indicators of snow grain history” exactly?

Response: We have specified the indicator of snow grain history in the paragraph on microstructure (p6L1-p6L9): “Snow layers are described through bulk physical properties (thickness, density, temperature, liquid water content) and microstructure parameters. The latter characterize the state of the snow crystals in terms of dendricity, sphericity, grain size and snow grain history. The parameter of snow grain history indicates whether there once was liquid water or faceted crystals in the layer (Brun et al., 1992; Vionnet et al., 2012). The changes in the morphological shape of snow crystals depends on snow metamorphism in response to atmospheric forcing and internal processes. To adequately treat the internal processes, the model employs a number of parametrizations derived from specific field and laboratory experiments.”

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Comment: Page 2812, line 12: The authors usually use the term “longwave radiation” (e.g., Page 2814, line 25) throughout the manuscript, but here they use the term “infrared radiation”. Please pick one convention and be consistent everywhere.

Response: The term “infrared radiation” has been changed to “longwave radiation” throughout the text.

Comment: Page 2813, Line 5: Earlier (page 2812, Lines 11-12) you noted that incoming solar radiation was a model input, but this line suggests that net solar radiation is used. Please clarify. How does the model calculate albedo?

Response: The phrase “Net radiation itself is composed of the sum of incoming and outgoing solar- and langwave radiation” describes the term NR in Equation 1 and does not refer to the model input. The model is forced by incoming solar radiation.

We have added the following paragraph to clarify the albedo calculation (p7L4-p7L8): “Crocus treats solar radiation in three spectral bands ([0.3-0.8],[0.8-1.5] and[1.5-2.8] μm) and empirical coefficients (0.71, 0.21, and 0.08) describing spectral albedo as a function of the near surface snow properties (Vionnet et al., 2012). For each band the spectral albedo is computed as a function of the snow properties (microstructure). The remaining energy absorbed by the snowpack is assumed to decay exponentially with snow depth.”

Comment: Page 2813, Lines 9-10: Instead of “Eq. 3, right hand terms”, I think you might mean “Eq.2, first terms on right hand” for the phase change. Referencing Eq. 3 does not make sense in this context because it is the mass balance.

Response: Yes, the reference has been changed accordingly.

Comment: Page 2815, Line 18 (equation 4): Please provide a citation for the new

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snow density equation. What study does CROCUS cite for this equation?

Response: Vionnet et al. (2012) does not cite any reference. The snow density equation has been proposed by the original paper of Brun et al. (1992). We included both references for this equation.

Comment: Page 2816, Lines 24-25: This sentence is misleading, as not all sensitivity analysis methods rely on variance decomposition. Please rephrase.

Response: We have deleted the part of the sentence which says "... by decomposing the variance of the model output."

Comment: Page 2819, Lines 3-22: Much of the text here is more appropriate for section 2 (data and methods) and not the results section.

Response: We have added a new chapter "Reference run setup" in section 2 (p11L5-p12L4). This chapter describes the boundary conditions and initial conditions for the reference run. Parts of the text from section 3 has been moved the new section.

Comment: Page 2819, Lines 25-27: You should state somewhere in section 2 what you assumed the snow emissivity was in order to calculate snow surface temperature from upwelling longwave radiation.

Response: We paragraph (p16L19-p16L21) has been changed as follows: "Fig. 2 shows the comparison of simulated snow surface temperature with observational data. The simulated snow surface temperature is derived from upwelling longwave radiation assuming a snow emissivity of 0.99." The snow emissivity has also been added to Table 1.

Comment: Page 2820, Line 4: Can you clarify what you mean by "diverse model

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uncertainties”?

Response: We have changed “diverse model uncertainties” by “structural model uncertainties”. Structural uncertainty refers to model inadequacy which comes from the general lack of knowledge. This uncertainty depends how accurately the snow model describes the natural/physical processes.

Comment: Page 2820, Line 7: It appears that the metrics used to describe model performance for the temperature variables (snow surface temperature and temperature profile) are inconsistent. For the former, they report the deviations in terms of a range (i.e., 95% within 1.1 K for snow surface temperature), but in the latter they use RMSE for the temperature profile. Please consider using more consistent evaluation metrics.

Response: We changed the evaluation metrics of the snow surface temperature to RMSE. The metrics should be consistent for all variables, now.

Comment: Page 2820, Lines 14-15: These stated albedo ranges are for the measured values, correct? Please clarify.

Response: The corresponding phrases have been revised to read as follows (p17L10-p17L14): “The The RMSE between the measured and modelled albedo over the entire simulation period is 0.06 [-]. Note that the measured albedo ranges between 0.65 in the ablation period and 0.92 in the accumulation period, which is characteristic for a site in the accumulation region (Armstrong and Brun, 2008; Greuell et al., 2007).”

Comment: Page 2821, Line 25: Please clarify whether this means 16 000 model simulations were evaluated, or if this means that you selected 16 000 points in the uncertainty space. These two are not equivalent, because the latter will result in at least $N(k+2)$ model evaluations where N is the number of points in the input uncertainty

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space.

Response: We have updated the complete section on “Global Sensitivity Analysis” and added a new paragraph (p14L15-p15L9) describing in more detail the estimation of the sensitivity indices, the sampling and the number of base samples. We have repeated the simulations with 20000 model runs ($k=8$, $N=2000$) to provide better accuracy of the indices. The accuracy of the sensitivity indices was assessed from 1000 empirical bootstrap samples. Similar to Raleigh et al. (2015) we estimated the 95% confidence regions for each total-order index (see Figure 4).

Comment: Page 2824, Lines 15-19: This argument depends strongly on whether one considers 7% interaction variance as a significant contribution. Also your argument would be strengthened if you examined your own results in Figure 7 and gave a specific example of how wrong conclusions might be drawn if only the first-order effects were considered (as is the case in SA methods that are designed for linear models). Currently this all seems like conjecture and the argument is not compelling.

Response: This was a serious mistake. The number refers to the maximum contribution of linear effects on the model variance (SHC) and not on the time averaged value. We have now given the averaged first-order indices for each target metrics (see Figure 4). The average values first-order indices are significantly smaller than the 93% given in the text and vary between 0.69 and 0.82. In the discussion section we (p22L7-p22L12) we write: “The overall results of this work show that on average about 80% of the total variance of SHC and SEB can be explained by first-order effects (Fig. 4). This means that the remaining 20% of the variance is due to non-linear interaction effects. There is no significant difference between the two sites at the glacier. This is in partial contrast to the findings of Raleigh et al. (2015), who performed similar investigations for different snow regimes and found that first- and total-order indices are of comparable magnitude.”

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Comment: Page 2828, Lines 7-8: The logic of this argument is unclear to me. Because you have calculated the first order indices, I would argue that you actually can compare your results to the first order effects found in Karner et al. (2013) and Obleitner and Lehning (2004). So this begs for additional discussion and comparison between the results here (which found low first-order sensitivity values for T and SW in the SEB) and those previous results (which had higher first-order effects from T and SW). Why do you suppose the first order effects are different?

Response: Karner et al. (2013) uses a different snow model, where measured albedo values are provided as input and do not depend on the snow microstructure. The station is also located in a different glaciological regime (equilibrium line altitude vs accumulation area, see also Table 3). In order to support our statement we have extended our analysis and simulated and analysed a second station, KNG1, on the Kongsvegen glacier located in the ablation zone. Consideration of sites other than KNG6 was motivated by the availability of correspondingly suitable data. We have rewritten and extended the discussion section and compare in detail our findings with the results of Karner et al. (2013) – see p23L27-p23L29; p25L26-p25L29; p28L17-p28L18.

In the conclusion we emphasized the differences between the two stations, KNG1 and KNG8 (p29L17-p29L25): “The overall impact of individual error sources on the sensitivity pattern varies for different zones on the glacier. In the accumulation zone, precipitation and longwave radiation are key factors for the evolution of the snowpack and contribute most to the model uncertainty. The significance of precipitation decreases with altitude, while other factors, such as wind velocity and surface roughness, gain importance. Uncertainties in the measurement of incoming shortwave radiation and air temperature have little influence on the model outcome, the former being biased by the specific, i.e. Arctic, conditions. The calculated seasonal sensitivity patterns are similar overall at both study sites.”

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Comment: Page 2829, Lines 5-8: I would disagree that your analysis supports this conclusion. As I understand the selected input uncertainty ranges, these are informed by manufacturer's specified accuracy. Hence, it may not be possible to reduce the LW uncertainty to anything better than +/-10%, but the results show that even in this "best case" (of having LW measurements instead of parameterizing the flux), the model outputs are still strongly controlled by the measurement uncertainty of LW.

Response: We agree that it may not be possible to reduce the LW uncertainty anything better than +/-10%. The phrase "..., however, can be gained more easily by using direct measurements of LW, ..." was misleading and has been removed.

Technical Corrections:

Manuscript Revision: Page 2808, Line 15: Replace "there" with "their".

Response: Changed accordingly.

Manuscript Revision: Page 2809, Lines 1-3: This first sentence reads awkwardly, perhaps because you use "which" three times in the sentence. Please rephrase.

Response: The sentence has been removed.

Manuscript Revision: Page 2809, Lines 21-23: This sentence reads awkwardly. I recommend rephrasing to "...scientists to quantify the uncertainty in the model outcome, and to provide information on its robustness."

Response: Changed.

Manuscript Revision: Page 2810, Line 7: Replace "have been" with "has been".

Response: Changed.

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Manuscript Revision: Page 2810, Lines 7-9: This sentence reads awkwardly. Please rephrase.

Response: The sentence has been rephrased: “In recent years there have been increasing efforts to quantify the parametric and predictive uncertainty of mass and energy balance models . . . “.

Manuscript Revision: Page 2812, Line 4: Replace “of the of” with “of”.

Response: Changed.

Manuscript Revision: Page 2813, Line 8: What do you mean by “according changes”? This does not make sense.

Response: The expression according changes has been changed to “The change of internal energy ...”

Manuscript Revision: Page 2813, Line 18: This is not grammatically correct. It should read “We refer to superimposed ice as ...”.

Response: Changed.

Manuscript Revision: Page 2813, Line 20: The comma after “showed” is not necessary.

Response: Changed.

Manuscript Revision: Page 2814, Lines 6-7: This sentence is not grammatically cor-

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rect. Please rephrase.

Response: The sentence now reads as: “This prevents percolation of water into the glacier ice and increases the refreezing potential at the snow-ice interface.”

Manuscript Revision: Page 2814, Line 17-18: Based on the inset map of Figure 1, this statement (“located in north-eastern Svalbard”) does not appear to be correct. Please correct.

Response: “north-eastern” was changed to “north-western”.

Manuscript Revision: Page 2814, Line 20: Based on Figure 1 (which shows the glacier flowing northwest), this description (“flows north-eastwards”) does not appear to be correct. Please correct.

Response: Changed.

Manuscript Revision: Page 2815, Line 5: Reverse the order here (“also showed”) to make this sound less awkward.

Response: Changed.

Manuscript Revision: Page 2815, Line 9: “Based to the distance” does not make grammatical sense

Response: Changed to: “Then the missing value has been replaced by randomly drawing on out the 20 most analogous days.”

Manuscript Revision: Page 2815, Line 11: It should be “physically” instead of “physical”.

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Response: Changed.

Manuscript Revision: Page 2816, Line 9: It should be “arbitrarily” instead of “arbitrary”.

Response: Changed.

Manuscript Revision: Page 2816, Line 17: Spelling error: it should be “sensor” instead of “senor”.

Response: Changed.

Manuscript Revision: Page 2817, Line 8: Please define “SD” for clarity.

Response: SD changed to “standard deviation”.

Manuscript Revision: Page 2817, Line 20: Remove the comma after “that”.

Response: Changed.

Manuscript Revision: Page 2817, Line 23: Add “the” before “following” and add a colon after “expression”.

Response: Changed.

Manuscript Revision: Page 2819, Line 24: Add “to” after “amounts”.

Response: Changed.

Manuscript Revision: Page 2820, Line 3: It should read “associated with” not “asso-
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ciated to”.

Response: Changed.

Manuscript Revision: Page 2820, Line 7: It should be “measurement shortcomings” instead of “measurements shortcomings”.

Response: Changed.

Manuscript Revision: Page 2820, Line 9: Why is Figure 5 reference before Figure 4? Consider renaming the figures to reflect the order in which they are introduced, or rewrite the text here to introduce the albedo figure before the density figure.

Response: The order of the figures has been changed.

Manuscript Revision: Page 2820, Line 16: “Following we indicate” does not make grammatical sense. Please revise.

Response: The sentence was removed.

Manuscript Revision: Page 2820, Lines 16-29 (and elsewhere): In all cases where the authors report energy fluxes, they omit the negative sign in the meters squared term. It should read “W m⁻²” but they consistently report it as “W m²”.

Response: Changed.

Manuscript Revision: Page 2820, Line 29: Reporting the mean annual energy surplus to the hundredths place is probably not warranted or useful.

Response: Changed.

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Manuscript Revision: Page 2821, Line 1: Missing a closing parentheses “)” after “Fig. 1”.

Response: Changed.

Manuscript Revision: Page 2821, Line 3: Add “for” before “considering”.

Response: Changed.

Manuscript Revision: Page 2821, Line 10: Add “the” before “case”

Response: Changed.

Manuscript Revision: Page 2822, Line 6: Add a comma after “simulation period”.

Response: Changed.

Manuscript Revision: Page 2822, Line 7: It should be “reaches” instead of “reach”.

Response: Changed.

Manuscript Revision: Page 2822, Line 15: Replace “are decisive” with “control”.

Response: Changed.

Manuscript Revision: Page 2822, Line 19: Remove the comma after “mind”.

Response: Changed.

Manuscript Revision: Page 2824, Line 1: This should read something like “to remind

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the reader” or “to note”. Also, the comma after “remind” is unnecessary.

Response: Changed.

Manuscript Revision: Page 2824, Line 15: Replace “proof” with “prove”.

Response: Changed.

Manuscript Revision: Page 2825, Line 4: Delete the comma after “showed”.

Response: Changed.

Manuscript Revision: Page 2826, Line 23: Either say “a negative feedback” or just “negative feedbacks” (no “a”).

Response: Changed to “negative feedbacks”.

Manuscript Revision: Page 2828, Line 18: It should read “scientists” (plural) instead of “scientist”.

Response: Changed.

Manuscript Revision: Page 2829, Line 1: Replace “proofed” with “proved”.

Response: Changed.

Manuscript Revision: Page 2829, Line 20: Why is humidity labeled as RH elsewhere but here it is Q

Response: Label has been changed to RH.

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Table and Figure Comments

Manuscript Revision: Table 2: For the units of PVOL, are you sure there should be a “%”? Because it is a fraction and because this value is usually around 5%, I think the uncertainty range should just read 0.03-0.05. Please confirm.

Response: That’s right. The % has been deleted.

Manuscript Revision: Figure 2: Due to the large number of points in the scatterplot, it is difficult to understand the distribution of points. Consider using a scatterplot with a density color scheme.

Response: The scatterplot uses a density color scheme. The original figure has a high resolution and should be easy to read.

Manuscript Revision: Figure 3: This figure would be improved by also plotting markers on each line that show the measurement and model nodes. This can be inferred from changes in the slope of the temperature profile, but it is not clear in all cases where the nodes are in the case of more subtle variations in slope.

Response: Markers have been added for both lines.

Manuscript Revision: Figure 3: Snow temperature is described in negative Kelvin, which is physically impossible. I think the label should either be in degrees centigrade or the numbers on the x-axis should change to be in Kelvin.

Response: The axis has been changed to degrees centigrade.

Manuscript Revision: Figure 6: It is difficult to read the text in this figure and it is not clear how well this will display in the final GMD paper format. Is it possible to change

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the aspect ratio and/or resolution and/or font sizes of the figure?

Response: We have improved the figure.

Manuscript Revision: Figure 7: Is Q supposed to represent RH in this plot? Is Q now a different humidity metric (such as absolute or specific humidity)? If this is still relative humidity, then it is best to remain consistent with the acronym usage and just keep RH. Please clarify.

Response: The acronym Q has been changed to RH.

Manuscript Revision: Figure 7: Because they are all the same, you could safely remove the legends from three of the four panels and just leave one.

Response: The legends have been removed from three of the four panels.

Manuscript Revision: Figure 8: Please specify at what temporal frequency (e.g., daily?) these sensitivity indices are calculated for snow depth.

Response: Caption changed to: “Evolution of the 6-hourly first-order ...”.

Manuscript Revision: Figure 8 caption: Table 1 does not include the indicated uncertainty factors. Do you mean Table 2?

Response: Changed to Table 2.

Interactive comment on Geosci. Model Dev. Discuss., 8, 2807, 2015.

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