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Geoscientific Model Development Discussions
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Interactive comment on "Assessment of the uncertainty of snowpack simulations based on variance decomposition" *by* T. Sauter and F. Obleitner

M. Raleigh

raleigh@ucar.edu

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Sauter and Obleitner present a study where uncertainties in meteorological forcings (due to specified measurement sensor accuracy) and two model parameters are concurrently propagated through the CROCUS snow model with a Monte Carlo style uncertainty analysis during 2010-2011 at the Kongsvegen glacier in Svalbard. They conduct a variance decomposition of four model outputs (snow depth, surface energy balance, sensible heat flux, and latent heat flux) to determine the relative contribution of the input uncertainties to the output spread via both first-order (direct effects) and total-order (effects that account for both first-order and interactions) sensitivity indices.

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Interactive Discussion

Discussion Paper

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 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.

GENERAL COMMENTS

1. 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

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our independent experiments in your discussion section.

2. 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.

3. 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.

4. 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.

5. 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?

SPECIFIC COMMENTS

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

- 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.

- 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?

- 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.

- Page 2815, Line 18 (equation 4): Please provide a citation for the new snow density equation. What study does CROCUS cite for this equation?

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

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

- 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.

- Page 2820, Line 4: Can you clarify what you mean by “diverse model uncertainties”?

- 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.

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

- 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 space.

- 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.

- 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?

- 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.

TECHNICAL CORRECTIONS

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- Page 2808, Line 15: Replace “there” with “their”.

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

- 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.”

- Page 2810, Line 7: Replace “have been” with “has been”.

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

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

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

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

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

- Page 2814, Lines 6-7: This sentence is not grammatically correct. Please rephrase.

- 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.

- 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.

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

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

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

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

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

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

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

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

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

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

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

- 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.

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

- 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 m^2”.

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

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

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

- Page 2821, Line 10: Add “the” before “case”.

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

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

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

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

- Page 2824, Line 1: This should read something like “to remind the reader” or “to note”. Also, the comma after “remind” is unnecessary.

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

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

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

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

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

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

TABLE AND FIGURE COMMENTS

- 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.

- 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.

- 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

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the case of more subtle variations in slope.

- 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.

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

- 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.

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

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

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

CITATIONS

Raleigh, M. S., J. D. Lundquist, and M. P. Clark (2014), Exploring the impact of forcing error characteristics on physically based snow simulations within a global sensitivity analysis framework, Hydrol. Earth Syst. Sci. Discuss., 11(12), 13745–13795, doi:10.5194/hessd-11-13745-2014.

Saltelli, A., P. Annoni, I. Azzini, F. Campolongo, M. Ratto, and S. Tarantola, 2010: Variance based sensitivity analysis of model output. Design and estimator for the total sensitivity index. Comput. Phys. Commun., 181, 259–270, doi:10.1016/j.cpc.2009.09.018.

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