Geosci. Model Dev. Discuss., 8, C1583–C1592, 2015
www.geosci-model-dev-discuss.net/8/C1583/2015/

Geoscientific Model Development Discussions
Open Access

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

T. Sauter and F. Obleitner

tobias.sauter@geo.rwth-aachen.de

Received and published: 3 August 2015

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

Comment: This paper tackles an interesting topic that is seldom properly covered and is based on a robust methodology. This paper has the potential to greatly benefit the community once its shortcomings would be addressed. Such shortcoming include the following points:

Thank you very much for reviewing this paper and the helpful comments.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

Comment: The reference scenario should be properly shown: very few information are given. A graph giving an overview of the forcing together with the temporal evolution of the snow height could really help the reader to make up his mind about this scenario. A graph showing the evolution of the energy balance components could also prove very useful when linked with the impact of the uncertainty on various parameters.

Response: The reference runs of KNG1 and KNG8 are shown in Figure 3 together with the confidence intervals. We added a new Table 3 with the mean and standard deviation of the meteorological variables and energy balance components for the two stations (KNG8 and KNG1) considered. We believe the table is informative than a graph showing the temporal evolution of the components.

Comment: The graphs showing the uncertainty provide some kind of a worst case scenario (multiple parameters combining their worst case values). This is very interesting but also very surprising at first because the amplitudes of the effects of such uncertainties are beyond common expectations and experience. I even set up a similar simulation with the SNOWPACK model in order to check the numbers because this seemed so surprising compared to regular simulations (and finally SNOWPACK shows very similar results to the CROCUS results shown here). To my understanding, even simulations with very poor datasets tend to fare better than the worst case combinations as presented here because some errors compensate each other (for example the Undercatch would be compensated by Incoming Long Wave parametrizations leaning toward clear skies). I think the surprisingly large amplitude of the uncertainty of the results should be better explained/demonstrated and potentially compared to real life data sets. For example, a graph showing the min/max/avg snow height development when only one parameter is changed; or showing how a few low quality datasets would compare to the findings presented here (although this would involve quite some work and would be based on other locations where both low quality and high quality data are available). An alternative approach would be to synthetically generate degraded

GMDD

8, C1583–C1592, 2015

parameters out of your data set mimicking the data quality issues of real, low quality sites. If these suggestions are impractical, in any case the authors should consider how they could bridge the gap between the common perception of model users (even when dealing with low quality data sets) and their findings

Response: The uncertainty estimation is based on the uncertainty given by the manufacture's specified accuracy. 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 compensating effect is still present since the data is only systematically perturbed by small biases. In fact, a synthetically generated dataset with the same characteristics as the measurements could be used for the analysis instead. We have carefully reworked the text and point out in the introduction and the discussion sections that the study focuses on systematic biases in the forcing data, e.g " . . . identifies how systematic measurement errors (biases) and uncertainties of some critical factors influence our confidence in glacier mass balance simulations." (p4L15-p4L17). We analyse in detail the interaction of variables in the Section 4 and relate the sensitivity pattern to physical processes (p22L19-p22L20).

Comment: The authors did not mention if (or how much) the CROCUS team was involved. Since the new snow density was tweaked to better fit the results, one is left to wonder if there was any discussion with the CROCUS authors on this topic (although this matches a similar value for Arctic conditions in the SNOWPACK model).

Response: We are closely cooperating with the CROCUS developer team and discussed the modifications. In particular, the modification of the water flow/refreezing module and the snow density calculations are the results of the close collaboration. We have mentioned the collaboration in the acknowledgements.

Comment: The authors emphasize the effects of the interactions although these only

GMDD
8, C1583–C1592, 2015

represent 7% of the ensemble spread... Doesn't this mean that first-order, linear effects are by far dominants (pages 2824 and 2828)?

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."

Comment: The figure 8 is very interesting and therefore should be better explained and emphasized in the text, the last paragraph of section 3.3 should be expanded.

Response: In contrast to the old version of this paper, the Figures 5 and 6 show now the total-order effects and not the first-order effects anymore. Since the GSA permits to recover the complete variance and not only first-order effect, we believe these indices are of more interest. At the end of Section 2.4 a new paragraph has been added describing in more detail how the indices and confidence intervals are estimated (p14L15-p15L9). Additionally, we have added a new Section 3.4 "Temporal evolution of the total-order sensitivity indices" describing the temporal variability of the total-order indices shown in Figure 5 and 6.

Comment: I would suggest writing equations (1), (2) and (3) in a more consistent way,

GMDD

8, C1583–C1592, 2015

making sure all parameters are described properly and maybe considering basing them on a positive energy change instead of negative.

Response: We have reworked the entire chapter and believe it is more consistent now.

One single year of validation data is a little short. Would it be possible to expand the reference period?

A longer period would certainly be of benefit. Unfortunately, reliable data is not available for a longer period. As an alternative we used the data from a second station, KNG1, located in the ablation zone and discuss the differences (see Reviewer 1). However, a one year period is sufficient to estimate the sensitivity pattern of the snowpack model.

Detailed comments: most of these comments relate to sentences that are not very well written and should be rephrased in a more natural manner.

Comment: the title should be improved to mention the Arctic conditions

Response: We have changed the title to “Assessing the uncertainty of glacier mass balance simulations in the European Arctic based on variance decomposition”.

Manuscript Revision: page 2809, rephrase lines 12 ("through to detailed"), 13 ("Besides all advantages"), 19 , 26-27, 29 (replace "apportioned" by "distributed" or something similar)

Response: We have changed “through to detailed” by “to detailed”, “Besides all advantages” to “Due to the increasing complexity of detailed models ...”, and replaced “apportioned” by “assigned”.

Manuscript Revision: page 2810, rephrase line 5

GMDD

8, C1583–C1592, 2015

Response: The phrase now reads as “To achieve a full understanding of the sensitivity pattern of highly interconnected and nonlinear models, ...”.

Manuscript Revision: page 2813, rephrase lines 5 ("measured input except of outgoing infrared") and 13-15

Response: We have deleted this passage, since the input data is specified in the subsection “Model input/output”.

Manuscript Revision: page 2814, the glacier flows north-westwards!

Response: The passage has been changed to “... the glacier flows towards the north-west coast of the archipelago.”.

Manuscript Revision: page 2815, rephrase line 4-5

Response: The text has been changed to “When the surrounding stations had missing values, the values were estimated by a stochastic nearest-neighbour resampling conditioned on the remaining variables (Beersma and Buishand, 2003).” (p9L13-ppL15).

Manuscript Revision: page 2816, line 16, remove the word "changes"

Response: The word “changes” has been removed.

Manuscript Revision: page 2819, the same things are said twice in the same paragraph

Response: The paper has been carefully restructured as proposed by Raleigh. The first paragraph of the Section “Reference run” and “Uncertainty estimation” have been moved to the methods section. A new chapter “Reference run setup” has been added

GMDD

8, C1583–C1592, 2015

describing the initial and boundary conditions of the reference run. We believe the paper is better structured now and makes a clear distinction between methods and results.

Manuscript Revision: page 2819, replace "starts at ..." by "starts on ..." and similarly for "ends at . . ."

Response: The passages now read as: “At both sites, the simulations start at the end of the ab- lation season, with the lowest recorded snow depth (defined by the minimum recorded surface height), and they are forced by hourly measurements.” (p11L20-p11L22).

Manuscript Revision: page 2819, line 20, consider specifying that the measurements are hourly?

Response: The text reads now as “The Crocus model is forced by air temperature (T), relative humidity (RH), wind speed (U), incoming shortwave radiation (SW), incoming longwave radiation (LW), precipitation rate (P) and atmospheric pressure (see Sect. 2.2). These time-dependent parameters were measured at both sites and are provided to the model by Netcdf-file for hourly time steps.” (p8L17-p8L24).

Manuscript Revision: page 2819, line 21 "these data", which ones?

Response: We have deleted this sentence.

Manuscript Revision: page 2819, rephrase lines 24-25

Response: The sentence has been changed to “In terms of water equivalent, the accumulated mass during the winter amounts to +0.76 m, compared to +0.82 m having been observed.” (p16L18-p16L19).

GMDD

8, C1583–C1592, 2015

Manuscript Revision: page 2821, line 1, a ")" is missing

Response: The “)” has been added.

Manuscript Revision: page 2821, rephrase line 4

Response: The passage was rephrased to “The overall impact of individual error sources on the sensitivity pattern varies for different zones on the glacier.” (p29L17-p29L19).

Manuscript Revision: page 2821, rephrase line 25

Response: We have rewritten this chapter and the sentence does not exist anymore.

Manuscript Revision: page 2822, rephrase lines 15-16, 19-20 and fix spelling on line 25

Response: We have rewritten this chapter and the sentence does not exist anymore.

Manuscript Revision: page 2824, line 15 replace "proof" by "prove"

Response: Has been changed accordingly.

Manuscript Revision: page 2825, rephrase line 26

Response: We have rewritten this chapter and the sentence does not exist anymore.

Manuscript Revision: page 2826, rephrase lines 7-8

GMDD

8, C1583–C1592, 2015

Response: The lines has been changed to “Hence, the sensitivity of net 5 shortwave radiation (∂G) to measurement errors (∂ESW) is given by $\partial G/\partial ESW = 1 - \alpha$, with α denoting albedo.” (p28L4-p28L6).

Manuscript Revision: page 2828, line 20 replace "firstly" by "first" and line 21 "secondly" by "second

Response: Have been changed accordingly.

Manuscript Revision: page 2829, line 1 replace "proofed" by "proved" and line 2 replace "provides" by "provided". Remove "by" on line 3

Response: Have been changed.

Manuscript Revision: page 2829, rephrase lines 4, 7, 10, 22 and 25-26

Response: We have rewritten this chapter and the sentence does not exist anymore.

Manuscript Revision: page 2829, line 13 "considerably" and "This lower proportion"

Response: We have rewritten this chapter and the sentence does not exist anymore.

Manuscript Revision: page 2829, please define Q on line 20

Response: Q has been changed to RH throughout the text and is defined in Section 2.1 (p8L18).

Manuscript Revision: page 2830, rephrase line 3-4

Response: Changed.

GMDD

8, C1583–C1592, 2015

Manuscript Revision: page 2838, fig. 1, rephrase "Map demonstrating"

Response: Changed to: “A map showing the location of the Kongsvegen glacier and the position of the automatic weather stations KNG8, KNG6 and KNG1 (Norwegian Polar Institute, 2014).” (p40).

Manuscript Revision: page 2841, fig 4, "snow albedo at the KNG8 location"

Response: Changed.

Manuscript Revision: page 2842, fig 5, idem

Response: The caption now reads as: “Spread of the ensemble simulation at KNG8 (upper panel) and KNG1 (lower panel) due to propagating uncertainties in the model inputs. The black lines represent the reference run. The intervals show the 99, 95 and 75

Manuscript Revision: page 2844, fig 7, please define the parameters. I also don’t find this graph very clear, improvements would be welcomed

Response: The figure has been replaced by a new figure. We hope the figure is more clear now.

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