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Interactive Comment

Interactive comment on "Simulation of the specific surface area of snow using a one-dimensional physical snowpack model: implementation and evaluation for subarctic snow in Alaska" by H. W. Jacobi et al.

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We appreciate the positive and constructive comments from Eric Brun and Pierre Etchevers. We also appreciate the comments from Michael Lehning although we disagree with his statement that the submitted manuscript does not deserve publication. We provide further arguments supporting our case. In addition, the reviewers Eric Brun and Pierre Etchevers seem to share our point of view. Below is our detailed response to the review comments. Please note that there was a typo on page 695, line 16 regarding the simulated melting of the snowpack. The snowpack disappears between 5





and 13 April, not March. This will be changed in the revised manuscript.

General comment reviewer Eric Brun:

The paper is very clear with relevant references and very comprehensive information on the experimental conditions and on the observations which have been collected to assess the performance of the simulated SSA. No doubt that this paper affords to be published in the Cryosphere Discussions.

RESPONSE We appreciate this very positive feedback on our study.

Specific comments reviewer Eric Brun:

SSA calculations mainly refer to dry snow conditions, which is reasonable regarding the test site located in continental Alaska. However, a short mention of SSA dynamics under wet conditions would reinforce the perspective for future simulation of SSA under any climate conditions. This could be easily done in the in §2.4 or in §4.

RESPONSE We agree with this statement and will amend the conclusions accordingly. SSA observations in a wet and / or melting snowpack are currently not available. Therefore, it is not clear how well the SSA development is represented by the applied equations, which have been obtained for a dry snowpack.

I do not understand line 18 page 699 which is probably due to a typing mistake.

RESPONSE Yes, there is an error in this line that slipped through the proof-reading process.

General comments reviewer Michael Lehning:

Altogether, calculating SSA with a snow cover model appears to be a useful thing for snow chemistry and other applications and also advancing some of the existing snow physics models is certainly welcome in the community. In particular, seeing revived activities with the CROCUS model is to be supported and the paper is well written.

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RESPONSE We appreciate this positive feedback on our study.

However, the advancement of science as expressed in the current paper is not yet visible. The implementation of a SSA calculation as a secondary parameter is a minor effort and does not warrant publication per se. Note also that all the data used here have already been published earlier. An interesting alternative to the approach taken here would be to replace one or more of the primary CROCUS parameters (dendricity, sphericity, grain size) with SSA and to formulate a snow model based on SSA, which would be more of an effort but also a more scientific approach. A main part of the paper then compares observed SSA with the simulated ones and basically states that there are still significant deviations including systematic errors in the SSA profile using the prognostic equation and unrealistic discontinuities and offsets of SSA using the diagnostic equation. The sensitivity runs used to try to explain the deviations remain inconclusive and other than an initial tuning of ground heat flux to better match the observed snow temperature development, no further efforts to improve the simulations is made. My impression is therefore that a first version of a tool has been created, which still needs to be improved and then applied to make some real science with it. A major general point is the cumulative uncertainty of the simulated functions. As the parametrizations have a large uncertainty, the prognostic SSA calculations have probably large error bars. Estimates of such errors should be provided, as this is essential to judge the guality of the proposed simulation. I therefore recommend rejection of the paper in its current form. To be more specific, I like the idea of the paper but I think that the authors could do this in a much better way.

RESPONSE We agree that there are two different possibilities for the further use of a parameter like the SSA in snowpack modeling. One option concerns the representation of chemical processes in the snow (so-called secondary parameter by the reviewer), which can either directly (heterogeneous reactions, ad- and desorption) or indirectly (reactions in the QLL) be quantified using the SSA. In this respect, the SSA can be regarded as a parameter calculated by a physical snowpack model and used as input

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for a chemical snowpack model. That was the major motivation to include for example the calculation of the SAI (section 3.6) in the manuscript presenting the seasonal cycle of the total surface of the snowpack available for physical exchange or chemical reactions. The second application of the SSA can be for an improved parameterization of the radiation transfer within the snowpack including the simulation of the albedo. The basis for this potential use of the SSA is discussed in the introduction of the manuscript. In this case, the calculation of the SSA would constitute a new parameter for snowpack modeling, which in turn provides feedbacks for the simulation of the thermal budget, the metamorphism, and the melting of the snowpack. In this respect, we believe that the SSA has a great potential in future snowpack modeling. Nevertheless, we are also convinced that a mere substitution of previous by new parameters is not useful. In our opinion, the available parameterization of the SSA must be carefully validated as presented in the manuscript. The SSA is still a parameter that has been observed at very few locations and on a very limited number of samples. To our knowledge, the observations used in the manuscript for the validation constitute so far the most comprehensive data set covering the full cycle of a seasonal snowpack. Therefore, it was obvious to base the first validation on this data set. In contrast, the model CROCUS has not been applied to an subarctic snowpack before. As a result, the manuscript presents a great deal of information including (1) a comprehensive data set for a subarctic snowpack, (2) a full range of simulation using the CROCUS model for subarctic conditions, (3) an extensive comparison of the observed thermal budget and snowpack heights with simulated results allowing the verification of the performance of CROCUS for a subarctic snowpack, and (4) a comprehensive validation of the SSA parameterization in the snowpack model. Additional information concern a comparison of simulated and observed density profiles, the formation of depth hoar, and the calculation of the SAI. In summary, we believe that such a validation of a newly implemented parameter is needed before it can be utilized for further calculations in a snowpack model. Our study presents existing deficiencies in the simulation of the SSA as well as sensitivities of the thermal budget of the snowpack model and of the simulation of the SSA.

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In our opinion, this constitutes the basis for further studies at this and other locations. We will further add the estimated errors of the SSA parameterizations. They are given by Domine et al. (2007b) for the diagnostic equations. The errors for the prognostic equations are estimated to be on the order of 20 %. This estimate is based on the comparison of predicted and observed SSA decrease as presented by Taillandier et al. (2007). Further errors introduced by deviations between observed and simulated snowpack heights, snow densities, snow age, and snow temperatures are not taken into account. The SSA errors will be presented in the Figures 6, and 7. We will waive the error bars in Figure 8 because they would make the figure illegible.

Additional major comments: p. 5: The temperature measurement string could also be used to produce SSA predictions based on measurements only. It would have been interesting to see how this compares to the simulated one. This may be the content of an earlier paper of Taillandier, which I did not read, though.

RESPONSE That is the case. Taillandier et al. (2007) have used data from field measurements at Fairbanks together with further field and laboratory experiments to obtain the applied relationship between SSA and snow age. Therefore, a prediction of the SSA based on the field measurements would be redundant.

p. 8: It is difficult to call CROCUS at the same time a model based on snow physics and a model that is "optimized" for warm and deep Alpine snow packs. Other than the boundary conditions, which are then adapted in the following, all snow physics models have been shown not to be particularly sensitive to different sites in the SNOWMIP project and CROCUS is known to nicely produce facets e.g. at Col du Lac Blanc.

RESPONSE We agree that this a confusing statement. The reviewer refers to the sentence saying that CROCUS "can be considered as an optimized tool for the simulation of a warm and deep snowpack...". This is misleading. We rather wanted to express that CROCUS has been shown to be an excellent tool for the snowpack in the Alps. We will change this sentence accordingly. Nevertheless, we like to note that different

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processes take place in different snowpack types. For example, water vapor redistribution takes place in snowpack types with strong temperature gradients, while it does not take place in a maritime or alpine snowpack. Convection takes place exclusively in taiga snow. Wind-pumping does not occur in taiga snow because of low winds and the near absence of surface structures. Therefore, CROCUS can very well be a physical snow model describing processes prevalent in alpine snow and neglecting processes unimportant in alpine snow. Even though if the neglected processes can be common in other snowpack types.

Eqs. (4) to (8): Using the available CROCUS microstructure parameters, it would have been easily possible to formulate a continuous diagnostic model of snow SSA, which avoids the problem of discontinuous changes discussed further below.

RESPONSE Field observations have demonstrated that the metamorphism of the snow leads to snow type sequences like fresh snow - recognizable particles - aged rounded grains or recognizable particles – aged faceted crystals – depth hoar. However, sharp boundaries between the different snow types do not exist in the real snowpack. The real transitions are rather diffuse. Therefore, implementing snow type classifications may be helpful to characterize a snowpack. However, in a snowpack model they necessarily lead to discontinuities in the simulations. In the case of the diagnostic SSA simulations, the smoothing of such discontinuities would require threshold densities to be imposed, which are arbitrary and sometimes in conflict with other model outputs that predict snow type independently from density. To illustrate, Fig. 1 (see below) represents the relationship between the SSA and snow density according to the equations (4) to (8) used in the manuscript. It shows a very limited number of intersections between the different curves, which could be used for a smooth transition between the snow types. For example, the transition from fresh snow to recognizable particles at a given density always involves a jump in the SSA. From a pure mathematical point of view, the sequence from recognizable particles to aged faceted crystals to depth hoar follows such a smooth transition. However, in that case the transitions

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between recognizable particles to aged faceted crystals and aged faceted crystals to depth hoar would have been fixed at snow densities of around 0.06 g cm-3 and 0.21 g cm-3, which both are unrealistic and arbitrary.

Eqs. (9) and (10): The formulation of the equations does not match the discussion in the text. From the text, a rate equation is expected and Eqs. (9) and (10) could easily be transformed to rate equations without using the discretized form presented in Eq. (11). Also, in my opinion, a unified rate equation which formulates SSA changes as a function of the temperature gradient and the temperature and not of the temperature at one time point alone would have made much more sense anyway. The equations given (even if they are already published) do not make sense because the SSA at a certain time should not depend on the actual temperature of this time but on the full temperature history or the history of the temperature gradient. This would easily be achieved by writing a real rate equation: d(SSA)/dt = f(T, dT/dz, t).

RESPONSE We believe that this comment is largely based on a misunderstanding of the basis of the equations and also of the physical processes taking place in a snow-pack. It has been well know for decades that there are two different regimes for dry snow metamorphism: ET and TG. This dates back to Sommerfeld and LaChapelle (1970) and has not been questioned. In fact, it formed the basis for subsequent quantitative treatment of snow metamorphism (e.g. Colbeck, 1983). The processes taking place under both regimes are different and it is not useful to suggest a unified rate equation. What might make sense is to provide an equation for the transition regime between ET and TG. Unfortunately, there is currently no basis to develop it, since no data exist in this region. As discussed in Taillandier et al. (2007), equations (9) and (10) make sense, because they do integrate the full temperature history of the snow layer and they are based on the average temperature experienced by the layer. Therefore, we conclude that there is currently no basis for writing an equation of the form d(SSA)/dt = f(T, dT/dz, t), and that, to the best of our understanding and knowledge, the equations we applied are the best that can be proposed with the data available

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today.

p. 13: The situation at the base of the snow cover is always much simpler to describe since only one heat flux (from the ground via conduction, in this case also supported by latent heat of a freezing soil) is really playing a role there. In addition, this flux has been fitted to match the observations and it is therefore clear that a good agreement of temperatures must result.

RESPONSE We noted this point in the manuscript, but will stress it further in section 3.2.

p. 13: The paragraph talks about "turbulent exchange" within the snowpack. It is easily shown with a simple Reynolds number calculation that any possible flow within a snow matrix caused by natural convection or atmospheric forcing will be laminar. Ventilation has in addition recently been shown to be only important very close to the surface (Clifton et al., 2008), at least as long as there is a rather flat snow surface. Therefore, instead of looking for air flow in the snow, I would expect here a discussion on the turbulent heat fluxes at the surface, especially with respect to assumed atmospheric stability. It is known that a Richardson number approach to estimate atmospheric stability e.g. results in too small turbulent fluxes in case of nominally very stable conditions. In other conditions the surface flux may be overestimated. Therefore, a sensitivity study with a simple increase or decrease of the fluxes over the full time range is insufficient.

RESPONSE The difficulties of calculating turbulent fluxes at the snow surface for stable conditions using the Richardson number is discussed in the first paragraph of section 3.3. It is also well known that no simple solution exists for a better parameterization of the fluxes under these conditions. This point is further discussed in the final paragraph of section 3.3 and in the second paragraph of our conclusions (section 4). However, as already discussed there additional measurements like observations of the turbulent fluxes are necessary to possibly obtain a better parameterization. Since such measurements have not been performed, developing a better description of the turbulent

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fluxes is beyond the scope of this manuscript. Nevertheless, the runs with changed turbulent fluxes indicate the model sensitivity demonstrating that for the temperature in the top layers of the snowpack the turbulent fluxes are crucial parameters. The reviewer is further referring to the paragraph that concerns the discussion of the cooling event of the snowpack between 5 and 9 March. As shown in Figure 2, this event does not only concern the top layers, but also deeper layers in the snow. Therefore, such events (which in fact have been observed previously at Fairbanks, see Sturm, 1991) can not be explained by different turbulent fluxes at the snow surface alone. The paragraph also refers to the previous observations and the already discussed possible driving forces (Sturm, 1991). The full mechanism of such events is currently not know and cannot be included in the snowpack model as concluded at the end of the same paragraph.

p. 15: I agree with the implicit assumption of the authors that the thermal conductivity in CROCUS is probably too small for the snow simulated in this application, although they state that CROCUS is already producing high values compared to a compilation of measurements by Sturm. Instead of worrying too much about matching published thermal conductivity values, it would have been good to investigate this in a more rigorous way by using the time series of measured temperatures available. You could estimate effective thermal conductivities simply from there. SSA or "appearance of depth hoar" are no good indicators as they are secondary and parameterized quantities.

RESPONSE Why should we expect to get much different thermal conductivities at Fairbanks than what has been previously compiled by Sturm et al. (1997) including many data points also obtained at Fairbanks? In fact, we have done some preliminary calculations of the conductivities giving the same result: The conductivity-density relationship used in CROCUS delivers values that are somewhat too high. We fully agree that the SSA or depth hoar cannot be used to evaluate the simulated thermal conductivities. However, we could not find a sentence or a phrase in the manuscript that could imply such a statement.

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Minor comments: p. 22: At this point, formulating radiation as a function of the CRO-CUS grain size is probably still better than using this parameterized SSA.

RESPONSE This remains to be examined and is not subject of this manuscript. Nevertheless, radiative transfer models in snow use optical grain size, which is related to SSA and not a grain size simulated with CROCUS. For example, Taillandier et al. (2007, their figure 1) show that using grain size rather than SSA leads to large errors.

p 683 line 13: Besides, the SSA can be measured by stereology (Arnaud, 1998) or, as shown by Kerbrat et al (2007) by tomography without any bias.

RESPONSE We will add these two references believing that Michael Lehning is referring to Kerbrat et al. (2008).

p 683, line 24: this relation was not given by Warren and Wiscombe, but by Grenfell and Warren 1999

RESPONSE This sentence gave the wrong impression, that the equation (1) was presented by Warren and Wiscombe (1980a). We will amend the text accordingly.

p 692, line 25 "a low (<9 K m-1) or a strong (>20 K m-1)" ... and which parametrization is used between 9 20 K m-1 ?

RESPONSE As described in the last but one paragraph of section 2.4, we used a threshold of 15 K m-1 to apply either equation (9) or (10).

General comment reviewer Pierre Etchevers:

This work appears me to be well structured and innovator. Indeed the SSA is a parameter which is now more and more frequently measured in the field with a sufficient accuracy and numerous methods are available to get large data sets. Moreover, it presents a real interest for snow models as a quantifiable variable which plays a major role in different physical and chemical snow processes. The paper is a first step to introduce the SSA in a sophisticated snow model. The methodology is clearly ex-

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plained and a particular attention is paid to the model results analysis (advantages and limirts). The references appear to be pertinent and the figures are convincing. The work presented in the paper is very valuable and offer three main outlooks : 1) the Crocus results analysis points out the physical processes which are dominant in a subartic snowpack and that should be improved in the model for this kind of snowpack, 2) the SSA simulations are very encouraging and the method needs to be extended to other snowpack types, based on SSA data bases which are actually or should be soon built. 3) The next step will be to use the SSA in a complete loop to modify snowpack characteristics (e. g. albedo), i. e. to simulate a complete feed-back between SSA and the other snow physical variables. Given the very good quality of this paper and its innovator results, I strongly support its publication in The Cryosphere.

RESPONSE We appreciate this very positive feedback on our study.

Specific comments reviewer Pierre Etchevers:

Page 70, line 6 and figure 5 : how is estimated the SSA for melting period? Is there a specific parametrization when the snow contains liquid water? This point should be précised in the paragraph 2.4.

RESPONSE See response above regarding a similar comment by Eric Brun.

Page 702, line 24 and following : this disadvantage seems a bit artificial, because it is due to the choice of a correspondence criteria between the snow type and the simulated snow grain characteristics (paragraph 2.4). If this correspondence was "smoother" from one grain type to another, the SSA discontinuities would disappear, which would be physically consistent.

RESPONSE See reply and figure above regarding a similar comment by Michael Lehning.

Page 706, line 7-8 : authors accord a better value to the prognostic equations because of a better agreement of the results. Since these equations have partly been estab-

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lished in the snow field on the Fairbanks site, it is probably a part of the explanation of their good results. Could the equations easily be transposed to other snowpack types? This point should be discussed in the paper.

RESPONSE We agree that data from Fairbanks were used for the development of the equations (9) and (10) leading to some redundancy. However, 6 out of total of 21 experiments were made at Fairbanks. In the other experiments snow from the Alps was used without significant differences. The validity of the proposed equations is discussed in detail by Taillandier et al. (2007) indicating that they are supposed to be used only for low- to medium-density snow. We will add this point to the revised manuscript in sections 2.4 and 3.5. We further like to note that the diagnostic equations were also partly obtained with data from Fairbanks. In fact, the equation for taiga depth hoar was obtained only from Fairbanks data. In the conclusions (section 4) we will stress further that the diagnostic equations are less satisfactory because the snow density is not predicted well by CROCUS for taiga snow (see section 3.4). As a result, differences between observed and simulated SSA values are caused by errors in the density simulations in CROCUS combined with the errors of the used SSA-density relationships.

Figure 5 (and other coloured figures) : the colour scale is ambiguous : for instance dark blue corresponds to values between 0 and 100 cm2 g-1 ? Moreover all the colours do not appear in the scale, which is a bit tricky. This should be improved.

RESPONSE We will improve the Figures 1, 2, and 5 accordingly.

Figure 6: the uncertainty for SSA observations and simulations is lacking. For the model for instance, it could be estimated from the different sensitivity runs and from the uncertainty of the equations (4) to (10). For the observations, the uncertainty is probably discussed by the papers which provide them (Taillandier and Dominé). This would help the reader to interpret the differences between observations and the two ways of simulating SSA.

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RESPONSE See response to similar comment by Michael Lehning regarding error estimates and error bars.

Figure 9 : are there enough observed SSA data in each snow pit to accurately evaluate the snow area index? Indeed, when one considers observed data on figure 6, one has the feeling that the vertical sampling is relatively weak, especially close to the surface (where SSA values are the highest). Did the authors take into account this uncertainty source in the error bars calculation presented on figure 9?

RESPONSE The observed SAI values as well as the error bars were taken from Taillandier et al. (2006) obtained by summing the products of the surface area, the snow density, and the height of each layer. Further details of the calculation procedure can be found there. They estimated an average error of 16 % for the SAI. We will add error bars for all observed points to Figure 9.

Additional reference

Colbeck, S.C., Theory of metamorphism of dry snow, J.Geophys.Res. 88, 5475-5482, 1983.

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Fig. 1. Relationship between SSA and snow density for fresh snow, recognizable particles, aged, rounded crystals, aged, faceted crystals, and depth hoar calculated with the equations (4) to (8).

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